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INSTRUMENTAL COMMUNITY

PROBE MICROSCOPY AND
THE PATH TO NANOTECHNOLOGY

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CYRUS C. M. MODY

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Instrumental Community

Probe Microscopy and the Path to Nanotechnology

Cyrus C. M. Mody

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for my parents, C. M. S. and Janet Mody

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Preface and Acknowledgments

The idea for a book about how the atomic force microscope became a useful and ubiquitous tool first occurred to me in the summer of 1998, in the course of ethnographic research in a materials science laboratory. In that ethnography, I was looking for ways that experimental engineering knowledge overflows the formal, technical considerations that textbooks usually present as sufficient for solving engineering problems. As an engineering undergraduate, I had seen hints of this insufficiency, and it contributed to my sense that there was something about engineering practice that I just didn't get. The materials science graduate students I worked with that summer, especially Ryan DiSabella and Marty Murtagh, showed me more clearly the aspects of experimental knowledge-making that aren't adequately captured in textbooks. They have my deepest gratitude for the discussions that led, circuitously, to the idea for this book.

Carrying out that idea occupied the next several years as I worked toward my Ph.D. in Science and Technology Studies at Cornell University. The members of the Cornell STS department shaped this project more than I can possibly describe. Most influential on my outlook were the members of my thesis committee: Mike Lynch, Ron Kline, and Trevor Pinch. Early in my time at Cornell, Javier Lezaun, Shobita Parthasarathy, Dan Plafcan, Jessie Saul, and Jamey Wetmore made Ithaca a vibrant home base. Later, Christina Dunbar-Hester, David Kirby and Laura Gaither, Erin McLeary and Ian Petrie, and Kevin O'Neill were cheery fellow travelers on the slow march to the end.

Along the way, I met many of the people you will be introduced to in this book. Thanks to funding from the National Science Foundation (under Grant SES 0094582 and Cooperative Agreement 0531184), the National Bureau of Economic Research/Sloan Foundation Science and Engineering

Workforce Project, the American Institute of Physics Center for the History of Physics, the Chemical Heritage Foundation, the Lemelson Center for the Study of Invention and Innovation, and the Institute of Electrical and Electronics Engineers, I had the resources and time to interview more than 150 people in Europe and North America about their experiences with probe microscopy. This book reflects the generosity and interest of those funders and interviewees.

A few interviewees deserve some singling out for their continuing help with this project. Virgil Elings, Randy Feenstra, Jane Frommer, Christoph Gerber, Joe Griffith, Paul Hansma, Stuart Lindsay, Jim Murday, Jun Nogami, and Craig Prater all gave feedback through ongoing discussions or email exchanges—sometimes encouraging, sometimes rebuking, always helpful. Clearly, some of these people will disagree with my findings and interpretations, as will other interviewees. Still, I hope all those I interviewed will accept my thanks and respect.

My understanding of the probe microscopy community has benefited enormously from participation in the communities of history, sociology, and anthropology of science, technology, and engineering. My debts to colleagues in those fields cannot be covered by any list, but special thanks are due to Davis Baird, Ben Cohen, Maggie Dennis, Paul Forman, Peter Galison, Hans Glimell, Michael Gordin, David Kaiser, Sarah Kaplan, Alfred Nordmann, Sonali Shah, Steve Shapin, Susan Silbey, John Staudenmaier, and Chris Toumey.

After graduate school, I was a fellow and then, thanks to Arthur Dammrich, an employee at the Chemical Heritage Foundation in Philadelphia. Arthur's confidence that science policy and history of recent science are two sides of the same coin gave direction to my rudderless post-thesis thinking. Moving to Philadelphia also introduced me to a never-dull network of current and former CHFers, starting with my fellow fellows (Mike Egan, Matt Eisler, Gabriella Petrick) and continuing through a rowdily charming group of friends, beer drinkers, and/or Quizzo enthusiasts.

While at CHF, I joined a small group of historians interested in nanotechnology. I thank my CHF colleagues—David Brock, Hyungsub Choi, Christophe Lécuyer, and Jody Roberts—for participating, perhaps against their better judgment, in the effort to understand this emerging field. In addition, Arne Hessenbruch, Ann Johnson, Tim Lenoir, Patrick McCray, Mara Mills, Joe November, and Joanna Radin have carried the history of

nano (broadly conceived) forward into profoundly interesting places. This book has taken a long time to come to final form partly because I wanted to build on discussions with these colleagues. Much of this book was worked out in those conversations, especially with Ann, Hyungsub, Jody, and Patrick. That it has come together, at last, owes much to the staff at the MIT Press, especially Margy Avery, and to the series editors and the manuscript reviewers.

The last four years have seen two big changes in my life. After CHE, I moved to Houston and to Rice University. The members of the Rice History Department, as well as Kevin Kelly, Kristen Kulinowski, and Chris Kelty and Hannah Landecker (if only for a nanosecond) have been unfailingly supportive and friendly.

At the same time, I got to know, and eventually marry, Karen Burk. She, my parents, and my family-away-from-family, the Inkeleses, put things like mere books in perspective. I thank, and love, them for it.

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1 Introduction: Communities, Innovation, and Knowledge

At 11 a.m. on January 21, 2000, President Bill Clinton took to the stage of the California Institute of Technology's Beckman Auditorium to announce a National Nanotechnology Initiative for the United States.¹ This moment had been in preparation since the early 1990s, when a small group of federal grant officers, Clinton administration science policy makers, and elite scientists had sought government investment and coordination for a new, distinct science of the very small. Clinton's speech merely recognized nanotechnology as a scientific movement already well underway. Yet that act of recognition transformed an informal, sometimes maligned research area into a well-funded and dynamic new field.²

The symbolism of Clinton's speech was carefully considered beforehand. The site—Caltech—was chosen because, as Clinton acknowledged, it was the academic home of Richard Feynman, the Nobel laureate physicist who first proposed an embryonic version of nanotechnology in an after-dinner speech for the 1959 meeting of the American Physical Society in Pasadena.³ Forty years and a few weeks later, Clinton stood on stage next to David Baltimore (a Nobel laureate biochemist and Caltech's president) and Gordon Moore (co-founder of Intel and the originator of Moore's Law, the rule of thumb for the pace of miniaturization in the semiconductor industry). Clinton, Baltimore, and Moore represented the "triple helix" of American science—government, academia, and industry—that would oversee and benefit from realization of Feynman's vision.⁴ Behind them, providing the literal and metaphorical backdrop, was an image selected by the White House as a nod to the politics and the science of nanotechnology: a five-million-fold enlargement of a pointillist portrait of the Western Hemisphere made from tiny clusters of gold (see figure 1.1). The symbolism was potent: the United States, by launching its nanotechnology initiative,



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Figure 1.1

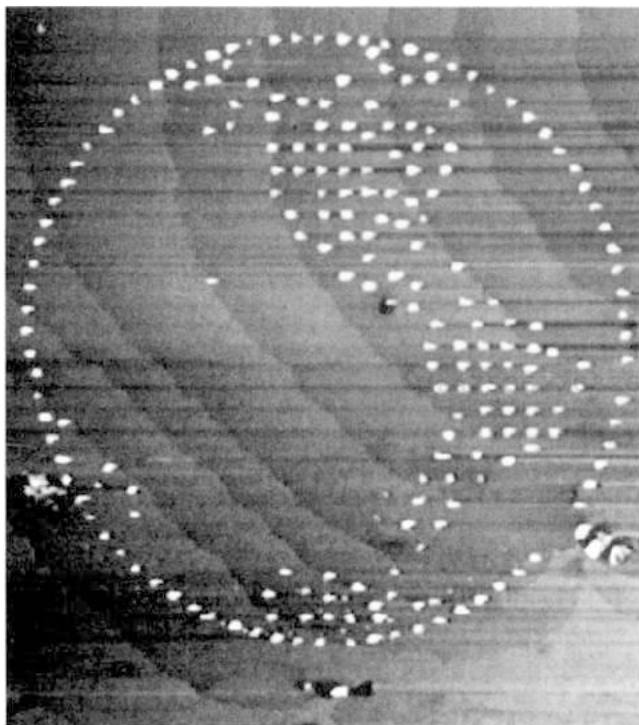
President Bill Clinton announcing the launching of the National Nanotechnology Initiative at Caltech, January 21, 2000.

would maintain its globally dominant economic and political position by way of its mastery of the science and technology needed to control even the smallest amounts of matter.

How much precision was needed to make this image? In the original gold depiction, the clusters representing New York and Los Angeles—in reality 4,000 kilometers apart—were only 200 nanometers (billionths of a meter) apart. Imagine an ordinary map of the United States, of the kind often found in elementary school classrooms.⁵ Now imagine that this map is a tiny version of the United States, with tiny cities, mountains, rivers, and people, and that there is a tiny version of you inhabiting the map. The tiny you is sitting in a miniature classroom, looking at a tiny map of the United States. The portion of the gold nanoclusters depiction representing the United States is that mini-map. Nanotechnology bears the same relationship to our ordinary sense of scale that a map bears to the continent it depicts.

The gold nano-map was constructed a decade before Clinton's speech by a group of IBM scientists using a scanning tunneling microscope (STM). They used the same STM both to cause a gold surface to form small clusters

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2000 Å

Figure 1.2

Image of gold clusters deposited and then imaged with a scanning tunneling microscope. Reprinted, with permission, from H. J. Mamin et al., "Gold Deposition from a Scanning Tunneling Microscope Tip," *Journal of Vacuum Science and Technology B* 9 (1991): 1398–1402 (copyright 1991 American Vacuum Society).

(which they assembled in the shape of the Western Hemisphere) and to image the clusters they created—the image that would later be adapted as Clinton’s backdrop.⁶ The remarkable control over small amounts of matter demonstrated in this image made the STM a potent symbol for nanotechnology proponents long before Clinton’s speech. Even before 2000, most of the thumbnail histories of nanotechnology found in policy documents, newspaper articles, and popular books presented the STM as the most significant tool of nanotech research.⁷ Many of the articles and reports promoting the National Nanotechnology Initiative in the wake of Clinton’s speech were filled with glossy STM images and claims about the STM’s potential.⁸ The White House chose an STM image as the backdrop for Clinton’s announcement because the STM was already the instrument most closely associated with nanotechnology.

Analysts of nanotechnology have also accorded the STM a special place in their studies of this new field. Philosophers of science celebrate its ability to see *and* manipulate single atoms—that is, its combining of the functions the philosopher and historian of science Ian Hacking aptly labeled “representing and intervening.”⁹ This, in turn, has led to extensive philosophical discussions of how images of inherently unseeable atoms should be interpreted.¹⁰ Likewise, economists have fixated on the STM (and its variants) as a “key enabling discovery for nanotechnology” that will unleash a Schumpeterian “gale of creative destruction.”¹¹ Sociologists and management scholars have generally concurred with the economists, claiming that nanotechnology research “became possible mainly through the invention of the Scanning Tunneling Microscope.”¹²

For both nanotechnology’s proponents and analysts, then, the invention of the STM set in motion the formation of a scientific field that, worldwide, absorbs nearly \$20 billion in dedicated funding each year.¹³ The output of that funding—the immediate economic value of nanotechnology research—has been estimated at nearly \$30 billion worldwide in annual sales of intermediate products that then find their way into almost 250 billion dollars’ worth of final products. If the STM really were responsible for all that, it would be a very special technology indeed, and the analysts’ and boosters’ praise would be apt.

The STM and its variants are certainly important tools, though I will argue that descriptions of the STM as the origin and enabler of nano are somewhat exaggerated. But even if that view is entirely accurate,

nanotechnology's proponents and analysts have not explained *how* these microscopes made this new field possible. The STM, after all, is an inanimate object. Its importance is a consequence of people using it. Presumably the nano proponents and analysts I have cited understand this and are simply invoking "the STM" as a synecdoche for an assemblage of microscopes and microscopists. Yet that shorthand fails to register the agency of microscopists in making the STM and its variants into economically and scientifically important tools.

The agency of microscopists matters because it wasn't self-evident that the STM would lead to anything, much less a new multi-billion-dollar area of research and industrial output. The initial results from the STM weren't very promising, and for a time it seemed to be on a path to obscurity (as had happened earlier to a similar instrument, the Topografiner). The STM's inventors needed to assemble a network of people who would be interested in their results, and eventually build their own STMs, for the instrument to amount to anything. That network, which I term an *instrumental community*, provided a basis for the creation, the evaluation, and the spread of innovations that made the STM a much more powerful technology than it otherwise would have been.

Of particular importance for the emergence of nanotechnology was the series of variants of the STM, including the atomic force microscope (AFM), the magnetic force microscope (MFM), and the near-field scanning optical microscope (NSOM), generated by that community. These variants, quite unexpectedly, have turned out to be at least as useful as the STM, and significantly more numerous. Collectively, they are known as scanning probe microscopes (SPMs), because most involve a small, solid probe that is rastered (i.e., "scanned") over a surface. Since not all variants of the STM raster a probe, and some are not even microscopes, these tools are also sometimes referred to as "local probe" or "proximal probe" techniques.

Sometimes shrewdly, sometimes fortuitously, probe microscopists organized their community in such a way that their instruments could move outward into a wide variety of disciplines and industries. They marshaled resources from various institutions, developed specialized areas of expertise, devised mechanisms for enrolling other communities, and eventually created new organizations for promoting, evaluating, developing, and selling probe microscopes. These community-building activities offered inspiration to the loose group of scientists (including a few probe

microscopists), policy makers, and futurists who lobbied for nanotechnology in the early 1990s. That is, nanotechnology's proponents invoked the probe-microscopy community as a model for other fields that gradually became part of the nanotechnology enterprise. The view that the STM set in motion the emergence of nanotechnology is deficient not only because it ignores probe microscopists' agency as innovators and users of a technology, but also because it ignores their agency as innovators in fashioning an instrumental community. The STM's importance to nanotechnology lay in the spread of innovations made by probe microscopists—both as technologists and as community builders—to other fields.

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Instrument and Instrumentality

President Clinton's Caltech speech nicely illustrates the double meaning of "instrumental community." What initially brought probe microscopists together was a common interest in *instrumentation*—a new technology for peering at very small objects in a hitherto unknown way. Yet probe microscopists also set out to create a new *instrumentality*—a new way of doing things that would propagate beyond their laboratories and change the world. The map of the Western Hemisphere made from gold nano-clusters was a product of novel instrumentation, yet the motivation for that experiment was to demonstrate a new technology (an instrumentality) for storing digital data. Similarly, the backdrop for Clinton's speech was chosen both for the striking visual appeal of STM images and because the probe-microscopy community was instrumental in forging consensus among bureaucrats, academics, and industry leaders that nanotechnology would be sufficiently important to national competitiveness to require a huge federal effort.

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As the gold nano-map implies, probe microscopy blurs any distinction between science and technology. Like much work done with probe microscopes, the Western Hemisphere map was simultaneously an experiment (for generating new knowledge and new questions about the behavior of gold surfaces) and a demonstration of a technique that might someday be incorporated into high-tech manufacturing. It is no coincidence that the gold nano-map was first published in a journal that contained both "science" and "technology" in its name. Science and technology—and instrument and instrumentality—were rarely stable categories for probe

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microscopists, but rather convenient poles between which experiments could continually be shifted.

Since the 1970s, a consensus has emerged among historians and sociologists of both science and technology recognizing that these categories are permanently blurry—that there is much that is scientific (meaning, roughly, generative of rigorous, generalizable knowledge) in the most applied and artifact-driven activities, and there is much that is technological (i.e., engineering-oriented and potentially instrumental and/or commercial) in even “basic” research.¹⁴ This synergy between instrument and instrumentality has been well documented, especially in studies of “Big Science” facilities (such as telescopes and particle accelerators) where scientists have had to put on their “engineering hat” in order to build and manage complex sites and organizations.¹⁵ The quintessential example of Big Science’s making itself instrumental is, of course, nuclear-weapons research, which evolved from a small community of physicists and chemists into world-changing megaprojects employing hundreds of thousands of people in only a few years.¹⁶

Yet small (or “bench-top”) sciences such as molecular biology and nanotechnology are widely seen as more important to today’s high-tech industries, such as biotechnology and microelectronics, than the Big Sciences inherited from the Cold War. In the 1990s, promoters of nanotechnology repeatedly insisted that government support of bench-top nanoscale research was necessary for continued innovation in microelectronics and other high-tech industries.¹⁷ In this book, I will endeavor to illuminate that interplay between bench-top science and high technology. To do so, I will try to knit together recent historical studies of bench-top experimental (and theoretical) tools and current work on the histories of microelectronics and biotechnology. The former literature offers a way of understanding the connections among pedagogy, knowledge creation, and tool development, and a means of tracing the local variations in design and use of tools as they propagate away from their place of invention.¹⁸ The latter literature emphasizes the role of craft skills in high-tech industries, the consequent regional concentration of knowledge and labor in those industries, and the importance of expectations about how long it will take research to bear economic fruit (and how research and manufacturing units should interact) in structuring competition among firms.¹⁹

At the intersection of the aforementioned literatures are studies of bench-top experiments that can double as tools for high-tech industry²⁰—tools

such as electron microscopes, lasers, spectrometers, centrifuges, and polymerase chain reactions. An important common feature of these tools is that they are simple and inexpensive enough that very small lab groups can pioneer their design and use, at the same time that very large firms exploit their industrial potential. The resources of large firms can be helpful in innovating such tools—researchers at IBM and AT&T, especially, were early pioneers of probe microscopy. However, the instruments of probe microscopy were small enough that corporate and government researchers could occasionally build or use them surreptitiously or with little managerial oversight.²¹ Presumably, had the technology been significantly larger or more expensive, it would have been more difficult to strategically hide it from view.

Small academic groups, too, are capable of pursuing bench-top experiments that aid industries such as biotechnology and microelectronics. In the 1980s and the 1990s, a university physics group consisting of one professor, two or three graduate students, and one or two postdoctoral fellows could build or buy an AFM and then make important innovations in the use or design of the instrument. In the same era, a state-of-the-art particle collider or optical telescope couldn't have been built or bought by such a small group. That such small university lab groups can make discoveries that later turn out to have great economic potential has generated intense interest in how academic research interfaces with industry. High-tech firms, university administrators, regional development offices, politicians (at all levels of government and in most industrialized nations), and scientists themselves have increasingly sought ways to commercialize bench-top academic research since about 1970.

Economists, sociologists, management scholars, and historians, in turn, have produced numerous studies of academic commercialization—some in favor of the phenomenon, some not.²² The hope, for most actors and analysts, is to better understand (and perhaps foster) the transformation of bench-top academic research instruments into marketable instrumentalities. This book is intended to contribute to that debate by focusing on aspects of university-industry relations that are usually bracketed. In it I argue that instrumental communities are indispensable to the commercialization of bench-top research. The probe-microscopy community was a multi-university, multi-firm, multi-disciplinary, multi-regional network in which ideas and people moved back and forth among government, academic, and corporate organizations. Interactions among people

widely distributed in that community allowed probe microscopy to alternate between instrument and instrumentality.

Analysts of academic commercialization often focus on a limited set of actors—for example, university administrations, members of university faculties, and start-up companies with which faculty members are associated. Such analyses also tend to focus on production (of patents, technologies, and firms) rather than on consumption. In this book, however, I will take a somewhat broader perspective. Professors and start-up firms will, indeed, play important roles, but so will scientists and engineers at government labs and established firms, federal grant managers, futurists, journal editors, and officers of professional societies. Moreover, we will see those different actors influence the form—and, ultimately, the market potential—of probe microscopy, both as producers of novel technologies and as active consumers of those innovations.

Ultimately, the lesson of this book for students of academic commercialization is that we cannot fully understand academic entrepreneurs unless we think about the complex roles they play within wider research communities that are only partially oriented to commerce. All the start-ups and some of the individual researchers we will encounter were, indeed, interested in probe microscopy's progress from an experimental technique to a marketable technology. In several cases, however, we will see that the best opportunities—for both commercial and academic actors—lay in moving probe microscopy away from marketable applications. Putting aside early hopes for commercial use, and instead focusing on answering basic research questions, linked probe microscopy to disciplinary communities that, in turn, inspired the innovations that were needed to move probe microscopy back toward its most lucrative markets.

That is, a market for probe microscopes developed not because the probe-microscopy community's members focused solely on applied research with commercial value, but because the community's members were moving in many directions at once. Probe microscopists pursued almost every gradation of basic and applied, curiosity-driven and problem-driven research. Movement in each of those directions allowed probe microscopists to form connections with new disciplines and organizations. Those disciplines and organizations sometimes presented potential markets for probe microscopes, but sometimes they also elicited or offered innovations that made probe microscopy more widely applicable. Thus, entrepreneurs and firms

that wanted to make money from probe microscopy needed to participate even in the least commercial activities of the probe-microscopy community. Entrepreneurial researchers and start-ups sought to recruit new members to (and from within) that instrumental community, to demonstrate skill in generating research results that would be ratified by other members of the community, and to create and control institutions (conferences, journals, funding streams) that would strengthen that community and enhance their standing within it.

Thus, in probe microscopy, and probably more generally, commercialization of bench-top small science can be a non-linear, distributed process over which any organization (a university, a government agency, or a high-tech firm) has limited control. Bench-top instruments benefit from the speedy innovation cycle that is typical of small science, yet the transformation of those instruments into marketable instrumentalities requires researchers to amplify their efforts by leveraging membership in a wider instrumental community. Though most instrumental communities are less formal and centralized than Big Science projects, they do exhibit a more complex degree of organization than a lone academic lab group. Instrumental communities are geographically distributed and internally differentiated. Local nodes control different kinds of resources, personnel, or expertise, and therefore can exert different kinds (and levels) of influence in the community.

Instruments, Organizations, and Disciplines

So instrumental communities are important, and complex. But what, exactly, are they? To a first approximation, the “probe-microscopy community” contained the people who mutually oriented to the STM, the AFM, or one of their variants, and who saw themselves as doing something in common with other probe microscopists around the world. More generally, an instrumental community is a network of individuals who view their involvement with a particular type of instrument and/or instrumentality as ratifying their connection to other nodes in the network.²³ The artifacts and techniques on which the instrumental community is centered are roughly those that Terry Shinn and Bernward Joerges describe as “research-technologies”—“multipurpose devices for detection, measurement, and control that were conceived and developed by a community connected to

both science and industry.”²⁴ The structure of an instrumental community roughly parallels that of what Sonali Shah terms a “user innovation community . . . characterized by voluntary participation, the free flow of information, and far less hierarchical control and coordination than seen in firms.”²⁵ Instrumental communities contain influential users who develop new variants of the form or application of a research technology, and who voluntarily share (some) information about those innovations with other members of the community in order to make the technique more robust and widespread.

Probe microscopists actively shared ideas, blueprints, samples, pieces of equipment, and even personnel in an attempt to build on each other's expertise. They also established institutions (including an annual conference series) to share knowledge and encourage the growth of the community. Indeed, probe microscopists defined membership in their community partly on the basis of participation in such institutions. People who attended one of the annual (later biannual) STM Conferences, for instance, could see who else attended and could conclude that those people had at least some stake in the technique. Indeed, conferences are the important institutions that they are because they allow participants to be co-present.²⁶ Attendees can judge, face to face, what *kind* of stake other attendees have in a technique. At the STM Conferences, attendees who were already heavily invested in probe microscopy could encourage newcomers to adopt the technique. Newcomers who were interested in working on probe microscopy could approach veterans to ask for advice or jobs, and to offer their own expertise and samples to be imaged.

Conferences weren't, however, the only means by which members (and non-members) of the probe-microscopy community identified who else was (or wasn't, or was only marginally or potentially) a member. For instance, they could identify other members of the community by seeing who published results from experiments in which an STM or an AFM was used. Grant officers at federal funding agencies could partially map the probe-microscopy community by monitoring who solicited money to build, buy, or use an STM or an AFM. Grant officers also encouraged their (or other officers') grantees to become probe microscopists by putting them in touch with STM users or AFM users or by arranging conferences and other networking opportunities, or by insisting that a microscope bought with their money be made available to a wide swathe of users. Likewise,

the companies that sold STMs and AFMs might see a probe microscopist as anyone who bought one of their firms' products, or one of their competitors' products, or who built a microscope that could be substituted (wholly or in part) by a commercial product. More controversially, they might see potential probe microscopists in anyone who could be convinced to do one of those things.

My point here is that probe microscopists had their own rules of thumb about where the boundaries of their community lay, but that they were also acutely aware that those boundaries were permeable, blurry, and dynamic.²⁷ Indeed, probe microscopists manipulated the boundaries of their community—and thereby provoked conflicts within their community. Not all the rules of thumb were the same. Probe microscopists disagreed profoundly about who should count as a probe microscopist, how big their community should be, and whose probe-microscope research best exemplified their community's work.

Still, no matter how much they disagreed about where the boundaries of their community lay, probe microscopists had to be mindful of the views, expertise, and achievements of the other members of that community. Though most probe microscopists worked in small teams, with little formal coordination with the rest of the community, the judgments of the community were influential in formulating research directions, interpreting results, obtaining funding, and hiring personnel. Those judgments were conveyed through a variety of different types of connections between members of the community—e.g., between an adviser and a student (or a former student), or between a funder and a fundee, or between a vendor and a customer, or between a colleague and a competitor, or between a user and a builder.

In many of these relationships, probe microscopy provided only part of the connection between the individuals. Though membership in the probe-microscopy community might be ratified by use of the same (or a similar) research technology, members of that community shared other ties that instigated, cemented, or grew from their common orientation to scanning tunneling microscopy or to atomic force microscopy. For instance, two physicists, both working with STMs and accustomed to meeting every year at the American Physical Society's meeting, might take those meetings as an opportunity to share information with regard to probe microscopy (e.g., where to buy components, how to clean the tunneling tip).

They might just as likely use that meeting to discuss each other's non-STM knowledge (e.g., what textbooks to use, how to get jobs for their students). Two STMers working for separate groups within IBM might (or might not) have shared information about their instruments, but they would have been just as likely to share mutual concerns about managers or company policies. In a few cases, both members of a married couple were members of the probe-microscopy community, presumably sharing more than an interest in the same research technology.

That is, no one was a member of *only* the probe-microscopy community. All probe microscopists owed loyalties to various other groups and institutions—loyalties that reinforced or contradicted demands made on them by other practitioners of STM and AFM. This simple fact, in all its permutations, will drive much of the narrative of this book. The probe-microscopy community grew because its members were able to exploit or create personal relationships with members of networks that weren't initially perceived by anyone to be relevant to probe microscopy.²⁸ Such extra-instrumental ties generated the trust that was needed to convince potential probe microscopists that the STM and the AFM were (or could be made to be) applicable to their needs. Thus, innovation in probe microscopy depended, in large part, on how participants structured this instrumental community to simultaneously satisfy the demands of membership in other networks. This struggle played itself out in the tiniest details of how STMs and AFMs were built, what samples they were used to characterize, and how their results were interpreted.

Three types of group membership will occupy most of this book: membership in instrumental communities, membership in disciplines, and membership in organizations.

First, instrumental communities: Many probe microscopists had been (or later became) specialists in other kinds of instrumentation. Some techniques were competitive with or complementary to the STM or the AFM. Probe microscopy manufacturers often marketed STMs and AFMs alongside spectrometers, diffractometers, ellipsometers, electron microscopes, and a host of other tools. Some people became interested in probe microscopy because they thought it could answer some research question or solve some industrial problem that they (and their colleagues) had unsuccessfully been trying to solve with a different research technology. Other people gravitated to probe microscopy because they had experience with

another research technology that they thought would give them an advantage in developing useful variants of the STM or the AFM. Thus, in a variety of ways, bridges to (or competition with) other instrumental communities allowed probe microscopy to propagate more widely.

Second, probe microscopists continually made use of their affiliations with various organizations: universities, large corporate research centers, government laboratories, start-up manufacturers, small analytical labs run out of spare bedrooms, federal funding agencies, professional societies, scientific journals, think tanks, industrial manufacturing and quality-control facilities, and so forth. These organizations gave probe microscopists a place to work or publish, situated them within a system of peer review or bureaucratic supervision, put resources at their disposal, allowed them to communicate with collaborators or be compared with competitors, and tied their employment to the objectives of the organization. Organizations defined the time scale on which probe microscopists produced results, and the ways those results were rewarded. Certain organizations formed nodes in the community, places from which other probe microscopists took their lead. Over time, major transitions in the structure of the community (and in the design and use of its technology) were associated with shifts in which organizations commanded the most influence or were the entry points for the most members of the community.

Most of the organizations examined in this book were formal entities even before the invention of the STM—networks of people bound by contractual obligations to the same entity and by other shared institutional forms (e.g., a letterhead, or an ability to locate themselves on different parts of the same organizational chart). However, an important contribution of this book is a clearer picture of how new organizations come into being, and formalize over time, in scientific and high-tech domains. The expansion of the probe-microscopy community created conditions for the emergence of *ad hoc* organizations that made it possible for information to travel more quickly around the community, or that facilitated entrance to the community—organizations such as tiny (one- or two-person) start-up companies and informally managed annual conferences. Eventually, the continued expansion of the community, and the increasing differentiation among types of probe microscopists, led these new organizations to incorporate and steadily formalize their procedures. For instance, manufacturers of SPMs slowly adopted organizational charts with intervening layers of

middle managers, and the annual STM Conference established a formal affiliation with a professional society: the American Vacuum Society.

Finally, probe microscopists made enduring use of ties to the scientific and engineering disciplines. From the beginning, probe microscopists came from a variety of disciplines, and spent much of their time trying to draw in members of yet more disciplines. Physicists, chemists, biologists, materials scientists, electrical engineers, mechanical engineers, mineralogists, and physiologists all routinely use probe microscopes today. In each case, the use and the design of the instruments have had to be adapted, sometimes drastically, to the values, practices, and interests of the discipline. At the same time, probe microscopy has made itself instrumental partly by effecting changes in the values, practices, and interests of those disciplines.

In return, disciplines made themselves useful to probe microscopy by contributing experimental materials that could be characterized with an STM or an AFM, “hot” or “interesting” questions these tools could be used to answer, interpretive schemas for judging the value of data generated with the microscopes, traditions for training new members of the discipline and for establishing the worth of their work, new markets for commercial STMs and AFMs, and a string of institutions (professional societies, conferences, journals, program areas within funding agencies, academic departments) that probe microscopists could occasionally bend to their purposes.

For the purposes of this book, the disciplines into which probe microscopy spread will be defined in part by the resources they offered probe microscopists: materials, criteria for judging knowledge and people, ways of training instruments and instrumentalists, means of publishing and meeting other practitioners, sources of funding, canons of open and closed research questions. However, probe microscopists themselves also identified which disciplines could aid them in terms of the pathways for recognition and reward those disciplines offered. As graduate students in disciplinarily defined departments of physics, for instance, some used their acquaintance with scanning tunneling microscopy or atomic force microscopy in order to finish their dissertations and find jobs—though often in their new jobs the mechanisms of recognition and reward led them to identify as “engineers.” Likewise, as postdocs and junior staff scientists IBM or at Bell Labs, some used their expertise in scanning tunneling microscopy

to gain recognition from more senior colleagues and managers who identified with the discipline of surface science. By publishing in surface-science journals, winning awards from surface-science professional societies, and solving canonical surface-science questions, young STMers could secure promotion within their organizations or move up to new jobs elsewhere.

For probe microscopists, then, the disciplines were identifiable and useful bodies in part because they offered pathways for recognition and reward that extended well beyond microscopists' narrow, parochial research area. Probe microscopy was never useful to all the practitioners of a giant discipline such as physics, or even to all of a large sub-discipline such as condensed-matter physics, or even to all of a subdisciplinary specialty such as semiconductor physics, or even to all the members of some very narrow research area (e.g., the atomic structure of semiconductor surface reconstructions). Interest in probe microscopy within any discipline usually nucleated from within very narrow research areas, yet adopters of probe microscopy were often able to use scanning tunneling microscopy and atomic force microscopy to reap recognition and reward from a much wider swathe of a discipline than just their immediate topical colleagues. Most famously, the 1986 Nobel Prize in physics was awarded to the inventors of the STM largely on the basis of their contribution to the solving of an important question in semiconductor surface-reconstruction research, which commanded the attention of semiconductor physicists more generally, which commanded the attention of condensed-matter physicists, and then physics as a whole.

By building up from very narrow research areas to larger professional constellations, therefore, probe microscopists used the disciplines to forge new ties and to reinforce old ones. From the beginning, one way that the instrumental community expanded was by probe microscopists' taking advantage of ties that were defined in part by disciplinary affiliations. For instance, many early adopters of the STM joined the field on the direct urging or example of people they knew from current or past membership in the same disciplinary department at a university, government, or corporate lab—people they had gone to graduate school with, or who had been their graduate or postdoctoral advisers, or whom they knew from having taken a sabbatical in that person's department.

More indirectly, probe microscopists carried news of the technique to the disciplines through talks at discipline-oriented conferences and articles in discipline-oriented journals. Sometimes, probe microscopists

didn't themselves identify as members of the discipline they were addressing. Some disciplines addressed in this way were reluctant to embrace the instrument; others did so quickly, but also dramatically reshaped the technology to complement their discipline's existing tool set. Other probe microscopists brought scanning tunneling microscopy and atomic force microscopy into disciplines with which they did identify as members. These SPMers were often recruited into probe microscopy to teach other SPMers their discipline's sample-preparation techniques and interpretive schema. In return, they gained familiarity with an instrument that could potentially give them a competitive advantage (e.g., in obtaining tenure or funding) over other members of their discipline. When they demonstrated that competitive advantage, many of their disciplinary colleagues followed suit by obtaining their own microscopes.

The overlapping networks of disciplines, organizations, and instrumental communities presented probe microscopists with a diverse and fluctuating landscape—or, as some analysts put it, an “ecology”—through which they could gain access to knowledge, practices, personnel, and resources.²⁹ The elements of that landscape interacted in complex ways. For instance, some organizations—especially universities—were strongly oriented to disciplinary categories, and largely routed resources and recognition through them. Others, including start-up companies, minimized discipline-based work. Yet other organizations, including government and corporate labs, were somewhere in the middle—and, perhaps for that reason, the disciplines that those organizations oriented to were sometimes disciplines, such as surface science, that weren't always recognized as disciplines in academic settings.

Moreover, the landscape that probe microscopists occupied contained some forms of group membership that were difficult to categorize. For instance, some instrumental communities offered probe microscopists many of the same resources that affiliation with a scientific discipline could. Electron microscopy, especially, looked initially like the natural home for probe microscopy, and indeed many probe microscopists published in electron-microscopy journals and spoke at electron-microscopy conferences (sometimes to the disgust of electron microscopists). I do not draw a sharp line between disciplines and instrumental communities, since doing so would ignore the similarities between these categories that probe microscopists themselves made use of.

What was salient to probe microscopists was that the landscape of disciplines, organizations, and instrumental communities was constantly changing, and that probe microscopy could be used both to influence those changes and to mitigate their consequences. Probe microscopists took advantage of open disciplinary questions in order to create constituencies for their technique—yet, in doing so, they sometimes answered those questions so well that the disciplines associated with those questions had to find whole new problem areas. Probe microscopists used their new class of instrumentation to carve positions and gain recognition at influential universities and in corporate laboratories—yet when they saw those organizations as stagnating, or going into decline, they used the recognition and resources those organizations offered to get new, more secure or personally satisfying positions.

This book is therefore a contribution to the literatures that analyze the complex interactions among elements in an ecology of knowledge. In particular, I will borrow some questions and insights from the New Institutional tradition in organizational sociology.³⁰ Among historians and sociologists of science, the New Institutionalists are probably best known for the observation that one reason organizations rarely behave “rationally” to maximize any simple set of objectives (e.g., profit, prestige, or power) is that an organization’s members usually have loyalties to professional and disciplinary communities outside (or even opposed to) the organization.³¹ At the same time, organizations have little choice but to rely on the professions. Communities of professionals supply organizations with bodies of expertise, with systems for training and judging newcomers, and with networks that allow innovations to move from organization to organization.³²

Certain disciplinary formations played important roles in particular organizations in which probe microscopy took root. For instance, the large numbers of surface scientists at IBM created the conditions for tunneling microscopy’s first notable successes, and then for its rapid proliferation within the organization. In a classic instance of what the New Institutionalists term “institutional isomorphism,” the competition, personnel exchange, and mutual regard of surface scientists at IBM Research and its rival, Bell Laboratories, stimulated the rapid spread of tunneling microscopy at Bell Labs.

However, the instrumental community is an important unit of analysis that complements and complicates the New Institutionalists’ framework.

Many organizations contain people whose work is oriented to some interdisciplinary and interorganizational community focused on implementing or perfecting some new instrumentality. Membership in such instrumental communities allows participants to generate, and then travel along, ties to new organizations or disciplines. In doing so, they stitch themselves into quite different professional networks and institutional environments. For instance, a graduate student trained in academic physics might use her expertise in atomic force microscopy to become director of marketing for a semiconductor process equipment manufacturer; or a corporate surface scientist might use his expertise in STM and AFM to secure an academic position and to collaborate with avant-garde artists.³³ Such transitions are somewhat anomalous in the New Institutionalism framework, but make much more sense if the instrumental community is added as a third strand in analyzing the interactions of organizations and professions.

Methodology

Almost all of the early contributors to STM and AFM are still alive; in fact, most are still working. It is possible to go and talk to them and recapture the excitement of the 1980s and the 1990s. This opens up methodological opportunities (and challenges). Luckily, the past 30 years have seen the emergence of a new field, Science and Technology Studies (STS), that combines sociological, anthropological, historical, philosophical, and other perspectives on technical work.³⁴ One hallmark of STS has been its focus on historical case studies of very recent technical episodes. STS therefore offers numerous methodological insights into how to reconstruct the history of a young community such as probe microscopy.

The basic conclusion of Science and Technology Studies is that knowledge is socially constituted and that judgments about what is true or false are inseparable from ordinary human social discourse—building trust in one another, making moral judgments, making friends and building alliances, and so on. Some community must be present to interpret the results produced by an instrument or to adapt that instrument for wider utility. Methodologically, one can approach the communal nature of knowledge in several ways. Important historical studies have done so for centuries-old episodes by drawing on manuscripts and archival collections.³⁵ In studies of more recent episodes, historians and sociologists have relied on interviews in which participants are asked to recall details of their work.³⁶

Finally, sociologists and anthropologists have compellingly illustrated the social texture of science and technology through ethnographic engagement with scientists and engineers in which the researcher goes to a current site of knowledge production and interacts with local participants over some extended period of time.³⁷

In this book, I attempt to integrate evidence from all three of the aforementioned methods. Ethnographic research was crucial in my developing an understanding of how probe microscopy is used in its most ordinary settings. Most of the ethnographic work that informs my analysis was done with groups of academic materials scientists at Cornell University at various times in the years 1998–2002. This consisted of being around the professors, postdocs, and especially graduate students using AFMs—hanging out, asking questions, and providing help or distraction while they argued over their results, ran samples through the AFM and other instruments, haggled with the AFM’s manufacturer, and slowly transformed their understandings of AFM into reliable intuitions and their AFM data into publishable results. Further ethnographic work at STM and AFM conferences, trade shows, and summer courses widened my perspective to include further-flung members of the probe microscopy network—manufacturers’ representatives, industrial users, casually interested practitioners from other research communities, and others.

Ethnography provided a substrate of practical knowledge on which I could build an historical study. Most of the data for that historical study came from interviews with scientists, engineers, and others involved in the development of probe microscopy.³⁸ Some of the interviews cited are publicly available. These are referred to as “oral histories” in the notes. The first reference to each oral history indicates where interested readers can obtain a transcript. The majority of the interviews, however, are not publicly available. All of the interviews I cite have been transcribed, and I have sought permission from interviewees for all portions of the interview that are quoted or referenced. Readers who are interested in reading more of any transcript will need to seek permission from the interviewee.

Almost all of the people mentioned in this book look back on the period depicted here with great fondness as the most exciting time of their careers—the moment when they were doing something recognizably new and instrumental. Evoking that process of establishing a technology or determining a scientific fact, and the excitement and disorientation that goes with it, was one of the joys of this research.

Appendix B lists the interviewees and the locations and dates of the interviews. In an effort to obtain the widest possible variety of perspectives in the probe-microscopy community, I interviewed researchers, technicians, and students in academic, corporate, and government laboratories; grant officers in government agencies; applications engineers, executives, and instrument designers in start-up companies; and others. This allowed me to see the diversity of alternatives for probe microscopy's development present at any given time, and to follow individual probe microscopists across a significant arc of their careers, from student to postdoc to senior researcher.

Of course, I couldn't talk with everyone who built or used an STM or an AFM between 1981 and 2000. Aside from the sheer numbers involved, many people bought or built (or began to build) microscopes and then gave up or dropped out of the community.³⁹ Thus, I limited myself to those people who were active in the larger network of probe microscopists and, in particular, developed some new design or application for the technique that they brought to the attention of their peers.

That said, my interviews were largely confined to North America, and the picture of probe microscopy I offer here dwells on its American variants. Probe microscopy was always a global phenomenon; it was invented, and first spread, in Europe, and early chapters of this book include material from interviews with European probe microscopists. However, in the period discussed here (up to 2000), the topics covered in this book—especially the interrelationship of government, academic, and corporate organizations—were particularly salient in the American context. A US government laboratory, the National Bureau of Standards, was responsible for important forerunners to tunneling microscopy in the 1960s. American firms, especially IBM and AT&T, stimulated the rapid growth of surface-science STM in the 1980s. American universities produced more (and, up to 2000, more influential) start-up STM and AFM companies (though European examples will also be included). As the final chapter will make clear, however, the probe-microscopy community's role in the emergence of nanotechnology was a transnational one. The policy implications of this book are focused on, but not limited to, the US case.

Finally, while ethnography and interviews are useful in revealing certain kinds of information about an instrumental community, they are also prone to distortions and gaps. Interviews were especially useful for tracing the movement of unpublished information (e.g., blueprints), samples,

and tacit skills, and for gaining insight into the relationships among probe microscopists: who did (or didn't) get along with who, and who was seen as expert (or inept) in which aspects of the technique. However, interview data should be treated with some caution. For instance, interviews reveal something about the motivations of probe microscopists at moments of transition, such as why someone left the community, or why someone abandoned the STM for the AFM. Motivations are always elusive, however, and difficult for participants to reconstruct after 10 or 20 years. The ways probe microscopists today describe their actions in the 1980s and the 1990s has as much to do with their current concerns as with the actual events of the past. I have therefore contextualized interview data with a variety of documentary sources—published articles, websites, advertisements, application notes, photocopies of lab notebooks, and so on. Probe microscopy is young enough that many documents are now available in electronic form, but old enough that important information has migrated into publicly accessible archival collections. In some cases, people I interviewed gave me documents that were not publicly available. Some interviewees also stayed in touch with me and offered their feedback on the interpretations I was beginning to form. This improved my understanding of a number of points, and I am especially grateful for the continued interest of these microscopists.

Outline of Chapters

Probe microscopy's historical moment arrived just as the research environment, especially for American science and high-tech industry, was changing dramatically. In 1981, when the STM was invented, large corporate laboratories were still in their heyday. Two of the central organizations in early probe microscopy, IBM Research and Bell Laboratories, were the research arms of firms that dominated their industries. Yet the early 1980s were also when the "biotech revolution" began to influence researchers and policy makers. The success of some professorial biotech start-up companies led the US Congress to pass legislation intended to facilitate patenting of academic research and led many universities to create institutions to foster academic entrepreneurialism. Some university-based STMers and AFMers were at the forefront of these changes. They quickly commercialized their research, sometimes by spinning companies out of their campus labs, with far-reaching consequences for their community.

Increasing global competition, and then the end of the Cold War, contributed to the decline or transformation of many big corporate labs. The recession of the early 1990s brought IBM to the brink of bankruptcy, and the rest of the decade saw Bell Labs lose most of its workforce. Many US companies began to integrate research more tightly with production and/or outsourced more research to university labs, accelerating shifts in the sources and character of science funding. Perhaps paradoxically, probe microscopy became both more academic and more applied as corporate researchers fled to universities and as start-up microscope manufacturers concentrated on selling to corporate customers in quality-control and reliability labs rather than just to the academic customers who once formed their base.

The chapters of this book are organized around these transitions. Probe microscopists always hailed from a variety of organizations and disciplines, but the external environment for research, and the internal evolution of the community, meant that some organizations and disciplines had more influence over the community at different points in its development. Thus, almost from the very beginning, there was differentiation among different sub-networks within the larger probe-microscopy community. Those sub-networks were defined in several ways: by proximity to the technique's original inventors, by association with a particular discipline, by the ability to generate ties to many different disciplines, and so on. Each chapter focuses on a different sub-network, and on the moment when that sub-network's members' version of the technology set the wider community's agenda. The drift of the chapters is generally chronological, in that I examine each sub-network in the order in which they achieved their greatest influence. However, there will be some chronological overlaps, especially among chapters 3–6.

The first sub-network encompassed the technique's inventors and their immediate colleagues when probe microscopy was still in its most rudimentary and precarious form. Chapter 2 begins in the 1960s, when a precursor of the STM called the Topografiner was built at the US National Bureau of Standards. The Topografiner's inventor, Russell Young, never successfully convinced others to replicate the instrument or to rally around the technology, so no instrumental community grew from his efforts. In 1981, the STM's inventors, Gerd Binnig and Heinrich Rohrer, faced a similar difficulty. The rest of chapter 2 explores how they played cannily on interfaces between their organization (IBM) and various disciplines—especially surface science—to generate interest in replicating their results.

Chapters 3 and 4 examine the first wave of these replications in North America. Chapter 3 focuses on the surface scientists who adopted the STM in the context of a long-running “cold war” between IBM Research and Bell Laboratories. These organizations employed large numbers of surface scientists in an enormous long-term effort to develop new microelectronics manufacturing techniques. As a result, surface scientists at Bell Labs and at IBM had better access to resources and many more intra-disciplinary peers and managers within their own organizations than most other early STMers. Thus, when it became clear that the STM could contribute to surface science, these organizations quickly fostered a large cadre of STM groups. Tunneling microscopy for surface science spread more slowly at other firms and in government and academic labs until the first generation of STMers began leaving Bell Labs and IBM to pursue careers elsewhere, bringing the technology with them.

Chapter 4 focuses on an alternative form of probe microscopy that had less allegiance to any one discipline. This sub-network was led by academic chemists, electrical engineers, physicists, and biologists, although it attracted corporate researchers (especially former students of the leading academics) too. These groups built smaller, cheaper microscopes than the corporate surface scientists, and they included a more diverse set of people (in terms of educational background) in their labs. Because their organizational positions allowed them a degree of disciplinary independence, leaders of these groups sought to widen the STM's applicability to materials other than those of interest to surface scientists. This led them to develop the AFM and other variants, and to recruit representatives from various disciplines in an effort to develop new uses for probe microscopy. Those representatives then became the conduits for the adoption of probe microscopy by their disciplinary peers.

As STM, AFM, and other variants proved capable of answering questions of interest within surface science and other disciplines, practitioners of those fields sought their own microscopes. Some newcomers built their own instruments, but this was a time-consuming task in an era when the instrumental community was moving very quickly. Thus, the demand for pre-built microscopes rose. Some pre-existing manufacturers of scientific equipment attempted to meet this demand by introducing new lines of commercial probe microscopes—a strategy that, for the first decade or so, was most successful for surface-science STM. In other cases, new

organizations emerged to develop and sell probe microscopes. The sub-network examined in chapter 5 is the one formed from those organizations and their ties to suppliers and users of innovations in probe microscopy. By making a relatively cheap instrument (as little as \$35,000 for a research AFM in the early 1990s) available to researchers who lacked the time or skill to build one themselves, these firms catalyzed a radical demographic shift in probe microscopy.

That is, by the late 1980s the probe-microscopy community was an “instrumental” community in the sense that its members were responsible for creating new firms (and redirecting pre-existing firms) to propagate the technique. Probe microscopy was also instrumental in reorienting various disciplines to new sets of questions, and in making it easier for members of those disciplines to establish collaborations with practitioners from other fields. Some probe microscopists—in academic, corporate, and government organizations—sought to make the technique instrumentally useful for high-tech manufacturing, especially in microelectronics.

Chapters 3–5 deal with different approaches to two recurring problems of instrumental communities. First, how could non-members of the community be made to trust members enough to believe, and even adopt, their technique?⁴⁰ Second, how could members know which innovations to pursue in order to generate trust among non-members? In exploring the different ways of answering those questions, readers will begin to see why the sub-network is the appropriate unit of analysis for each chapter. Trust in probe microscopy was built link by link, person by person, through the gradual growth of networks of practitioners; however, the nature of the trust, and of those personal links, depended on which sub-network practitioners most closely associated with. Chapter 3 describes a sub-network built largely through trust in disciplinary canons that were embedded in organizational systems of review. Chapter 4 examines a different sub-network in which trust in the technique was facilitated by personal familiarity among the sub-network’s members—familiarity that either pre-existed the technique or had to be manufactured along with it. Chapter 5 shows how one probe-microscope start-up used that personalized, charismatic form of trust-making to handle inputs from microscope builders, and to transform microscope users into customers.

By 1990, each of the sub-networks examined in chapters 3–5 had grown dramatically. Each of those sub-networks was itself somewhat segmented,

owing to the proliferation of distinct variants of probe-microscope technology. The demographic shifts precipitated by the diversification and proliferation of probe microscopy could therefore have led to the disintegration of the community. Indeed, some early probe microscopists tried to foment such a disintegration; others simply lost interest in learning about what members of other sub-networks were up to. However, probe microscopy's demographic growth and segmentation coincided with the end of the Cold War and the consequent changes in many nations' science-policy regimes. Some influential probe microscopists saw emerging late-Cold-War and post-Cold-War trends in science policy as providing a way to overcome problems generated by their instrumental community's changing demographics. Chapter 6 shows that their most successful strategy for maintaining the coherence of the probe-microscopy community was to take advantage of the emerging nanotechnology movement. Not all probe microscopists reacted favorably to the nanotechnology label, given that label's association with futurist speculations about space colonization, cryonics, and radical life extension. Yet nanotechnology, as a movement within various nations' science-policy circles, offered a way for probe microscopy to continue expanding into new fields (fields that were similarly oriented to nanoscale phenomena) without fragmenting into distantly related specialties. Thus, the probe-microscopy community was "instrumental" in cultivating institutions and audiences that were influential in the early growth of the nanotechnology enterprise. Conversely, the community was *an instrument of* promoters of nanotechnology, who offered it as a legitimization of their vision for how science and science policy should work, and a model for their burgeoning movement. Therefore, the book ends with an examination of the fusing of nanotechnology proponents' use of probe microscopy and probe microscopists' use of nanotechnology.

2 Inventing a Community

Not every research technology becomes the focus of an instrumental community. Building a community is, after all, difficult and often thankless work. Cultivating potential members requires a different skill set than inventing an instrument. It is not always obvious whom an inventor should approach to join a nascent instrumental community, or where to find such people. Some inventors may even prefer that their instruments not be widely adopted, since an instrumental community can dilute their influence over what their invention looks like or how it is used.

For probe microscopy, as for many other technologies, inventors found the raw materials for building an instrumental community in already-existing disciplines, organizations, and instrumental communities. We cannot see the invention of probe microscopy as a purely technical matter of wiring feedback circuits and sharpening tips. The invention process was also a matter of skillfully moving the new technology to just the right intersection of discipline and organization at just the right time. New innovations in design or use suggested new avenues for community building; conversely, instrument design was shaped by the possibility of recruiting certain types of users.

One discipline, surface science, was particularly influential in the invention of a probe-microscopy community. Yet the influence of surface science was mediated through a particular kind of organization: the large research laboratory. This chapter focuses on two organizations that were at the center of both surface science and early STM: the US National Bureau of Standards (NBS) and IBM's research laboratory in Rüschlikon, Switzerland (better known as IBM Zurich).

More than ten years before the STM was invented at IBM, researchers at the NBS constructed an instrument, called the Topografiner, that contained

most of the elements of a tunneling microscope. Both the Topografiner and the STM were initially objects of managerial puzzlement and even opposition, requiring the instruments' inventors to seek internal and external audiences who would ratify the value of their technology. The NBS researchers were never able to find or create that constituency, despite their close ties to a discipline, surface science, that later welcomed the STM. Conversely, members of the IBM team—despite an uneven relationship with surface scientists—used their organizational ties to that discipline successfully. IBM's surface scientists gave the Zurich STMers strategic advice (on where to focus their efforts so as to elicit interest from surface scientists) and practical tips (on how to prepare samples). As a result, the STMers made a startling discovery related to one of the central mysteries of surface science. Ever since, surface science has been one of the strongest constituencies for tunneling microscopy.

The eerily comparable cases of the STM and the Topografiner therefore allow us to see just how difficult, and how important, it is to build an instrumental community. The STM is now seen as having an air of inevitability, but at the time its promise was ambiguous. The STM managed to avoid the Topografiner's fate because its inventors steered its design and its use so as to appeal to members of a discipline (surface science) that was powerful within their organization (IBM Research) and its peers (including the National Bureau of Standards and Bell Labs).

Surface Science and the Bureau

Surfaces are important in many technologies and natural phenomena: many chemical reactions happen at surfaces, most mechanical engineering is affected by friction and adhesion between the surfaces of moving parts, optical components gain many of their characteristics from light's journey across a surface, rust and corrosion begin at surfaces, and so on. But a field of surface science didn't take shape until the early 1960s. This field distinguished itself as scientific relative to other research on surfaces by virtue of its practitioners' ability to precisely control the composition of the surfaces being studied. It did so by exploiting the remarkable progress made in the 1950s in technology for generating (and measuring) very low-pressure vacuum environments.¹ Surface scientists defined themselves in part by

their ability to use these “ultrahigh-vacuum” (UHV) chambers to create and maintain samples of unprecedented purity at their surfaces.

Surface scientists could place tools for preparing samples (by heating or sputtering them, evaporating metals onto them, or exposing them to various gases) within their UHV chambers. Vacuum chambers usually also enclosed instruments (diffractometers, spectrometers, microscopes) used to characterize samples. By preparing and characterizing samples in the exquisitely clean conditions of the UHV chamber, surface scientists could claim that their samples were “well defined.” That is, they could claim to know the exact composition of the first few atomic layers (picturesquely known as the “selvedge”) of their samples, even if they didn’t know how the atoms in those outer layers were arranged or bonded to one another.

One point of having well-defined samples was to create surfaces that could be represented by mathematical models simple enough to be generated and manipulated with contemporary levels of computing power. That desire, in turn, drew surface scientists’ attention to crystalline materials (usually metals and semiconductors) or to simple molecules deposited on crystalline substrates, since crystals are highly ordered materials that are relatively simple to represent mathematically. More complex materials that were less amenable to mathematical analysis or that could not survive ultrahigh vacuum—especially biological materials—were seen as only marginally interesting to a rigorously scientific study of surfaces.

Surface science drew extensively, therefore, on instrumentation, sample-preparation techniques, and theories borrowed from crystallography. In particular, surface scientists took many of the instruments invented in the 1920s and the 1930s for interrogating bulk crystals with beams of electrons and adapted them for examining just the very outermost layers of atoms in a crystal. This adaptation yielded a suite of surface-analysis tools, such as low-energy electron diffraction (LEED), x-ray photoelectron spectroscopy (XPS), and Auger electron spectroscopy (AES).

Many of these instruments descended from a tradition in electron physics of experimental “tubes” that could be turned into commercial products: the Crookes tube, the thermionic valve (or “vacuum tube,” used in radio, computing, etc.), the Coolidge tube (used to produce medical x-rays), the light bulb, the cathode-ray tube (used in televisions), and so on.² Whether as experimental apparatus or commercial product, these tubes usually

consisted of an evacuated glass “envelope,” a cathode (often made from a sharp metal filament) for generating electrons, and various geometries for controlling the stream of electrons or monitoring their interactions with matter. The early surface scientists inherited the bench-top culture of the electron physics tube makers. Surface scientists of the early 1960s prided themselves on their expertise with glass, tungsten, and sealing wax—on being able to blow a glass tube in a matter of minutes, seal it, and have it keep its vacuum for decades. If you visit one of these surface scientists even today, you probably will see several of these old tubes lining their book-cases, still ready to be used.

The purpose of this thumbnail description is merely to show that surface scientists fashioned a self-consciously new and rapidly evolving discipline in the 1960s. Members of this discipline extensively adapted instruments from electron physics to analyze ultraclean, ordered metal and semiconductor surfaces. They also, increasingly, developed their own, new classes of characterization tools. That is, surface scientists provided many members for various instrumental communities that were centered on tools that either were developed within surface science or were borrowed from other domains (e.g., electron physics).

Thus, during the period covered in this chapter (roughly, 1960–1983), surface scientists developed expertise in inventing, appropriating, evaluating, and replicating new characterization tools. One important difference between the Topografiner and the STM was that the Topografiner emerged in the late 1960s, when this disciplinary expertise was still rudimentary. One prominent surface scientist, Charles Duke, identifies 1967–1982 as the transitional period when the widespread adoption of AES, XPS, and LEED led to a Kuhnian “paradigm shift” in surface science.³ That shift, which enabled the adoption of many more characterization tools in the 1970s, benefited the STM but was not yet in place to aid the Topografiner.

That paradigm shift was accelerated by surface scientists’ increasing influence within certain organizations. Surface scientists adopted the American Vacuum Society (AVS) as their “home professional society” in the 1960s after their request to have stand-alone topical sessions at the annual March meeting of the American Physical Society was turned down.⁴ Many surface scientists remained members of the APS, but they increasingly took leadership positions within the AVS. This allowed them to use the AVS’s resources—its peer-reviewed *Journal of Vacuum Science and Technology*,

its annual meeting, its regional chapters, its short courses, and a Surface Science Division set up in 1968—to allow information about the new characterization tools to move much more quickly.

Another set of organizations that fostered the adoption of the new tools of surface science were large government and corporate research laboratories. In the United States, Bell Labs, the National Bureau of Standards, General Electric, IBM, Xerox, Ford, and a few other corporate and government labs employed a large percentage of the surface-science community at any given time. An even larger percentage of the discipline had worked in those organizations at some point in their careers. At some of these organizations, such as the Naval Research Laboratory, there were formal surface-science-oriented departments through which resources, and career trajectories, were funneled.

In contrast, though surface science was well represented in American universities, there were practically no academic departments of surface science. Most academic practitioners in the United States were housed in physics departments, though some had appointments in chemistry, electrical engineering, or other departments. Very few universities employed more than a few faculty members who specialized in surface science, whereas, by the 1970s, Bell Labs and IBM each employed more than a dozen senior surface scientists, in addition to numerous junior staff scientists and postdoctoral fellows. Surface-science tools could therefore be evaluated, and then spread, within Bell and IBM much more quickly than was generally possible in academic surface science.

The National Bureau of Standards never gathered the same mass of surface scientists that Bell Labs and IBM Research did in the 1970s, yet its researchers played an important role in the early professionalization of the field. The NBS was the premier American metrology laboratory, equivalent to the German Physikalisch-Technische Bundesanstalt or the French Bureau des Poids et Mesures.⁵ The NBS was a government agency, not a commercial research lab, but its mandate was to aid American industry (through pre-competitive standards setting) and to provide certain products and services (such as Standard Reference Materials used in calibration and quality control) to industrial clients.

In 1961, the NBS hired Russell Young, an electron physicist who had recently done PhD work with Erwin Mueller at Penn State.⁶ Mueller was the inventor of two important surface-analysis tools: the field emission

microscope (FEM) and the field ion microscope (FIM). In fact, during Young's time in Mueller's group a fellow student had discovered that the FIM could provide images showing the placement of individual atoms at the sharpened tip of a crystal of tungsten—the first atomic-resolution microscope.⁷ Young himself specialized in research on the energy distributions of electrons as they were field-emitted from a surface. At the NBS, Young joined a group run by an eminent electron microscopist, Ladislaus Marton. Young aided the rest of Marton's group in understanding how electrons traveled through their microscopes and interacted with samples.

Marton had been one of the earliest electron microscopists, famed for adapting the technique to image biological samples.⁸ Yet by the time Young arrived, Marton's group was seen by the management of the NBS as needing revitalization. Senior managers criticized the Electron Physics Section for its inability to connect either to other groups within the NBS or to industrial clients. Young was in part a victim of this attitude, and in part intended as a solution. He was always at the margins of the group, but that allowed him to develop his own interests and make connections to other parts of the NBS. In particular, he helped mobilize other young chemists and physicists from around the organization to form a local surface-science community. That is, he encouraged others in the NBS not to identify themselves exclusively as electron physicists or physical chemists or solid-state theorists, but rather to think collaboratively about surface phenomena in crystals (particularly metals), and to work together on the kinds of benchtop, blown-glass, high-vacuum experiments that were becoming standard in the new surface science.

One of these researchers, Ted Madey, describes this period as follows:

The first people whom we would recognize as UHV surface scientists hired at NBS were all field emission microscopists, and that'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 . . . 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. . . . It was really kind of an exciting and exhilarating time.⁹

This passage highlights the important aspects of early surface science: its origins in (but also movement away from) electron physics, the mandatory nature of clean samples and ultrahigh vacuum, and the growth of tight-knit local communities of surface scientists at a few large laboratories.

The Topografiner

Russell Young's managers applauded his role in the formation of the NBS surface-science group, both for improving the Electron Physics group's "interaction . . . [with] the outside world" and for creating "very commendable cross linking between workers in three different Sections."¹⁰ Young's cross-linking dovetailed well with the managerial philosophy of Marton's successor as leader of the Electron Physics Section, J. Arol "John" Simpson. According to Karl Kessler (chief of the Optical Physics Division, and Simpson's direct boss), Simpson was guided by an "amoeba theory of maintaining a central core of competence in electron optics and using this competence to build up special measurement capabilities directed toward specific problems in physics," which, "once developed, were spun off to other groups and the central core turned to the next problem."¹¹ Young, who had never been part of the group's "central core," was therefore the first of Simpson's supervisees to be "spun off." He was soon followed by other Electron Physics surface scientists, among them Ward Plummer, Bill Gadzuk, and Cedric Powell.

Simpson's "amoeba theory" offers one way for managers to exploit the intersection of discipline, organization, and instrumental community. His strategy began with building up disciplinary expertise in one part of the organization, where talented individuals could be identified and evaluated by their disciplinary peers for their ability to develop new research technologies ("special measurement capabilities") of relevance to their discipline. Then, he encouraged those individuals to seek out other parts of the organization where their research technology could be applied to some practical problem.

By 1965, Russell Young had found one problem that seemed amenable to his electron physics expertise. One of the Bureau's x-ray spectroscopists, Richard Deslattes, was interested in measuring very small atomic spacings in a crystal lattice. A common issue in measuring small distances is vibration—if two points are shaking relative to each other, it is hard to know

how far apart they are. Unfortunately, small-scale vibrations are ubiquitous, especially in laboratories. People walking and talking, equipment being moved, fans and pumps churning—all cause experimental equipment to shake.¹²

When he heard about Deslattes' vibration problem, Young realized he could supply a solution from his work on field emission of electrons. In field emission, a relatively high voltage placed on a material causes electrons to "tunnel" out from the surface and move ballistically through open space. Tunneling is a quantum-mechanical phenomenon in which the indeterminacy of a particle's momentum and position allows it to unexpectedly cross an energy barrier. In field emission, a high voltage placed on a cathode causes electrons that would normally be bound to the cathode material to tunnel out, forming a measurable current.¹³ Up to the 1960s, much research had been done on the effect of the cathode material and geometry on the field emission current. Young's insight was that this current could also be used to measure very small changes in the *distance* between the cathode and an anode on which the emitted electrons impinged. He estimated that if the cathode and the anode were kept about 3 microns (millionths of a meter) apart, changes in the anode-cathode distance of less than an atomic diameter could be measured.¹⁴

To test this insight, Young devised an elaborate vibration sensor in the form of a classic electron physics experiment: a blown-glass, evacuated envelope containing a sharp tungsten "emitter" (i.e., a cathode) and a nearby flat metal anode (see figure 2.1). With a high enough voltage between the emitter and the anode, the latter would measure an emitted current, the strength of which would indicate the spacing between the two. If the cathode were vibrating, the field emission current would fluctuate, giving a proxy for the amount of vibration.

There are many simpler ways of dealing with experimental vibration, so Deslattes quickly lost interest in the project. But Young began to think about general applications of this device for measurements of small distances. These speculations on what he now called an "ultramicrometer" were squarely in line with the Bureau's mission to aid industrial clients. For instance, Young suggested that the emitter could be moved in a straight line across a sample surface, continuously measuring the field emission current. This would give a measure of the sample's surface roughness. Instruments for measuring surface roughness (such as surface profilometers) were

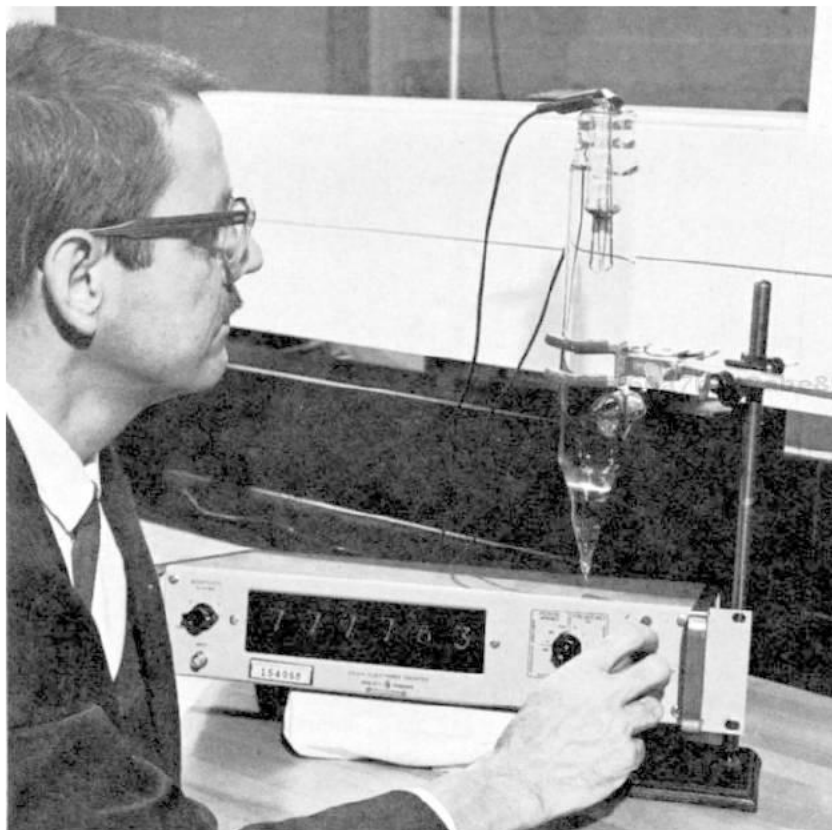


Figure 2.1

Russell Young and a field-emission ultramicrometer test rig. The emitter is the downward-facing metal triangle inside the glass envelope. The anode is the tantalum strip (bent into an upward-facing arc) directly beneath the emitter. Source: Anonymous, "Field Emission Ultramicrometer Checks Dimensions, Profiles Without Contact," *Machinery*, August 1967. Reprinted, with permission, from the current publisher of *Machinery*, Findlay Media.

already being used by one of the Bureau's most important constituencies: precision engineers responsible for quality control in the manufacture of paints, coatings, lenses, gears, bearings, and silicon wafers. However, Young argued that profilometers, in which a stylus scrapes along a sample's surface, damaged samples. The ultramicrometer, in which the emitter doesn't touch the sample, might be a non-destructive alternative to profilometry. Thus, Young identified precision engineers as the professional group most likely to adopt the ultramicrometer, and began publishing articles in their journals and making presentations at their conferences.¹⁵

A profilometer, as the name implies, gives a single two-dimensional profile of a sample's surface. Young's experimental ultramicrometer gave only a one-dimensional measurement of the emitter-anode distance. Yet Young wondered whether the ultramicrometer might surpass profilometers to give *three*-dimensional measurements. What if one moved the ultramicrometer along a line in one direction, then at the end of that line moved it one increment in the perpendicular direction and repeated (a pattern known as "rastering")? Your eyes are doing something similar as they read this book—moving from left to right along a line of text, then incrementing down to the next line and repeating. But instead of a page made up of lines of text, Young imagined a picture made up of a sequential series of two-dimensional profiles of a surface. Collectively, those profiles would form a three-dimensional image of the surface's topography (see figure 2.2). Hence, Young dubbed this instrument the "Topografiner."

Since the early 1980s, the Topografiner has been seen—by Young's colleagues, by the Nobel Prize committee, and by the US Patent Office—as having contained most of the essential elements of a scanning tunneling microscope. In both the STM and the Topografiner, a voltage is generated between a metal or semiconductor sample and a sharp (usually tungsten or platinum/iridium) probe. The voltage produces a current of electrons, which is measured and fed back into a piezoelectric crystal, which controls the height of the probe. Piezoelectric crystals change their shape when a voltage is applied to them, so small changes in voltage can precisely nudge the probe up and down in increments of less than the diameter of an atom.

In both the Topografiner and the STM, the probe can be held in one spot over the sample and the probe-sample voltage can be swept over a spectrum of values; in this form, both instruments act as spectrometers. Alternatively, a second set of piezoelectric crystals can raster the probe over

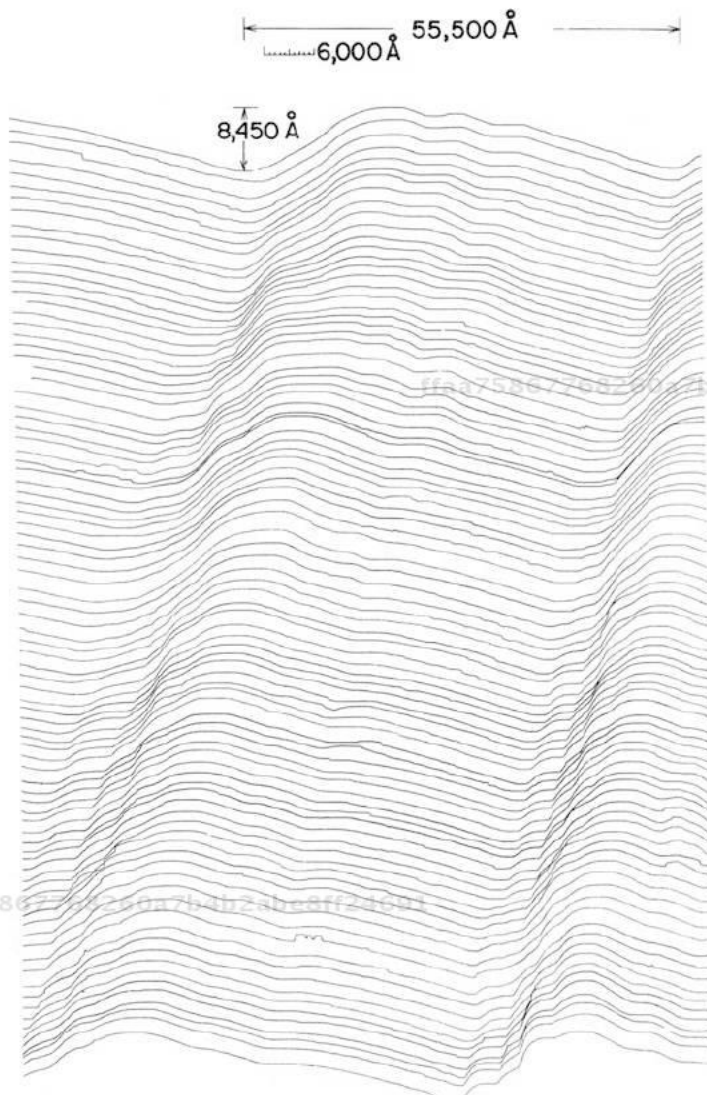


Figure 2.2

Line tracings of a diffraction grating made with the Topografiner. The swales running from the upper right to the lower left are the rulings of the grating. The lines running from left to right are the sequence of two-dimensional profiles measured by the Topografiner's emitter. Reprinted, with permission, from Russell Young, John Ward, and Fredric Scire, "The Topografiner: An Instrument for Measuring Surface Microtopography," *Review of Scientific Instruments* 43 (1972): 999-1011 (copyright 1972. American Institute of Physics).

the sample; in this form, the feedback current controls the height of the probe so that it doesn't crash into the surface. The same current is also fed into an oscilloscope, chart recorder, or other output device, giving a profile of the height of the surface; each profile is offset from the previous one, yielding a three-dimensional effect.

The most important difference between the STM and the Topografiner is that, in their rastering forms, the STM measures a current of electrons that tunnel directly between the probe and the sample; whereas in the Topografiner the distance and voltage between the probe and the sample are greater than in the STM, and the electrons move through the space between the probe and the sample. Still, Young's estimates of the resolution of the Topografiner were not far different from those initially made by the inventors of the STM, and both instruments were plausibly applicable to similar problems. The US Patent Office saw the instruments as similar enough that it denied the first twelve claims on the American STM patent "as being obvious over Young et al."¹⁶ Why, then, was only one Topografiner built, while the STM has been replicated hundreds if not thousands of times?

The answer has to do with differences in the ways the instruments' inventors harnessed their organizations' resources to win over various disciplinary audiences. The resource base of Russell Young's organization was tightening rapidly just as he proposed the Topografiner. The late 1960s and the early 1970s saw a backlash in the United States against federal funding for basic research and mounting pressure on scientists to demonstrate the "relevance" of their work to applied, civilian problems.¹⁷ The NBS, which conducted a great deal of basic research, now found itself under scrutiny. According to Ernest Ambler, when the NBS's new director, Lewis Branscomb, appeared before Congress in 1969, "the chairman of the Senate confirmation committee . . . made it clear that Branscomb should pay attention to ["consumer product testing and safety"] and not devote all his attention to science. . . . Lew took that pretty seriously."¹⁸ Ambler recalled that this caused some friction between NBS management and researchers: "[T]here was still a reluctance on the part of the brightest people to get involved in some of the basic standards work of [the Institute for Basic Standards]. They wanted to do academic kinds of research. The best people weren't working on the more mission-oriented things."¹⁹

Young's managers (John Simpson, head of the Electron Physics Section; Simpson's boss, Karl Kessler, chief of the Optical Physics Division; and

Kessler's boss, Ernest Ambler, director of the Institute for Basic Standards) saw the Topografiner as much too "academic," and Young as the type of researcher who should be steered toward "mission-oriented" research. The problem was *not* that Young was not qualified to do "academic kinds of research" and was being demoted to something more mundane. Just the opposite. Young's managers saw the Topografiner as a difficult, innovative tool that demonstrated Young's abilities as an experimentalist, and therefore they wanted to promote him to front-line tasks they regarded as urgent to the organization's survival.

Thus, in 1971, Simpson acknowledged to Young that "the topografiner [*sic*] is the one jewel in a rather dingy crown. It is the one program element which has ties to 'high science' or for that matter to 'high technology.' It is the one element which represents an attempt at other than linear incremental progress."²⁰ But, as Richard Deslattes put it in an interview with *Science*, NBS was then being

"mandated to produce results only of immediate and tangible public benefit, at the cost of losing touch with the deeper and longer term benefits of rationally conceived and executed programs." [Deslattes also said] that the message passed on to the laboratory scientists is that it is necessary to hide the best scientific work of the bureau which may not bear directly on politically popular problems.²¹

In such an environment, upper management was very reluctant to encourage any project that might appear to be "high science."

One outcome of that reluctance was that Young struggled to acquire the resources needed to build the Topografiner. In particular, he never had access to a good enough vacuum chamber to push the limits of the instrument. With the ultramicrometer demonstration of 1966, Young had simply sealed the emitter and anode in a glass envelope; he was only trying to show the concept worked, and therefore only experimented with one configuration. With the Topografiner, though, Young wanted to image many different samples, meaning that he had to have a vacuum chamber that he could move samples in and out of—i.e., a metal chamber with an air-lock. But the only vacuum "chamber" he could afford was a cast-off naval gun barrel, cut down to size. Frustrated, Young wrote the following in his notebook: "Must obtain a good vacuum! We are Diogenes in search of a good vacuum system!"²² Unfortunately, this makeshift chamber actually amplified the stray experimental vibrations that had previously bedeviled Deslattes. The oscilloscope traces showing the Topografiner probe's movement were "noisy and sensitive to audio pickup such as doors closing at a

distance, quiet talking, etc.”²³ Young had to wait to take data when the air conditioner was “off for repairs.” “To take advantage of the quietude,” he continued, “we move ourselves and the X-Y recorder out in the hall and turn off the noisy overhead fluorescent room lights.”²⁴

Still, Young believed that if he could get decent results with this *ad hoc* set-up then he could get the backing to make further improvements. To that end, he worked with specific audiences in mind, hoping those groups would become patrons. For instance, he focused on obtaining images of diffraction gratings (see figure 2.2). Such gratings consist of very narrowly spaced, high-precision grooves ruled into a surface. Optical spectroscopists use gratings to tune a beam of light to a narrow set of frequencies and, not coincidentally, Karl Kessler was an optical spectroscopist. Young’s images revealed little new about diffraction gratings, but they had the potential to pique Kessler’s interest. At the same time, Young sought to attract interest from precision engineers looking for a better way to characterize high-precision structures. In his publications on the Topografiner, Young emphasized its ability to image objects used in precision engineering where he thought his instrument could offer an advantage over profilometers and other kinds of microscopes—objects such as gage blocks, gears, and ball bearings.

Interestingly, it was a member of exactly this precision engineering community who coined the term “nanotechnology” at just the moment when Young was publishing his Topografiner results. Though he probably didn’t know of Young’s work, Norio Taniguchi pointed to a future requirement for instruments like the Topografiner in his now-famous 1974 paper “On the Basic Concept of ‘Nano-Technology.’” Taniguchi astutely noted that the tolerances for “manufacturing of mechanical parts of high precision machineries, for instance, block gauge, injection pump, pneumatic or hydraulic bearing, memory disc or drum of electronic computer, aspheric lens” were getting smaller and smaller. “The usual precision finishing technology has aimed to get the preciseness and fineness of 1 μm ” (one micron, or a millionth of a meter) or “micro-technology,” but in the next few years even greater tolerances would be required.²⁵ Once those tolerances dipped down to billionths of a meter, “the finishing technology aimed to get the preciseness and fineness of 1 nm [nanometer] would be called ‘nano-technology.’”²⁶ Taniguchi also saw that in the microelectronics industry, new techniques would be “necessary for finishing of silicon

wafer with no surface damage and high grade of flatness, coating of very thin film with precise thickness onto silicon wafer."²⁷

Taniguchi's 1974 assessment of (near-) future needs closely echoed Young's 1971 observation that "in the field of thin-film [microelectronic] devices, manufacturers have progressively reduced the size of their electronic elements to the point where one can anticipate devices employing single layers of atoms or molecules."²⁸ In preparing such thin films, "elaborate polishing, cleaning, and smoothing techniques cannot replace a detailed knowledge of the actual surface topography"—the kind of knowledge only a microscope like the Topografiner could provide.²⁹ Yet precision engineers were unpersuaded. As John Simpson put it, looking back from 1986, in 1971 "there was no industrial interest in surface characterization on the atomic scale. . . . [T]he topographiner [*sic*] [*was*] a solution looking for a problem which within, [*sic*] the industries we were charged with serving, at that time did not appear to exist."³⁰

If precision engineers dismissed the Topografiner because its resolution was too high for them to exploit, the discipline Young had done so much to advance at the NBS—surface science—was uninterested because the instrument's resolution was not high enough. Surface scientists were already accustomed to working at the atomic scale, where microscopes were at the time of little use. True, the field ion microscope and transmission electron microscope (TEM) could both give atomic resolution, but only under very constrained conditions. Of much more general interest to surface scientists were techniques, such as low-energy electron diffraction and x-ray photoelectron spectroscopy, that gave powerful, if visually indirect, evidence of the position, composition, and chemical bonding of the atoms and molecules at metal and semiconductor surfaces.

All of Young's calculations indicated the Topografiner would be no better at answering surface-science questions than FIM and TEM. Yet Young hoped it could help surface scientists characterize samples *in preparation* for using diffraction and spectroscopic instruments. He also envisioned the Topografiner as a bridge between the two communities of precision engineering and surface science. It would be a means for healing the "abrupt separation between the 'arts and sciences' of surface finish metrology on the one hand and surface science on the other."³¹

The story of probe microscopy and the path to nanotechnology has, in fact, turned out to be the healing of this "abrupt separation" through

the creation of a common instrumental community. Young's boss, John Simpson, understood this, even as he opposed the Topografiner program. In 1971, in a memo to Young, Simpson wrote: "[M]icro-metrology, i.e. metrology on the submicron [i.e. nano] scale is probably the area that will be of primary program importance in the middle range future. The topografiner [*sic*] is our foothold in this field."³² Young failed to exploit that foothold. He could not make the Topografiner appealing enough to the disciplines which might have adopted the instrument. Precision engineers saw it as too "high science," while surface scientists saw it as not scientific enough.

In desperation, Young sought out one last audience: the electron tunneling community. Remember that in the Topografiner an electron is made to tunnel out of the emitter into free vacuum and then travel ballistically until it hits the anode. Young knew that if the emitter and the sample were brought close enough together (within a few atomic diameters), electrons would tunnel *directly* from the emitter to the anode (or vice versa). He also knew that there was already an instrumental community interested in how electrons tunnel from one solid to another. These researchers used "sandwich tunnel junctions" made by growing a very thin oxide layer "sandwiched" between two metal or semiconductor electrodes. By growing a thin enough oxide layer, tunnel junction specialists were able to get electrons to tunnel from one electrode, across the oxide, and directly into the other electrode.

Occasionally, some sandwich junction researchers tried to build "vacuum tunneling" experiments in which the two electrodes would be separated by a narrow gap, rather than by a solid oxide. Yet in the early 1970s none of them had ever succeeded at vacuum tunneling. Thus, they always returned to sandwich junctions as a much easier set-up, with enough inherent flexibility to allow a wide range of experiments. Young gambled that by building a stationary version of the Topografiner he could demonstrate vacuum tunneling and entice sandwich junction researchers to copy his apparatus. He even published data that purported to show he had achieved metal-vacuum-metal tunneling, although these results were ambiguous at best.³³ Tunneling researchers remained unpersuaded.

If anything, Young's foray into vacuum tunneling simply convinced his managers that he was pursuing a "high science" program that could jeopardize the NBS. Looking back from 1986, Simpson viewed Young's work

as a “complete proof of feasibility of a vacuum tunneling scanning microscope.”³⁴ Of course, that evaluation contains some 20/20 hindsight. As of 1971, the Topografiner hadn’t operated as a vacuum tunneling scanning microscope—it had scanned in field emission mode, and perhaps operated in *stationary* tunneling mode, but it could only have been a very weak proof for the feasibility of an STM.

It is more likely that, at the time, Young’s supervisors saw the Topografiner more as a proof of Young’s technical and managerial abilities. He had demonstrated that he could do “high science.” Now, in a move that would be repeated throughout American science in the early 1970s, NBS researchers were being encouraged to drop high science in order to focus on immediate “national needs.” Ernest Ambler put it this way in 1970:

My concern to strengthen the basic metrological capabilities [i.e. services for industrial customers] has caused fears that more purely scientific activities of the Institute [for Basic Standards] would suffer. . . . I simply would not think it responsible of me to expand [purely scientific activities] in a large way until our metrological arm is strengthened. . . . Obviously, this process can be greatly hastened if talented people from data [i.e. purely scientific] programs will step into the metrological arena.³⁵

The Topografiner was interpreted locally as a demonstration that Young was a talented person who should “step into the metrological arena.” The connections he had made with precision engineers in attempting to persuade them to adopt the Topografiner could now be applied to more pressing problems: “calibrations, calibration upgrading, [and] determination of national needs” for surface finishes.³⁶ Thus, Simpson offered Young a promotion to head a new Precision Engineering Division, on the condition that he abandon the Topografiner.

Looking at Young’s work from a vantage point outside the NBS, precision engineers, surface scientists, and tunnel junction researchers saw a different kind of proof. They knew Young as a capable experimenter, and the NBS as one of the best government laboratories of its day. Thus, if even Young, with (they assumed) the full backing of the NBS, couldn’t produce an unambiguous tunneling signature, then (they concluded) no one could. Bob Jaklevic, perhaps the leading tunnel junction researcher of the day, put it this way:

You could build a vacuum tunneling system in the ’60s. 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, and were able to produce some excellent images. But there were already other topographic instruments capable of similar resolution as theirs. No one expected that an instrument like theirs could someday produce stable images with atomic resolution. That I did not carry on with my own initial attempts along these lines was a direct result of the respect I had for the work of the Bureau group. In truth I could not justify the outlay of my time and resources on a project which I felt would produce little beyond what they had already done.³⁷

At this point, it was quite unlikely that anyone who was established in precision engineering, surface science, or tunnel junction research was going to abandon proven techniques in favor of the Topografiner.

As it happened, Russell Young did succeed in enrolling one sandwich junction experimenter precisely because he was *not* an established member of that field. Clayton Teague was a graduate student at North Texas State University, where he was trying to build conventional tunnel junctions. Having learned of Young's work, however, Teague became intrigued by the possibility of doing vacuum tunneling measurements without an insulating layer. So he came to the NBS, first as a student, then as an employee, and built his own, stationary version of the Topografiner to continue Young's efforts. By 1978, Teague had demonstrated, much more convincingly than Young, that this set-up could produce the vacuum tunneling signature.³⁸ However, for most materials that the tunnel junction community was then interested in, vacuum tunneling seemed to offer little or no advantage over sandwich junctions. Moreover, the continuing crisis at the NBS forced Young and Teague to hide the vacuum tunneling apparatus from managerial view, and Teague (like Young) eventually had to abandon the project in order to take a promotion at the Bureau. Teague didn't publish his results until 1986, long after the advent of the STM had diminished their news value.³⁹

Josephson Junctions and the STM

As Russell Young was attempting to interest sandwich tunnel junction researchers in the Topografiner, IBM was putting millions of dollars into a technology derived from those same sandwich junctions. In 1968, the company started a program to develop a supercomputer based on so-called Josephson junctions. These are named after Brian Josephson, the British solid-state theorist who, as a 22-year-old graduate student, predicted several intriguing properties of a sandwich tunnel junction in which the

electrodes are formed from superconducting materials (i.e., materials that lose all resistance to the flow of electricity below some very low temperature). When no voltage is placed across these superconducting layers, a direct current will nevertheless continue to flow between them.⁴⁰ When a voltage is placed across the junction, a high-frequency alternating current will switch back and forth through the sandwich.

In the 1970s, many people hoped the Josephson Effect would be technologically useful for sensitive magnetometers (e.g., for detecting submarines), in metrological applications (e.g., for calibrating the standard volt), and, in particular, in faster computing. At the time, the path to higher computational speed was a matter of profound disagreement between different microelectronics manufacturers. One route to higher speeds was to make silicon transistors smaller, thereby reducing the distance electrons have to travel during a logic operation. At IBM, though, there was deep skepticism that miniaturization could be pushed much further. Thus, while IBM continued to invest heavily in new miniaturization techniques for silicon, it hedged against silicon by exploring more radical alternatives.⁴¹ Robert Keyes (a sort of public intellectual for electronics at IBM) put it this way in 1969:

Transistorized [silicon] computer logic has made steady progress toward higher speeds by reducing the dimensions of circuits and devices. However, . . . dissipation of power at increasingly high densities [of transistors per square inch—high densities made possible by miniaturization] seems to be leading to difficult thermal problems that eventually will limit the progress of logical circuitry toward higher speeds. Progress beyond this point can only be made by radical deviations from the current lines of development. The most straightforward new method seems to be lowering the temperature at which the circuitry is operated.⁴²

Keyes argued that lowering the temperature of silicon transistors would lead to some gains, but that a more effective strategy would be to abandon silicon in favor of logic elements made from superconducting materials, since these dissipate much less heat, and have faster switching speeds, than semiconductor transistors.

By the early 1980s, IBM was putting nearly \$20 million per year (in 1980s dollars) into a program to build a supercomputer based on Josephson junctions.⁴³ The program, which at its peak employed about 150 people, was supported in part by the National Security Agency, the US government's cryptographic arm, which saw the high theoretical speed of Josephson computing as advantageous in breaking codes.⁴⁴ IBM initially

coordinated the program from its flagship research center in Yorktown Heights, New York, with smaller efforts at its Zurich lab and at its production facility in East Fishkill, New York.

One issue confronted by the Zurich group was that the thin insulating layer between the two superconducting layers contained “pinholes”—very small regions where the two superconducting layers came in contact and short-circuited the Josephson Effect. In 1978, one of the Zurich-based Josephson team’s colleagues, Heinrich “Heini” Rohrer, was looking for a new research topic that could, at least indirectly, aid the superconducting computing program. As Rohrer recalled,

The one thing [the Josephson effort] couldn’t handle was the tolerances of the oxide layers. They couldn’t get them accurate enough to regulate predictably the amount of tunneling needed for reasonable logic circuits. . . . I thought it would be interesting to investigate the growth and electrical properties of these thin insulating layers on a very local scale.⁴⁵

Rohrer asked a newly hired physics PhD, Gerd Binnig, to develop new approaches to extremely localized spectroscopy of very thin, possibly inhomogeneous insulating layers.

Even before Binnig officially took the job, he and Rohrer discussed various methods. Because the pinholes were small, they needed an instrument with very high resolution. Probably because both had experience in electron tunneling, and because the Josephson project had a tunneling aspect, they quickly gravitated to using the tunneling phenomenon to probe for pinholes. They imagined a sharp metal tip, kept very close to a thin insulating film deposited on top of a superconducting electrode (an “open-faced” sandwich junction, as it were). If the metal tip were kept close enough to the superconducting substrate, electrons could tunnel between them. If there were pinholes in the insulating film, the tip would be able to travel into them and thereby move closer to the superconducting layer. This would cause the tunneling current to increase; therefore, by moving the tip around the sample, Binnig and Rohrer would be able to measure the tunneling current to tell where the pinholes were, how big they were, and how many there were—useful information to the Josephson team in devising new ways to manufacture junctions.⁴⁶

Binnig and Rohrer might have gotten similar information with a regular scanning electron microscope. Back-of-the-envelope calculations (the same calculations Russell Young had made) seemed to indicate that their

tunneling probe would, at best, be only a slight improvement on electron microscopy, and might become useful only after many years of development. It is a testament to the organizational values of IBM Research that Binnig and Rohrer could gesture to the Josephson project in embarking on a project that almost any other organization (e.g., the National Bureau of Standards) would have seen as offering a limited and prohibitively delayed return on investment.

A few weeks after his initial discussions with Rohrer, it occurred to Binnig that this tunneling probe could easily be turned into a microscope. On the basis of that insight, he and the two technicians assigned to the project, Christoph Gerber and Edmund Weibel, began to assemble an instrument that (like the Topografiner) contained the essential ingredients of a scanning probe microscope: a device with piezoelectric control crystals moving a sharp tip up and down and in a raster pattern over the sample, a feedback circuit to keep the tip from crashing into the surface, and a chart recorder printing an offset sequence of profiles (of the tip's vertical movement on each scan) to form a three-dimensional image.

At this point, some of Binnig's colleagues began to question his acumen, if not his sanity. As far as anyone at Zurich knew, vacuum tunneling had never been achieved, though there had been many attempts. Owing to the far greater difficulty of tunneling *while* scanning (when the moving parts make vibration a bigger issue), Binnig's goal seem doubly impossible. Later, Rohrer recounted that "when we . . . told people at the lab what we wanted to do, they said 'You are totally crazy—but if it works, you'll get the Nobel.'" To demonstrate that their idea was experimentally feasible (and perhaps therefore to show that they were not crazy), Binnig, Gerber, and Weibel built an interim instrument that resembled Teague's stationary vacuum tunneling apparatus. But Young and Teague's attempts at vacuum tunneling were perceived by their organization as proof that they were more interested in esoteric, academic phenomena than industrial application and should be discouraged from pursuing those phenomena any further. The Zurich STMers (especially Binnig, the most recently hired) *needed* to do vacuum tunneling to prove to their organization that they were serious, capable experimentalists who hadn't bitten off more than they could chew.

Over the years, some of Russell Young's advocates have claimed that the STM's inventors must have known about the Topografiner when they

began their experiments.⁴⁸ Binnig and Rohrer, however, have consistently said that they didn't hear about the Topografiner until well after they had built a working STM. Because little contemporary evidence is available, priority in the invention of the STM is a tricky question. I have seen no evidence that could lead me to doubt Binnig and Rohrer on this point. However, it would not surprise me if some of the colleagues with whom Binnig and Rohrer discussed their experiments were aware of Young's work, and that this may have been one reason why the STMers were told that their idea was "crazy." After all, to people outside the National Bureau of Standards, Young's experiments appeared to have shown that a tunneling microscope was not feasible. If anything, that makes it seem less likely that the members of the Zurich team were aware of the Topografiner—why follow a path widely seen as a cul-de-sac?

Significantly, by the time the team demonstrated a vacuum tunneling signature (on March 16, 1981, more than two years into the effort), the IBM Josephson project that had inspired the STM was in serious trouble. In a technical audit that year, only three out of fifteen members of a review panel recommended continuing the program. IBM management believed that "members of the Josephson group had very low morale" and that "the Zurich group had begun to evidence a general dissatisfaction with the technical direction of the program."⁴⁹ The program continued, but its research-oriented director, Wilhelm Anacker, departed and was replaced by a production-oriented manager, Joseph Logue. Logue moved the program's focus to the East Fishkill manufacturing facility and reduced its complement of PhD researchers in order to proceed more quickly with processing of commercial chips.⁵⁰ From the time Logue took over, the Josephson project was on probation. Indeed, after two years he and senior management concluded that Josephson technology could not profitably outpace silicon-based microelectronics and canceled the program.⁵¹

The consequences of the Josephson program's demise for the STM team were ambiguous. On the one hand, as Binnig and Rohrer later put it, the STM had "emerged as a response to an issue in technology" and had been "inspired by the specific problem of inhomogeneities in thin insulating layers—a central challenge to our colleagues working on the development of a computer based on Josephson tunnel junctions."⁵² On the other hand, the STM team was not part of, and didn't directly report to, the Josephson effort. The STMers didn't require the specific technological horizon

of superconducting computing in order to justify their work. Because IBM supported large amounts of fundamental research, the team was able to cast the STM as a general tool for very localized spectroscopy regardless of the technological relevance of the materials they experimented on.

Yet there was no guarantee that the team members' colleagues would agree that the STM was a worthwhile experiment. After all, many of those colleagues initially regarded it as "crazy." As Ambrose Speiser, a former director of the Zurich lab, notes, the STMers' work "was severely criticized by the physics establishment inside and outside IBM in the early stages. Their ideas were called irrelevant, their judgment was considered misguided, and, accordingly, it was predicted that their work would lead nowhere. It took a fair degree of stubbornness to continue to the end. After success had arrived, it was speculated that these projects could not have proceeded in the large and powerful IBM Research Center at Yorktown Heights. The reason was not lack of managerial support; rather the scientists would have been discouraged by pressure and criticism from their peers."⁵³

In such a precarious environment, the STMers didn't fully commit to the technology until they had interesting data that would answer their critics. As Othmar Marti (a student who worked alongside Binnig in the early 1980s) remembers it, during 1980–81 the STM was "hidden."⁵⁴ (Perhaps it would be more accurate to say that nearly a third of Binnig's time, and most of Rohrer's, was visibly committed to other projects in that period.) Today, Christoph Gerber describes the very early days of the STM as a "side project" pursued in addition to his, Rohrer, and Binnig's individual work in low-temperature physics—a side project that became their main preoccupation after the instrument's first breakthroughs.⁵⁵

Speiser was not the only member of IBM Research management to believe that the Zurich lab's organizational and physical distance from Yorktown Heights gave the STM room to grow in a way that was denied to the Topografiner. One surface scientist at rival Bell Labs recalled:

I was kidding one of the [IBM] research managers . . . 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.⁵⁶

For part of 1980–81, therefore, the STM was in an ambiguous position—not yet promising enough to bring into the light, but (unlike the Topografiner) not so “well managed” as to be shut down. If they were to commit themselves fully to the technology, the STMers would have to produce interesting results—and then they would need a constituency that would interpret their results as “interesting.”

STM and Surface Science

One strategy for improving the STM technically while building a network of supporters was for Binnig and Rohrer to ask colleagues to suggest samples for them to image. They used the first samples they received to calibrate the instrument, rather than to generate new knowledge about the samples themselves. Dick Gambino, a materials scientist visiting from Yorktown, gave the STMers a piece of calcium-iridium-tin, and Hans-Jörg Scheel, an important early advocate of STM in the Zurich lab, offered a sample of gallium arsenide (a technologically important semiconductor).⁵⁷ In both cases, the samples were known beforehand to have high, steep terraces that would stand out in an STM image. The STMers discovered nothing new about these materials, but they learned to calibrate the piezo crystals that moved the probe up and down by comparing their images to the already-known heights of these materials' atomic steps.

Now they were ready to generate images that revealed something new about a sample. But what samples should they look at? From the name of the discipline, “surface science,” it might have seemed obvious to ask surface scientists for samples. The STM, after all, is acutely sensitive to surface phenomena but, unlike some other microscopes, doesn't peer very far into a material. However, there were good reasons not to look to surface science. That discipline had ignored the Topografiner, after all, largely because surface scientists were almost exclusively interested in atomic-scale phenomena. Since the best available theories indicated that the STM was incapable of atomic resolution, there was little reason for surface scientists to adopt it.

One reason Binnig did approach his surface science colleagues was that he suspected that the resolution of the STM might be much better than conventional calculations indicated. Those calculations were based on a physical model of the STM in which the tip ends in a smooth curve with

a constant radius of curvature. Binnig's insight was that, for almost any metal tip, the radius of curvature is not constant at the atomic level. One atom at the end of the tip must stick out closer to the sample than all the rest. Because the tunneling current is exponentially dependent on the distance between the tip and the sample, almost all of the tunneling current would go through that one outermost atom.⁵⁸ Thus, the beam of tunneling electrons with which an STM "feels" a surface could be less than an atom wide, allowing the STM to distinguish between individual atoms.

To explore this possibility, by mid 1981 the STMers were asking IBM surface scientists what surfaces they most wished to inspect at the atomic scale. The answer they got was something called the silicon (111) 7×7 .⁵⁹ This was a surface that had been known since the 1950s, but its particular importance at Zurich in 1981 had much to do with changes that had occurred in surface science in the past ten years. The 1970s had seen a rapid increase in the computing power available to some surface scientists—particularly those at IBM, Bell Labs, and a few other corporate research centers. These also happened to be places with a keen interest in semiconductor microelectronics. Though surface science as a whole was split evenly between metal and semiconductor specialists, semiconductor researchers were more numerous and more prominent at IBM and at Bell Labs. By the early 1980s, these corporate semiconductor surface scientists had elevated certain computing-intensive questions to commanding importance in their field.⁶⁰ Perhaps the most central of these questions concerned the atomic structure of "surface reconstructions." Bulk crystals are composed of repeating units of small numbers of atoms—a "unit cell." The atoms in one unit cell may bond to each other, but they also bond to atoms in the neighboring cells. At the surface, however, atoms no longer have a unit cell above them with which to bond. In metals this is relatively unimportant, since electrons in metals are pooled into a "sea of electrons." In semiconductors, however, electrons remain closely associated with an atom and, in the language of surface science, would "prefer" to provide a bond to another atom. If they cannot (as at the surface), they form a "dangling bond"—an energetically unfavorable state. To minimize dangling bonds and move to a lower potential energy, semiconductor atoms reposition themselves ("reconstruct") into a different geometry at the surface than in the bulk material.

Glimpsing reconstructions requires an extremely clean surface—otherwise impurities disrupt the underlying unit cells and absorb the dangling

bonds. Studying surface reconstructions requires both a sophisticated, expensive ultrahigh-vacuum chamber and the experimental skill to prepare a clean sample. Even then, reconstructions are elusive. In the 1970s there was no microscope that could directly visualize where the atoms were in the semiconductor samples that corporate surface scientists were interested in. Thus, they relied on indirect methods—primarily various kinds of diffraction (shooting particles at a surface and watching how they bounce around before they come out) and spectroscopy (shooting particles or electromagnetic radiation at a surface with a range of energies and observing the range of energies at which particles and radiation are emitted back out of the surface).

The 1970s saw the invention of many new diffraction and surface spectroscopy techniques—what surface scientists refer to as their “alphabet soup” of instrumentation.⁶¹ Because none of these techniques gave complete information about the positions of atoms in a reconstruction, researchers coordinated results from many different instruments. Surface scientists became adept at inventing new techniques and then using them to learn more about the most interesting surface reconstructions. To define which surface reconstructions were “interesting,” surface scientists relied most heavily on one technique: low-energy electron diffraction (LEED). Other instruments, such as various kinds of ion diffraction or energy-loss spectroscopy, were useful in understanding reconstructions, but in the early 1980s LEED was “the major modern source of surface atomic geometries.”⁶²

When Binnig and Rohrer asked surface-science colleagues what samples to look at, they were directed to a surface reconstruction with a famously mysterious LEED pattern: the silicon 7×7 . LEED had long ago revealed that this was a very large surface unit cell, and that its atoms were arranged in a particularly complex manner. Over a period of 20 years or more, many different instruments and analysis methodologies were used to study this reconstruction, yet its structure remained unknown. Most surface scientists felt that results from these various instruments must, in some way, be compatible. As long as the structure of the 7×7 remained unsolved, however, it was unclear whether the toolkit of surface science could be used to determine the structures of very complex reconstructions. Later, when a 7×7 model was agreed upon that was compatible with data from the full menu of instrumentation, some saw this reconstruction as a “Rosetta Stone,” the deciphering of which indicated that even the most difficult surface

reconstructions could then be solved.⁶³ Worldwide, by one prominent surface scientist's estimate, between \$100 million and \$250 (in 1980s dollars) was spent on deciphering the mystery of the 7×7.⁶⁴

The growing body of knowledge about how to prepare and study the 7×7 made it the "fruit fly of surface science."⁶⁵ Molecules and metals were deposited on top of it, new kinds of instruments were tested on it, and graduate students became surface scientists by learning how to prepare and study it. Any new instrument that revealed something novel about the 7×7 could be assumed to reveal information about all the important reconstructions. Thus, it was no surprise that this was the surface Binnig and Rohrer were directed to.

Not being surface scientists, Binnig and Rohrer lacked a full understanding of that discipline's techniques for preparing 7×7 samples. Nor did they always adhere to the conventional wisdom of surface science. For instance, surface scientists "knew" that the 7×7 had to be kept under pristine UHV conditions at all times for the reconstruction to be seen. Binnig and Gerber prepared their 7×7 samples in one vacuum chamber, took them out, carried them down the hall by hand, and placed them inside the vacuum chamber housing the STM. Not surprisingly, they had difficulty making a 7×7 sample that would yield clear STM images.

After a few months, Binnig and Rohrer began casting about for a less finicky surface reconstruction. Probably on the advice of Karl-Heinz Rieder (an IBM Zurich surface scientist who specialized in metals), they turned to the gold (110) 2×1—a much simpler, less dramatic reconstruction that few surface scientists were interested in. However, gold is more chemically inert than silicon and therefore easier to keep clean, which allowed the team to push the limits of the STM without pushing their own limits as preparers of specimens. By the summer of 1982, they had images of gold that purported to show the atomic geometry of its surface reconstruction. They sent off an article to *Physical Review Letters* and, while they waited for a reaction, moved back to the silicon 7×7.⁶⁶

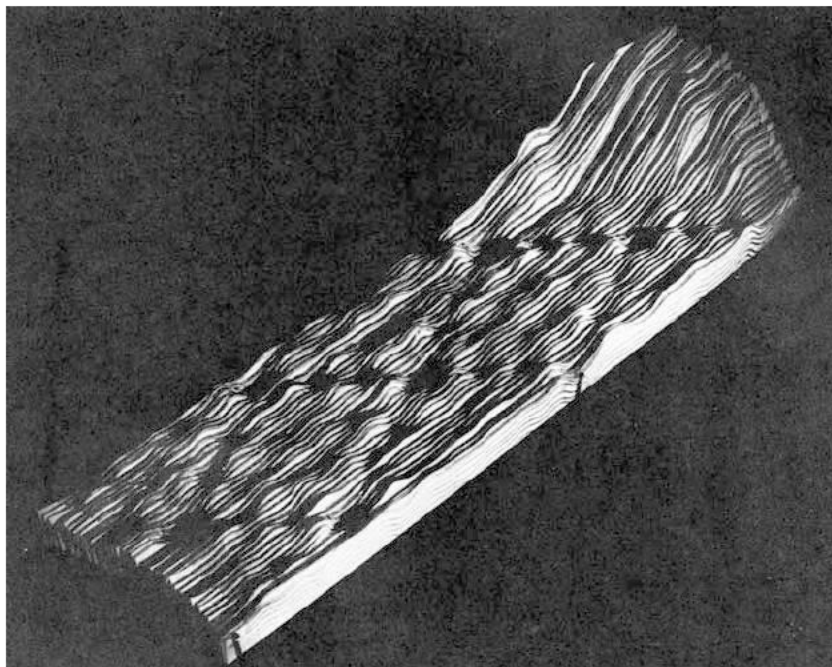
Most surface scientists found little of interest in Binnig, Rohrer, Gerber, and Weibel's gold paper. Surface scientists who were aware of the paper either doubted that it showed what the STMers thought it showed (an atomic reconstruction) or believed that the gold 2×1 was a trivial case in which the reconstruction was not very dramatic and was already well understood and that therefore the STM didn't add much.⁶⁷ Thus, imaging

the gold 2×1 brought the Zurich team no closer to convincing the wider surface-science community to adopt, or even be interested in, the STM.

However, intramurally Binnig and his colleagues took advantage of their personal acquaintance with IBM's surface scientists. In returning to the 7×7, one particularly important contact was Franz Himpsel, an IBM Yorktown surface scientist specializing in photoemission spectroscopy who was visiting Zurich for a few weeks. Himpsel gave the STMers advice on recipes for preparing silicon. That advice soon paid off.⁶⁸ By fall of 1982, the STM had begun to reveal tantalizing regularities in the 7×7's surface. Binnig and Rohrer knew of models of the 7×7 that predicted that at the corners of adjacent surface unit cells there would be a large gap between the adatoms of the adjoining cells—a so-called corner hole. These corner holes were close in size to the plausible resolution of the STM. Thus, when Binnig and Gerber began seeing a pattern of regular depressions in their STM images, they concluded that these were the corner holes and calibrated accordingly. From there, Binnig pressed forward—in the unorthodox conviction that the STM was capable of even higher resolution—until he saw a series of regular bumps in the chart recorder that he believed could only be traces of individual atoms (see figure 2.3).

Roots of Success

Nearly overnight, the fortunes of the STM rose dramatically. For the next few years, presentations on the STM at international conferences drew standing-room-only crowds. Looking back in 2010, Christoph Gerber described the emotional impact of the reception of the 7×7 results—"the reaction and response from the scientific community . . . was enormous. Certainly one of the precious moments in a lifetime. I still feel the shivers."⁶⁹ Yet what, exactly, elicited the new enthusiasm for the STM, especially among surface scientists? It is often claimed in popular writings on nanotechnology that the reason the STM is so special is that with it "the direct 'visualization' of individual atoms first became possible."⁷⁰ That claim, however, requires considerable qualification. In fact, the STM was not the first instrument, or even the second, to "see" individual atoms—the field ion microscope had been doing so since the 1950s, and the transmission electron microscope since the 1970s.⁷¹ Moreover, Binnig and Rohrer had claimed that the STM could achieve "true three-dimensional



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Figure 2.3

The IBM Zurich STM team's celebrated image of the silicon 7×7 (actually a photo of a three-dimensional model). Christoph Gerber took the traces from the STM's chart recorder, glued them onto cardboard, cut out the cardboard-backed traces, then glued the stack of traces together in sequence. Reprinted, with permission from Elsevier, from Gerd Binnig and Heinrich Rohrer, "Surface Imaging by Scanning Tunneling Microscopy," *Ultramicroscopy* 11 (1983): 157–160 (copyright 1983).

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topography of surfaces on an atomic scale" months before they published their 7×7 results, yet very few people found that claim interesting.⁷² Rather, it was the STM's ability to image the *particular* atoms of the 7×7 that turned the Zurich team into stars and convinced surface scientists to adopt the instrument.

The STM didn't, however, "solve" the atomic structure of the 7×7 surface reconstruction. In their first paper on the subject, Binnig and Rohrer (with the help of a Zurich theorist, Alexis Baratoff) provided a new model for the surface reconstruction based on their atomic-resolution image. However, some flaws in their model were quickly pointed out, and the 7×7 surface reconstruction was not solved to the discipline's satisfaction until 1985,

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when Kunio Takayanagi brought transmission electron diffraction results to bear on data from a variety of other instruments.⁷³ The STM certainly contributed to the determination of the 7×7 's structure, by pinpointing the locations of the adatoms on the top of the surface unit cell. The 7×7 's surface unit cell, however, extends several atomic layers into the surface. The STM could not be used to locate all the atoms in the surface reconstruction.

Thus, the first STM images of the 7×7 caught surface scientists' eyes not because they *answered* the question, but because they allowed the discipline to actually see what it had talked about for so long. After decades of having only indirect data, and having to visualize reconstructions in the "inverse space" offered by LEED, surface scientists now had a "real-space" image against which to compare their models. One early STMer, Jun Nogami, recalled: "As a surface scientist . . . teasing out aspects of surface structure through indirect means, when I saw those atoms traced out [with an STM] in green lines on the CRT screen, I immediately knew that . . . I had to get into [tunneling microscopy]."⁷⁴

Despite the euphoria, surface scientists didn't give up their indirect methods with the introduction of the STM. For one thing, they knew that an STM image didn't tell the whole story of a reconstruction. In fact, it quickly became apparent that STM images could be misleading: what appeared to be variations in topography might actually be variations in electronic structure (and vice versa). Surface scientists, however, had sophisticated mechanisms for recognizing, and working around, such pitfalls of new instrumentation. That task of evaluating the STM's relevance to surface science and incorporating it into the discipline was quickly taken up after the 7×7 , especially (as we will see in the next chapter) at IBM and Bell Laboratories.

The STM's inventors, meanwhile, were not very interested in becoming surface scientists themselves. In fact, they were sometimes impatient with the discipline in general and with some of its practitioners at IBM's US labs in particular. It is not too much of an exaggeration to say that the Zurich STMers' approach was to make quick demonstrations of the microscope's relevance to various disciplines and then allow those disciplines to polish the rough edges. The first applications they concentrated on (imaging surface reconstructions and, later, scanning tunneling spectroscopy of surface states) happened to be important to surface science. But they quickly moved on to applying the STM to electrochemistry, biophysics, and other

areas, thereby attracting more (and more diverse) members to their fast-growing instrumental community.

Nevertheless, surface scientists' interest in the STM was useful *organizationally* to the Zurich team. Because of the prominence of surface scientists at IBM Research headquarters at Yorktown, the 7×7 images instantly committed the company to growing an STM community. IBM Research put the STM high on the Zurich lab's agenda, and many researchers there soon took up the new instrument: Dieter Pohl, Urs Dürig, James Gimzewski, Giorgio Travaglini, S. E. Alvarado, Bruno Michel, et al. Some of these people (e.g., Gimzewski) continued using the STM for surface-science research. Others took it into other fields, especially biology. Binnig, Rohrer, and Gerber, meanwhile, were given a great deal of liberty, which they used to concentrate on building an instrumental community by traveling as emissaries, organizing community-building activities, and inventing modifications to the STM to make it more friendly to other disciplines and instrumental communities.

Through their experience with the 7×7, Binnig, Rohrer, and Gerber learned successful techniques for recruiting a discipline into their instrumental community. They applied those techniques over and over in later years. Inventing a reliable *instrument* was, of course, an important first step. The STM would have gone nowhere without the Zurich team's (especially, by most accounts, Binnig's and Gerber's) extraordinary experimental skills. Technically, the STM was a more sophisticated instrument than the Topografiner. Yet we cannot explain the STM's success and the Topografiner's failure solely by pointing to their differences in apparatus. Had Russell Young persuaded a critical mass of precision engineers, surface scientists, or tunnel junction specialists of the Topografiner's immediate value, they might have given NBS management a reason to allow the program to continue until the Topografiner matched the STM's capabilities. The Topografiner's poor resolution resulted in part from a lack of a good vacuum chamber and other technical problems. Yet Young's inability to amass the resources needed to improve the Topografiner's characteristics was, in turn, a product of his inability to assemble a constituency for the instrument.

In contrast, the Zurich team (especially, by most accounts, Heinrich Rohrer) demonstrated great skill in growing a constituency for the STM. An indispensable element of that constituency building was the soliciting of advice that the Zurich STMers could use both to improve STM technology

(e.g., finding samples with which to calibrate the microscope's piezos) and to link it to networks of practitioners inside and outside IBM. At first, the STM was linked into such networks in piecemeal fashion, through a few individual colleagues who gave the STM their support. During that time, the STM team had to scramble to ensure their instrument didn't meet the Topografiner's fate. With the 7×7, however, the STM was suddenly linked into a large disciplinary network, leaving Binnig, Rohrer, and Gerber free to move in new directions.

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3 Adopting, Adapting, Departing: Early STM at IBM and at Bell Labs

Scientists at IBM Research and at Bell Labs were not the only early adopters of the STM, but in the mid 1980s one could get that impression. For instance, Jim Murday, a program officer at the Office of Naval Research who funded many surface scientists and probe microscopists (and who assembled his own surface-science STM group at the Naval Research Lab), notes that when he compiled statistics on which nations sent participants to early STM conferences, he listed IBM as a country all by itself.¹ If we think of the STM community as a network that, at the beginning, had Binnig, Rohrer, and Gerber as its central nodes, the view from that center largely confirms the dominance of IBM and Bell Labs in those early years. For instance, in 1986, when it was still possible to comprehend everything that members of the STM community had published and to weight their work by quality, Binnig and Rohrer reviewed the literature in the *IBM Journal of Research and Development*.² In that review, about a quarter of the citations to tunneling microscopy research were of works co-authored by Binnig and Rohrer themselves. About another quarter were by others at IBM Zurich (or who had been there when the STM was invented), and a little more than a quarter were from Bell Labs or from IBM's US research centers. The remaining quarter was a mixed bag, though one can identify four university groups that were emerging as important nodes in the STM network: the University of Basel, the Universidad Autónoma de Madrid, Stanford University, and the University of California at Santa Barbara.

This chapter focuses on the first few years of STM, when IBM and Bell Labs were the giants of research in tunneling microscopy. It was in those years that an STM community came into being. At the beginning of the period, STM was carried on, in relative isolation, by people whose knowledge of the technique came almost entirely from Binnig and Rohrer.

Between 1984 and 1987, however, STMers established an annual conference series and other mechanisms that allowed microscopists outside Zurich to form connections with each other, rather than relying solely on their ties back to Zurich.

The period was brief, but it left an enduring mark on the probe-microscopy community. In particular, it established a lasting distinction between surface-science STM and other varieties of probe microscopy. That distinction was never abrupt—surface-science STMers interacted amicably with other probe microscopists, and a few eventually transitioned to AFM and other variants. Yet most surface scientists held quite different views than the rest of the community about the proper scope, application, and design of probe microscopes. In the early 1990s, some of these surface-science STMers became dissatisfied enough with other kinds of probe microscopy that they tried to secede and form a new community. As we will see in chapter 6, other probe microscopists—those who opposed such balkanization—latched onto the label “nanotechnology” as a way to consolidate their instrumental community. By the end of the 1990s, nanotechnology had become a valuable resource for surface scientists as well, and many early surface-science STMers are now widely recognized leaders in nanotechnology policy and research.

This perhaps temporary and certainly never complete disaggregation of surface-science STM from the rest of probe microscopy came about, in part, because of the way the original Zurich team’s work was first replicated. As Binnig and Rohrer’s 1986 literature review shows, nearly all the other Zurich STMers learned the technique through sustained, cooperative interaction with the inventors. In fact, nearly all the Zurich papers cited in that review were by people who co-authored other papers with Binnig and Rohrer. Similarly, the four university groups that can be seen in that review to have been emerging as centers of probe microscopy (those in Madrid, Basel, Palo Alto, and Santa Barbara) all had close ties to Zurich. Nicolás García and Arturo Baro brought the STM to Madrid after sabbaticals at IBM Zurich, and co-authored extensively with the Zurich team in the mid 1980s. Hans-Joachim Güntherodt’s group at Basel was less than 50 miles from, and interacted frequently with, the Zurich lab. The Santa Barbara and Stanford groups became major contributors to probe microscopy with the help of prolonged visits by (and collaborations with) members of the Zurich team.

Replication of the STM at Bell Labs and at IBM's US facilities proceeded quite differently, however. In this early period, some scientists at those sites did need to talk face to face with Binnig, Rohrer, or Gerber before they could successfully build an STM, but prolonged interaction doesn't seem to have been an absolute requirement for replication. Virtually no one at Bell Labs or at IBM's US sites co-authored or collaborated extensively with members of the Zurich team. Once surface-science STM gained a foothold at Bell Labs and at IBM's US research centers, it spread quickly within those organizations with limited input from Zurich. Surface scientists already formed a dense network within and across IBM and Bell Labs that facilitated the replication and adaptation of STM. Their common ties to that surface-science network meant these STMers oriented more to each other than to other parts of the probe-microscopy community.

The replication of techniques has been studied often by historians and sociologists of science. This chapter draws on that literature's demonstration that replication is rarely a mechanical, effortless process.³ Usually a scientist seeking to replicate a technique must interact face to face with someone who already possesses the technique in order to acquire the "tacit knowledge" required to successfully reproduce the original results. Even then, techniques are rarely replicated without distortion—even if perfect reproduction were possible, it is usually not desirable since techniques must be adapted to local conditions and objectives.

Most studies of replication, however, focus on the transfer of a technique from one site or organization to another, rather than on replication within a single site or organization. After all, when an experimental technique is still in its early, poorly replicable phase, most research organizations will not encourage more than one or two of their lab groups to commit to such an undependable enterprise. Site-to-site transfer is therefore important in the early growth of most instrumental communities, including probe microscopy. But occasionally conditions can encourage the multiple adoption of a technique within a single organization even in the tenuous period when replication requires significant commitment of resources. In probe microscopy, the specific configuration of surface science within—and between—IBM and AT&T made it both desirable and possible for STM to be repeatedly replicated in those organizations. Yet replication was not simple duplication; in fact, IBM and Bell Labs surface scientists were modifying and adding to the technique every time they

reproduced it. By examining the growth of surface-science STM, we gain a better understanding of how organizations make use of disciplines to foster replication *and* innovation.

Once replication of the STM had been achieved repeatedly within IBM and Bell Labs, many of the early adopters of the technique moved on to positions outside those companies. Their involvement with STM gave them unique qualifications that vaulted many of them to the forefront of the discipline of surface science. As they moved away from IBM and Bell Labs, they nucleated new sites for surface-science STM. Their demonstration of the STM's relevance to surface science caused the technique to proliferate. Eventually, that proliferation profoundly changed surface scientists' conceptions of what counted as a disciplinarily interesting question. Thus, in examining the growth of surface-science STM, we also gain a better understanding of how disciplines make use of organizations to foster the proliferation of techniques and the generation of knowledge.

Basic Research in a Corporate Environment

IBM and AT&T were commercial entities, so it would be reasonable to assume there was some economic motivation for their adoption of tunneling microscopy. Yet neither company ever made any direct profit by manufacturing STMs to sell on the market. Replication of the technique within these companies was not a first step toward mass production of the STM. Nor did IBM or AT&T make much money off their intellectual property in probe microscopy relative to how much they initially invested in the technique. When the STM was invented, both companies were still bound by consent decrees from the US Department of Justice that restricted their ability to defend, or profit from, patents on their research.⁴ Those consent decrees eventually expired, and IBM tried to collect license fees on its STM patents. Yet the expectation of such fees played little or no role in encouraging the initial spread of tunneling microscopy within IBM or Bell Labs.

Even if AT&T and IBM didn't plan to sell STMs (or STM patents), did they hope to derive economic benefit from the STM by using it in manufacturing? The STM was invented, after all, partly in hopes of improving the production of Josephson junctions for IBM's superconducting supercomputer. Similarly, the gold nano-map described in chapter 1 was created partly in hopes that STMs could be used to read and write digital data.

It is clear that some probe microscopists at Bell and IBM justified their adoption of the technique by pointing to its eventual use in quality control or in direct manufacture of commercial products. However, manufacturing applications do not seem to have driven the initial spread of STM within these organizations. Virtually all of the IBM and Bell Labs papers cited in Binnig and Rohrer's 1986 review described uses of the STM for esoteric surface-science research far removed from manufacturing. The IBM and Bell Labs researchers who eventually developed manufacturing applications for probe microscopy did so *after* the STM had already been taken up for basic surface-science research. To some extent, the adaptation of STM for answering esoteric surface-science questions established an organizational infrastructure that aided the later turn toward more applied versions of probe microscopy.

The initial spread of STM within IBM and AT&T, then, was justified by pointing to its relevance for solving basic surface-science questions with only indirect, prospective relevance to these companies' bottom lines. Such justifications were credible because when the STM was invented IBM and AT&T were still committed to large-scale basic research. In 1981, Bell Labs' total budget was \$1.6 billion (\$3.7 billion in 2009 dollars), about 10 percent of which was allocated to longer-term research rather than shorter-term technology development or trouble-shooting.⁵ Bell Labs employed about 22,000 people at the time, more than 1,300 of them in its basic research departments.⁶ IBM's research and development spending and employment were of a similar order.⁷ It was generally recognized at the time that such high levels of support ensured "that Bell Labs was the world's premier institution doing condensed-matter physics and a leader in some other fields too."⁸ Likewise, there was general agreement that IBM Research was a close competitor of Bell Labs in a few areas of basic research on condensed matter—including surface science.

Basic corporate research was, and is, a contentious issue.⁹ Kumar Patel, the director of physical research who oversaw most of Bell Labs' early STM work, took great pains to insist that AT&T "funded fundamental research in the past because it was a sound business decision and not out of charity or as a means of supporting a luxury."¹⁰ Such denials were necessary because basic research could seem wasteful, its benefits too delayed and unpredictable for the corporate environment. As the Bell Labs Nobel laureate Philip Anderson put it, basic research presented

the problem of infinity multiplied by zero. The probability of producing something which develops into a major change in technology—something like the transistor—is very low. That is the zero in the equation. On the other hand, if you do discover something like the transistor, the benefits not just to the Bell System but to the entire world are incalculable. That's the infinity in the equation.¹¹

For Anderson, the possibility of a commercial breakthrough (an “infinity”) justified Bell Labs’ “maintain[ing] a certain cadre of people who are free to look where they think it’s worth looking” rather than just doing work “directly tied to mission-oriented operations.”¹²

Whatever its ultimate payoffs, basic research presented a short-term dilemma for corporate managers: how to manage a cadre that was free to define its own goals, and therefore somewhat independent from direct corporate guidance? The solution, in large part, was for companies to rely on the larger scientific disciplines that members of their basic research cadre belonged to. Both IBM and Bell Labs used the opportunity to do basic research to recruit top graduates from academic departments. Those people were then given significant leeway: as the Bell Labs vice president and Nobel laureate Arno Penzias put it, “we hire bright people, point them in the right direction, and then get out of their way.”¹³ Yet those junior researchers knew they would be judged on their ability to solve questions of importance to members of their disciplines, win awards from their disciplines’ professional societies, publish in the journals rated highly by members of their discipline, and train new practitioners of their discipline—much the same evaluation criteria used in discipline-based academic departments.²⁴⁶⁹¹

Thus, as Penzias articulated, “if you don’t produce outstanding research, you’re not going to get much support the next time resources—money, lab space, technical help—are allocated.”¹⁴ Those who did work judged as “outstanding” by their disciplines were promoted to senior management positions, where they could judge and reward their junior colleagues’ basic research on the same disciplinary criteria. That is, basic researchers at Bell Labs and at IBM—including the early STMers—were indeed “free to look where they think it’s worth looking,” but they knew that freedom depended on impressing senior members of their disciplines both inside and outside their organizations.

Discipline-based evaluations were clearly not the only means of judging corporate basic research. Nor did the fact that corporate basic researchers were nominally “free” to be guided by curiosity mean that they were not

also influenced by their firms' "mission-oriented" agenda. Many consulted or collaborated regularly with their mission-oriented colleagues. Managers heaped public praise on those researchers who had identified commercial possibilities in their fundamental research and shepherded their discoveries toward the product line. Neither Bell Labs and IBM managers, nor researchers, nor I would recognize an absolute distinction between basic and mission-oriented work; in Penzias' words, "each feeds on the other."¹⁵ Yet "basic" and "mission-oriented" were sufficiently clear categories for researchers and managers to routinely deploy. One important characteristic in distinguishing those categories was the greater reliance on discipline-based evaluation in basic research.

Some executives from the manufacturing arms of both IBM and AT&T argued, however, that the discipline-oriented metrics for evaluating basic research robbed scientists of the desire, or even ability, to work toward corporate goals. For instance, Joseph Logue, the manufacturing-oriented manager who took over IBM's Josephson computing program in 1981, lamented that discipline-centered metrics gave researchers too much autonomy and too little cognizance of organizational needs:

One internationally known metallurgist thanked me for what he had learned on the [Josephson] program. He told me that before I took over, he had investigated whatever subject caught his attention. When I came on board, he found that he was asked to target his attention to subjects that were important to the overall program. I thanked him very sincerely, but it caused me to think about the structure of the program when I joined it. What I found was that many of the 50 PhDs on the team were more interested in work that would lead to an individual publication than in working as a team to advance the program.¹⁶

As we will see, such criticisms gained traction over the course of the 1980s. As AT&T lost its regulated monopoly status and IBM's market dominance softened, both firms "reverse[d] the trend toward university-like research."¹⁷

When the STM first arrived in the United States, though, discipline-based basic research was still strong at both IBM and Bell Labs. Researchers perceived significant incentives to investigate questions of importance to their disciplinary peers, even when the relevance of those questions to their employers' needs was ambiguous. Managers saw the ability to answer those questions as an important criterion in evaluating both techniques and personnel. Managers compared researchers to their disciplinary peers both inside the organization and at other leading organizations in that field. In surface science, a field in which Bell Labs and IBM Research were

perhaps the top organizations, researchers from each firm monitored—and tried to leap ahead of—developments at the other. Once the STM was ratified as an important tool of surface science, therefore, its proliferation within these organizations was almost inevitable.

The Early Adopters

Surface science's gatekeeping function can easily be seen in the way tunneling microscopy was first adopted at Bell Labs and at IBM's US research facilities. When the technique was initially being imported from Zurich, established surface scientists held back until the STM's relevance to their discipline had been demonstrated. Junior researchers therefore took on the task of proving both their own surface-science capabilities and the STM's to those senior researchers.

For instance, the most important early theoretical understanding of STM images was developed by a Bell Labs postdoc, Jerry Tersoff, with the guidance of a senior theorist, Don Hamann. In late 1982, Hamann was asked to referee Binnig and Rohrer's *Physical Review Letters* article on the silicon 7×7 . As he puts it, "I looked at that and I said 'Hot damn, this is so exciting.'"¹⁸ But, as a referee, Hamann couldn't act on the 7×7 data until they had been published. He knew, however, about the earlier (in his words) "hohum" STM images of gold that were already published. He tasked Tersoff with using the Zurich team's gold (110) data to derive a simple, easy-to-understand theory of how an STM images surfaces. Whereas many experimentalists considered the STM theories then coming out of Zurich too complex to be useful, the Tersoff-Hamann approximation quickly became a handy heuristic for surface-science STMers.¹⁹

Note the importance of the silicon 7×7 in stimulating Tersoff and Hamann's interest in the STM. Hamann *could* have taken action when he first saw Binnig and Rohrer's work on gold. Gold, however, was not of great importance to the network of surface scientists at Bell Labs in the way that the 7×7 was. Yet this was not because the 7×7 was of significantly more direct practical relevance to AT&T. True, silicon was a commercially important material for the telecommunications giant; but the silicon used to make integrated circuits has a different crystalline orientation than the 7×7 —so fundamental knowledge of the 7×7 didn't translate easily to improved methods for making microchips. Moreover, research into the

atomic geometries of surface reconstructions was predicated on a much higher degree of cleanliness and precision than the microelectronics manufacturing of the early 1980s. Semiconductor firms today are approaching atomically precise manufacturing, but at the time basic research in surface science was decades ahead of manufacturing capabilities.

Fundamental knowledge of surface reconstructions was certainly not irrelevant to manufacturing, but its relevance was largely indirect or mediated. One prominent surface scientist at Xerox sums up corporate research on semiconductor reconstructions as “an elaborate recruiting and socialization scheme,” a “funnel” for transferring useful skills (e.g., familiarity with ultrahigh-vacuum equipment) to new personnel and moving some of them to more mission-oriented parts of the organization.²⁰ Such diffuse objectives, however, did little to specify the content of corporate surface science. Practitioners were relatively free to elevate some questions over others on disciplinary criteria rather than on the criterion of relevance to manufacturing. The 7×7 was “probably the most studied of the reconstructed surfaces of semiconductors,” a “long-standing question of fundamental importance for semiconductor surface physics,” more because of its clear, intricate LEED signature and its perplexing indecipherability than its importance in manufacturing.²¹ Yet those disciplinary criteria mattered a great deal. A new technique that shed light on a “ho-hum” reconstruction counted for little in the eyes of senior surface scientists like Hamann, but a technique that shed light on the 7×7 commanded instant attention.

Thus, without the 7×7 it would have taken much longer for the first STM to be built at Bell Labs. In 1982, a young x-ray physicist, Jene Golovchenko, moved into a Bell Labs surface physics group and began looking for a new project. Golovchenko hadn't been trained as a surface scientist and therefore “didn't have a very big investment in surface physics,” even if his short-term prospects depended on proving himself in that field.²² As a result, the standard techniques of surface science, particularly LEED, held less attraction for Golovchenko than for more established surface scientists: “I hadn't been brainwashed into [believing] something [i.e. LEED] was *the answer*.”²³ That may have made him more willing to take a risk on an unproven instrument to which established surface scientists were not yet ready to commit.

Golovchenko learned about the STM “a little before the big home run” (i.e., the silicon 7×7), and decided it “had all of the romance of being a challenging instrument to make work.”²⁴ He assumed this would be an easy

project, largely because his previous x-ray experiments involved very fine motion control using piezoelectric crystals—much like the piezoelectric motion control for the STM tip. His supervisors, however, were reluctant to provide the resources needed to build an STM. For them, Golovchenko's lack of identification with surface science counted against him. They assumed that if "this was really a worthwhile direction to go in . . . some of the people more ensconced in surface physics at Bell Labs would've done it."²⁵ In fact, at least one Bell Labs surface scientist, John Arthur, had tried to reproduce some of Russell Young's work in the 1970s, but his colleagues had "greeted [the Topografiner] with a ho-hum attitude."²⁶

Their response in the 1980s was no different, and Golovchenko's proposal was put on hold. In desperation, Golovchenko invited Rohrer to give a talk at Bell Labs. Between the invitation and the lecture, word of the first 7×7 papers began to spread. As a result, Rohrer spoke before a packed crowd. Golovchenko: "I was just so thrilled because when I started I didn't really detect much support, and now people were sitting in the aisles in this auditorium. It was the most crowded I'd ever seen it."²⁷ From that point on, Golovchenko's supervisors committed both funding and substantial political capital to setting up an STM program.

Before the 7×7, managers at IBM's flagship lab, the Thomas J. Watson Research Center (often referred to informally as IBM Yorktown), were marginally more interested in importing the STM than their Bell Labs counterparts. However, the best path for tunneling microscopy to cross the Atlantic was unclear. Yorktown management, and Binnig and Rohrer themselves, may have thought that electron microscopists would be the group most interested in, and equipped for, building an STM. Thus, Oliver Wells, a senior Yorktown electron microscopist, went to Zurich to learn the basics and recruited a postdoc, Mark Welland, to help him build an STM. Yet this effort never came close to repeating the Zurich team's triumphs. After the 7×7 made it clear that surface scientists were the constituency most interested in STM within Yorktown, Welland drifted into the group of Joe Demuth, a senior Yorktown surface scientist who was beginning an STM program.

The first successful replication of the STM at Yorktown was done, not by an electron microscopist or by an established surface scientist, but by Randy Feenstra, a newly hired staff scientist fresh from graduate school at Caltech. Like Golovchenko, Feenstra had no training in surface science. But he could see that his own field, the physics of defects in semiconductors,

was (in his words) “mature.”²⁸ Feenstra believed surface science was “still on the upswing, it was by no means a mature field. . . . Most of the problems were unsolved at that time.” Thus, like Golovchenko, Feenstra was looking for a project that would introduce him to surface science, but which more senior colleagues were too cautious to attempt.

While traveling with a large Yorktown delegation in the summer of 1982, Feenstra visited the Zurich lab and saw the STM. Even though the Zurich STM was still notoriously difficult to operate at that point, and hadn’t yet revealed anything that most surface scientists found very interesting, Feenstra was interested. At dinner that evening, he later recalled,

Seymour Keller who hired me at IBM . . . said “well I think somebody from Yorktown should come here and look at the STM and go back to Yorktown and build one.” I said “I’ll do it!” Really, it was just like that. He sort of looked and said “why don’t you think about it a little bit?” So I thought about it and I still wanted to do it, because it was surface science, it was something new, it looked like a good project.²⁹

Within a few months, Feenstra was back in Zurich, like Wells, to learn the basics of STM.

Replication

In fact, a long succession of STMers made what Jim Murday calls the “pilgrimage to Zurich.”³⁰ Many found it useful just to take a look at the original microscope and ask basic design questions. But it didn’t seem to matter whether these visitors spent six months or an afternoon in Zurich. Feenstra’s pilgrimage was particularly complicated by overblown promises by Yorktown management that Feenstra knew enough surface science to help the Zurich team with its sample-preparation problems. More generally, early visits to Zurich were hampered by the slow, unreliable state of the instrument. “Back then,” Feenstra later recalled, “it was damn slow and very painstaking. . . . But nevertheless I fiddled around and learned about the instrument and had a good time, went out drinking beer with the people from Zurich. It was fun.”³¹

By the time Feenstra returned to the United States, IBM management seems to have learned two lessons. First, initial replication of the STM at IBM’s US sites should be undertaken by surface scientists. Second, trips to Zurich should be kept short. When the IBM lab in San Jose (known as IBM Almaden) assembled an STM group in 1983, management hired two surface

scientists: Robert Wilson, just completing a postdoc at Bell Labs, and Shirley Chiang, just finishing her PhD work at Berkeley. Wilson went to Zurich for a few weeks, before he and Chiang had gotten very far. Later, Chiang stopped by the Zurich lab for a day, as a side trip during her honeymoon.³²

All of these IBM groups in North America struggled at first to build a working STM. Wells eventually gave up on his STM. Feenstra and the Wilson-Chiang team toiled for nearly two years without achieving atomic resolution—the yardstick of whether one had a working STM.³³ Jene Golovchenko could not get atomic resolution either, nor could three other Bell Labs scientists—Joe Griffith, Young Kuk, and Russell Becker—who began building STMs soon after him. The delay in replicating the STM made life difficult for these junior researchers. As Joe Griffith remembers it, Kumar Patel came back from seeing a rudimentary STM at Stanford

and announced “if a Stanford graduate student could build an STM in six months an MTS [member of the technical staff] at Bell Labs should be able to do it in a week-end.” So we all said “okay, we’ll do that.” And *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 Rohrer were having difficulties getting it to work again.³⁴

What Patel didn’t know was that the STMers at Stanford—and Basel, Madrid, Santa Barbara, and everywhere else—were also struggling with atomic resolution. Building “an STM,” and even getting it to do stationary vacuum tunneling, seems to have been a relatively easy task in this period. The secret to getting an STM to image single atoms, however, refused to travel from Zurich through the end of 1984.

New STMers who went to Zurich were apparently unable to bring this secret back with them. One alternative was to bring Zurich to the new STMers instead. In 1985–86, Binnig and Gerber traveled extensively, helping new STMers achieve atomic resolution. As one such researcher remembers it, his STM “was sort of running but not running very well [until Binnig visited], flipped some switches and played with it, and, wow, magically this beautiful image just popped out.”³⁵ Another way of transferring the tacit knowledge needed for atomic resolution was for a group of new STMers to meet with a member of the Zurich team. But there were so many inter- and intra-organizational rivalries at IBM and at Bell Labs that none of their staff members moved to convene such a meeting.

The call for an emergency “workshop” came from Calvin Quate, a Stanford professor of electrical engineering and applied physics. Quate had spent most of the 1950s at Bell Labs and had a large network of former students and postdocs at IBM, so he knew what the corporate STMers were facing and could act as a neutral party. In late 1984, he convinced a former student, Alex de Lozanne, to organize a small meeting in Cancun for about a dozen aspiring STMers plus Heinrich Rohrer. Despite the scenic surroundings and the wintry conditions back home, most of the attendees say they were too focused on the difficulties they were having with their STMs to enjoy the resort. Instead, they sat in a hotel suite and gave a series of frustrated, shoulder-shrugging presentations describing their STMs and trying to figure out how to get atomic resolution.³⁶ Attendees included Feenstra from Yorktown; Golovchenko from Bell Labs; Wilson from Almaden; Bob Jaklevic from Ford; Quate and de Lozanne; Paul Hansma from the University of California at Santa Barbara; Rohrer; Nico García from Madrid; John Clarke, a Berkeley physicist; and Lynn Swanson, a field emission researcher at the University of Oregon who was probably there to comment on the anomalous behavior of STM tips. The tone of the meeting was downbeat. Most groups were nearing the end of their tether, and some attendees recall that other participants expressed doubt that the Zurich results *could* be replicated.

Yet whatever inside information the new STMers learned from Rohrer or taught each other at Cancun (how to prepare tips, where to buy piezo crystals, how to use a voltage pulse to clean a tip, etc.) worked. At the March 1985 meeting of the American Physical Society—just four months later—Golovchenko, Quate, and Feenstra were able to show atomic-resolution images to a packed audience. The other attendees, slowly but steadily, followed suit. In some cases, the knowledge of how to build an STM then spilled over from the groups that had been at Cancun to other STMers working cooperatively and in close proximity in the same organizations. For instance, Joe Griffith, whose STM was housed in the same cramped tractor shed at Bell Labs as Golovchenko’s, attained atomic resolution not long after Golovchenko returned from Cancun. In other cases, different STM teams within the same organization were barely on speaking terms. Competition among STM groups limited both the willingness of already successful groups to share information and the willingness of newcomers to ask for it. For these people, access to disciplinary knowledge (of

surface science), as transmitted to them through their organizations, aided replication.

A prime example is Joe Demuth's group at Yorktown. Unlike Feenstra, Demuth was already an accomplished surface scientist *and* a senior research manager when he embarked on STM. When STM was still an unproven instrument of little use to surface science, Demuth—like most senior corporate researchers—shied away. But in 1984, after the silicon 7×7 images had appeared, Demuth began to build his own STM. Unlike Feenstra, Demuth could increase his STM effort quickly by drawing on resources and personnel that weren't available to more junior staff scientists. Demuth hired a team of specialists, each of whom contributed some piece of the tacit knowledge needed to build an atomic-resolution STM. First, Mark Welland moved to Demuth's group from Oliver Wells', allowing Demuth to draw on the experience of someone who had tried to build an STM. By early 1985, Demuth, Welland, and another postdoc, Everett van Loenen, had built an STM that was capable of vacuum tunneling but could not resolve individual atoms. Then, that February, Demuth hired another postdoc, Bob Hamers, a talented instrument builder with the skills in electronics they needed to make their STM less noisy.³⁷ Finally, Demuth brought in Ruud Tromp, another recent PhD whose graduate research had been on ion scattering as a means of determining the atomic structure of several semiconductor reconstructions including the silicon 7×7.³⁸

Tromp provided the disciplinary know-how for preparing samples and interpreting images—something the STMers who were new to surface science (including Golovchenko, Feenstra, and Quate) lacked at first. Thus, the time between when Demuth assembled his team (February) and when they got their first atomic-resolution images (May) was much shorter than for the other groups. My point here is that, as sociologists of science have noted, communities make replication of experiments possible. Distributed networks of practitioners both facilitate the movement of tacit knowledge needed for replication and establish the criteria for deciding whether an experiment has been successfully replicated. However, more than one community may possess the resources and criteria needed to accomplish any given replication.

Some early STMers learned to replicate the technique largely through interactions with other members of the instrumental community—as at Cancun. For others, though, the network of practitioners that supplied the

tacit knowledge they needed to achieve atomic resolution was the corps of non-STM surface scientists within their organization. These surface scientists gave their STM colleagues advice, especially about how to prepare samples.³⁹ The problems that interested these surface scientists (semiconductor surface reconstructions, especially the 7×7) framed the criterion for a successful tunneling microscope—atomic resolution of those surface reconstructions—by which most early STMers were evaluated. Proximity to the surface-science community could aid in achieving that criterion as much as proximity to the growing STM community.

Competition at All Scales

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The race to have a working STM had serious implications. The big corporate labs, particularly IBM Yorktown and Bell Labs, were fiercely competitive organizations, and Machiavellian intrigue was common. Rivalries between the two companies, between research groups within them, and even between individuals within a group influenced how science was done. The competition between Yorktown and Bell Labs in surface science was a kind of cold war, with its own arms race of theories and experiments. Managers at Yorktown, therefore, were enthusiastic about the STM's potential for scoring points against Bell Labs.⁴⁰

Within both IBM Yorktown and Bell Labs, each of the STM groups that sprouted in the mid 1980s competed for limited resources: money, personnel, lab space, and so on. The jousting for lab space is particularly indicative of the competitive atmosphere in which STM groups worked. In the early days, an important impediment to atomic resolution was the vibration problem that bedeviled Russell Young at the National Bureau of Standards. Thus, many STMers tried to grab the quietest, least-trafficked areas of their buildings. Junior researchers were sometimes given unsuitable lab space at first, particularly if their group leaders were supervising many other projects and saw the STM only as a side bet. Bob Hamers and Ruud Tromp, for instance, had to build their first STM in a lab next to a noisy freight elevator, and they could get vibration-free images only if they ran the instrument late at night or on weekends.

Grabbing quiet space became a test of an STMer's bravado and of his or her supervisor's political skills. For instance, when the electronics shop at Yorktown moved into new rooms, Hamers and Tromp simply (in Tromp's

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words) “squatting” in the abandoned quarters until they were kicked out. After that, Tromp recalled, “we had spotted this empty office so we took all our stuff—our STM, which was on casters, and the electronics—and at night we wheeled it to the back lab and we started squatting in that office. And the office actually never got converted back to an office again, it became our lab.”⁴¹ At Bell Labs, Jene Golovchenko went around the building testing the floors with vibration sensors. Eventually he commandeered the quietest spot he could find for his STM: the projection room behind the auditorium. Even there, there were too many vibrations from people and equipment, so Golovchenko set his sights on a tractor shed at the far end of the Bell Labs property. It took fierce bureaucratic infighting to secure this space, though. According to Joe Griffith, “the Buildings people were not 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 [Area 15, materials research]. Jene went after that lab first. . . . The owner, Bob Laudise, fought them off ferociously. . . . Laudise was just absolutely livid because I was associated with them. He called me into his office one day and just let me have it. . . . But finally, Kumar [Patel] was very powerful in those days. . . . When he wanted something to happen it happened. So they got the tractor shed set up and we were out there for some years.”⁴² This shed became quite famous in the STM community. Until the early 1990s there were often multiple microscopes in operation there. The close confines allowed the quick circulation of ideas, designs, samples, software, equipment, and everything else needed to make an STM work.

Most STM groups at Bell Labs and at Yorktown didn’t have as close a camaraderie as the denizens of the shed. In fact, management pitted these groups against each other, so that advancement depended on outmaneuvering local competitors.⁴³ This led to bitter rivalries, and occasionally to skullduggery. Stan Williams, a former Bell Labs surface scientist, recalled:

There was a significant fraction of people there who were ultra-competitive and . . . absolutely determined to rise to the top by any means necessary, including sabotaging each other’s experiments and stealing each other’s data. Somehow the upper level management of Bell Labs thought that was okay. . . . There were bitter, bitter feuds between people within Bell Labs working on similar things. . . . I’d show up at my lab in the morning and I wouldn’t know which door there would be blood pouring out of from the actions of the previous night.⁴⁴

In such an environment, it was important for a junior researcher to have the right supervisor:

Unless you had a very, very strong department head, the end of the story was you were screwed. My department head was a very nice guy, but he wasn't very politically connected. He wasn't very strong and it was clear to me that my long-term prospects at Bell Labs wouldn't be very good unless I joined this group of throat-cutters.⁴⁵

A good supervisor secured resources (such as the tractor shed) and navigated the bureaucracy, leaving the STMers they managed to focus on getting their instruments to work.

Sameness and Difference

The most helpful group leaders were those who knew surface science well and could suggest appropriate research problems. Pursuing such problems allowed STMers to impress both the wider discipline of surface science and the large surface-science contingent within their organizations. Yet, because corporate surface-science STMers took their experimental cues from the same disciplinary canon, and often from the same pool of senior managers in their organizations, their work (especially at the start) clustered around a relatively small set of problems. This narrow focus, combined with the intense competitiveness of the corporate labs, meant that these STMers looked to each other almost to the exclusion of the rest of the STM community. Joe Griffith recalled that when he and a co-author wrote a review article on STM in 1990 "all of the examples that we gave in that article were either from this building [Bell Labs' Murray Hill site] or [IBM] Yorktown Heights. It was as if the rest of the world didn't exist."⁴⁶

The enforced sameness sometimes produced convergences that, to the outside world, looked self-defeating. For instance, at one point the Wilson-Chiang group at IBM Almaden and the Hamers-Tromp-van Loenen-Demuth group at IBM Yorktown were studying the same surface reconstruction of silver deposited on silicon without being aware of the duplication of effort. In fact, the two groups came to exactly opposite conclusions. Both groups produced STM images showing what appeared to be individual atoms lying on top of the deposited silver. One group thought these atoms were silver; the other thought they were silicon. When the two groups submitted papers to *Physical Review Letters* only four weeks apart, the editors, perhaps as a joke, placed them back to back in the issue dated January 26, 1987. As Ruud Tromp recalled, this "gave rise to some amusement in the community" and to some embarrassment within IBM.⁴⁷ Thus, even though circumstances pushed IBM and Bell Labs surface-science STMers to converge

on the same set of problems, there were great disadvantages to working on *exactly* the same problems. Supervisors evaluated these STMers in part by comparing their work to that of their colleagues, but supervisors also evaluated whether STMers possessed the initiative and creativity to branch off in new directions.

Perhaps no episode illustrates this balancing act better than Joe Demuth's attempt to batch-produce his STM design. Demuth was not the first to supply standardized STM parts to IBM researchers. Othmar Marti, a graduate student at the ETH (Swiss Federal Institute of Technology) in Zurich had earlier made a batch-produced electronics package called the "Blue Box" for IBM Zurich researchers. Demuth's more ambitious aim was to make a complete STM available to every group at Yorktown that wanted one. Thus, his group helped the Central Scientific Services (CSS) unit at Yorktown design and then produce about a dozen STMs, primarily for use at Yorktown and at other IBM Research sites near there. Management made these STMs essentially free—groups didn't need an extra line item in their budget to acquire one. It's an indication of how large and inward-looking IBM was that the internal STM market was so important, but no attempt was made to commercialize these STMs externally.

The people who used these microscopes tended to be surface-science postdocs whose supervisors were finally taking an interest in STM. In fact, the design was so tuned to the needs of surface scientists that postdocs who tried to use these microscopes for something other than surface science sometimes couldn't even get them to work. That was one reason so few of the instruments left Yorktown. Dawn Bonnell, one of the postdocs who used the CSS microscope, recalled:

IBM was concerned that these machines [the CSS STMs] would go out [of Yorktown]. The instruments were really touchy. . . . Everything [on those STMs] was tuned to be high resolution . . . which meant it was also easy to become unstable. You could just tweak it [and] you could have circuits oscillating. IBM was worried that people not familiar with the instruments would be frustrated with that level of sensitivity and would blame them. It was not a product and hadn't gone through all the things that attorneys like it to go through to be robust and to have no bugs in the program.⁴⁸

For postdocs who wanted to quickly get ahead in surface-science STM, the batch-produced STM saved a significant amount of time. Yet because the CSS instrument was optimized for such a narrow range of experiments, it had the dangerous potential to channel users into research that was too similar to what other groups were doing.

Thus, Yorktown postdocs approached the CSS STM knowing that they would have to show their supervisors that they had their own experimental skills, that they could have built their own STM had they wanted to, and that they could devise approaches to surface-science problems that were better than those of the Demuth team, or that were too sophisticated to have been packaged into a batch-produced instrument. As a result, many of them rewrote the batch-produced STM's software as they devised new capabilities that departed from its origin in the Demuth group.⁴⁹

The challenge for these early STMers at Bell Labs and at IBM was to operate within the shared framework of surface science while distinguishing themselves from other STMers enough to establish a distinct experimental identity. If they wanted to be recognized by surface scientists, there were some red lines that were difficult to cross—for instance, they had little choice but to build microscopes for operation in ultrahigh vacuum, since that was the most conducive environment for keeping metal and (especially) semiconductor samples well defined and clean enough to satisfy other surface scientists.

But surface science also offered a menu of experimental hardware that the Bell Labs and IBM STMers could productively pair with the STM. Thus, they began using their microscopes in conjunction with surface science's standard tools for preparing samples, such as evaporators and sputterers. Several STMers at Bell Labs and at IBM were also among the first to build instruments that could operate at low or variable temperatures, so as to better observe phenomena of interest to surface scientists (e.g., the diffusion of adsorbates). These STMers also began to place their microscopes in vacuum chambers with established surface-science instruments.

At the time, semiconductor surface science's most important tool was low-energy electron diffraction, the primary method for generating data on surface reconstructions. It was deeply entrenched at the big corporate labs, so STMers in those organizations were mindful of the views of its practitioners. Yet because STM was a potential competitor to LEED, some practitioners of the older technique were skeptical. For LEED's adherents, STM images seemed too localized—there was always a possibility the STM image came from some small patch of the sample that had reconstructed differently than the rest of the surface. STMers at Bell Labs and at IBM had to answer this criticism if they wanted continued backing from their organizations. Some did so by mathematically transforming their real-space STM images into inverse-space images very roughly akin to those produced by

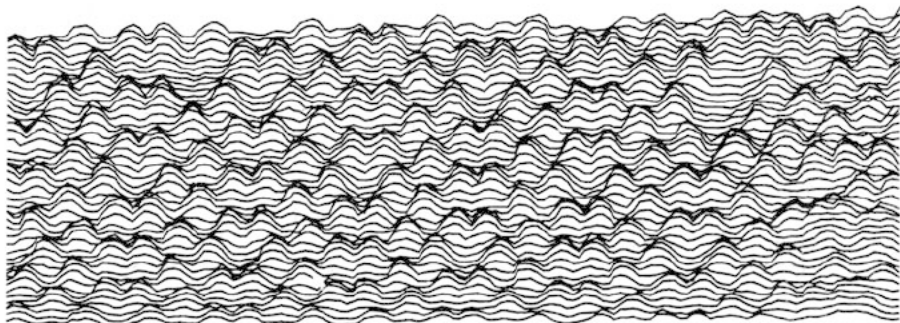


Figure 3.1

An image of the silicon 7x7 made by juxtaposing a series of line scans on a chart recorder. Reprinted, with permission, from J. E. Demuth et al., "A Scanning Tunneling Microscope for Surface Science Studies," *IBM Journal of Research & Development* 30 (1986), no. 4: 396–402.

LEED.⁵⁰ Comparing these faux-LEED images to real LEED patterns allowed the STMers to assert that they really were observing the surfaces they said they were. A more robust response that some pursued was to increase the area that the STM tip could scan. The more area covered, the less LEED specialists could dismiss an STM image as a fluke.

As was noted in chapter 2, one feature of surface science at IBM and at Bell Labs was that its practitioners had access to high-power computation, and they relied much more heavily on it than surface scientists at most universities or government and corporate labs did. This characteristic also showed up in Bell Labs' and IBM Yorktown's appropriation of the STM. The STMers there experienced far more organizational pressure to make their instruments compatible with computer control than other members of the instrumental community did. In contrast to STMers at Yorktown and at Bell Labs, academic probe microscopists of this period often built large racks of analog electronics and used them to produce subtle, virtuoso images that would have been difficult then to make with a digital controller. They archived those images by taking photos of oscilloscope patterns or by running the microscope's output through a television and using a videocassette recorder to record their data, rather than storing results in computer memory.⁵¹

Even Binnig and Rohrer, though working for IBM, resisted computer control. One Bell Labs STMer recalled:

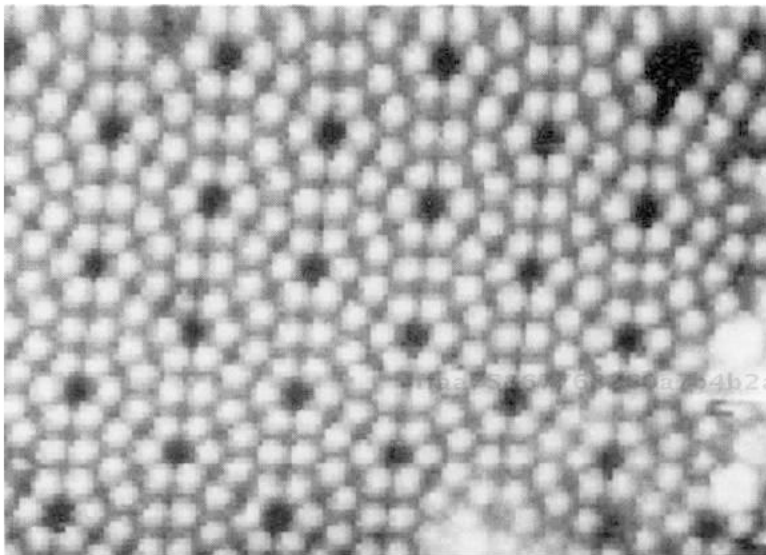


Figure 3.2

An image of the silicon 7×7 made by digitizing STM data and then using software to fill in space between line scans. Note how much easier this version is to “read” than figure 3.1. Reprinted, with permission, from Demuth et al., “A Scanning Tunneling Microscope for Surface Science Studies.”

At dinner [Rohrer] was bragging that they had real knobs on their STM, they don’t do all those computer things, they don’t like those computers. That really hobbled them in the year or two after [the 7×7] because they were using a strip chart recorder to make the traces while the rest of us immediately set about writing programs to run personal computers.⁵²

Rohrer even tried to get Yorktown STMers to follow suit. As Bob Hamers recalled, “Rohrer came in one day and . . . said ‘you should throw away that computer, you should get a chart recorder.’”⁵³ Yorktown management, on the other hand, aggressively encouraged researchers to adopt each new version of the IBM PC as it came out. STMers at Yorktown and at Bell Labs wrote copious amounts of software for their instruments. This software made understanding and analyzing STM images far easier than with analog methods such as chart recorders and photographs (compare figures 3.1 and 3.2).

How a research group implemented such software said a lot about its position in an organization like IBM Yorktown. For instance, the most distinctive difference between the Feenstra and Demuth groups’ STMs lay in

computer control for scanning tunneling spectroscopy. Using an STM as a spectroscopic tool was a nearly automatic choice for surface scientists. After all, most of the instruments invented by surface scientists in the 1970s were spectrometers of various kinds. To integrate the microscope fully with surface science, STMers would need data that could be compared against results obtained by means of infrared spectroscopy, x-ray photoemission spectroscopy, and other varieties of spectroscopy. "Spectroscopy" here just means the ability to put some range of inputs into a surface and get some output dependent on that range. The idea, usually, is to learn about the energies associated with electrons or molecules at the surface. The way to do this with an STM is to pass the tip over a point on the sample and vary the voltage between the tip and the sample over some range.

The Feenstra group—younger, less established, less connected to Yorktown's resources than the Demuth group—devised a spare, elegant, low-cost technique for tunneling spectroscopy that could be done with a personal computer rather than a mainframe. Randy Feenstra and his post-doc Joe Stroscio simply scanned the tip in one direction at one voltage, then back at a different voltage. The technique yielded an image of gallium arsenide that was featured on the cover of *Physics Today*. Electron energy states associated with the gallium atoms (depicted in blue) showed up at one voltage, the states associated with the arsenic atoms (depicted in red) at another voltage.⁵⁴

The Demuth group, meanwhile, devised a technique that pushed the boundaries of computer memory and required an expensive mainframe computer to process images. Their algorithm stopped the tip at each pixel in the scan and ran the voltage through a range of 24 values. That is, they produced 24 images for each image that other groups produced. Such a large amount of data (by early 1980s standards) could be exploited efficiently only by a large lab group with access to the top-line resources of a very large research organization. Hamers describes the process thus:

[I]t would take about half an hour to make an image. Then in order to see it in a grayscale format, we used to have to copy it to the mainframe computer system over a 9,600-baud line, and then run a FORTRAN program to convert the data to a grayscale format, walk down to the graphics laboratory where they actually did have grayscale monitors, and after about half an hour you'd be able to see what the image you just took was like. So sometimes . . . you just had to have a team, one person watching the instrument and one person trying to analyze the data and just run back and forth to see what was going on.⁵⁵

Using this technique, the group gained acclaim for images showing different bond energies at each adatom of the silicon 7×7 and for correlating those bond energies to spectroscopic measurements made with other instruments.

Departing

Once they replicated the technique, the surface-science STMers at IBM and at Bell Labs were extraordinarily productive. They were able to use the disciplinary canon of surface science to devise courses of experiments, each new experiment building on the previous one in ways articulated by surface-science questions. For instance, the Golovchenko group was probably the first to replicate the Zurich team's atomic resolution of the silicon (111) 7×7 reconstruction. This then led to a quick series of papers on closely related reconstructions of the silicon (111): the 5×5 , the 9×9 , even the 11×11 . Similarly, the Demuth group, having been recognized for its spectroscopic work on the 7×7 , quickly moved on to spectroscopy of the silicon (111) 2×1 and silicon (001) 2×1 . Randy Feenstra began with silicon, then decided to specialize in more exotic semiconductors: germanium, gallium arsenide, and eventually gallium nitride.

At Almaden, Shirley Chiang and Robert Wilson, after some initial work on semiconductor samples, moved on to metals, including gold, platinum, copper, and rhodium. They then deposited organic molecules on metal substrates—for example, naphthalene, azulene, and several methylazulenes on platinum. Don Eigler, who had been a postdoc at Bell Labs before moving to Almaden, started out in STM by chasing a surface-science dream of doing vibrational spectroscopy of the bonds in a single molecule adsorbed on a surface. That project turned out to be so difficult that it was not solved until ten years later. In the meantime, Eigler found he could produce publishable results quickly by systematically putting atoms and single molecules on a metal substrate and imaging them: iron on copper, benzene on platinum, oxygen on niobium, xenon on nickel.

All these researchers found surface science useful because it gave them a menu of options that they could move through quickly, and ready-made local and international audiences of people who would understand why they were moving from one sample to the next. These courses of closely related experiments allowed surface-science STMers to generate large

amounts of publishable data quickly. In the short time available before their postdoctoral fellowships ended or their staff positions came up for review, they skillfully used the STM to accrue the credentials they needed to climb the career ladder of surface science.

Thus, surface-science questions simultaneously aided the early STMers' careers and fostered the initial proliferation of, and innovation in, probe microscopy at Bell Labs and at IBM Yorktown and Almaden. Those discipline-based questions, however, were generally in fundamental research areas with relatively indirect or long-range relevance to AT&T's and IBM's product lines. Once local surface scientists ratified the STM as an important tool, however, managers at IBM and at Bell Labs began to encourage the search for more "mission-oriented" variants: atomic force microscopy, near field scanning optical microscopy, magnetic force microscopy, STM- and AFM-based data storage.

The turn toward mission-oriented probe microscopy was, in part, a product of the changing prospects of AT&T and of IBM. In 1984, AT&T divested itself of its regional operating companies in order to settle an antitrust suit brought by the US Department of Justice. As a consequence, about 3,000 people left Bell Labs to form a new company, Bell Communications Research (or "Bellcore"), that would be the research arm of the now-independent regional operators. Some STM research was transferred to Bellcore, though with a very mission-oriented flavor—for instance, investigations of the surfaces of battery electrodes or fiber-optic cables.⁵⁶ At the same time, AT&T found itself in a crowded marketplace where it was less able to justify basic research. By 1991, "the era of university-style research at [Bell Labs] was over,"⁵⁷ or at least in rapid decline. The remaining STMers faced intense pressure to switch to more mission-oriented fields, such as metrology for microelectronics manufacturing.⁵⁸

The collapse of IBM's mainframe market (and a general recession) in the early 1990s almost led to that company's demise. As a result of the ensuing austerity measures, IBM Research lost nearly 20 percent of its staff, and the company as a whole shed a similar percentage of personnel.⁵⁹ More fundamentally, upper management came to believe that IBM's basic research was too insulated from its manufacturing arm, and that the long time horizons of basic research allowed competitors to exploit the best ideas coming out of Yorktown, Zurich, and Almaden before IBM could.⁶⁰ IBM still does

high-quality fundamental surface-science STM today, but at a much lower level than in the mid 1980s.

The disciplinary criteria by which the first STMers at IBM's research sites in the United States and at Bell Labs were judged—and by which they judged others—somewhat impeded their transition to this more mission-oriented world. As Joe Griffith says of Bell Labs,

most of the people here doing probe microscopes were very serious surface scientists, they wanted to see exactly where every little atom was on some surface that had been very carefully prepared in ultrahigh vacuum. AFM was kind of a messy thing, it was operating out in air and it wasn't completely clear that the theory was exactly right on how the thing was working and it probably wouldn't do atomic resolution. So everybody sort of turned their nose up at it.⁶¹

At IBM, research in AFM, MFM, and non-surface-science STM was often pushed by people with ties to Calvin Quate's Stanford group, rather than by members of IBM's cadre of surface scientists.

With the corporate environment at Bell Labs and at IBM becoming less conducive to their research, many of the first-generation surface-science STMers moved elsewhere. Some went to national laboratories that had strong traditions in surface science. For instance, researchers at the National Institute of Standards and Technology (the successor to the National Bureau of Standards) had tried without success to copy an early Zurich STM design, but they were not able to achieve atomic resolution until Joe Strosio joined them, having left Yorktown. Likewise, Bob Wolkow moved from Yorktown to Bell Labs to the Canadian National Research Council, one of the institutions that had championed surface science's adoption of the American Vacuum Society as its professional society in the 1960s.

More often, however, the early Bell Labs and IBM STMers took positions at universities. Jene Golovchenko was one of the first, going to Harvard in 1987. A little later, Barney Webb and Max Lagally's surface physics groups at the University of Wisconsin recruited Golovchenko's technician, Brian Swartzentruber, to do his PhD work there and build them an STM. Bob Hamers later joined the faculty at Wisconsin. After the shake-up at IBM, Randy Feenstra went to Carnegie-Mellon, and Shirley Chiang to the University of California at Davis. This outflow of personnel was, in fact, an element of the business model of IBM and Bell Labs. As Kumar Patel put it,

A significant fraction of the people who leave our research division return to teaching. There isn't a university solid-state or condensed matter physics department that

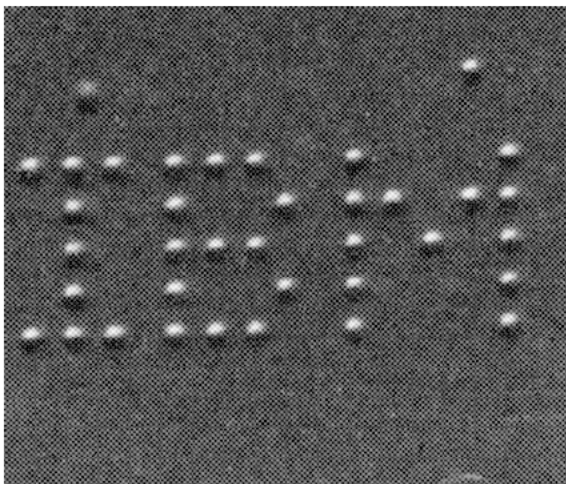
is not heavily populated by people who were once here. It is a two-way street. We send universities people who have been here 10 to 15 years and who then become excellent faculty members. In turn, they send us their best students.⁶²

Changing economic conditions clearly accelerated this process, however.

Thus, the early proliferation of surface-science STMs within IBM and Bell Labs later facilitated repeated site-to-site transfer of the technique away from these organizations. The tunneling microscopists who left IBM and Bell Labs took with them tacit skills that were useful in getting an STM to work. They also sometimes took with them tangible aids to replication, such as software. Their continuing ties to former colleagues at IBM and at Bell Labs occasionally gave them access to surplus equipment (vacuum chambers, dewar flasks, microscopes).⁶³ By moving elsewhere, they established new centers of STM, where they then trained new generations of practitioners—making STM an ever more prevalent surface-science tool. As we will see in chapter 5, veterans of Bell Labs and IBM were not responsible for commercializing surface-science STM, but they did establish the disciplinary rationale for STM that led other surface scientists to form a market for commercial instruments.

The adoption of STM, in turn, changed what it meant to do surface science. By making real-space visualization of surface reconstructions possible, the STM made one of the discipline's favorite problems seem far less exciting. Whereas in the 1970s a graduate student could write a whole PhD dissertation on a surface reconstruction, today a grad student might devote a chapter or less to solving a reconstruction.⁶⁴ As Stan Williams (who took up STM after his postdoc at Bell Labs) puts it, once the 7×7 had been solved "people sat back and said, 'OK, what did we really learn from doing this?' It was really pretty cool to solve it, but the question was 'Are we going to put more and more money into doing nothing but solving surface structures?' The answer was No. It just wasn't interesting any more."⁶⁵

The STM also contributed to the rising costs of surface-science research. In Williams' words, surface science "was just so damned expensive to do. . . . A lot of young surface scientists [wanted to do] something that was not as expensive, so surface science turned out to be a dinosaur in some sense."⁶⁶ By the late 1990s, many of these people, including Williams, were accommodating themselves to the use of "nanotechnology" as a new, more dynamic label for their research. Many of the 1980s-vintage corporate STMers who are still working would today describe their work as



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Figure 3.3

Atoms of xenon arranged to spell "IBM" on a nickel surface. Source: D. M. Eigler and E. K. Schweizer, "Positioning Single Atoms with a Scanning Tunneling Microscope," *Nature* 344 (1990): 524–526. Reprinted with permission from Macmillan Publishers Ltd.

nanotechnology or nanoscience. Indeed, many of them now run the organizations that make up the nanotechnology enterprise. As of 2010, Dawn Bonnell (formerly of IBM Yorktown) runs the Nano-Bio Interface Center at the University of Pennsylvania; Bob Wolkow (Yorktown, then Bell Labs) helped found Canada's National Institute for Nanotechnology; Joe Stroschio (Yorktown) is with the Center for Nanoscale Science and Technology at the National Institute of Standards and Technology; Mark Welland (Yorktown) founded the Nanoscience Centre at the University of Cambridge and is a former editor in chief of *Nanotechnology*; Jim Gimzewski (Zurich) is the director of the Nano & Pico Characterization Core Facility of the California NanoSystems Institute (CNSI); and Paul Weiss (Bell Labs, then Almaden) is the director of the CNSI.

These scientists' achievements in STM give them the credibility to be leaders in nanotechnology. Yet in many cases their achievements that are most obviously "nanotechnological" are exactly those that grew out of corporate surface science. Perhaps the most famous image in nanotechnology, for instance, is Don Eigler's STM micrograph of 35 xenon atoms spelling out "IBM" (figure 3.3). Yet Eigler's technique for moving atoms

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was adapted from a trick he learned from Russell Becker in the tractor shed at Bell Labs. Moreover, the reason Eigler was moving xenon atoms around in the early 1990s was to answer questions posed by the Yorktown surface-science theorist Norton Lang, and the reason he was working in STM in the first place was because he had set out to do vibrational spectroscopy of the bonds in a single molecule adsorbed on a surface. As we will see in chapter 6, proponents of nanotechnology often invoke the field's inherent interdisciplinaryity. For a significant portion of the probe-microscopy community, however, the path to interdisciplinary nanotechnology ran through organizations that relied on the disciplinary tools, puzzles, networks, and evaluative criteria of surface science.

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4 Variation and Selection: Probe Microscopy Comes to California

As we saw in chapter 3, surface scientists at Bell Labs and at IBM quickly formed a dense STM network (defined by both collaboration and intense competition). This sub-community of probe microscopy initially expanded within these two firms, but then carried UHV STM out into the wider surface-science community.

The technique's inventors, however, were not strongly or permanently tied to surface-science STM. Binnig, Rohrer, and Gerber became adroit enough at surface science to prepare samples and interpret images of skillfully chosen reconstructions, and as a consequence their 7×7 data secured the STM a powerful constituency within IBM. The Zurich team aided various surface-science STMers in getting started, and for a few years they continued publishing data on surface reconstructions themselves. Yet none of the members of the original Zurich team were identified with surface science in the way that early adopters at Bell Labs and at IBM Yorktown were. Instead, their attention shifted relatively quickly from a search for new surface-science applications for STM to a search for practitioners who would develop new applications and variants of probe microscopy that would not be as starkly defined by a single discipline or a small number of organizations.

Initially, Binnig and Rohrer focused that search on European laboratories with some tie to IBM Zurich. However, by 1985 two groups in California—Calvin Quate's at Stanford and Paul Hansma's at UC Santa Barbara—were emerging as the Zurich team's most influential and like-minded partners. This chapter traces the evolution of that relationship, and the development of the Quate and Hansma groups into leading centers of probe microscopy. As "centers," Quate and Hansma's labs were places that other probe microscopists looked to for high-quality innovations in design and

application of the technique. Yet they were also “centers” in the sense that they recruited, trained, or aided many new practitioners, served as clearinghouses for information about the technique, and presented a model of how to do probe microscopy that others followed. Bell Labs and IBM were “centers” in that sense for surface science. The Quate and Hansma labs became “centers” of probe microscopy for a wider range of fields.

Yet these centers didn't necessarily hold. Many of the first probe microscopists in biophysics and biochemistry, molecular biology, electrochemistry, geology, materials science, electrical engineering, and other fields sought Quate's and Hansma's advice or collaboration when learning how to work with this new technique. They turned their attention to their separate disciplines, however, in making careers out of their mastery of STM, AFM, and other variants. By the late 1980s, the “centripetal” motion of new probe microscopists toward Stanford and UC Santa Barbara was balanced by a “centrifugal” motion out toward various disciplinary audiences. As the probe-microscopy community grew, it also diverged, so it became increasingly difficult for practitioners working on different variants or in different application areas to grasp the content or the importance of each other's work.

The relationship of new probe microscopists to central groups such as Binnig's, Quate's, and Hansma's illustrates the complex intertwining of technology and epistemology in an instrumental community. These central groups improved the technology of the probe microscope in ways that made it more applicable and more friendly to a wider range of disciplines. Yet Binnig, Quate, and Hansma were not themselves expert in the disciplines they were catering to, and members of those disciplines were not expert in probe microscopy. These conditions, therefore, created a great deal of uncertainty about the proper interpretation of probe-microscopy images and the proper design of the microscopes themselves. Binnig, Quate, and Hansma were as innovative in developing *ad hoc* methods for overcoming that uncertainty as they were in developing new variants and applications of probe microscopy itself.

Zurich and California

Binnig and Rohrer made forays beyond surface science and beyond IBM almost from the invention of the STM. Their most notable early success

was in recruiting Nico García, and then Arturo Baro, to bring the STM to Madrid after sabbaticals at IBM Zurich. García and Baro, like many early STMers even outside Bell Labs and IBM, initially explored the STM's application to surface science. Yet they quickly departed from surface scientists' chosen materials and criteria for successful STM. For instance, as early as May 1985, García, Baro, and the Zurich team co-authored a study of STM of bacteriophages that came nowhere near atomic resolution but did establish that the STM was capable of imaging biological materials.¹

Binnig and Rohrer also encouraged nearby German and Swiss researchers to take up the STM. Some of these, including Henning Neddermeyer at Ruhr-University Bochum and Karl Besocke at the Institute for Nuclear Physics in Jülich, were surface scientists. The Zurich team also took an interest in fostering development of non-surface-science applications. For instance, though neither Binnig nor Rohrer co-authored with Neddermeyer or Besocke, they did with Jürgen Behm at the University of Munich. Behm, a former visiting scientist at IBM Almaden, was one of the first to develop applications for STM in electrochemistry. This was, in some ways, a small conceptual leap from the Zurich team's work on surface reconstructions—Binnig characterized electrochemistry, which seeks atomic-level information about surfaces immersed in an aqueous solution rather than in a vacuum, as "a kind of underwater surface science."²

The Zurich team's early strategy for recruiting STMers seems to have been to identify an application beyond, but easily extrapolated from, surface-science STM, and then work with, or aid, a professional contact who was in a position to realize that application. Many of these contacts came through Rohrer's large network of acquaintances, built over a long career. For instance, one of the obvious early directions for STM was a machine that could operate at very low temperatures. (Certain chemical and physical phenomena are more easily observed or controlled at very low temperatures.) Othmar Marti, a student from ETH Zurich who had spent some time working at IBM in 1980–81, expressed an interest in building such a low-temperature STM. Rohrer then convinced his own doctoral adviser at the ETH Zurich, J. L. Olsen, to be Marti's primary adviser; Rohrer was his second adviser and oversaw his research at IBM.³ Marti's dissertation went on to become a widely circulated primer on the basics of STM building that other groups used to help them get started.

The Zurich team recruited Paul Hansma from UC Santa Barbara in a similar fashion. Hansma was a specialist in electron tunneling spectroscopy whom Rohrer knew from a sabbatical he had taken at Santa Barbara in the 1970s. Reaching out to electron tunneling specialists such as Hansma was a relatively obvious move for the Zurich team. After all, Russell Young had recruited Clayton Teague from that community to work on the Topografiner in the 1970s. After publication of the Zurich team's silicon 7×7 results, a few sandwich tunnel junction experimentalists, including Bob Jaklevic at Ford Research, spontaneously built their own STMs. In Jaklevic's case, the transition from sandwich tunnel junction spectroscopy to tunneling microscopy was aided by the fact that his laboratory director, Norman Gjostein, was a surface scientist. At Ford, just as at IBM Yorktown and at Bell Labs, the Zurich team's 7×7 results were pivotal in convincing managers to free up resources to build an STM. (The Ford Research STM was built by Jaklevic's postdoc Bill Kaiser.)⁴

The Zurich team had made some initial forays into using an STM to do tunneling spectroscopy, and their initial results helped inspire IBM colleagues such as Randy Feenstra to develop the technique further.⁵ Even before then, however, Rohrer identified Paul Hansma as a perfect candidate for adapting the STM for tunneling spectroscopy. After all, Hansma had literally written the book on tunneling spectroscopy.⁶ So in the summer of 1981, Binnig (on Rohrer's recommendation) stopped at Santa Barbara while on his way to a conference in Los Angeles to convince Hansma to move from sandwich tunnel junction spectroscopy to STM-based experiments.⁷

The next spring, one of Binnig and Rohrer's earliest and most important converts simply fell into their laps. Calvin Quate, a professor in applied physics and electrical engineering at Stanford, read about the STM in *Physics Today* while on a flight to London to pick up the Rank Prize for Optoelectronics.⁸ On arrival, Quate discussed the article with Eric Ash, a colleague then at University College London. Ash had a former student at IBM Zurich, through whom Quate arranged an impromptu visit. Impressed, Quate returned to Stanford and refashioned his group into an STM lab.

Like Hansma, but unlike the earliest adopters of STM at IBM Yorktown and Bell Labs, Quate was a senior researcher with a long track record. He received a PhD in electrical engineering from Stanford in 1950, then spent ten years in industrial research, largely at Bell Labs. In 1961, he returned to Stanford, specializing in microwave research at a time when the university

was using its expertise in microwave technology to secure funding from federal defense research agencies and to connect to high-tech firms in the Bay Area.⁹ In the 1960s, Quate made his name studying the coupling between acoustic waves and microwaves—an area with applications in signal processing for radar and electronic communications.¹⁰

The political and (perhaps more important) budgetary turmoil of the late 1960s and the early 1970s encouraged Quate to turn toward biomedical applications (and funding) for his research.¹¹ To that end, he co-invented the scanning acoustic microscope (SAM), in which an ultrasonic beam launched through a sample carries information about the sample's microscale properties to a detector. By rastering the ultrasonic beam, the acoustic microscope builds up an image of, for example, organelles within a cell. Quate found it difficult, however, to forge collaborations with biomedical researchers, so by the mid 1970s he had turned to an alternative application: imaging defects inside integrated circuits. That application created a small but enduring niche for SAM in the microelectronics industry. Microelectronics firms particularly valued SAM for its ability to do “non-destructive testing” (NDT)—that is, it allows a manufacturer to see whether a process step (e.g., applying a thin film) has worked, without having to destroy or damage the silicon wafer.¹²

By the time he boarded his flight to London in 1982, Quate was beginning to extricate himself from acoustic microscopy. He and his students had generated images of biological materials with a resolution near the instrument's theoretical limits, but Quate hadn't been able to elicit as much collaboration from life scientists as he and his funders had hoped. Quate's group had also demonstrated the instrument's relevance to microelectronics manufacturing, and commercial manufacturers had begun to market microscopes for that application with little need for further innovation from Quate. Reading about the STM, Quate must have seen an opportunity to take on a new project while retaining much of what he had learned in acoustic microscopy. The STM and the SAM used very similar scanning control electronics and very similar image display and processing techniques. Perhaps more important, the STM seemed to offer the same non-destructive testing capability as SAM, on the same semiconductor samples, but at potentially higher resolution. Thus, Quate's network of contacts—and former students and postdocs—in the microelectronics industry would be a ready-made audience for anything he did with STM.

Technologies of Community

By 1983 a small non-surface-science network of STMers had begun to emerge. This network was initially centered on Zurich—nearly every non-surface-science STMer had some tie to Binnig and Rohrer. Hansma and Quate were, at that point, geographic and intellectual outliers of this group. Quate remembers that his first visit to see the Zurich microscope left little impression on Binnig, and that the Stanford group's "connections weren't good enough to get preprints" from Zurich until after he sent Binnig copies of his STM's first images.¹³ But in the next few years, the ties between the Zurich and California portions of the STM community became much stronger. Binnig and Gerber spent most of 1985–86 in the San Francisco Bay Area, splitting their time between Quate's group and IBM Almaden, and Othmar Marti spent 1986–88 as a postdoc in Hansma's group. When Binnig and Gerber returned to Europe to set up an IBM research outpost in Munich, Quate's student Doug Smith joined them as a postdoc. Later, a student of Binnig's from Munich, Franz Giessibl, went to the Bay Area to work for a start-up company affiliated with Quate's group—a company Marti also considered working for.¹⁴

At the same time, the network of California STMers began to grow. Many of the new members of this network had prior associations with Quate or Hansma and also with each other. Two of Quate's former students who were working at IBM Almaden, John Foster and Dan Rugar, followed their adviser's path and took up STM around the time Binnig and Gerber were on sabbatical there (as did two former Quate postdocs, Kumar Wickramasinghe and Clayton Williams, at IBM Yorktown). IBM funded Quate's early STM research, and continued to hire his former students (e.g., Andy Bryant and Tom Albrecht) and postdocs affiliated with his group (e.g., David Braunstein).

In 1983, Rohrer gave a talk on the 7×7 at the American Physical Society's March meeting in Los Angeles that pulled in two more members of the California STM network. John Clarke, a Berkeley superconductivity specialist for whom Hansma had worked as a graduate student, came back from the meeting and convinced one of his graduating students, John Mamin, to continue on as a postdoc and to help Clarke's group venture into STM. In building their STM, Mamin and Clarke's students David Abraham and Eric Ganz benefited from their proximity to Quate's group at Stanford. They

received advice from Quate's postdoc Sang-il Park, and they used some elements of the Quate group's design, such as a spring system with magnetic damping (for vibration isolation) and later a magnetic walker (for bringing the tip down to the sample), along with their own innovations. Later, when Mamin got a job at IBM Almaden, he and Dan Rugar became long-time collaborators in AFM, then in STM, then in MFM.¹⁵ Clarke didn't stay in STM for long, but in that short time his group generated several much-cited articles on the subject.¹⁶

At the other end of the state, a Caltech chemist, John Baldeschwieler, became interested in STM after one of his postdocs reported on Rohrer's presentation in Los Angeles. Baldeschwieler was an experienced instrument builder with a strong reputation in nuclear magnetic resonance spectroscopy and in ion cyclotron resonance spectroscopy; he was also deeply involved in science policy circles in Washington.¹⁷ Though he started out behind Quate and Hansma and never quite caught up, Baldeschwieler did manage to mount a high-profile STM effort. He seems to have had only limited contact with Quate and Hansma (indeed, Quate collaborated with Baldeschwieler's departmental colleague, Nathan Lewis, but not with Baldeschwieler). However, his efforts paralleled, and had at least an indirect effect on, those at Santa Barbara and Stanford.

The relationship that made California the source of so many innovations used by the worldwide probe-microscopy community, however, was that between Quate and Hansma. Though they co-authored a few articles, their interaction was largely informal. They sent each other drafts of articles for comment, experimental materials, and sometimes students, and occasionally they negotiated a loose division of labor as to what research topics each should pursue. In turn, each built a network of collaborators largely located on (though certainly not confined to) the West Coast of the United States. As Binnig, Gerber, and close affiliates of the original Zurich team (e.g., Marti) formed strong ties to Stanford and Santa Barbara, a Zurich-California network emerged as a recognizable and important early sub-community of probe microscopy. As we will see in the next chapter, its importance was eventually amplified by its close affiliation with start-up companies that commercialized STM and AFM.

One characteristic that set this sub-community apart somewhat was its members' interest in making it easier (for themselves and others) to build, operate, and learn about probe microscopy. Binnig and Gerber set the tone

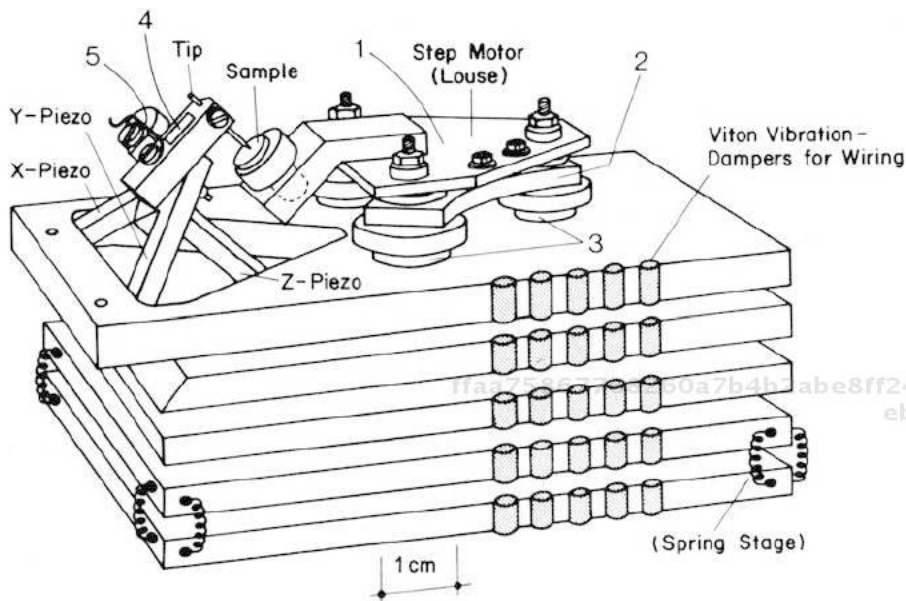


Figure 4.1

A small, vibration-free STM with Viton dampers developed by the IBM Zurich team and copied by many early STMers. Reprinted, with permission, from C. Gerber et al., "Scanning Tunneling Microscope Combined with a Scanning Electron Microscope," *Review of Scientific Instruments* 57 (1986): 221–224 (copyright 1986 American Institute of Physics).

early by developing, and then sharing knowledge of, STM innovations that made the microscopes less expensive and time-consuming to build, and more stable to operate. For instance, Gerber, in an early attempt to broaden tunneling microscopy's appeal beyond surface science, set out to design an STM that could fit inside a scanning electron microscope.¹⁸ In the process, he discovered that such a compact STM virtually eliminated the vibration problems that had plagued the Zurich team (as well as Russell Young and everyone else). As figures 4.1 and 4.2 show, this compact STM design, with a "louse" for moving the probe and with stacks of Viton rubber for vibration insulation, was disseminated widely and was copied in detail.

During their year in the Bay Area, Binnig and Gerber were together responsible for another innovation that made probe microscopes dramatically easier to build and operate: the tube scanner. Earlier designs used three perpendicularly stacked piezo crystals to control the fine movement

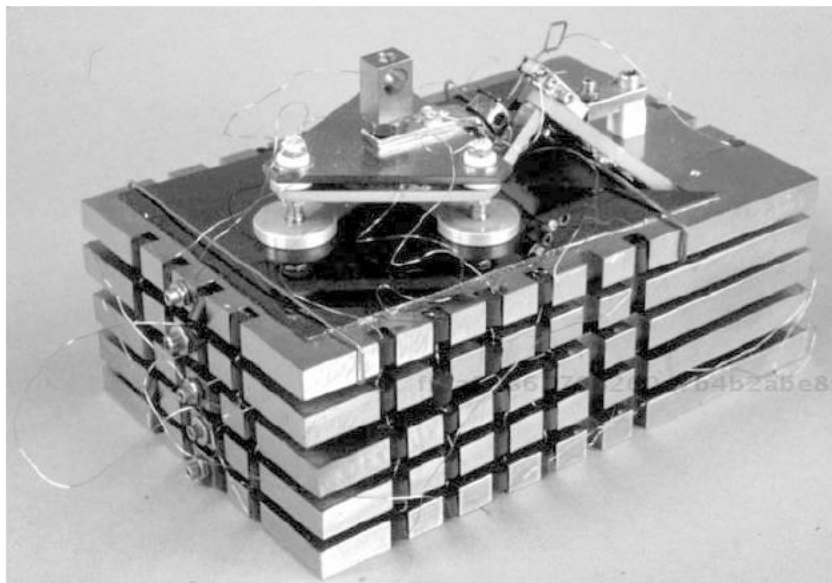


Figure 4.2

An early STM constructed by members of John Baldeschwieler's group at Caltech. The three circular pads are the feet of the "louse" that carries the sample toward the STM tip (the needle above and to the right of the louse). Total size: about 4 inches by 2 inches by 1 inch. Photograph by Gregory Tobias. Courtesy of Chemical Heritage Foundation Collections. Note the strong similarity to figure 4.1.

of the probe. In those designs, the piezos were thin and brittle (and hence easily broke off), and the process of gluing them together (and keeping them glued) was laborious and unreliable. Binnig and Gerber, in the course of helping the Stanford and Almaden teams devise more rugged STMs and AFMs, saw that *one* stack of piezos could control fine motion in all three directions. This "tube scanner" (so-called because the piezo stack usually formed a hollow cylinder) was thicker and hence less fragile, and it proved easier to assemble. Like the compact STM, this innovation spread rapidly through the tunneling-microscopy community.

Quate and Hansma readily adopted this orientation to improving microscope design so that STMs and AFMs could be built and operated more easily. At the same time, the Zurich-California sub-community also took the lead in establishing networking mechanisms that would allow faster dissemination of those design innovations. We saw in chapter 3 that Quate convened the Cancun workshop that was instrumental in enabling

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replication of atomic resolution. Even before that workshop, Rohrer had arranged for a conference the next summer to be held in Oberlech, Austria, under the auspices of the IBM Europe Institute. This meeting featured most of the Cancun attendees, plus other early adopters from Europe and North America. Whereas the Cancun meeting had been fraught with anxiety, the Oberlech conference was boisterously good-natured. Probe microscopists look back on it with exceptionally happy thoughts, especially about friendships they struck up with other early adopters.¹⁹

The following summer, Nico García hosted an International Conference on Scanning Tunneling Microscopy in Santiago de Campostela, Spain. This event then became the basis for an annual meeting, known colloquially as the STM Conference. Baldeschwieler's group brought STM '87 to Oxnard, California, about halfway between Caltech and Santa Barbara. After that, the STM Conference migrated annually around the world, steered by its organizing committee to places where the conference could encourage local interest in the technique and thereby expand the community: the United Kingdom in 1988, Japan in 1989, and the East Coast of the United States in 1990. The STM Conference became an important site for linking probe microscopy to the emerging nanotechnology enterprise.

Air Operation

Few things demonstrate the Zurich-California sub-community's common orientation to microscopes that would be easier to build and operate than its members' discovery and exploitation of STM operation in air. At the beginning, almost everyone assumed that an STM couldn't operate in air, since the electric field between the probe and the sample would cause arcing. Besides, the yardstick of a successful STM was atomic resolution, and most researchers thought a surface exposed to air would get dirty too quickly to allow images of single atoms. Thus, Quate, Baldeschwieler, and Clarke all initially aimed to get atomic resolution of surface reconstructions in vacuum. Yet these groups lacked Bell Labs' and IBM's in-house expertise in UHV, in sample preparation, and in the interpretive practices of surface science. This made for slow going.²⁰ Quate, especially, saw his early lead evaporate.

Hansma, however, didn't initially pursue atomic resolution, or even microscopy. Instead, he devised an experiment that would resemble standard

sandwich tunnel junctions closely, but with a vacuum separating the two electrodes instead of an oxide layer. Recall from chapter 2 that in sandwich junctions one electrode is “grown” (by evaporating a metal or semiconductor onto a substrate); then an oxide layer is grown to a precise thickness (by exposure to gas) on top of that first electrode; then a second metal or semiconductor is evaporated and deposited to form another electrode on top of the oxide layer. Hansma adapted this design for his STM-inspired “squeezeable” tunnel junctions. In these squeezable junctions, thin gold wires are suspended, cross-wise, over each other, separated by a small gap. With a voltage across the two wires, an electromagnet positioner slowly pulls one wire toward the other until electrons begin tunneling between them.

These squeezable junctions were designed to work in vacuum, but for debugging purposes, or perhaps just by intuitive leap, Hansma and John Moreland (a grad student) experimented with operating them in air. Surprisingly, they didn't see the electrical discharge between the wires that back-of-the-envelope calculations had led them to expect. Tunneling seemed to work in air just as well in vacuum. Hansma could easily have shrugged this discovery off. Indeed, the Zurich and Madrid groups may have already known about air operation, but they were moving in so many directions that its significance was drowned out. Some surface-science STMers also tinkered with prototype microscopes in air, but few would have dreamt of publishing images taken in anything other than “ultra-clean” (i.e., vacuum) conditions.²¹

Hansma's past experiences led him to disregard some of the precepts of ultrahigh-vacuum surface science, and encouraged him to exploit air operation. In the late 1970s, Hansma had taken a sabbatical at Berkeley with Gabor Somorjai, a prominent surface scientist. There he had developed a preference for quickly built, “kludged” experiments that was incompatible with the agonizing time scales of UHV surface science, in which modifications to sample or apparatus are limited by the week-long pump-down and bake-out time of the vacuum chamber.²² With the insight that squeezable junctions worked in air, he could now build tunneling experiments that could be modified rapidly and iteratively. Moreover, the success with air operation inspired him to try tunneling in water, in oil, and in nitrogen.

By the time he heard about the Zurich group's silicon 7×7 results, Hansma was already thinking about moving into tunneling microscopy because of disappointments with the squeezable junctions. The experiments

that could be done easily with a squeezable junction provided little information that wasn't already available from Raman spectroscopy.²³ At the same time, the discovery of tunneling in air created new opportunities in STM. Before the possibility of air operation occurred to him, Hansma had looked to the resource disparity between his own group and those at IBM and at Bell Labs and saw little chance of competing in the expensive world of ultrahigh-vacuum STM. Tunneling microscopy in air, however, offered a simpler, cheaper alternative in which he could compete on a level playing field with surface-science STMers.

Air STM was the first step toward a transformation in the interactions among tunneling microscopists' instrumental, organizational, and disciplinary commitments. Before, newcomers to tunneling microscopy had little choice but to design a UHV instrument, primarily for surface-science applications. After air operation became known, surface science became just one application among many for the STM. Moreover, surface-science STM was an application area that people who were not already surface scientists now could avoid. UHV chambers are expensive, as are the special materials and design features an STM needs in order to survive the harsh UHV environment. As was noted in the preceding chapter, the specimen-preparation tools and other characterization instruments that accompany a surface-science STM are also expensive, and require a significant investment of time and training to master.

In addition, at the time, UHV chambers took several days to "pump down" to ultrahigh-vacuum conditions, and early surface-science STMs broke down often, so operation of the microscope (when it was working) tended to be done in manic bursts separated by periods of inactivity. The mantra among some UHV STMers was "never leave a working machine," even if that required taking round-the-clock shifts.²⁴ Air STMers, conversely, didn't have to buy an expensive UHV chamber, and they could modify their microscopes in minutes. Air STMers could even tinker with the instrument while it was running, change samples and tips quickly, or look at a sample with a light microscope while characterizing it with the STM. Samples that would not survive UHV (including many biological samples) could now be examined. Since exposure to air no longer mattered, samples could be prepared by hand, at an ordinary lab bench or under a fume hood, whereas in UHV samples had to be prepared inside the chamber, where the experimenter couldn't touch or even see them.

Obviously, samples examined in air would not count as “clean” or “well defined” by the standards of surface science. Indeed, surface scientists raised a number of awkward questions about air operation. How could a tunneling current pass through the non-conducting contamination layer that inevitably builds up in air? Were electrons in the tip actually tunneling to the sample, or was the tip simply making ohmic contact with the sample?²⁵ There were many good technical reasons to be suspicious of air STM. Air STM eventually buckled under the weight of such objections. Yet the technique enjoyed a tremendous boom until the early 1990s. For new STMers, tunneling in air meant a lower barrier to entry (in resources and expertise) to a hot instrumental community, and a wider space of experiments on which to work.

For Hansma, air STM was attractive partly because it offered an opportunity to follow Binnig and Rohrer's path by collaborating with members of his large (and varied) network of professional acquaintances. For instance, Hansma quickly used the air STM as the basis for a collaboration with Bob Coleman, a physicist at the University of Virginia (UVA) whom he had known since the early 1970s. Coleman specialized in making and characterizing layered compounds such as tantalum selenide. In 1985, Hansma, on sabbatical, went to UVA to teach Bob Coleman STM and to add layered compounds to the list of things a tunneling microscope could image. After that, Hansma began to move toward biological STM. One reason for this was that his wife, Helen, had a degree in biology but was then teaching yoga at UC Santa Barbara and science at her children's elementary school; biological probe microscopy allowed her to return to laboratory work, and eventually to have her own lab group in the physics department at Santa Barbara.²⁶ The possibility of having his wife work with him may have been in Paul Hansma's mind in late 1985 when he heard a talk by Stuart Lindsay, a biophysicist at Arizona State University (ASU). Hansma invited Lindsay to return a few weeks later and begin a collaboration on STM of DNA.²⁷ Afterward, Lindsay returned to ASU and began building his own biophysical STMs. Finally, since biological materials are best studied in aqueous solution, Hansma adapted his STM to run in water as well as in air. That, in turn, led him (like Binnig) into electrochemical STM, then into a brief collaboration with Bruce Schardt, a former Santa Barbara electrochemistry postdoc who had recently taken a faculty position at Notre Dame.

The AFM Revolution

In 1985-86 members of the Zurich team joined the California groups for extended stays, which accelerated a convergence of their experimental styles. Othmar Marti, after considering an offer to take a postdoc with John Clarke at Berkeley, chose instead to go to UC Santa Barbara, largely because of Paul Hansma's new interest in biological microscopy.²⁸ Quate, meanwhile, invited Binnig to spend most of 1985-86 at Stanford. At the time, Binnig may have been tired of traveling and lecturing so much about the STM and having little opportunity to do his own research. Quate's invitation was therefore quite open-ended. As Quate recalled later, "I said 'Gerd, please don't invent anything while you are at Stanford.' He replied, 'Don't worry, I want to spend the year doing science. There will be no work on devices.'"²⁹ Instead, Binnig moved into an entirely new area: gravity-wave physics. For a while after he arrived at Stanford, he tinkered with some odd liquid helium funnels, trying to make a low-budget gravitational radiation detector.³⁰

While Binnig was on sabbatical at Stanford, Christoph Gerber was seconded to IBM Almaden, less than an hour away. Binnig, Quate, Gerber, and the other STMers in the Bay Area therefore had ample opportunity to discuss new directions for the technology. Binnig, in particular, was pondering comments made by John Pethica, a friction researcher who had gotten to know the Zurich team while a staff scientist at the nearby Brown Boveri research lab in 1980-82. Now back at the University of Cambridge, Pethica and a surface-science postdoc, Mike Pashley, had built an STM in which they had observed that tunneling seemed to be occurring even at anomalously low bias voltages. Pethica suggested (at the Oberlech conference, and perhaps before) that this might indicate that the tip and the sample were in contact, separated not by an air or vacuum gap but by dirt or oxide on the surfaces of the tip and the sample.³¹ This meant that STM images might be distorted by the tip pressing down into the sample, making the heights of surface structures and adsorbates appear anomalously large.

Pethica's observations were meant to caution STMers that some of their images were distorted by forces acting between the tip and the sample. However, Binnig, in collaboration with Quate and Gerber, began to consider ways to use Pethica's insight to their advantage. In this, they were perhaps also influenced by a close reading of the doctoral dissertation on metal-vacuum-metal tunneling that Clayton Teague had completed as

Russell Young's protégé at the National Bureau of Standards. In one section of the dissertation, Teague tried to calculate the forces acting between the two tunneling electrodes in his apparatus. Binnig, Gerber, and Quate, extrapolating from the calculations of Pethica and Teague, came to the conclusion that the forces acting between the tip and the sample could be used to measure surface topography, even for insulating materials.³² That is, if one could bring a somewhat "springy" probe close to (or even into contact with) a surface, the forces between the probe and the sample would be strong enough (as indicated by Teague's and Pethica's work) to deflect the probe. If you could measure that deflection, you could image the surface in much the same way the STM did. With this insight, Binnig and Gerber assembled a crude "atomic force" microscope with a probe made from a small, flexible cantilever (early cantilevers were often just pieces of aluminum foil) weighted at one end (with, for example, a small shard of diamond). As the cantilever was scanned over a sample, variations in the height of the sample caused the weighted tip to move up and down. An STM placed on the back of the cantilever, operating somewhat like Young's ultramicroscope, then measured those deflections and turned them into an image.

The invention of the AFM was, therefore, inspired in part by questions that had arisen *within* the nascent STM community. But the new technique was also inspired by a desire to radically expand the membership, and the instrumentality, of that community. Calvin Quate, in particular, was driven by the desire to turn STM into a useful instrumentality in the same way he had for acoustic microscopy. As early as 1983, he wrote the following in his lab notebook:

[T]he entire field of surface science—all of the manifold phenomena of surfaces—can now be studied in a new and meaningful way. Vacuum tunneling has now given us the tools that we need to progress. This area, this field, underlies so much of science[,] so much of technology. . . . It is a fundamental of operation that underlies vast areas of US technology—oxide coatings, corrosion resistance, materials of [all] kinds have surfaces. One might put it this way—is there an area of US industry, US technology that doesn't involve surfaces of some kind[?] The field of biotechnology that looms so large in our future is dependent on molecular structure—can we afford not to move on new techniques for viewing, for imaging molecular structure[?]³³

This passage makes it clear that Quate saw surface science as the application area that demonstrated STM's relevance, but also that he defined "surface science" much more broadly than the practitioners of that discipline.

For surface scientists, STM, done in vacuum, was a sufficiently revolutionary technique, whereas air STM, and the other variants that followed, were unrigorous and problematic. For Quate, though, STM's great drawback was that it could image only electrically conducting or semiconducting surfaces. The materials of interest to "US technology" couldn't be imaged with the STM. "Oxide coatings" used in microelectronics, "molecular structures" used in biotechnology, and all kinds of materials exposed to air and therefore prone to corrosive reactions—these are all electrically insulating and therefore can only be imaged with great difficulty (if at all) with an STM. One reason Quate readily picked up air STM from Hansma was that most technologically useful surfaces are made and used in air, rather than in a vacuum. Yet in air almost all materials form an insulating layer at the surface that makes it difficult or impossible to image them with STM. AFM, crude as it initially was, offered a way around that. With it, Quate could move beyond, or could radically redefine, "surface science."

Because they held very different views of what constituted "surface science," Quate and most surface scientists held very different opinions of the worth of the AFM. The AFM moved glacially slowly into mainstream surface science, but very quickly through the Zurich-California subcommunity. Paul Hansma, for one, was told about the AFM even before Binnig, Quate, and Gerber's first publication. After some personal lobbying from Quate, Hansma decided to retool his group to focus almost exclusively on force microscopy.³⁴ In hindsight, Hansma's decision to move from STM to AFM needs little justification; some early AFMers are even reluctant to acknowledge that they ever did STM at all. Yet Binnig, Quate, Hansma, and a few other early AFMers were taking an enormous chance on a finicky, difficult technique. By 1986, air STM was becoming more reliable and easier to build and use. AFM was neither reliable nor easy. Cantilevers were very difficult to make and impossible to standardize; the AFM's resolution was lower than the STM's; and at first AFM was shown to work only for a small (relatively uninteresting) class of samples such as graphite and mica.

Worst of all, the AFM's original detection system—an STM mounted on the back of the cantilever—was so erratic that even a good instrument only yielded images a small percentage of the time. By one Hansma graduate student's estimate, group members spent 85 percent of their time on maintenance of the continually breaking early AFMs.³⁵ In the remaining 15 percent, they might generate one publishable image in a month. This

was exactly the kind of snail's pace Quate, Hansma, and Binnig had hoped to avoid by switching from UHV to air operation in the first place; now, their switch to AFM seemed to be undoing those gains. Hansma, frustrated, imposed a rough rule: "one instrument, one paper." Each AFM would produce enough data for one journal article, then the group's focus would move on to the next-generation microscope.³⁶

With the STM-based cantilever detection system, however, getting data on an interesting sample and then replicating that data for publication became a considerable bottleneck. In practice, "one instrument, one paper" was only ever a rough guideline. The group's articles often incorporated data from more than one microscope, and graduate students might continue using a microscope even as Hansma, Marti, and Barney Drake (Hansma's longtime technician) worked on the next prototype. Still, if the AFMers were to continue to lead probe microscopy, they needed some technical improvement that would enable them to work faster.

The improvement came from an unexpected direction—though the Zurich-California groups were skilled at cultivating, and then exploiting, the unexpected. Some years before, Hansma had lent some space in his lab to a retired high school physics teacher named Sam Alexander. Alexander used the space for experiments intended to contradict or revise the theory of relativity. In Alexander's version of relativity, gravity operates differently at the atomic level than in Einstein's version. For instance, when rotated in the Earth's gravitational field, a long rod made from a dense material would change in length more than a lighter rod. Alexander's experimental apparatus, therefore, depended on measuring very slight changes in length by means of an "optical lever"—a laser beam bounced off the rods onto a photodetector several feet away. Small changes in the length of the rod would change the angle at which the beam reflected off, resulting in very large changes in where the beam hit the photodetector.³⁷

By 1988, Alexander's experiments were becoming increasingly sophisticated, yet his data continued to be inconclusive. Seeking a new tack, he and Hansma brainstormed ways Alexander could, instead, involve himself more in Hansma's research. Together, they hit on the idea of importing Alexander's optical lever to improve the finicky AFM. After all, both techniques involved measuring very small changes in position. So Alexander began working with Othmar Marti and Barney Drake to turn the AFM cantilever into an optical lever. At first, this meant simply gluing a

reflective piece of glass to the back of the cantilever and pointing a laser at the glass. As a sample was scanned underneath the cantilever, the piece of glass would go up and down by a few nanometers; the laser spot, bounced onto a wall a few feet away, would correspondingly dance by a few *inches*.

Over the next few years, the Hansma group iteratively improved the optical-lever detection scheme, making AFM a much more reliable and useful technique. This process took advantage of a resource to which a lab group at a research university usually has greater access than a group in a corporate or government lab: graduate and undergraduate students. During their time at Santa Barbara, Hansma's students would pick up some instrument-building skills, especially in the process of repairing skittish AFMs; primarily, though (at least in the 1980s), Hansma's students focused on characterizing new classes of materials and developing new applications for AFMs. Hansma, Marti, and Drake then imported the students' experiences as *users* to improve their next microscope design. As a result of this feedback, the group's AFMs became smaller, more reliable, and more user-friendly.

By the seventh generation of optical-lever prototypes, the Hansma AFM was reliable enough that in 1989 a local start-up, Digital Instruments, adapted it as an AFM "add-on" to its commercial STM. The next chapter will detail how, through commercialization, AFM became the overwhelmingly dominant mode of probe microscopy. But even before commercialization, the optical lever spread rapidly from Santa Barbara to Stanford and to other AFM groups. Though Hansma was not, in fact, the inventor of optical-lever AFM (two IBM researchers, Gerhard Meyer and Nabil Amer, preceded him slightly), he is usually regarded as the most effective popularizer of the technique.³⁸ As Calvin Quate put it, "the beam-bounce method was introduced by Meyer and Amer at Yorktown, and it is now widely used as a result of the excellent work by [Sam] Alexander" and the rest of the Hansma group.³⁹

Getting rid of STM detection made AFM easier than before, but not easier than air STM. The AFM probes remained problematic. Members of the Hansma group usually hand-assembled a probe from three parts: a foil strip or wire for the cantilever, a mirror for the optical lever, and a diamond shard for the tip.⁴⁰ Each probe, however, displayed the idiosyncratic preferences of its maker; Sam Alexander reportedly liked to use bee stingers for his cantilevers.⁴¹ Mirrors were made by hand-blowing a bubble of

glass until it popped, then coating one of the resulting pieces with reflective paint. Diamond tips were made by putting cheap diamonds bought in pawn shops between metal plates and shattering them with a hammer. The mirror, the tip, and the cantilever were all small and delicate, and gluing the pieces together, using a tiny brush (sometimes made from the group members' own eyebrow hairs), required a deft, trained hand.

The probes took considerable time to prepare and wore out regularly. Moreover, each probe was unique, so there was significant variation among images taken with different tips. At the time, such variation was not a major problem—AFM images were so new and rare that demand was high even without standardization. Looking back, AFMers today see the late 1980s as a time when “almost any” image could get published, often on the cover of *Science* or *Nature*.⁴² Yet they could see that once the instrument's novelty had worn off there would be a need for reproducible images produced with standardized cantilevers that were less time-consuming to make and use.

The solution, this time, came from Palo Alto. Stanford, after all, sits in Silicon Valley, home to an industry that specializes in manufacturing billions of very small, standardized, rigid objects—transistors. Since the 1960s, researchers had been using the lithographic techniques developed for semiconductor manufacturing to make other kinds of small structures—exotic electronic devices such as quantum wells, microfluidic channels, and tiny gears. Specialists in Calvin Quate's own field of surface acoustic wave research, for instance, were using lithographic techniques to make tiny, interdigitated structures for signal-processing devices in the early 1970s.⁴³ In the late 1980s, Stanford was one of the leading centers for such microfabrication techniques.

One of Quate's graduate students, Tom Albrecht, immersed himself in Stanford's clean rooms to learn how to make tiny, standardized silicon cantilevers.⁴⁴ The first were crude—less durable and lower in quality than the best hand-made ones, but more uniform and (discounting the overhead in development) easier to make. Later students created more and more variations—different shapes, sizes, materials, even cantilevers made from piezocrystals that would sense deflection directly, eliminating the need for the bulky optical lever.⁴⁵

It is difficult to overstate the importance of microfabricated cantilevers. Handmade probes were so labor-intensive that disagreements within a lab

group about the ownership of a probe could become quite heated.⁴⁶ Early AFM veterans who remember the days of handmade probes express profound gratitude to Tom Albrecht for developing microfabricated cantilevers. Indeed, according to Quate the AFM “evolved under the tutelage of three people: Gerd Binnig, Christoph Gerber, and Tom Albrecht. Binnig and Gerber were veterans of the STM. They fashioned the first instrument. Tom Albrecht was a fresh graduate student. He fabricated the first silicon microcantilever.”⁴⁷ That is, for Quate, the microfabricated cantilever was an innovation to be hailed in the same breath as the AFM. Not surprisingly, then, Quate was quick to distribute Albrecht’s cantilevers to Hansma and others. Groups that received these cantilevers began enthusiastically using them.⁴⁸ AFM images became easier to generate and—because they were more standardized—somewhat easier to interpret.

Outward Focus, Expanding Network

The arrival of Marti in Santa Barbara and Binnig and Gerber in the Bay Area and the frequent exchanges of ideas and materials between the Quate and Hansma labs contributed to the emergence of similarities in the ways the two groups approached experiments. This convergence evolved in tandem with the group’s innovations to probe microscope design. That is, air STM appealed to Hansma because it afforded a more flexible style of work that looked beyond surface science to a broad range of collaborators. Binnig and Quate’s experimental values were similar enough to Hansma’s that they borrowed air STM from him. Hansma’s values were similar enough to theirs that he then followed them into electrochemical STM. Once all three had committed to that expansive style of probe microscopy, AFM was a natural next step because it promised to solve some of air STM’s lingering problems.

Perhaps the distinguishing characteristic of the convergent Zurich-California approach to experiments was the tremendous volume of design modifications and new applications, many of which didn’t pan out.

At Stanford, that high volume of new designs and uses was possible because Quate recruited a large crew of students and set them working on multiple microscopes at once. With many projects going on at the same time in a tight-knit group, people, designs, equipment, and samples circulated freely between experiments. The AFM was invented when it was

partly because Binnig prospered in that environment. He could dream up a creative new idea, slap together a prototype (sometimes in less than a day), test it, scrap it, and start over with a fresh idea, borrowing a few elements from the last prototype.⁴⁹ Whereas in IBM's and Bell Labs' surface-science groups microscopes tended to last for years (occasionally even decades), in the Zurich-California groups microscopes had lifetimes of weeks. This was evolutionary variation and selection at an extraordinary pace.⁵⁰

This fevered style contrasted sharply with the way surface-science STMers worked. For surface scientists, ultrahigh vacuum and the requirement of "well-defined" surfaces demanded painstaking sample preparation and instrument design. After all, in UHV a microscope might fail to achieve atomic resolution for small, difficult-to-pinpoint reasons—perhaps a screw made from the wrong material, or an ion gauge (used to measure vacuum pressure) outgassing contaminants from its hot filament.⁵¹ Members of the Quate and Hansma groups, in contrast, could afford to be less careful about what samples they looked at and what materials they built their microscopes with. Hansma's group tried disposable razor blades as electrodes in a squeezable tunnel junction and, for a while, used surgical tubing, a coffee can, and a cement block for vibration isolation.⁵² In the Quate group, students might spend an afternoon using an AFM to image Scotch tape, or might put an AFM in a refrigerator in order to playfully obtain images of ice, or use an electrochemical STM to compare images of samples immersed in Coca-Cola and Pepsi.⁵³

Such an open-ended variation-and-selection strategy created the problem of identifying materials that could be usefully and intelligibly imaged with an STM or an AFM. One possibility, curiously, was to simply put found objects into the microscopes. Hansma demonstrated to visitors the capabilities of his seventh-generation optical-lever AFM, for instance, by imaging a leaf on a potted plant kept in the lab (figure 4.3). Another strategy was to identify congenial collaborators among Quate's and Hansma's local academic colleagues and try to image the materials those people specialized in, and/or to combine probe microscopy with the instruments they specialized in. For instance, in 1987 Hansma began working with Joe Zasadzinski, a recently arrived biological electron microscopist at UC Santa Barbara, to use electron microscopists' specimen-preparation techniques (freezing a sample, then coating it with a metal film to make it conductive) to image phospholipid membranes (Zasadzinski's specialty) in an STM.



Figure 4.3

The seventh iteration of the optical-lever AFM developed by Paul Hansma's group at UC Santa Barbara. Note the houseplant with its leaf placed in the sample holder of the AFM. The AFM's ability to image ordinary objects, with little preparation, was an important reason for Hansma's transition from STM to AFM. Source: Albrecht Ludwig Weisenhorn, *Atomic Force Microscopy in Liquids*, PhD dissertation, University of California, Santa Barbara, 1991. Photo by Paul Hansma.

This strategy didn't narrow the search for applications for STM and AFM much, though—research universities house specialists in a very wide variety of materials and techniques. One way to filter among this range of possibilities was to work with specialists in materials that the wider scientific community deemed “hot” at the moment. For instance, in the late 1980s, when high-temperature superconductors were all the rage, Quate's group quickly formed collaborations with Stanford's specialists in those materials. More lastingly, Hansma could see that the success of the biotech industry, and the talk of a large-scale Human Genome Project, made DNA a compelling material to image. As we have seen, that led to his early collaboration with Stuart Lindsay. Within the University of California at Santa Barbara, Helen Hansma prepared DNA samples, and provided interpretive know-how, that allowed Paul's students (and, later, her own group) to image genetic material. Paul and Helen's combined expertise then allowed them to establish further extramural collaborations in DNA imaging, such as one with Carlos Bustamante at the University of Oregon.

The impression I want to convey here is that Quate and Hansma were continually using STM and AFM to widen, and vary, their network of professional acquaintances, and to strengthen their ties to people who were already in that network. The latter were a somewhat motley crew: friends from grad school, spouses, people whose offices were down the hall, and so on. Over time, the Quate and Hansma groups came to be seen as willing collaborators. With that reputation, Quate and Hansma no longer had to rely on prior acquaintances, or search for new members of their network; instead, all kinds of specialists *came to them* seeking advice or collaboration.

Initially, each of the people who looked toward Santa Barbara and Palo Alto in this way already had some weak tie to the Quate and Hansma groups. For instance, in early 1987, Andy Gewirth, a postdoc in Al Bard's electrochemistry group at the University of Texas (UT), began trying to build an electrochemical STM. At first, Gewirth visited Alex de Lozanne, a former student of Quate's who was now on the physics faculty at UT.⁵⁴ De Lozanne was discouraging, though, so Gewirth decided to attend the 1987 STM Conference in order to look for help. There, he ran into Rich Sonnenfeld, one of Hansma's graduate students, whom Gewirth happened to know from their college days at Princeton. Sonnenfeld gave Gewirth the advice he needed to build his own electrochemical STM. Then, a few years later, Gewirth was able to use that tie to UC Santa Barbara to secure an invitation

from Hansma to visit for a few weeks to collaborate on modifying the AFM for use in electrochemistry. Likewise, in 1986 Jun Nogami, a graduate student at Stanford specializing in surface science, heard that Quate's group was working on STM and volunteered his services in specimen preparation. The results were so promising that Nogami decided to put off a planned postdoc in France, and instead "begged Cal for about a month to give me a job and finally he relented. And so I stayed with that group for about four years."⁵⁵

As the reputations of the Stanford and Santa Barbara groups grew, however, would-be postdocs and collaborators began to contact Quate and Hansma out of the blue, with little or no prior connection. Even though only a small fraction of those people struck up collaborations with the Stanford group or the Santa Barbara group, the absolute number of visitors to Palo Alto and Santa Barbara was quite large. Hansma, in particular, became famous in the probe-microscopy community for hosting a long series of postdocs who expanded the technique's application into broad areas of physics, chemistry, materials science, geology, and molecular biology.

Contending with Uncertainty

This portrait of how Quate and Hansma (and, to some extent, Binnig and Rohrer) worked might seem unremarkable. After all, exploiting the "strength of weak ties" in moving ideas from one social group to another is a well-known strategy.⁵⁶ Nor is it surprising that one or two probe-microscopy groups would develop many, many more such ties than their peers—that is how emergent networks often form.⁵⁷ More surprising, perhaps, are the *consequences* of the Zurich-California sub-community's network building. As they pushed the limits of probe microscopy, these groups increasingly contended with epistemic uncertainty and fragmentation.⁵⁸

To understand these consequences, it is useful to contrast the Quate and Hansma groups with their surface-science peers at Bell Labs and at IBM Yorktown. There, the greatest uncertainty was whether STM would work at all—hence, before successful replication, only junior researchers were willing to invest effort in it. After replication, the disciplinary canon of surface science (often embodied in STMers' senior managers) provided a framework for devising a series of experiments. At that point, the biggest

uncertainties facing a surface scientist were whether their STM would be operational long enough for them to write a sufficient number of articles to be promoted and whether their interpretation of an STM image would be accepted by their colleagues. These practitioners were relatively confident, however, that they would be able to choose, and correctly prepare, samples that would command attention from those peers.

The Zurich-California groups lacked such confidence. In modifying their microscopes to image a wider range of samples, members of these groups found it increasingly difficult to learn various disciplines' techniques for choosing and preparing those samples. It soon became clear that they would need help in regaining the certainty provided by a disciplinary canon. Barney Drake, Hansma's technician, recalled:

I remember [Hansma] saying at some point, "we have to have a biologist in the lab, to do biology. We're not biologists, we don't know how to prepare the samples." . . . I really felt like I got a great education, imaging so many different samples from these people who really knew their sample. . . . [W]e'd break out the biology textbook and try and learn about what we were trying to do. I remember one time flipping through a textbook and [thinking] "Oh, yeah, I've tried to image that."⁵⁹

Bringing in visitors, though, merely displaced uncertainty. If Hansma found a competent biologist to work with, his lab could do credible biology—but how to know which biologists were competent? How to know which ones had the right samples and sample-preparation skills?

As we have seen, one initial solution to this problem was to rely on collaborators who were already personally known to Quate, Hansma, Binnig, or Rohrer. But as the volume of people visiting their labs and seeking collaborations grew, pre-existing personal ties were insufficient. Another solution, therefore, was to manufacture these personal ties concurrently with collaborators' stays. Members of—and visitors to—these groups spent informal time with each other in ways that would have been somewhat unusual among surface-science STMers at Bell Labs and at Yorktown. Binnig and Gerber, for instance, became fond of playing golf with Quate's students during their year in California. Quate himself successively took up new "extreme" sports (kayaking, windsurfing) as his students discovered them, and the Quate group as a whole regularly played volleyball together.⁶⁰

Yet another solution was to use *ad hoc* rules to filter out potential collaborators who weren't serious enough. Remember, for instance, that Binnig

paid little attention to Quate until the Stanford group began to send him their first, crude STM images. Likewise, Hansma eventually articulated a few rules of thumb for allocating space in his lab: visitors had to be committed enough to bring their own funding and materials, and they could stay either for “over six months or one day or less” (the longer period for serious collaborators, the shorter stay for people passing through town).⁶¹ A stay of any other duration, Hansma thought, would distract his students but would be unlikely to produce any worthwhile research. As Steve Shapin has shown, such rules of thumb often flourish in research communities that contend with uncertainty. *Ad hoc* rules are flexible enough to accommodate surprises, yet they can still guide action in the moment.

Hansma became known for his “proverbs,” which his former students and postdocs repeat to this day. Most were closely connected to his group’s potentially chaotic variation-and-selection strategy. To stimulate variation, he encouraged his team to “make as many mistakes as you can as quickly as you can” and to “do everything as poorly as possible” (that is, to throw experiments together with little polish or gloss, concentrating on quickly getting the basics to work).⁶² Conversely, to force a selection from those variations, Hansma sometimes set arbitrary limits on how long a particular microscope could be used—for example, as Scot Gould (one of Hansma’s graduate students) recalled, “one image, one paper, and then we’d build the next microscope.”⁶³ If an image wasn’t forthcoming, then Hansma would set a deadline. As Barney Drake remembers it,

[W]e had a lot of successes on Friday afternoon, because he would give me a deadline, “okay, you can work on this thing for another week.” Usually I would get emotionally attached to a project because I had put so much into it that I didn’t want to change direction. Paul was great, he would see when something was a dead end, much before I could. . . . He would usually [say to] me, “okay, this is the last week.” So I’d be fiercely trying to get something done. A lot of Friday afternoons, when this was the last day we were going to run, we got images and then, “okay, we’re done, it’s time to move on.”⁶⁴

Instrumental Artifacts

The proliferation of STM and AFM use in new areas was so rapid that no amount of “proverbs” could prevent missteps. Misinterpretations caused by deceptive features (“artifacts,” in microscopists’ lingo) in STM and AFM

images were unavoidable in the early years. As Jun Nogami remembers it, image interpretation “was all over the map. I’ve published some things that are wrong, like everyone did. Basically we were saying ‘these are the atoms’ and that’s a tremendously naive point of view.”⁶⁵ Naive interpretations sometimes emanated from the Zurich-California groups, and sometimes from Bell Labs and IBM surface scientists. Meticulous care could prevent some overinterpretation, of course; Nogami points to Randy Feenstra’s Yorktown group as one that was careful not to equate the “blobs” in their STM images with atoms. But in a rapidly moving field, some missteps were bound to be made. The reactions of the Zurich-California groups and the surface scientists at IBM and at Bell Labs to misinterpretation and overinterpretation tell us a great deal about these different regions of the probe-microscopy network. For example, early on, many STMers chased the idea of doing vibrational spectroscopy of a molecule adsorbed onto a substrate. In 1987, Calvin Quate’s group published a graph of tunneling current versus bias voltage for one such system: sorbic acid adsorbed onto graphite immersed in liquid helium.⁶⁶ “Discrepancies” in these data were noted very quickly, however—for instance, in a 1989 review article by Bob Hamers, an IBM Yorktown surface-science STMer.⁶⁷ Over time, a consensus emerged that the Stanford paper was too hasty: as Hamers noted in 1996, “while at least one early study reported observing . . . vibrations of sorbic acid, more recent studies . . . have generally not found any reproducible structure in the [tunneling current versus bias voltage] curves that can be attributed to vibrational information.”⁶⁸

Yet Hamers himself was involved in one of the more famous early missteps of surface-science STM. As was noted in chapter 3, the Demuth group at IBM Yorktown (including Hamers) and Bob Wilson and Shirley Chiang’s group at IBM Almaden simultaneously published images of the same surface reconstruction of silver deposited on a silicon substrate. But the groups’ interpretations “yielded completely different models.”⁶⁹ Subsequently, other surface scientists used their discipline’s full toolkit of instruments to show that both IBM models were incorrect.⁷⁰

My point in describing the sorbic acid and the silver-on-silicon episodes is *not* to condemn either Calvin Quate or Bob Hamers—just the opposite. I want to show that the innovation process in probe microscopy—whether at Stanford, at Santa Barbara, at Yorktown, or at Murray Hill—involved taking interpretive risks that sometimes didn’t pan out scientifically but still

advanced the technique. Members of the Quate and Hansma groups were (and are) refreshingly candid about the misinterpretations generated in the rush to publish of the late 1980s, but are also quick to point out the positive net effects even of results that were later disputed.

For example, Mike Kirk, a Quate student, noted in his 1989 dissertation how the Quate group's research on high-temperature superconducting materials put them in good company, but also led them to make mistakes:

[T]he 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. . . . [J]ust 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 [APS] meeting so it had to be written quickly and, as a result, the interpretation was not entirely accurate.⁷¹

Other members of these groups freely acknowledge that, because knowledge of exactly how an STM or an AFM actually worked was so incomplete, *some* of their results from those days could today be seen as "wrong," "garbage," or "artifacts."⁷²

Even debatable data were valuable precisely because they elicited debate. Images, even poor ones, of samples that were important to some disciplinary community would spur interest in STM and AFM among members of that community. Jan Hoh, a postdoc at UC Santa Barbara, sums up Hansma's attitude toward the instrumentality of image artifacts this way:

I learned from him that you can actually be wrong but still make an important contribution. This paper on hydrogen bonding, I don't know if it's wrong but it's not clearly demonstrably right. There were papers like that, and other papers that Paul published, that are incredibly inspirational. That's one thing that I learned from Paul, is that you can be wrong but a properly placed and properly written paper, something that puts things into context, can be incredibly important for a field because it stimulates thinking, it motivates people.⁷³

In an important sense, this mode of operation had governed the Zurich-California groups from the beginning. The first atomic-resolution silicon

7×7 images, for instance, “stimulated” enormous interest among surface scientists, even if that discipline concluded that the surface-reconstruction model put forward by the Zurich team was in need of correction.

One advantage of “stimulating” and “inspiring” interest in various disciplines was that the Zurich-California groups could leave it to those disciplines to polish probe microscopy’s rough edges. The Zurich team, for instance, revolutionized surface science with their images of the silicon 7×7, but they themselves didn’t have to become full-fledged surface scientists. That way of working was then taken up by the Hansma and Quate groups. As Paul Hansma puts it, “[I’m not] trying to become established in a particular field. Because that just doesn’t make much sense. [There are] people who’ve spent a lifetime learning electrochemistry, and I don’t want to have to spend that same lifetime. I’d rather build new instruments.”⁷⁴ That attitude, perhaps, defines the difference between the Zurich-California groups and the early surface-science STMers at IBM and at Bell Labs. The surface scientists were committed to spending a lifetime learning their discipline. If their images were to be challenged by other surface scientists, they would have to defend or correct their interpretations. Quate and Hansma, on the other hand, accrued their reputation not by inhabiting one discipline, but by innovating the technology of probe microscopy so that it (and they) could move from one disciplinary audience to the next.

Graphite and DNA

No incident illustrates the differences between the aforementioned sub-communities more than the strange case of STM images of DNA deposited on graphite. When it was discovered that a tunneling microscope could operate in air, it was not clear at first what an air STM was good for or how reliable its images were. There was no immediately obvious or generally accepted way to calibrate the quality of an air STM (or its operator). For ultrahigh-vacuum STM, atomic resolution of the silicon 7×7 was the yardstick by which machines and people were measured, but there was no similar material for air operation.

Eventually, highly oriented pyrolytic graphite (HOPG) took on that role. Graphite was simple to clean (the top atomic layer could simply be peeled off with Scotch tape), and atomic-resolution STM images of HOPG were easy to produce. Moreover, the Quate group discovered that very

pure samples could be obtained for free (or at nominal cost) from Arthur Moore, a researcher at Union Carbide. That information spread quickly, and Union Carbide's graphite suddenly became the material of choice for many STMers.⁷⁵

Graphite was a user-friendly material, but data from it were problematic. In STM images, the apparent vertical distance from the outermost atoms on the surface to the atomic layer beneath them (the "atomic corrugation") was much larger than had been predicted. As was noted earlier, John Pethica eventually explained that the probe was pushing into the graphite, exaggerating vertical distances. Later, a Stanford graduate student, Howard Mizes, concluded that graphite samples could tunnel to multiple locations on an STM tip at once, so the resulting image exhibited a Moiré pattern or other artifact. Such artifacts meant that the relationship between the apparent "graphite atoms" in an STM image and the actual placement of graphite atoms on the sample surface was difficult to interpret.⁷⁶ These effects were more pronounced in air than in vacuum. Surface scientists, distrustful of air operation anyway, were therefore especially skeptical of images of graphite. As Bob Hamers puts it:

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 [of] maybe a tenth of an angstrom. So I thought, "am I doing something wrong?" My manager, Joe Demuth, was saying "why can't you get this? Everybody else is doing this in air and seeing height changes that are 2 angstroms high." Then a few months later . . . people were reporting corrugations of 20 angstroms high and 200 angstroms high between atoms that were only 1.97 angstroms separated. . . . So I kind of got disgusted at that point and figured this graphite doesn't look like a good place to spend my time.⁷⁷

Yet these criticisms did little to dampen the popularity of graphite. As Jim Gimzewski, a surface-science STMer at IBM Zurich, recalled (with some disgust), at the 1987 STM Conference "suddenly the world went graphite because everybody could image suddenly graphite but not other things. . . . It really got a bit boring with the graphite."⁷⁸

One reason for graphite's popularity was that STMers could deposit other molecules—particularly biological materials—on top of it and image them. At the time, the biological molecule that STMers were most keen to image was DNA, partly in hopes of support from the nascent Human Genome Project. This large-scale effort to sequence the human genome was funded by the US Congress starting in 1987, and was formally founded

in 1991 as a government entity jointly supported by the Department of Energy and the National Institutes of Health (NIH). Similar state-sponsored genome projects sprouted up at the same time in Europe and Japan. One thrust of the Human Genome Project sought new technologies for accelerating gene sequencing, such as polymerase chain reaction, chromatography, robots for automating routine procedures, and microfabricated arrays or "gene chips."⁷⁹ Some probe microscopists hoped to attract interest from the Human Genome Project by showing that STM or AFM could be used alongside these other techniques. That is, they argued that a probe microscope might be able to physically "read" the base-pair sequence in a strand of DNA. As Paul Hansma puts it, "that dream drove a lot of us. In fact, imaging DNA is probably the project that I spent the most intellectual effort on without ever publishing a paper."⁸⁰ This dream was one reason Hansma switched from STM to AFM, since he found it easier to believe that an AFM could image organic molecules. Others, however, continued trying to image DNA with STM, and were more willing than Hansma to publish such images.

One image, in particular, raised hopes, and then invited criticism, of STM of DNA. Originating in John Baldeschwieler's group at Caltech and featured on a 1990 cover of *Nature* (see figure 4.4), it purported to demonstrate that an STM could produce atomic resolution images of DNA.⁸¹ If Baldeschwieler was correct, the STM was close to being able to determine the base pairs in a DNA sequence. This image of DNA on the cover of a prestigious scientific journal raised interest at the NIH in the possibility of using an STM to sequence DNA. Over the next few years, that agency awarded several grants dedicated to topics such as "AFM and STM in Novel Approaches to Sequencing" and "Feasibility Studies for STM/AFM-Based DNA Sequencing."⁸²

By 1991, a consensus had begun to emerge that Baldeschwieler's image, and STM of DNA on graphite more generally, were problematic. Surface-science STMers, of course, had long insisted that STM images of biomolecules were unreliable. But more challenging for Baldeschwieler and other DNA-on-graphite researchers were people who had already published STM images of DNA but who now pointed out the need for better protocols, especially when using graphite substrates. Two of the most influential doubters were Stuart Lindsay (Hansma's collaborator at ASU) and Tom Beebe (an assistant professor at the University of Utah), the principal

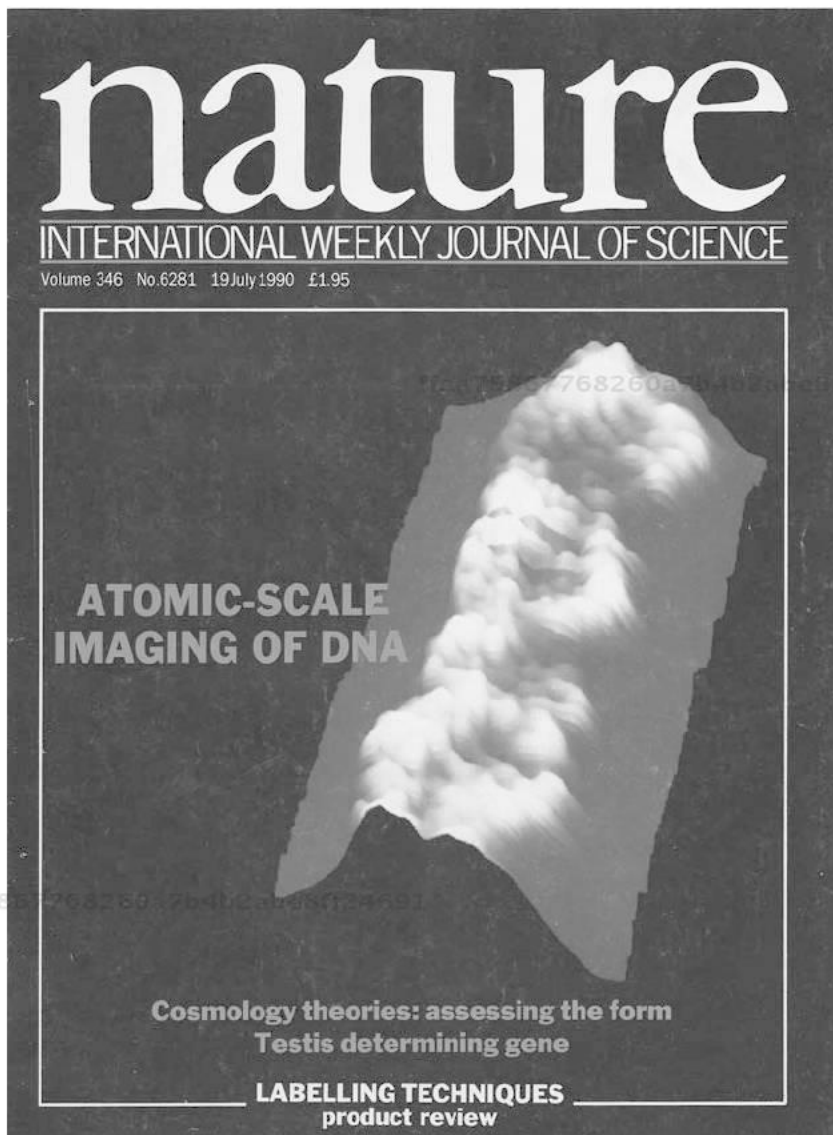


Figure 4.4

Cover of *Nature* 346 (1990), number 6281. Reprinted by permission of Macmillan Publishers Ltd.

investigators on the two DNA-sequencing NIH grants just mentioned. Like Baldeschwieler, Lindsay and Beebe had published STM images that purportedly resolved the grooves of the double helix of DNA (Lindsay on gold, Beebe on graphite).⁸³ Gerd Binnig and Heini Rohrer, too, had published images of DNA on graphite of similar resolution, but now Binnig joined the skeptics of DNA-on-graphite images.⁸⁴

In part, this new-found skepticism was a reaction to the astonishing growth of STM research on DNA in the late 1980s and the early 1990s. "I don't regard that as my highest quality science by any means," Stuart Lindsay now says. "Just because of the tremendous pressure at that time. . . . There was just this tremendous rush to publish."⁸⁵ Unlike some other groups' images of DNA on graphite, Lindsay's images of DNA on gold were later reliably reproduced by other groups. Yet at the time Lindsay felt the strain of keeping pace with the large volume of (what he saw as) occasionally dubious research on DNA deposited on graphite.

Eventually, as the people who were doing STM of DNA gained experience, and got to know each other at the STM Conferences, some of them decided to push their colleagues for a slower pace and more rigor. As Tom Beebe puts it,

I knew Stuart from meetings and we would always talk together. We would often talk in the same sessions. For perhaps the past couple of years at that point [in 1990], it was now becoming known in the STM community that graphite is a dangerous substrate for biological experiments and you have to really be careful about statistics and publishing what you know is reproducible and is not an artifact, do the right controls, and so on. Within the STM community, this was becoming a nagging and somewhat embarrassing issue, that other people who didn't appreciate this need to do controls and to be careful about graphite were continuing to publish a lot of what I thought, quite frankly, was garbage. It was not really the biomolecule that they said it was. So, by that time I had collected hundreds of images of beautiful-looking DNA artifacts and, and I called Stuart and said, "Do you guys see a lot of artifacts on graphite that look just like DNA?" He said, "Oh yeah. Of course." And I said, "Well, so do I."⁸⁶

To correct this "embarrassing" tendency, Beebe and Binnig each wrote an article indicting images of DNA on graphite.⁸⁷ They argued that graphite is so protean that isolated images of it could look like almost anything. Some STMers joked that they could find STM images of graphite defects in the shape of any letter in the alphabet. One common defect happened to have the same "pitch" (repeat distance) as DNA, so an STM probe wandering

over a surface could seem to find DNA even if no nucleic acids had been deposited on that piece of graphite.

After the publication of the Binnig and Beebe articles, researchers quickly lost interest in air STM, and in STM of DNA, and the National Institutes of Health lost interest in funding such work. Probe microscopists complained for many years that reviewers at the NIH had over-interpreted the Binnig and Beebe papers as providing grounds for dismissing almost all STM research (even when substrates other than graphite were used) and even much AFM research (even though AFM is not prone to the same artifacts).⁸⁸

Yet skeptics such as Binnig, Beebe, and Lindsay hadn't intended their criticism to be interpreted as a blanket condemnation of all STM research on DNA. Binnig's article, for instance, raised objections to graphite as a substrate material, but suggested that STMers should instead begin depositing and imaging DNA on molybdenum disulfide. Beebe pointedly excepted from his criticism groups (such as Lindsay's) that were imaging DNA on gold and other non-HOPG substrates. Indeed, a few holdouts, notably Reinhard Guckenberger (at the Max Planck Institute in Munich), and Tomoji Kawai (at Osaka University), ultimately showed that reproducible atomic-resolution STM images of DNA could indeed be obtained.⁸⁹

Yet these results attracted only modest interest, and they are not widely known. As Lindsay puts it,

[T]he fact that you can image DNA on metal surfaces is . . . probably not known 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.⁹⁰

That is, probe microscopists' (and others') interpretations of DNA-on-graphite research were strongly influenced by the presence or absence of alternative experiments—"more important stuff"—for them to pursue.

So long as air STM was much easier than AFM or UHV STM, many probe microscopists stuck with it, despite persistent qualms about graphite defects and other sources of artifacts. As AFM became an easier, more reliable technique, however, many air STMers came to see those qualms as damning. There were—as Binnig, Beebe, Lindsay, and others pointed out—ways to alleviate anxieties about air STM by undertaking more rigorous protocols. However, after about 1991, the difficulty of doing such

experiments made them unappetizing to probe microscopists in comparison with the many easier experiments they could now do with AFM. The existence of those easier experiments was, as we have seen, dependent on the continuing innovation in probe-microscope technology coming out of UC Santa Barbara, Stanford, Zurich, and elsewhere—innovations that made the STM and especially the AFM faster and more reliable, their images easier to interpret, and specimen preparation less demanding. STM of DNA might well have continued to boom had it not been for developments such as optical-lever detection and microfabricated cantilevers. As it was, most of the people who started out in STM of DNA drifted, one by one, to AFM—even such holdouts as Lindsay and Guckenberger.

The Demographics of Uncertainty

As the graphite case shows, the continual appearance of innovations in usability (compact STM, air operation, the tube scanner, AFM, optical-lever detection, microfabricated cantilevers) in the late 1980s and the early 1990s had complex epistemic consequences, especially for the Zurich-California portion of the probe-microscopy community. Technical innovations such as air STM and social innovations such as an annual STM Conference made it much easier for large numbers of people to join the probe-microscopy community. Developments such as optical-lever detection and microfabricated cantilevers made it possible for Quate's and Hansma's groups, and for other groups, to forge ahead with new *uses* for the microscope, rather than focusing entirely on fixing finicky, unreliable instruments. Those new applications of probe microscopy created possibilities for collaboration with new disciplines, and therefore created opportunities for representatives of those disciplines to enter the probe-microscopy community.

Thus, the late 1980s saw a dramatic increase in the number of probe microscopists, and an increasing diversity of types of probe microscopists. We will see in the next two chapters that those trends were amplified even further by the appearance of commercial instruments. The growth and diversification of the probe-microscopy community brought some clear benefits to Binnig, Quate, Hansma, and their close collaborators, but those trends also entailed some epistemic risks. The newcomers to the technique sometimes repeated mistakes that old-timers had overcome years before, and their presence increased the pressure on everyone to publish quickly.

In the graphite case, veteran probe microscopists stepped in to discipline newcomers' use of the technique—but not before the credibility of air STM images had suffered some permanent damage.

Quate and Hansma tried to overcome these epistemic uncertainties by welcoming into their groups visitors who brought with them discipline-specific knowledge of how to frame research questions, prepare samples, and interpret images. Yet even close-knit collaborations with disciplinary experts couldn't completely resolve the epistemic uncertainties that accompanied probe microscopy's demographic shift. With so many collaborators passing through their groups, learning the technique, and then returning to their home disciplines, Quate and Hansma couldn't possibly monitor developments in every application area that originated in their groups. Nor could they be sure that everything produced by their groups would withstand scrutiny—to do so would have required them to spend a lifetime learning the values and practices of every discipline they wanted to reach out to.

The same innovations in usability that created epistemic uncertainty could also be used, however, to sidestep them. By demonstrating the relevance of probe microscopy to many different disciplines, and by making probe microscopy user-friendly enough for members of those disciplines to adopt the technique, the Zurich-California groups formed both strong and weak ties to a very large network of other scientists. The leaders of the Zurich-California groups—Binnig, Quate, Hansma—eventually learned to trust that the various networks to which they were tied would work out the best way to use STM or AFM on their own. Facts and procedures might be uncertain at the centers of probe microscopy, but those centers were at the periphery of the disciplines that were evaluating the images produced with STM and AFM.

One aspect of Binnig, Quate, and Hansma's response to uncertainty was, therefore, to simply set it aside and concentrate on moving the technology forward. Mistakes might occasionally be made, and images produced too hastily, but any epistemic ill effects could be confined. Because of innovations in usability, the probe-microscopy community had grown and diversified enough that it now contained niches that were only vaguely aware of each other. One niche might no longer be able to communicate easily with another—surface-science STMers might, for instance, be less and less able to talk to or evaluate biological AFMers. Epistemic uncertainties could,

therefore, be consigned to separate niches, to be worked out by the specialists in the disciplines most affected by them. Meanwhile, technical innovations and feedback from user experience could be transported from one niche to the next. The central nodes of the probe microscopy network were those that could develop and demonstrate such innovations and broker them from niche to niche. The Zurich-California groups pioneered such brokering, and dramatically amplified their productivity as a result. Soon, however, they would have to share that task with a new set of organizations: start-up companies founded to broker probe microscopy for profit.

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5 Digital Instruments: Commercialization in a Changing Community

In the 1980s, the commercialization of academic research suddenly became a much-touted, much-disputed, much-studied phenomenon. In a sense, of course, the selling of professors' knowledge is as old as the university itself. In the United States, the expectation of commercial return has encouraged university research since at least the middle of the nineteenth century. What was new in the 1980s was not the invention of commercial possibilities, but the discovery of commercializing activities that were already ongoing. That heightened awareness of academic commercialization has generated a voluminous literature by economists, management scholars, sociologists, and (to a lesser extent) historians.¹ That literature, in turn, has evolved in tandem with changes in university and government policy intended to amplify or accelerate commercialization of academic research—changes such as the founding of university technology-transfer offices, and legislation intended to encourage professors to patent their research.²

Probe microscopes became commercial products in the late 1980s and the early 1990s, largely through the efforts of start-up companies affiliated with the university groups discussed in chapter 4. These companies are therefore an excellent test case for understanding how the policy changes and institutional innovations of the 1980s affected the process of academic commercialization. In some ways, probe microscopy exemplified the benefits of the new commercialization paradigm, in that STM and AFM companies generated substantial royalties for the University of California system and a few other schools. However, in important ways, the founders of these companies were driven by developments within the probe-microscopy community, and by their encounters with commercialization in the 1970s, rather than by the structural spurs to commercialization effected in the 1980s.

These companies ensured the widespread adoption of STM and AFM in a variety of disciplines and industries. When nanotechnology initiatives were being formed in various countries, in the early 2000s, the existence of STM and AFM start-ups was often invoked as a necessary first step toward—and a reason for optimism about—the reaping of large-scale, economy-wide benefits from the funding of nanotechnology research. Proponents repeatedly compared nanotechnology to the California gold rush of the 1840s and the 1850s, declaring that the entrepreneurs who made the earliest, most reliable profits were those selling “picks and shovels” that then allowed other prospectors to find gold.³ The STM and AFM, on that telling, were the picks and shovels of nanotechnology; the companies that sold them (to mix metaphors) were the “weathervanes” indicating that the storm of nanotechnology was coming.⁴

Commercialization, therefore, was central to probe microscopy’s transformation from an instrument-oriented community into an instrumental community able to influence events beyond the walls of the laboratory. Commercialization extended, and radically amplified, the network-building activities we examined in chapter 4. STM and AFM companies were, therefore, subject to the same epistemic and normative uncertainties that the Zurich-California groups had to contend with. Alliances with those groups—exchanges of ideas, personnel, artifacts, and rules of thumb—aided SPM companies in overcoming those uncertainties.

In time, manufacturers of STMs and AFMs both supplemented and supplanted the Zurich-California sites as “centers” in the probe-microscopy network. One manufacturer, in particular—Digital Instruments (DI), based in Santa Barbara—essentially doubled the size of the probe-microscopy community between 1987 and 1990, and continued to sell thousands of microscopes in the 1990s. DI became the entry point to probe microscopy for at least a plurality of the community; its role will, therefore, be the primary focus of this chapter. By selling so many microscopes, many of them to people who didn’t intend to do research or to get involved in the probe-microscopy community, DI dramatically changed the nature of the network connected through use of the STM and AFM. (In the final chapter, we will see that in the 1990s the policy makers who promoted nanotechnology took advantage of the proliferation of weak ties enabled by DI’s expansion of probe microscopy.)

What Is “Commercialization”?

It is tellingly difficult to pick out a starting point for the commercialization of academic probe microscopy. In the beginning, STMers built their own microscopes from a mix of off-the-shelf parts, custom-ordered components, and pieces they made themselves or had made in their organizations' machine shops. Probe microscopists always depended on commercial suppliers; indeed, knowing where to buy the right parts was an important element of the tacit knowledge needed to build an early STM. But STM builders—whether in corporate, academic, or government labs—were also very active consumers of commercial products. They petitioned suppliers of vacuum chambers, piezoelectric crystals, and other parts to design products specifically for the STM market. Some suppliers brought early STMers in as consultants to help them do so; a few of those suppliers even eventually sold complete STMs.

If, therefore, “commercialization of academic research” refers to the conversion of professors' knowledge into marketable products, there was no point at which academic probe microscopists weren't involved in such activities. Moreover, the formation of the probe-microscopy community was facilitated by a lack of clear distinctions between “academic” and “corporate” researchers. For instance, Heinrich Rohrer's sabbatical at UC Santa Barbara in the 1970s led to Paul Hansma's taking up STM. Conversely, the STM moved to the Universidad Autónoma de Madrid because Nico García and Arturo Baro each made extended stays at IBM Zurich, and it moved to Cambridge University via John Pethica's stint at Brown Boveri. Othmar Marti, a graduate student who received his degree from ETH Zurich for work primarily done at IBM, built the analog portion of the control electronics for several of the first STMs at IBM Zurich—electronics that, by 1988, were being marketed by VG Instruments.⁵

Calvin Quate's group at Stanford probably best exemplifies the ways in which academic probe microscopy was always already commercialized. His acoustic-microscope work in the 1970s was supported by IBM, and Quate routinely sent IBM researchers SAM images of technologically important samples, such as gallium arsenide wafers and materials used in magnetic bubble memory.⁶ Quate also promised to (and did) “provide IBM with a supply of well-trained students,” and he gave advice to IBM

(and other companies') researchers who were building their own acoustic microscopes.⁷ In turn, he used the fact that companies were replicating his invention as evidence of his group's progress in reports to his Air Force funders.⁸ His experience with acoustic microscopy in the 1970s and the early 1980s therefore gave him an excellent sense of how new instrumentation could answer IBM's specific manufacturing needs

When Quate moved into STM, he quickly applied for, and received, a grant from IBM in support of both acoustic and tunneling microscopy. Several of his former students and postdocs who were working at IBM followed him into tunneling microscopy. And, as we have seen, his hosting of Gerd Binnig in 1985 and 1986 resulted in the invention of the AFM. Thus, before 1986, Quate's activities were deeply imbued with commercial priorities—even though he was not, at that point, much involved in the activities that were supposedly most emblematic of academic commercialization in the 1980s. He was not, for instance, listed on the original AFM patent, nor did he show any interest in founding a probe-microscope company. Indeed, he was initially somewhat discouraging toward entrepreneurial activity inside his lab. Yet it is clear that the knowledge being produced by Quate's group regularly flowed to IBM (and to other companies). Quate's research seems, in fact, to have been more closely tied to IBM's manufacturing capability than that of the IBM surface scientists we examined in chapter 3. Certainly, the possibility of contributing to marketable products was an important consideration in Quate's shifts from SAM to STM to AFM.

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Thus, commercial activities were ubiquitous among probe microscopists from the very beginning of their community—perhaps more so among some academic participants than among those based in corporate labs. However, not all forms of commerce were present from the start. Looking back, probe microscopists do identify the advent of a certain kind commercial activity as an important turning point in the history of their community: the sale of microscopes, for money, by newly formed organizations dedicated to that task.

It's easy to see why this moment is remembered distinctly. The majority of probe microscopes today are bought from commercial manufacturers. The probe-microscopy community would be much smaller, and narrower

in its focus, had STM and AFM start-ups not emerged. Yet the transition to the era of purchasable microscopes was, on closer inspection, quite gradual. One could buy an STM even before separate organizations existed to handle such transactions. Once such organizations did come into being, they sometimes found it necessary to barter their products in exchange for services or knowledge rather than cash. Quasi-commercial trading for probe microscopes (and their components) existed in parallel with more formally commercial markets for quite some time.

For instance, IBM's Zurich and Yorktown labs both created internal markets for batch-produced microscopes that were made available to their researchers. As we have seen, the "consumers" of the Yorktown microscopes treated them as incomplete kits that had to be modified or rebuilt if anything useful was to be gotten out of them. A few of these Yorktown microscopes accompanied their users when they left IBM to take up faculty positions. Similarly, when Bell Labs researchers took new jobs elsewhere, they were sometimes able to take with them the STM software that they and the other denizens of the tractor shed had developed together.

Likewise, the Hansma group's most trusted collaborators sometimes were offered a microscope at the end of a visit. Such gifts saved the collaborators valuable time. In return, the UCSB group's productivity would be increased, since collaborators who continued research that they had begun in Santa Barbara could use these gift microscopes to quickly generate data for articles on which Hansma and his students could be listed as co-authors.⁹ For the person who received such a microscope, the time saved was an enormously valuable commodity. Probe microscopy was moving quickly, and there was a lot of low-hanging fruit—samples that were obvious candidates to be imaged—that would not wait to be plucked.

The desire to save valuable time drove some newcomers to probe microscopy to ask established researchers to make them a microscope, either for free or for a relatively low price. Those veteran researchers were apparently more likely to oblige if the request was for a specific type of microscope that they specialized in, that few others had replicated, and that hadn't yet been commercialized. For instance, Stuart Lindsay gave away a few controlled-environment STMs (in which *in situ* chemical processes could be observed) because he was one of the pioneers of the technique, and because the leading STM manufacturers didn't yet make high-quality instruments of that kind.

A few grad students were willing to make microscopes at their colleagues' requests because they were looking to trade their know-how for a little money and/or reputation. For instance, Brian Swartzentruber, Jene Golovchenko's former technician at Bell Labs, built four or five STMs for university faculty members (mostly his adviser's former students) and for government researchers, on his PhD adviser's behalf. He also sold STM software to those groups (and a few others) for "supplemental income" while in graduate school.¹⁰

Probably the graduate student who went furthest in selling STM kits on the side was Douglas Smith, at Stanford. Smith's Tunneling Microscope Company, founded in 1986, might even qualify as the first probe-microscope start-up company. However, the Tunneling Microscope Company was a rather ambiguous marker of the new start-up era. The "company" had only one "employee" (another Quate student, Mike Kirk, who helped with product assembly), and was in existence for about a year. People today remember it mostly for the bright red hats with the company logo that Smith gave away. Yet, even as informal as the Tunneling Microscope Company was, Quate saw it as an unwelcome distraction. As Mike Kirk puts it, "basically Dr. Quate said '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 [his dissertation], graduate, and move on."¹¹

Smith's product was a cheap (less than \$10,000), bare-bones STM for which customers supplied their own software and electronics. His customers, like users of the IBM batch-produced microscopes, had to demonstrate their instrument-building abilities to get Smith's incomplete kits to work.¹² These customers generally had the expertise to build an STM, but they needed Smith's kits to save them time. For instance, one buyer was Rich Colton, a senior surface scientist at the Naval Research Laboratory who returned from a sabbatical with John Baldeschwieler's Caltech group intent on building an ultrahigh-vacuum STM. While that instrument was under construction, he bought one of Smith's instruments in order to do a "feasibility study" of various applications of STM.¹³

Smith's customers seem to have been people who were closely connected to the Quate group but not active collaborators. For instance, Rich Colton's boss, Jim Murday, was also a grant officer at the Office of Naval Research who funded Quate (and corresponded with him frequently). John

Foster, one of Quate's acoustic-microscope students who moved to IBM Almaden, bought a Tunneling Microscope Company instrument to see if an STM could be used to modify surfaces or adsorbed molecules and therefore function as a data storage read/write device. Gerd Binnig seems to have been a conduit for another sale—after Stanford, Smith went to Munich to do a postdoc at Binnig's IBM-sponsored outpost, and at the same time one of Smith's STMs wound up in the hands of Wolfgang Heckl, a grad student at the Technische Universität München who later succeeded Smith as Binnig's postdoc.¹⁴

Digital Instruments

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For most of the 1980s and the early 1990s, therefore, probe microscopes were being exchanged—for money, co-authorship, interesting samples, know-how, and so on—before, and then in parallel with, the existence of firms dedicated to selling the instruments. These quasi-commercialized microscopes were often deliberately incomplete, so that the buyer could modify them for his or her particular uses. The “markets” for such instruments were usually confined to their makers' organizations, or to a close network of collaborators and other personal contacts. Building these microscopes was a sidelight, not a job unto itself.

Through 1985, STMs were so difficult to build, and so unreliable in operation, that it would have seemed crazy to think that selling STMs for cash could be a way to make a living. Moreover, at that point the STM was demonstrably useful for only a very narrow range of applications—the potential market for a commercial STM cannot have seemed very large. Between 1985 and 1987, though, innovations appeared that made the technology more reliable and more widely applicable: air operation (and operation in water and other fluids), the tube scanner, the compact STM, and so on. At the same time, the annual STM Conferences made it much easier for people who had heard of tunneling microscopy but had no personal connection to its practitioners to become acquainted with the technique.

These factors—or, at least, the insight that these factors would soon come into play—were enough to stimulate the founding of commercial STM manufacturers. One of the first (and certainly the most significant in the 1980s and the 1990s) was Digital Instruments, the brainchild of Virgil

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Elings, a physics professor at UC Santa Barbara.¹⁵ Elings first considered the possibility of selling commercial STMs after talking with Nico García while García was on the UCSB campus to give a lecture. From that encounter, Elings decided to attend the STM '86 meeting in Santiago de Campostela. After returning from the conference convinced that a market existed for a commercial microscope, he approached his departmental colleague Paul Hansma and suggested that they form a company to sell STMs.¹⁶

Like Calvin Quate, Hansma was initially wary of involvement with a start-up, so instead he offered Elings the blueprints and advice that he gave other colleagues building STMs. Elings then invited one of his former students, John "Gus" Gurley, to co-found a company, which they named Digital Instruments (DI). The name was meant to highlight the fact that their microscopes would have a digital electronic controller. At the time, most STMs had multiple racks of analog controls with knobs that the user could turn to set the microscope's parameters. A digital instrument, in Elings' view, could be more automated and more flexible than an analog one.

Despite their company's name, Elings' and Gurley's first prototype used a roughly assembled analog controller as a kind of placeholder while Gurley tried to perfect the digital electronics. With the STM '87 conference in nearby Oxnard coming up, and with Gurley struggling to improve the digital controller's signal-to-noise ratio, Elings improvised a commercial package from the analog prototype. Thus, Digital Instruments' first product turned out to be the analog NanoScope I. In April of 1987, DI was already advertising this \$25,000 STM ("atomic resolution—guaranteed, operation in air or liquid").¹⁷ With the NanoScope I providing cash flow, Gurley had an opportunity to push ahead with an all-digital controller. By July of 1988, DI was advertising that product, the NanoScope II, as "a new dimension in microscopy."¹⁸ Those early ads, primarily in *Physics Today*, helped DI tap a reservoir of demand for a commercial STM. Indeed, with the very first ad, Elings claims he could tell what parts of the United States had just received the magazine by noting where sales calls came from.¹⁹ By early 1990, after only three years in operation, DI had sold about 300 microscopes, "more than half of all the STMs in the world."²⁰

The Long Shadow of the 1970s

The success of the NanoScope I and of the NanoScope II was due in part to Virgil Elings' commercial and quasi-commercial experiences of the 1970s

and the early 1980s—experiences that long pre-dated most of the administrative and policy changes intended to spur academic commercialization. Elings had started his research career as a high-energy physicist, surviving the Cambridge Electron Accelerator explosion of 1965 while getting a PhD from MIT.²¹ By 1972, however, just a few years after taking a faculty position at UC Santa Barbara, Elings was moving into research areas outside high-energy physics. That year, he and a departmental colleague, Vincent Jaccarino, founded a Master's of Scientific Instrumentation (MSI) program “to teach people from diverse backgrounds—engineering, physics, biology, even psychology—about how to design instruments.”²²

Elings and his colleagues touted the MSI program as a possible answer to the turmoil that was facing American academic physics departments in the 1970s: a dreadful job market for graduates, reductions in federal research funding, and declining enrollments. MSI students brought much-needed revenue to UCSB, accounting “for roughly half of the total entering physics graduate-student population” in the late 1970s. Upon completing their two-year commitment, these students found jobs in “university research groups, primarily in physics, biology, and medicine . . . [and] industrial research labs, often in the semiconductor area.”²³ In the program, students learned quasi-entrepreneurial skills while saving UCSB scientists the expense of buying instrumentation:

The students are largely responsible for finding these ideas [for projects] from within the [physics] department or from other science faculty on campus. The projects must have real “clients”—those who have a vested interest in the timely and successful completion of the project.²⁴

In many cases, these skills later served students well in start-ups such as DI:

One lesson from the master's program that Elings carried into DI was that “the areas that students had done undergraduate work in made little difference in their ability to design instruments. Any deficiency, except of knowledge of math, could be repaired by some reading and talking with other students. All those esoteric courses made little difference.”²⁵ Perhaps as a result, personnel came to DI from a wide variety of sources. Elings' sons, Mike and Jeff, made contributions to the company's technical work. Dennis Adderton, a high school friend of Jeff Elings, was an intern at DI during summers while in college at UCSB; after graduation, he became employee number 40, and later he implemented designs for products such as a scanning capacitance microscope and a tunneling AFM.²⁶ Another

early employee, Matt Thompson, who had a degree in history and was running a bookstore when he joined DI, became one of the company's top applications scientists. James Massie, a psychology major before he entered the MSI program, later led the team that designed the AFM on which DI's fortunes eventually rested.

Virgil Elings' philosophy of business—emphasizing the inherent uncertainties of selling a new and ever-evolving research technology—may explain his reliance on this motley collection of early employees. As he put it in 1995, "We try to hire smart people. Their areas of education are secondary. None of us knew about scanning probe microscopes when we started, and I hope there is still a lot more we don't know."²⁷ In terms of design, Elings and Gurley opted for digital control for precisely the same reason—because it could be adapted more easily in the face of uncertainty: "[W]hen you make an instrument you actually don't know what the solution is it turns out. . . . [Y]ou have to be flexible, so when one day you wake up and figure what the hell it is you're doing, you can actually do it without saying 'Oh, it's too late, I already got the circuit boards done.'"²⁸ This, too, was a lesson from the MSI days. In the MSI program, students were encouraged to use digital circuits because "one exciting aspect of microelectronics is that the resulting 'intelligent' instruments may be easily redesigned to meet altered specifications, often by simple software changes."²⁹

The MSI program was clearly quite different from a traditional academic degree program. It was, for some students, "a rude awakening from the spoon-feeding of most undergraduate experiences."³⁰ Seeing that his best students used little of their undergraduate education in instrument building (but instead learned best by doing), Elings became convinced that formal academic pedagogy was counterproductive: "[S]chools at all levels, practically down to kindergarten, do almost nothing to foster innovation and invention. . . . [A]cademia can afford to spend some time on innovation since, in my opinion, a lot of what is done now is a waste of time."³¹

In view of his objections to how universities were run, it isn't surprising that Elings began looking for opportunities beyond UCSB. In the early 1970s he filed for a patent on—and began selling—a device made from two parabolic mirrors that produced an illusion of an object floating in space; it was sold as a toy in science museums.³² Over the next decade and a half, he either commercialized (sometimes with business partners) or explored the commercial possibilities of several technologies inspired

by his own research or that of his master's students. Examples include a digital cardiometer, a dermofluorometer (for measuring blood flow in skin flaps), and a light-scattering detector for measuring small particles. The last of these start-ups before DI was Nicomp, a company that Elings co-founded with David Nicoli to sell a particle-sizing system. Elings used part of his share of the profits from Nicomp as his initial investment in Digital Instruments.

Charismatic Authority and Technology Transfer

The MSI program and related ventures provided Virgil Elings with seed money for DI, with important personnel (including Gus Gurley and James Massie), and with insights into how to stimulate and manage innovation. From his descriptions of the MSI program, it appears that Elings' experiences as a UCSB faculty member also left him with a profound skepticism of academic culture and knowledge. Yet because DI's profit margins depended on academic customers, and university research generated innovations that found their way into DI's products, Elings' skepticism had to be strategic rather than absolute. That is, his skeptical stance acted as a filter or a crucible for determining *which* customer demands and innovations were worth pursuing, rather than an impermeable barrier to all inputs from academic researchers.

The most important academic sources of innovations for DI were the UCSB and Stanford groups that we examined in chapter 4. We saw in that chapter that Paul Hansma and Calvin Quate faced a problem of trust and credibility in pushing probe microscopy into new fields, in making STM and AFM noteworthy and useful to disciplines of which they had little knowledge, and in determining which representatives of those disciplines they could trust to help them make the technique more useful and credible. In turning probe microscopes into commercial products, Elings faced similar problems: What cues would indicate which design innovations were robust and marketable? How could Elings tell which, if any, of the researchers clamoring to have their ideas commercialized by DI were worth listening to? How could he tell which of his own employees' design innovations should be incorporated into the NanoScope and which should be left out?

As Steve Shapin has shown for high-tech entrepreneurs from Robert Boyle to Robert Oppenheimer to Robert Swanson, such uncertainties

cannot be overcome solely by the application of bureaucratic-rational rules.³³ No one had ever created an organization to sell probe microscopes before; no one could say exactly how such an organization should be run or how it should manage inputs from the instrumental community that formed its customer base. One response to these open questions was simply to be flexible—to hire smart people who could adapt, and to use an easily reprogrammable digital controller. As Elings says today, since “no one has any experience in the future,” one should “forget the plan and look for surprises.”³⁴

Complete flexibility, however, introduces the possibility of chaos. A company whose employees have no guide to action is potentially one in which effort is duplicated or wasted and good ideas go unnoticed. We saw in chapter 4 that Paul Hansma offered his small lab group guides to action through proverbs and rules of thumb. Elings, too, had a store of maxims, the most important being “You can’t know what you’re doing. (Keep your eyes wide open to the possibilities and don’t think you know exactly what needs to be done next).”³⁵ These proverbs were then supplemented by personalized management, in which the contradictions and ambiguities of applying flexible maxims were ironed out by Elings’ charismatic authority. Instead of a layered organization chart with middle managers making bureaucratic-rational decisions, “DI had 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.”³⁶

Shapin notes (with a nod to Max Weber) that this charismatic authority resides, at least in part, in the entrepreneur’s physical presence—his or her ability, as perceived by those with whom he or she interacts, to make things happen simply by being there. That Elings’ personal presence was seen at DI as a catalyst of knowledge is clear. As Jerome Wiedmann, a former MSI student who ran DI’s early marketing efforts, puts it, “We used to say about Virgil that if you went into [his] office, your IQ went up six points.”³⁷ Yet Elings’ charisma often guided by indirection rather than by precept. According to Wiedmann, “He chose absolutely to be an enigma at all times. I think that is a large part of why people are so conflicted about this guy.”³⁸

Elings’ physical presence and enigmatic indirection provided an answer to the questions of which innovations were worth marketing and how to

bring them into the company. For instance, when researchers at IBM and elsewhere began to talk about using a magnetized AFM tip to do magnetic force microscopy, Elings had to decide whether DI would commercialize MFM. He could have created a formal unit within the organization and given it concrete instructions for implementing MFM. Instead, he hired Ken Babcock, a recent UCSB PhD with a background in magnetism but not AFM. The second day Babcock was at DI, "Virgil walked by the cubicle and stuck his head over and looked down at me and said 'so, magnetic force microscopy' and before I could look up he was gone."³⁹ Elings' presence, in such a flat organizational structure, made such utterances worth listening to, but it was partly because their content was so unspecified that they could stimulate the kind of innovation that DI needed. As Babcock puts it, "I realized that in most of the things he said, you should sit and ponder. It was like a Zen master: my mission is [to] make magnetic force microscopy into something, but it's up to you to figure out how to do that."⁴⁰

This is not to say that Elings had magical powers, nor is it to say that the opposite of bureaucratic-rational decision making is irrational decision making. Rather, Elings' management style was a reasonable and ultimately a successful response to the inherent uncertainties of selling a research technology that was continually evolving unforeseen uses and designs. For Elings, presence was a litmus test: facts that could be made to evidence themselves in his presence served as a basis for future action; facts that were simply asserted or derived from theory didn't. People who were willing to challenge Elings to his face and parry his opposition could be trusted to develop marketable innovations; people who wilted in Elings' sometimes blistering presence could not.

Academic researchers who wanted DI to sell their particular innovation, or to modify the NanoScope for their particular application of probe microscopy, could expect to be tested intensely. For instance, when Stuart Lindsay developed an AFM variant that could be used in electrochemistry and biophysics, he was initially unable to get DI to market it. Elings was adamant that DI would not make custom microscopes, and he was reluctant to add anything to the NanoScope merely at the urging of researchers. Eventually, Lindsay formed his own start-up company, Molecular Imaging (MI), which tried to negotiate a deal with DI:

Bill Offenbergl, the CEO of Gatan [which then owned MI], . . . said [to Lindsay and MI employees] "well, if your microscope's so damn good, you can sell it to Virgil Elings."

Crikey! Terror. All the faces in the room went ashen. Bill was going to invite Virgil over to Tempe and we were going to give him a demo that was going to be so good that Virgil was going to agree that he had to market our microscopes because it would fill his niche in electrochemistry. In fact I actually have a T-shirt somewhere in my drawer which says "I Survived V-D" and underneath it says "Virgil's Demo." Bill had those made for everybody involved at the time. It was a great demo, Virgil could not crash that microscope, he was stomping on it and [yet we still saw] atoms.⁴¹

Lindsay was, perhaps, lucky to be let off that easily. As Jan Hoh, a Hansma postdoc, recalled, "Virgil loved to swear and be very brash. . . . [I]f you took that personally you wouldn't be long-lived [at DI]. He just loved to test people."⁴²

Probably the most important such test, ultimately, was that applied to Paul Hansma and the AFM. By early 1989, Hansma

had really become an AFM believer. . . . But the AFM didn't work very well, especially back in the days when we were using tunneling to detect the deflection of the lever. Virgil would come over and we would tell him that the AFM was the way of the future, and we would show him some images we'd gotten. He would look at them and say, "Ah yeah, maybe," and then he'd go away and not do anything. . . . Because it was one thing to have an AFM that worked well enough that we could get images to show him when he came, but that was never enough to convince him that he had something that looked like a product. What finally convinced him is when he came in the lab and the AFM worked while he was in the lab.⁴³

After Virgil had seen the Hansma AFM operate in person, he agreed to commission a commercial prototype. One of DI's newest employees, James Massie, and Hansma's technician, Barney Drake, then set about transferring the technology to DI.

As Craig Prater (then a Hansma student, later one of DI's top technologists) recalled, Massie "took an already elegant design and then made it even more elegant, easier to use. Over time he found the things that ended up not being reliable and came up with more elegant, more reliable designs."⁴⁴ But no matter how elegant Massie's design was, for Elings the AFM was not a marketable device until it worked in the inherently contingent, physical world directly in front of him, rather than just in the rule-bound, pristine, foreseeable world of the research laboratory. As Prater tells it,

[W]e brought over to show him this AFM that we had operating and of course he said "ah, looks like a piece of crap. . . ." He reached over and grabbed a Polaroid picture and got a pair of scissors and cut off a little piece of the Polaroid picture, [and] said "*image this.*" I had the same sort of mindset as the people at IBM in the early days, that the things that you image should be atomically flat, precision surfaces.

The idea of taking a piece of paper and sticking it in was just crazy. I said “Well, I don’t know if this’ll work” and he [said] “Try it. This is what our customers will do, they’ll put any kind of shit under there, you try it.”⁴⁵

DI, Surface Science, and the AFM

As Prater’s anecdote indicates, Elings took an extraordinarily open-ended view of what a commercial STM or AFM should be capable of imaging—more so than the Hansma group at the time, much more so than IBM’s surface scientists. Yet the samples DI employees imaged and published in promotional material were not chosen at random. From the very beginning, DI publicity highlighted the use of the NanoScope to image technologically important objects (in the condition in which they were manufactured), rather than specially prepared research materials. Early ads showed STM images of, among others, an “optical disk” (June 1988), “a nickel stamper from which compact disks are made” (July 1988), a “cobalt-chrome layer, a material used for vertical recording on magnetic disks . . . no surface preparation was required” (September 1988), “photographic film grains” (January 1989), an “integrated circuit” (August 1989), and a “laser-formed microencoder” (March 1990).⁴⁶

Thus, even in DI’s first year, Elings was targeting industrial customers, and some of DI’s earliest orders came from research and analytical laboratories at the Nippon Telegraph and Telephone Corporation and Standard Oil of Ohio.⁴⁷ The philosophy of building a cheap, rugged, air STM that would be useful to PhDs doing basic research but that could also be operated by non-PhDs and/or in non-research settings made the NanoScope attractive to a wide range of customers. But since no one yet knew what a commercial STM was good for, buying one involved taking some risk. By making its STM as cheap as possible, and eliminating the need for expensive, laborious specimen preparation, DI alleviated some of that risk and allowed customers to simply play with the NanoScope until they found a use for it. Indeed, Elings now refers to DI’s early days as the “toy business.”⁴⁸

Yet that philosophy effectively precluded DI from targeting surface scientists, the largest sub-community of probe microscopists during the company’s first few years. Surface scientists, at this point, had a reasonably clear idea of what an STM could do for them. To meet their requirements, though, an STM had to be housed in an elaborate, expensive UHV chamber, usually alongside other tools of surface science. Thus, in the late 1980s,

when commercial STMs designed specifically for UHV surface science began to appear, the companies selling them were almost all established makers of surface-science equipment that had found some way to acquire STM know-how: Burleigh, LK Tech, VG, Microscience, Omicron, McAllister, RHK Technology, JEOL.

The one major non-incumbent firm that tried to cater to the surface-science market was Park Scientific Instruments, a start-up associated with Calvin Quate's lab. Yet, as we will see, despite Park's close association with one of the top two or three probe-microscopy groups in the world, the company struggled to sell to surface scientists—incumbent firms retained a significant advantage in marketing to that discipline. One reason Park struggled was that most of its competitors for the surface-science market were able to sell their STMs along with some combination of UHV chambers, UHV-compatible micropositioners, Auger and ESCA spectrometers, LEED and RHEED diffractometers, molecular-beam epitaxy machines, electron microscopes, and so on. That is, surface scientists were much more likely to buy a commercial STM if it was part of a package of equipment designed specifically for their discipline.

DI, meanwhile, avoided the surface-science market almost completely—though a few companies did target surface scientists by offering to retrofit a NanoScope for operation in UHV. Thus, the partial disaggregation of surface scientists from the rest of the probe-microscopy community, which was underway even before the introduction of commercial instruments, was reinforced by the formation of a market for UHV STMs that was served by different companies than the market for other kinds of probe microscopes. Moreover, as DI's sales increased, UHV STM became a smaller and smaller sector of probe microscopy. In the first three years of DI's existence, its customers nearly doubled the size of the probe-microscopy community. Very few of those new STMers were operating their instruments in UHV or looking at samples prepared according to the disciplinary canon of surface science.

Some surface-science STMers saw the emergence of commercial air STMs as an invitation for bad research to swamp high-quality work in the probe-microscopy community. As Bob Hamers, a surface scientist at IBM's Yorktown lab, recalled,

One of the things 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 [besides graphite], or anything that you'd really want to study, it was no longer true.⁴⁹

The introduction of commercial air STMs, according to some surface scientists, made things too easy. As Jun Nogami, Calvin Quate's surface-science postdoc, recalled,

[I]t didn't help when it was discovered . . . that you could image in air. . . . Air instruments are a lot easier to build, they're a lot easier to sell. Then you get companies like Digital Instruments that have very sophisticated software that can make anything look pretty. That leads to . . . overinterpretation on a much more diverse set of materials.⁵⁰

Of course, as Nogami freely admits, surface scientists who built their own STMs also sometimes fell prey to "overinterpretation." The problem was that, with commercial air STMs available, the probe-microscopy community grew too quickly, and the newcomers had too few personal ties to their predecessors. Knowledge of image artifacts simply couldn't be transmitted quickly enough to keep up: "As the commercial instruments got better and people started buying them, then you would see people republishing the same old mistakes that we made a good number of years ago."⁵¹

The consequences of the rapid expansion of probe microscopy presented DI with a dilemma. On the one hand, selling STMs was DI's business—the more the better. Elings had little interest in telling customers that some uses were forbidden. In fact, he had reason to think that surface scientists' skepticism of air STM's capabilities were suspect. Back when he was debugging the NanoScope I prototype in early 1987, his routine was as follows: "Every morning I would go in and run the damn thing and get hooked on it. . . . We always cranked the scan down [to] try and see atoms on anything."⁵² According to Elings, he then

started to see an array of "atoms" on a gold sample. Now we only had two of the four scan electrodes hooked up (we didn't have 4 amplifiers yet) which made the X and Y scales very different. I showed it to Paul [Hansma] and he was worried that the array didn't look right, being skeptical about my explanation. Then he got hold of [Jerry] Tersoff at IBM who said it was impossible to see close-packed atoms on gold, so Paul was not interested in writing a paper, thinking, I guess, that the data was crap.⁵³

Pressed for time, Virgil Elings let his son Mike use the STM at night to come up with better gold images and present them in a school science fair.

As Virgil Elings is quick to point out, an IBM Almaden group *later* published atomic-resolution STM images of a close-packed gold surface in air and UHV.⁵⁴

Still, if air STM's reputation became tainted by "overinterpretation," there was the possibility that DI's market could collapse. Thus, Virgil Elings was sometimes just as skeptical toward air STM as the surface scientists. For instance, as Stuart Lindsay remembers it, once when he and Elings saw a conference speaker present air STM images of a bacterial sheath on graphite, "Elings got up and said [to the speaker] 'Well I don't know what you think your images are but we've seen an awful lot of things that look just like that on bare graphite.' That's the first time that I ever heard that accusation made for graphite."⁵⁵ That is, even before Gerd Binnig and Tom Beebe demonstrated convincingly that graphite defects could produce false STM images of DNA and other biomolecules, Elings was publicly making the same criticism. Today, in conversation, Elings ardently insists—even more than most surface scientists—that not a single air STM has been proved to operate in tunneling mode rather than via ohmic contact between the tip and the sample.⁵⁶

Yet it is also true that, in the late 1980s and the early 1990s, DI encouraged customers to believe that STM images of biological materials or organic molecules deposited on graphite were credible. For instance, a September 1989 advertisement trumpeted "The NanoScope II Scanning Tunneling Microscope" and its ability to image "uncoated Z-DNA strands on graphite" "without coatings that would have obscured the helices [*sic*]."⁵⁷ In April of 1989, another ad touted molecular-resolution images (made with a NanoScope STM) of a layer of liquid crystals deposited on graphite.⁵⁸ As the problems with air STM and graphite became known, however, it became clear to some at DI that those criticisms could have repercussions for DI's market.

The consequences of that backlash were minimized by the introduction of DI's commercial atomic force microscope. That atomic force microscopy would become DI's mainstay was, initially, quite unexpected. Elings, after all, had been reluctant to commercialize force microscopy, and people who worked for DI at the time remember him speaking very skeptically (even dismissively) about the potential size of the commercial AFM market. Today, DI veterans fondly point out that the first ad for Digital's AFM, in November 1989, relegated it to the status of a mere add-on: "The NanoScope II

Scanning Tunneling Microscope, *Now with an Atomic Force Microscope Option.*"⁵⁹

Elings' skepticism of force microscopy's commercial prospects had some validity at the time. After all, the NanoScope STM was already capable of imaging many industrially important objects. The potential market for STM seemed unlimited. As DI's ads announced, its STMs were "opening new doors in such diverse areas as Biology, Chemistry, Electrochemistry, Semiconductors, Materials Science, and Process Control" [though not, of course, surface science].⁶⁰ It was by no means clear that the AFM market would ever approach, much less dwarf, the market for STM.

By 1990, however, the criticisms of air STM were becoming widely known. Plenty of people were still doing air STM, but the leading centers of probe microscopy—UC Santa Barbara, Stanford, IBM, Bell Labs—had moved away from it and toward either AFM or UHV STM. Those who stuck with air STM found that they had to do more difficult experiments, on a narrower range of samples, in order to convince their peers. AFM, meanwhile, was becoming easier, and that it could image a very wide range of samples was becoming clearer. A DI ad from September of 1990, for instance, showed

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 new employees did it out of curiosity [sic].⁶¹

The AFM was certainly not yet free from criticism. It was not yet well understood, for instance, whether those "atoms" of salt were really individually resolved. Yet a consensus quickly emerged that the AFM was, as a May 1990 DI ad explained, simply "an ultra-low force profilometer." Since profilometers had been trusted by industry for decades, their similarity to AFMs allowed the newer technique to avoid many of the doubts that dogged air STM.

The consensus that AFM could be trusted but air STM couldn't formed almost simultaneously with DI's introduction of a commercial AFM. As a result, AFM quickly displaced STM as DI's main product. By January of 1990, DI's ads were declaring that "NanoScope II capabilities include STM, AFM, direct three-dimensional measurements, spectroscopy, surface roughness, bearing ratio, cross sections, two-dimensional Fourier transforms, and many others."⁶² At that point, just two months into selling an AFM, the

technique was gaining prominence. But it was still just one of many things a NanoScope could do. By May of 1990, however, DI's ads had dropped any references to STM of DNA or graphite forever. Most DI ads after that either mentioned AFM exclusively or mentioned it alongside such variants of AFM as magnetic force microscopy and lateral force microscopy. That is no wonder—by February of 1991, only 15 months after the introduction of DI's AFM “add-on,” DI's ads estimated that more than half of the 200+ NanoScopes sold in that period had been AFMs.⁶³ For the rest of its existence, DI would be primarily an AFM and MFM company.

Competition and Credibility

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As DI shifted to AFM, its connection to the Hansma group became much tighter. Hansma's students and postdocs, who initially had few interactions with DI, now began to take jobs there. For a time, UCSB offered the right of refusal on exclusive licenses to DI for patents coming out of the Hansma lab.⁶⁴ In return for an exclusive license on the AFM that Barney Drake and James Massie commercialized, DI agreed to pay the regents of the University of California 10 percent of “net sales of licensed products” to “fund further research in the field of Atomic Force Microscopy at the University of California, Santa Barbara.”⁶⁵ In addition to these licensing fees, DI supplied the Hansma lab with NanoScopes—which meant that any innovations developed at UCSB were already on the same platform that they would be implemented on at DI. In the mid 1990s, DI also seems to have given some NanoScopes to Calvin Quate's lab.⁶⁶

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For Paul Hansma, having the leading AFM manufacturer just down the road made it easier to pursue experiments other labs couldn't pursue. For instance, Jan Hoh, a molecular biology postdoc in Hansma's lab, remembers scavenging parts from DI:

Once, Jason [Cleveland, a graduate student,] and I were working on a calibration method for atomic force cantilevers. So we went down [to DI] and there was a place where there were cantilevers that had been returned or had minor defects, a table just full of these things. We needed cantilevers that had slightly different thicknesses for this calibration. So we just sat there and broke out cantilevers from 20 different wafers, which you would never be able to do anywhere else.⁶⁷

Proximity to DI also made it easier for members of the Hansma group to persuade DI engineers to modify the commercial microscopes. As Hoh puts

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it, “you’d go down and you could talk to . . . Gus Gurley about ‘software should do this, software should do that.’ Some things they did and some things they didn’t. There are things in there now that I remember I specifically suggested.”⁶⁸

Thus, the closer relationship between DI and UCSB in the early 1990s brought mutual benefits of a sort that are not usually encompassed by debates about “technology transfer” or the “commercialization of academic knowledge.” DI’s technology—for example, its superior image-processing software—made it easier for UCSB researchers to publish eye-catching, high-profile articles. Those articles, in turn, increased DI’s visibility and credibility with potential customers. For instance, in April of 1991 DI began a widely discussed ad campaign with the tag line “when you need to do science” (later changed to “We have science covered”—see figure 5.1).⁶⁹ These ads featured a NanoScope surrounded by six issues of *Science* featuring on their covers images generated by DI microscopes. Five of those six images were generated by the Hansma group or its collaborators, and some of the articles were co-authored with Virgil Elings.⁷⁰ A DI ad in *Physics Today* noted that “more scientific publications have been produced with a NanoScope II than with any other SPM.”⁷¹ And some of those papers had been published in the scientific community’s most general, most prestigious journals.

DI took greater advantage of its connection to UCSB in order to attract research customers in the early 1990s, when the probe-microscope market was growing more competitive. As we have seen, commercial UHV STMs had appeared at the same time as the NanoScope but weren’t really competing for the same market. But by the early 1990s, a few suppliers of surface-science equipment were branching into DI’s market. Burleigh, for instance, began offering an “instructional” air STM and AFM for \$25,000 alongside its “personal” UHV STM (\$50,000).⁷² At the same time, DI lowered the price of the NanoScope II from \$69,000 to \$35,000 in anticipation of introducing the NanoScope III. Burleigh’s instruments were designed for teaching labs, but the company claimed they “could, in fact, function nicely as a basic research instrument.”⁷³

Burleigh and other surface-science-oriented firms never ate too deeply into DI’s sales. The competitors that attracted much more attention at DI were new companies that began to form around the other nodes of the Zurich-California network described in chapter 4. For instance, in the mid

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Figure 5.1

An advertisement from Digital Instruments' "We Have Science Covered" campaign (*Journal of Vacuum Science and Technology A* 11 (1993), no. 3: A7). Note the similarity between the NanoScope and the Hansma design shown in figure 4.3. Reprinted with permission from Bruker Nano, Inc. Digital Instruments is currently a part of Bruker Nano's AFM Business Unit.

1980s, in Pasadena, Paul West—formerly one of John Baldeschwieler’s postdocs—founded QuanScan. A few years later, after QuanScan went under, West started a new firm, Topometrix, which competed for DI’s market. Similarly, in Arizona, two brothers, Larry and Darryl McCormick, founded Angstrom Technology in 1987 with help from Paul Hansma’s collaborator Stuart Lindsay. The McCormicks’ business plan was to apply for Small Business Innovation Research grants and to sell STMs on the side. The McCormicks based their STM on a digital controller that Uwe Knipping, an ASU technician, had developed for Lindsay.

Conceivably, Angstrom Technology could have had many of the same early-mover advantages that Digital Instruments did. Knipping’s design, however, was so complex that, although it could yield “unbelievably sophisticated” data, it “would [work only] for so long and then in the middle of a critical experiment—crash. . . . The machines in a sense were *amazing*, . . . but they were just hopelessly unreliable.”⁷⁴ Eventually, Lindsay—like many STMers who just wanted to get on with generating images—bought a DI NanoScope. Soon after, Angstrom Technology closed its doors. In 1993, Lindsay founded his own company, Molecular Imaging, which would alternately compete and collaborate with DI for the rest of the 1990s.

Of the new companies that formed around the Zurich-California groups and their close collaborators, DI employees seem to have been most worried by Park Scientific Instruments, founded in 1988 by two former Stanford University postdocs, Sang-il Park and Sung Park (who were not related to one another). Before 1990, Park Scientific Instruments was more tightly integrated with the Quate group than Digital Instruments was with Paul Hansma’s lab. Sang-il Park built Quate’s first STM, and several Quate students followed him to Park Scientific Instruments, bringing nearly unaltered instrument designs with them. Park Scientific Instruments’ ads of 1990 proclaimed “six years of R&D are in this package”—meaning a little over a year’s worth of R&D by the company and almost 5 years’ worth by the Quate group.⁷⁵ As Jun Nogami, a Quate postdoc who was never formally affiliated with Park Scientific Instruments, puts it, “the very first STM brochure with Park was 100% data taken in [Quate’s] lab, mostly by either Sang-il Park or me.”⁷⁶

Initially, Park Scientific Instruments built on Nogami’s work by targeting the surface-science market. Like other companies in that sector, Park tried to offer the full line of surface-science equipment: “complete, multi-functional UHV systems are available, including vacuum chamber, STM,

LEED, and Auger.”⁷⁷ However, given their close relationship with Quate (who was on Park’s board and who paid frequent visits), the company was better positioned than other UHV STM companies to move past surface science and into new markets. By February of 1990, for instance, Park had added an AFM line—just three months after DI had done the same.⁷⁸

Perhaps the biggest difference between Digital Instruments and Park Scientific Instruments was the latter’s accommodating attitude toward its customers. Whereas DI never made custom modifications for individual customers, Park regularly worked closely with customers on one-off variants.⁷⁹ Park also introduced an “open” architecture that allowed customers to write their own software, whereas at DI open architectures were seen as unnecessary and unreliable. In part, Park’s open architecture and readiness to do custom modifications exemplify a tendency in Silicon Valley for customers and suppliers to work closely on product design.⁸⁰ As an ad from 1990 put it, “after we deliver it, we don’t just walk away.”⁸¹

Park’s self-presentation as a full-service company was probably meant to draw a stark contrast with DI. Before 1990, DI took a minimalist approach to customer service, simply FedExing microscopes to customers and leaving it to the customers to figure out how to use them. Park’s founding, however, coincided—perhaps accidentally, perhaps not—with DI’s becoming more responsive to customers’ needs and more proactive in highlighting the research potential of its instruments. For DI to become more responsive to research customers, however, it needed some way to interface with various disciplines—some way to convince members of those disciplines that a NanoScope would be useful to them, while conveying (but also filtering) suggestions from the members of those disciplines for DI’s designers.

We saw in chapter 4 that the Hansma group solved a very similar problem by working closely with representatives of various disciplines who taught Hansma’s students how to prepare samples and interpret images, provided user input that fed back into successive generations of microscope design, and then did what Craig Prater calls “missionary work,” bringing evidence of probe microscopy’s relevance back to their disciplinary colleagues.⁸² In the early 1990s, DI began doing something similar: hiring experimentalists from various fields that hadn’t yet embraced probe microscopy in the hope that they would persuade their disciplinary colleagues of probe microscopy’s usefulness.

That is, Digital Instruments and the Hansma lab were not merely sharing technology. In some strong sense, they were sharing an *ad hoc*

methodology for overcoming shared epistemic uncertainties. Both DI and the Hansmas faced challenges to the credibility of STM and AFM images. Recruiting disciplinary representatives as collaborators was one way to establish the credibility of probe microscopy for the audiences to which those experts were linked, whether the objective was to get members of those experts' disciplines to cite your articles (the Hansmas' objective) or to get them to buy your microscopes (DI's objective).

Thus, whether these disciplinary representatives worked for the Hansmas or for Digital Instruments, their work and their intellectual products were very similar. They submitted articles to leading journals, traveled to conferences to give talks, and—by example—led their colleagues toward probe microscopy. In view of the commonality of circumstances at Digital Instruments and UC Santa Barbara, it is not surprising that several of these people were referred to DI by the Hansmas—for example, Ken Babcock (for magnetic force microscopy) and Mike Allen (for biology). A few—e.g., the husband-and-wife team of Roger Proksch and Irene Revenko—had been postdocs in the Hansmas' groups before moving into very similar roles at DI.

Whether as postdocs at UC Santa Barbara or as applications scientists at Digital Instruments, these people played a double role. Credibility was, after all, a two-way street. DI and the Hansmas needed these experts' disciplines to trust the technology of the probe microscope. At the same time, those disciplines often had very specific demands for how probe microscopy should develop, and for what functions and variants and applications should be pursued. These postdocs and applications scientists therefore needed DI and the Hansma group to trust them—and, by extension, the disciplines they represented—in deciding what technological path to follow.

Building that trust was, for these postdocs and applications scientists, a highly personalized endeavor. We have seen that Jan Hoh and other Hansma-group postdocs used their geographic proximity to DI—and their personal acquaintance with DI's engineers—to encourage the company to incorporate their disciplinarily informed user experiences into new software and hardware. The applications scientist Mike Allen found that beer was as important as reason in convincing DI's engineers to listen to input from his disciplinary colleagues:

Allen: I would tour labs and see how they were applying the instruments. . . . They would always have a list of things they wanted changed for me to

take back. . . . [such as] “There’s a bug in the software version when I try to do this filtering” or . . . “Why can’t I move the tip over to this spot if I want to?” . . .

Mody: What would it take for a request like that to actually end up in DI’s products?

Allen: Well, you go down to the corner store, buy a six-pack of cold beer, and take it to the software engineer. I found out too late when I was there that’s how things got done.⁸³

That is, once again, there was no solely bureaucratic-rational solution to the two-way problem of credibility and trust.

Making an Instrument Instrumental

Knowing whose judgment to trust was an increasingly urgent problem after 1990 because the landscape of probe microscopy was increasingly diverse. As we have seen, the number of disciplines using probe microscopes increased dramatically in the late 1980s and the 1990s. At the same time, the technology itself saw an astonishing proliferation of new variants: scanning ion conductance microscopy (SICM), scanning capacitance microscopy (SCM), scanning thermal microscopy (SThM), magnetic force microscopy (MFM), lateral force microscopy (LFM), ballistic electron emission microscopy (BEEM), near-field scanning optical microscopy (NSOM or, in Europe, SNOM), tunneling AFM (TunA), scanning electrochemical microscopy (SEcM), scanning Kelvin probe microscopy (SKM), and so on. In 1999, a commission of the International Union of Pure and Applied Chemistry found that descendants of the STM included more than fifty named probe microscopies.⁸⁴ This proliferation of new variants meant that DI needed more eyes and ears to help identify which variants were worth pursuing, and to explain those variants to customers in a multiplicity of academic disciplines and industrial sectors.

A surprising number of the variants were initially developed or suggested by researchers at IBM and at Bell Labs, especially by former members of the Quate lab. Yet IBM and AT&T were ambivalent about the commercial possibilities of new variants—an ambivalence that made it easier for Digital Instruments and other start-ups to commercialize those variants instead. The example of Joe Griffith, one of the few surface scientists at

IBM or Bell Labs who made the transition to developing such variants, is telling in this regard. Like other surface scientists at Bell Labs, Griffith had started out in ultrahigh-vacuum STM. However, because he was situated in Bell Labs' materials-oriented Area 15, rather than its basic-physics-oriented Area 11, Griffith may have been better prepared than his colleagues to respond to AT&T's increasing insistence on mission-oriented research. Thus, around 1990, Griffith became aware that he needed to drop UHV STM and instead pursue ways to adapt force microscopy to solve AT&T's manufacturing needs.

Griffith and a North Carolina State graduate student, Dave Grigg, began working on an AFM-like instrument that could probe integrated micro-electronic circuits and inspect patterns on the photomasks used in making those circuits. Though the circuits Griffith and Grigg had in mind were those manufactured by AT&T, their force microscope was, in the end, commercialized extramurally, by a small West Coast start-up trying to break into the semiconductor metrology market. Moreover, Griffith couldn't even interest AT&T in patenting his technology:

[T]he attorneys turned their noses up at it. . . . The attorneys were really geared to products of which you might be making thousands a day. So we would tell them about what the anticipated market [for our microscope] would be and they would [say] "Ah, nah, forget it, that's not worth the trouble."⁸⁵

Grigg, meanwhile, completed his postgraduate studies and went to work for Digital Instruments. There, he was one of the lead designers on the Dimension, DI's AFM product line targeted primarily to industrial users. Grigg's and Griffith's expertise in force microscopy thus accrued mostly to start-up companies, rather than to AT&T.

The early 1990s were a busy time in the development of probe microscopes for semiconductor inspection and metrology. Joe Griffith himself was most anxious about competition from two sources: the US National Institute of Standards and Technology (successor to the National Bureau of Standards) and IBM. At NIST, Russell Young's protégé Clayton Teague (now a senior manager in his own right) developed a metrological STM known as the Molecular Measuring Machine. At IBM Yorktown, Calvin Quate's former postdoc Kumar Wickramasinghe invented an AFM inspection tool known as the SXM.

Like Joe Griffith, Kumar Wickramasinghe was located in a different part of his organization than most of his UHV STM colleagues: he was

in Yorktown's Manufacturing Research area, rather than in Physical Sciences.⁸⁶ Thus, like Griffith, Wickramasinghe was perhaps better positioned than IBM's surface-science STMers to accommodate to the increasing emphasis on mission-oriented and product-focused research at IBM in the late 1980s and the early 1990s. During those years, he and other Quate group veterans now working for IBM, including Clayton Williams at Yorktown and Dan Rugar at Almaden, invented or improved a slew of SPM variants, including SThM, SCM, MFM, SKM, scanning chemical-potential microscopy, and apertureless NSOM.

In the 1990s, engineers at Digital Instruments developed implementations of several of these modes. In some cases, once DI employees became aware of these new techniques, they independently developed a commercial implementation. In other cases, they built prototypes that were refined into a marketable form by getting feedback from outside researchers. For example, in the case of capacitance microscopy, DI obtained advice on their prototype from Clayton Williams (by then at the University of Utah).⁸⁷ As Dennis Adderton, who worked on DI's capacitance microscope, puts it, researchers such as Williams "were always happy to use the prototypes because they were able to get reliable results with the new type of measurement. Publications often resulted from these tests."⁸⁸ In addition, DI sometimes recruited people from labs that had developed a new microscope. For instance, Andrew Erickson was recruited from an Intel lab researching capacitance microscopy to work on that type of microscopy at DI.

Thus, DI engineers were well aware of the variants that Wickramasinghe and other Quate veterans were developing at IBM, and when Wickramasinghe, like Griffith, began developing a metrological AFM for inspecting integrated circuits, DI paid attention. This instrument, the SXM, operated primarily as a non-contact AFM: the tip never came in contact with (and hence would not damage) the sample. Instead, the tip hovered over the sample and vibrated, with the detector set to observe minute variations in the frequency of the tip's vibration due to changes in the sample's topography or material properties. The SXM also had a special "boot" tip that allowed it to probe *sideways* as well as up and down (see figure 5.2). This was an important feature in semiconductor manufacturing, where the steep trench walls cut into silicon to make tiny transistors were difficult for regular AFMs to access.

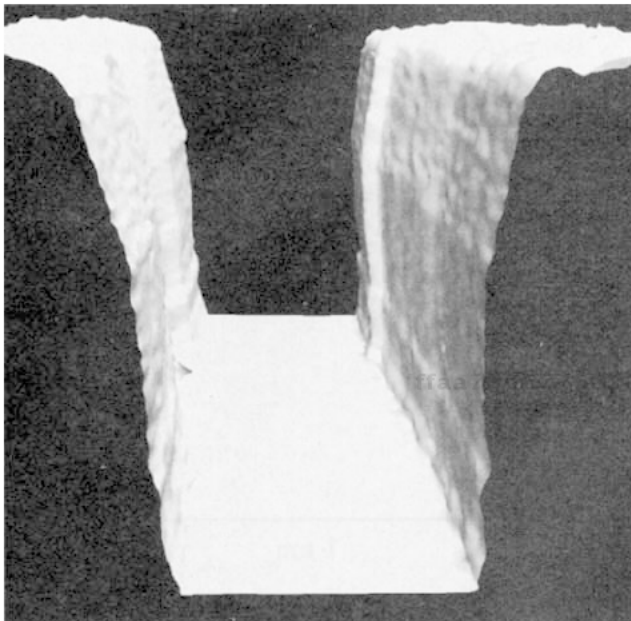


Figure 5.2

An image of the sidewalls of a trench in a dynamic random-access memory chip, taken with a prototype of the IBM/Wickramasinghe commercial SXM microscope. Source: Y. Martin and H. K. Wickramasinghe, "Toward Accurate Metrology with Scanning Force Microscopes," *Journal of Vacuum Science and Technology B* 13 (1995): 2335–2239 (copyright 1995 American Vacuum Society). Sidewalls such as these are very difficult to measure with a standard AFM, but measuring them is very desirable for semiconductor manufacturers. This trench is 700 nanometers deep and 400 nanometers wide.

Between 1987 and 1990, Wickramasinghe's group, along with engineers at IBM's facilities in Boca Raton, Florida and Sindelfingen, Germany, developed the SXM into "the first manufacturing hardened instruments that went to all the major IBM manufacturing and development labs."⁸⁹ The exquisite quality of the SXM's images sparked interest, and acclaim, from the semiconductor industry. As a result, IBM began marketing a commercial version to other semiconductor manufacturers.

Yet the commercial SXM was a temperamental tool relative to its buyers' needs. As Joe Griffith puts it, "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."⁹⁰ One microelectronics company that bought an SXM was

looking very very hard for somebody who wasn't a PhD who could run the thing. And they finally found a guy who was able to run the thing and his record said he wasn't a PhD. Well it turned out that the record was wrong, because the guy had lied on his employment application. He had a PhD and didn't tell them. Under normal circumstances that would've been grounds for firing but he was so good that they just moved him off to another area.⁹¹

For some purposes, such as research into new microfabrication processes, the SXM's unique capabilities justified the labor and expertise required to operate it. For routine manufacturing, however, most semiconductor companies wanted an instrument that gave a simple numerical output (or even a Yes or a No answer to the question "Is this wafer up to spec?"); they didn't want exquisite images. They also wanted a machine that would work 24 hours a day and could be run by a low-wage operator rather than a PhD.

The SXM had difficulty filling that role, and apparently IBM had difficulty filling the role of an instrument sales company. In 1993, IBM contracted with Veeco, a supplier of semiconductor equipment, to sell and service the SXM (which IBM continued to manufacture).⁹² For Veeco, the move added a new semiconductor-characterization tool to complement its other surface-analysis products. Yet Veeco spent much of the rest of the 1990s trying to figure out how to sell the SXM.

At the same time that he was working on the SXM, however, Wickramasinghe decided to pursue a parallel development of a non-contact AFM outside IBM as a hedge in case the SXM didn't pan out. Having put out calls for bids from several instrument manufacturers, he decided that DI's proposal for an industrial non-contact tool looked best. Then he

got some IBM money and went to Digital Instruments and said "look we want to do non-contact AFM for manufacturing applications." . . . [DI] came up with a prototype . . . and the first few systems went to [IBM] San Jose for magnetic force microscopy and for scanning roughness of discs. We had a deal with them, the first X units would come to us at a reduced price, and then after that they could sell them wherever they wanted.⁹³

DI was well placed to conclude such a deal, in part because its engineers had long occupied the same innovation space as Wickramasinghe and other IBM personnel. IBM, after all, employed many researchers who were "pushing the envelope" of microscope design and whom DI engineers could therefore compete with, collaborate with, draw inspiration from, or provide inspiration to.

Of course, IBM also employed many researchers and manufacturing specialists who could potentially *use* scanning probe techniques to help the company improve its products and/or manufacturing processes—people who might conceivably be in the market for DI's (or other companies') commercial microscopes. Indeed, IBM's own probe-microscope developers often had that internal market in mind. The SXM was the most successful instrument to close an innovation loop connecting IBM's microscope builders and users, but it was by no means the only such instrument. For instance, even as she was pursuing STM studies of semiconductor and metal surfaces and organic molecules deposited on metals, Shirley Chiang collaborated with her IBM Almaden colleagues Mathew Mate, Gary McClelland, and Ragnar Erlandsson to develop a type of AFM that could measure frictional forces—a measurement useful to IBM's disc-drive business.⁹⁴ Even the STM itself originated in part as a response to the Josephson computing team's need for better characterization of thin oxide films.

Thus, for Digital Instruments, IBM was both a source and a potential market for AFM innovations. DI engineers kept abreast of developments coming out of IBM in areas (such as magnetic force microscopy) that could then be commercialized for customers at IBM and other high-tech companies. DI was therefore well positioned to answer Kumar Wickramasinghe's proposal to develop an industrial AFM. By doing so, DI inserted itself into the loops that circulated innovations between IBM's microscope developers and its internal users.

The major requirement that IBM and other manufacturing customers demanded of an industrial AFM was the ability to handle partially processed manufactured objects—say, semiconductor wafers—in such a way that those objects could continue through the manufacturing process and eventually be sold. Simple as that may sound, that requirement entailed several major AFM innovations. For one thing, an industrial AFM had to handle much larger samples than a research AFM, since industrial users wanted to put products into their microscope without cutting them into small pieces. Building a large-sample AFM, however, required changing how the probe and the sample interacted. In small-sample AFMs (whether commercial or homemade), the sample is mechanically scanned back and forth while the probe stays still. This ensures that the probe is more stable than it would otherwise be, and that it provides higher-resolution images. Scanning large samples is slower and more cumbersome—the extra weight

makes it harder to shift the sample back and forth. The optics for DI's large-sample AFM had to be completely redesigned, therefore, so that the probe would scan while the laser sensing the probe's vertical movement tracked along with it.

Another important requirement for industrial AFMs—at least for some users—was that the microscope not damage the sample. Semiconductor manufacturers, in particular, wanted to inspect their wafers after each process step without having to sacrifice any part of the wafer to the inspection. Yet this requirement was in tension with the same customers' demand that a microscope have a high throughput and be relatively easy to use. Early AFMs operated either in "contact mode" (with the probe scraping along the sample surface) or in "non-contact mode" (with the probe hovering above the surface). A contact-mode AFM tends to be easier to build and operate, but often damages the sample. Non-contact-mode AFMs, such as the SXM, preserved the sample but tended, in that era, to be very difficult to operate. Semiconductor manufacturers, especially, required an instrument with both the minimal sample damage typical of non-contact-mode *and* the ease of operation typical of contact-mode AFM.

Around 1990, therefore, Digital Instruments was looking for new ways of operating an AFM that would combine the best features of contact-mode AFM and non-contact-mode AFM. What DI's engineers stumbled upon—and were well placed to take advantage of—was a variant that soon became known as tapping mode AFM. In tapping-mode AFM, the cantilever is forced up and down so that the probe comes into contact with the sample repeatedly while the sample topography is being measured but is not in contact with the sample while the cantilever is being scanned (so that the probe doesn't scrape along, and therefore damage, the sample). Tapping-mode AFM made it much easier to image semiconductor and data-storage devices, polymers, lubricants, and other materials of industrial importance. It has also proved enormously useful in imaging soft biological samples of interest to basic researchers.

Thus, innovations in DI's industrially oriented product lines often spilled over to benefit its basic research customers, and vice versa. Making that spillover happen, however, was hard work—the applicability of new variants was rarely self-evident, especially in market niches that hadn't yet adopted probe microscopy in any form. Thus, DI engineers and applications scientists spent much of the 1990s researching new uses for both its

industrial AFMs and those meant for basic research. DI then coached whole industries on how to use its products by distributing “application notes” with titles like “Applications of Atomic Force Microscopy for Contact Lens Manufacturing” and “IC [Integrated Circuit] Failure Analysis and Defect Inspection with Scanning Probe Microscopy.”⁹⁵ At the same time, the company stimulated new disciplines to adopt its microscopes by having its employees publish peer-reviewed journal articles with titles like “Magnetic Dissipation Microscopy in Ambient Conditions” and “Magnetic and Acoustic Tapping Mode Microscopy of Liquid Phase Phospholipid Bilayers and DNA Molecules.”⁹⁶

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Consolidation and Fragmentation

Digital Instruments’ efforts in building microscopes and in building awareness of those microscopes among potential customers paid off handsomely. DI’s profits were always larger than those of any other probe-microscope company, but the tapping mode and the industrial AFM line helped DI pull even further ahead of its competitors. Industrial customers were willing to pay much more than the academic researchers with whom Park Scientific Instruments and Topometrix were generally dealing—by 2000, perhaps ten times as much for a top-line tool for inspecting semiconductor wafers as for a standard research instrument. Those extra profits allowed DI to fund further innovation that gave it a competitive advantage.

New money also allowed DI to pursue litigation against some of its competitors. In 1993, DI initiated patent litigation against Topometrix that dragged on for three years.⁹⁷ Patent claims in probe microscopy have sometimes been difficult to defend, especially owing to the ambiguities of IBM’s patents (which Topometrix had licensed) on the STM and the AFM. (Calvin Quate had co-invented the AFM but is not listed on the patent.) Neither DI nor Topometrix was able to win a clear legal victory, but DI was in a much better position to absorb the costs of litigation than Topometrix was. In the wake of the lawsuit, therefore, Topometrix sold itself to an instrumentation holding company, Thermo Electron. Thermo Electron then acquired the struggling Park Scientific Instruments. In 1998, Topometrix and PSI merged under a new name: ThermoMicroscopes.

Meanwhile, Digital Instruments’ orientation to industrial customers increased the company’s value in the eyes of semiconductor process

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equipment firms looking to buy a stake in probe microscopy. By 1997, the industrial product lines had matured enough that Virgil Elings began negotiating to sell DI to one such company, Zygo. Up to then, Zygo's strength was in optical tools for measuring surface roughness, an important variable in making a silicon wafer ready for processing. While looking to acquire DI, Zygo was also seeking to license and sell IBM's SXM instrument so that it could offer a broad suite of tools—optical and probe-based—to the semiconductor industry.⁹⁸

In August of 1997, Zygo “signed a letter of intent to buy Digital Instruments for stock then worth \$250 million.” DI was “said to have revenues of roughly \$51 million for 1997.”⁹⁹ According to the trade press, by October of 1997 the value of the stock being offered had declined by 25 percent, and the deal was canceled. Elings then turned to a Zygo competitor, Veeco, which took on DI, reportedly for about \$150 million in stock, in early 1998.¹⁰⁰ After a few years with the merged company, Elings took his share of that money and embarked on a new career in philanthropy, giving millions to universities, halfway houses, museums, schools, and municipalities. UC Santa Barbara, in particular, has benefited from his generosity. In 2007, Elings and his former wife, Betty Elings Wells, gave \$12.5 million for UCSB to build Elings Hall—the home of the Santa Barbara portion of the California NanoSystems Institute.¹⁰¹

Veeco went on to acquire more than a dozen firms in addition to DI between 1997 and 2003, including IBM's SXM division (in 2000) and Thermomicroscopes (in 2001).¹⁰² Veeco's evolution since 2000 is largely beyond the scope of this book, but it is relevant to my argument to observe that Veeco's attempted consolidation of the probe-microscopy market has, in some ways, led to even more fragmentation. Veeco's buying spree seems to have encouraged some entrepreneurs to believe that the company had become too large and bureaucratic to respond to demands from niche communities within probe microscopy. Thus, the turn of the millennium saw a new wave of probe-microscope start-ups, each scrambling for its own application niche (electrochemistry, biophysics, teaching labs, etc.) or region (Israel, Russia, Korea, Switzerland, Germany, Britain, etc.) within which to prosper.

Some of these new start-ups were founded as a direct result of the consolidation of the first-wave companies. For instance, Sang-il Park returned to Korea in 1997 when Park Scientific Instruments was absorbed by Thermo Electron. In Korea, he founded a semi-independent spin-off company, PSIA,

which acted as Thermomicroscopes' East Asian AFM distributor. However, when Veeco acquired Thermomicroscopes in 2001, Park terminated that relationship and marketed his own AFM. Similarly, Paul West, founder of QuanScan and Topometrix, helped form a new company, Pacific Nanotechnology, in the late 1990s, in anticipation of the National Nanotechnology Initiative.

Another important driver of this new round of entrepreneurship was the stark difference in the approaches of Elings and Veeco executives to the limitations of bureaucratic-rational authority. Veeco is certainly a much more "business-like" operation than DI ever was. It has more MBAs, more middle managers, slicker advertising, and stricter rules about who can visit its work sites. Longtime DI employees saw Veeco's new rules as clamping down on the open-ended informality required for innovation. For instance, after Veeco merged with DI, Veeco executives decided to put a lock on the stockroom at its Santa Barbara facility (formerly DI's headquarters). As a result, engineers could no longer freely scavenge parts at any time of the day—something that had been routine under Elings.¹⁰³

For some DI employees, the final insult came when Veeco began paying commissions to salespeople. In an interview, the electrical engineer Dan Bocek put it this way:

[A] sale, at least in a company that sells high-tech instrumentation, actually involves a lot more people than the salesman. 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 samples and get data. Maybe the guy's going to have some technical questions; [so] 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?¹⁰⁴

That is, Veeco approached the problem of sales from a bureaucratic-rational perspective: salespeople, with sales experience, housed in a separate sales department, should get an industry-standard commission. DI veterans, however, were used to Elings' flexible, personalized approach—sales were handled by *ad hoc* networks of employees that informally conveyed information to, and simultaneously built trust with, customers.

Partly as a result of this mismatch between Veeco's bureaucratic-rational business practices and DI's charismatic traditions, the merged company became an incubator for start-ups founded by its former employees. Some of these were formed in partnership with former Quate students, and some have had the tacit or explicit support of the Hansmas and even Elings.

Perhaps the most revealing of these spin-offs is Asylum Research, founded in 1999. Jason Cleveland, Asylum's founding CEO, and Roger Proksch, its president, were both veterans of Paul Hansma's group at UC Santa Barbara and of Digital Instruments, and several other Asylum employees came there via DI/Veeco and/or Paul and Helen Hansma's lab groups. The founding idea of the company—conveyed by the double entendre in its name—was that it would provide an asylum for disaffected Veeco employees, but also that it would be a place where “crazy” suggestions for how AFM technology should move forward would be encouraged and listened to.¹⁰⁵ Asylum's founding, therefore, was an explicit nod to the logic of charismatic authority and the limits of bureaucratic-rational management.

Veeco's spawning of Asylum and other spin-off companies is, in some ways, a very old story in high-tech industries. It is reminiscent of the spawning of multiple “Fairchildren” out of Fairchild Semiconductor in the 1960s.¹⁰⁶ Yet the nature of probe microscopy's instrumental community was perhaps especially encouraging to the continual formation of start-ups like Asylum. As we have seen, new variants and applications of probe microscopy were constantly appearing in the late 1980s and the 1990s, creating enormous uncertainty about which variants and applications were worth pursuing. Virgil Elings' way of dealing with that uncertainty was to adopt a flexible technological platform and a flexible corporate organization, both of which could be restructured quickly to take the company in new directions.

Another approach was to limit uncertainty through specialization and market segmentation. Starting with surface-science instrumentation companies in the 1980s, some probe-microscope manufacturers chose to concentrate on a narrow slice of variants and applications. By the end of the 1990s, controlling the inputs and outputs from all of probe microscopy's niches was becoming challenging even for Digital Instruments, and niche-specific firms were becoming ever more competitive. Then DI's merger with Veeco made such niche-specific start-ups even more attractive. Spin-offs from Veeco formed, in part, because DI employees saw ways to innovate more rapidly and more responsively for specific segments of the diversifying probe-microscopy community than the larger, increasingly rule-bound parent company could.

The invention and innovation of all these variants and uses of STMs and AFMs has always been a product of networks that criss-crossed any

conceivable boundary between academic and commercial institutions. Sometimes academic researchers have worked directly with start-ups to commercialize their ideas. Sometimes start-ups have taken advantage of their members' earlier experiences in universities, and in earlier start-ups, in adapting—or reinventing—academic innovations. Sometimes start-ups have shadowed emerging subfields (such as MFM or SCM) that were made up of government, academic, and corporate researchers. The diversity of probe microscopy continually co-evolved with the diversity of forms of interaction among different types of commercial and academic actors.

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6 Probe Microscopy and the Path to Nanotechnology

Commercial production of STMs and AFMs accelerated the dramatic growth of probe microscopy. It would be next to impossible to locate every piece of research generated with a probe microscope or every commercial SPM sold, but we can get a clear sense of how commercialization amplified research output by examining some crude proxies. By the rough metric of the Science Citation Index, annual production of probe-microscopy articles rose slowly from the STM's invention through 1989, only hitting 220 to 250 articles per year at the end of that period.¹ But in the 1990s, annual publication rates rose much faster, zooming to more than 4,000 articles per year at the end of the decade. (See figure 6.1.)

That inflection point at 1990 coincided almost exactly with the introduction of Digital Instruments' AFM and with a similar inflection point in NanoScope sales. Information from DI's advertising and newsletters suggests that the company shipped about eight NanoScopes per month before 1990, and that in the years 1990–2000 it shipped more than 19 units per month.² The total number of commercial STMs and AFM must have been much larger; after all, DI wasn't the only supplier. In 1993, *Business Week* estimated DI's market share at about 50 percent—a reasonable estimate for the rest of the decade too.³ Insofar as nearly all of the first-wave commercial SPM makers entered the business around 1989, it is likely that their sales rose at least as sharply after 1990 as DI's. In any case, it is evident that there were many more probe microscopes in existence after 1990 than before, and that the community's research productivity rose accordingly.

Somewhat less obviously, commercialization led to a diversification in the ways an individual could be a probe microscopist. In the 1990s, as was noted in chapters 4 and 5, probe microscopy spread to a number of

disciplines, including physics, chemistry, materials science, biology, surface science, electrical engineering, and geology. STM and AFM were usually first carried into these disciplines by individuals who had either built a microscope for themselves or worked closely with a builder. But the technology spread much more quickly once those early adopters' colleagues began buying commercial microscopes for routine use. The availability of commercial microscopes was also necessary for their spread through various industrial sectors. A few corporate basic research labs could afford to build their own instruments, but industrial process control or analytical labs needed to make measurements, not microscopes.

Quite a few people still built their own microscopes, of course. But, again, commercialization introduced new ways for an individual to be a microscope builder. Some abjured commercial instruments as not sensitive enough for their very precise or specific experiments. Some bought all or part of a commercial microscope and then modified it to meet their needs. Some developed improvements to probe-microscope technology in the hopes that a microscope manufacturer would commercialize their work.

My point is Digital Instruments and other companies made it possible for users of probe microscopes to connect to other users in an entirely new way. Some tried, as before, to stay at the forefront of innovation in the technology, but now companies like DI were there to compete with them or to co-opt them. Others remained active users, but after 1990 they increasingly directed their feedback and demands to DI and its competitors rather than to research groups like Hansma's, Quate's, or Binnig's. Customers could now add an AFM to their complement of other instruments, perhaps using it only occasionally, with little attention to the nuances of its operation and little interest in connecting with other users. An academic department—or a multi-department center, or a shared equipment facility—could now buy an AFM for use by graduate students from all over campus.

Probe microscopes therefore became a technology to which people could orient either quite strongly or very weakly. STM users or AFM users could make a large investment of time, money, or expertise in these tools if they desired; or they could tell one technician or grad student in their lab to get AFM training and hardly ever think of probe microscopy again. They could see it as something that they alone, among their organizational or disciplinary colleagues, were interested in; or they could see it as something that almost everyone in their organization or discipline was interested in.

This mix of strong and weak ties was, in large part, the basis for probe microscopy's role in the formation of the nanotechnology enterprise. The proponents of nanotechnology came from a variety of disciplines, industrial sectors, government agencies, and non-governmental organizations. Their visions for nanotechnology were just as varied, but all saw it as a way to suture together different approaches to nature and different sectors of society. All saw nanoscale science and technology as bridging commercial, governmental, academic, and civil-society organizations, and as a site for transnational competition and harmonization. The probe-microscopy community, with participants in many different sectors, nations, industries, disciplines, and organizations, offered nanotech proponents ready-made connections from which to build the bridges that they envisioned.

The STM and NANO Conferences

It wasn't inevitable that probe microscopy would come to occupy a position at the center of the various networks connected together by the label of nanotechnology. Some probe microscopists argued against attaching the nanotechnology label to their community, and many were ambivalent about it. The probe-microscopy community's commitment to nanotechnology was never an all-or-nothing proposition. Indeed, SPMers were quite creative in finding ways to appropriate some (but not all) elements of nanotechnology discourse while waiting to see what others did before committing further. The "path" to nanotechnology was really more a ping-ponging of growing commitment among a variety of organizations, disciplines, and instrumental communities. That is, once one network of people (defined by their common connection to an organization, a discipline, or an instrument) adopted some element of nanotechnology discourse, other nearby or connected networks became more likely to do so.

"Nearby or connected" could mean many different things. For example, two networks might have common members or funders, might compete for the same markets, might share the same instruments, or might read or publish in the same journals. Proponents of nanotechnology did their best to *create* connections between networks in order to facilitate the process of bootstrapping those networks' adoption of nanotechnology discourse. Proponents also sometimes used networks that had adopted nanotechnology discourse—especially probe microscopy—as models and ambassadors for teaching peer networks how to practice nanotechnology.

The adoption of nanotechnology discourse wasn't, however, impressed on probe microscopy from the outside.⁴ Rather, the idea of using probe microscopy to spread nanotechnology discourse into a variety of disciplines and organizations, and of using nanotechnology discourse to spread probe microscopy into new disciplines and organizations, was championed by those probe microscopists who were themselves positioned as interdisciplinary and interorganizational mediators. Moreover, these mediators wielded nanotechnology discourse not simply for purposes of empire building—though there was some of that—but as a tool for resolving demographic and structural tensions that arose within probe microscopy and within other research communities.

An examination of the diffusion and adoption of the prefix “nano” in the titles of scientific conferences demonstrates just how such demographic and structural tensions provided an entry point for nanotechnology discourse into a variety of research communities. As we will see, many conferences “nano-ized” in the late 1980s and the early 1990s, partly because of changes in the number or interests of their members and partly because leaders of those conferences looked to similar nano-ization going on at peer conferences.

The annual international conference dedicated to probe microscopy was one of the first to adopt the nano label, and one of the most important models for conferences that did so later. One important reason for probe microscopists to import the “nano” label for their conference series was that the demographics of their instrumental community were changing dramatically in the late 1980s, partly as a result of sales of commercial STMs and AFMs. While the proportion of probe microscopists represented by IBM and Bell Labs researchers and by ultrahigh-vacuum STMers was declining, the proportion represented by universities and by air STMers and AFMers was increasing.

The tipping point for these demographic changes was the 1987 meeting of the International Conference on Scanning Tunneling Microscopy, held in Oxnard, California. This second meeting of the STM Conference was organized primarily by John Baldeschwieler's group at Caltech, about 60 miles away. Baldeschwieler was aided by a six-person organizing committee composed largely of people who would go on to run future STM Conferences in their home regions. For instance, Andrew Briggs, from the University of Oxford, took the conference to the United Kingdom in

1988, and Osamu Nishikawa, of the Tokyo Institute of Technology, ran it in Japan in 1989. Also on the committee was James Murday, a surface scientist at the Naval Research Laboratory and a grant officer at the Office of Naval Research.⁵ Murday had sent one of the surface scientists from his NRL group, Rich Colton, to Baldeschwieler's group for a sabbatical year to learn STM and then bring it back. Thus, Colton ended up running the 1987 conference's local organizing committee, and he and Murday arranged for the Office of Naval Research to be the meeting's primary non-local sponsoring organization. In return, Murday and Colton won the right to host the 1990 STM Conference in Baltimore, 40 miles from the Naval Research Lab.

Despite the increasing popularity of probe microscopy, the STM Conference had no assurance of continued growth or even existence. As Randy Feenstra puts it, some probe microscopists, including Heinrich Rohrer, were "always opposed to perpetuating an STM conference. . . . [Rohrer] said 'let's have a conference for a few years until people sort of learn how to do it and the instruments become a bit more widespread, and then let's just go out to our respective fields and use STM.' And he was right. That's to some extent what happened."⁶

Several constituencies favored the continuation of the STM Conferences, however. For newcomers to probe microscopy, the STM Conferences were still a good place to quickly learn about the technique and to get to know its practitioners. Because innovations to STM and AFM technology were emerging so frequently, and were often applicable across many domains, veteran probe microscopists still found it useful to attend a single conference organized by their instrumental community, rather than at all the niche conferences associated with their disciplinary communities. For STM and AFM manufacturers and allied firms, the STM Conference still represented the most convenient way to reach their (or their competitors') current customers.

Thus, through the 1990s there was always enough interest to keep the STM Conference alive. Indeed, the conference began to acquire some institutional permanence. The initial meetings had their proceedings published in a different journal every year—the journal selected not because its readers had a deep interest in probe microscopy, but because the main organizer of the STM Conference that year had connections to the journal's editor. But beginning with the Oxnard meeting in 1987—though only uninterrupted beginning in 1993—almost all of the conference's proceedings

appeared in the *Journal of Vacuum Science and Technology*, a publication of the American Vacuum Society. James Murday, at the time a trustee of the AVS, was probably responsible for this connection.⁷ Over time, the Vacuum Society also took on some back-office functions of the STM Conference (for instance, processing registrations).

The demographic turn evident at the Oxnard meeting, however, made the sponsorship of the AVS somewhat anomalous. The earlier Santiago de Compostela conference and the Cancun and Oberlech workshops were dominated by IBM and Bell Labs researchers, most of whom were using tunneling microscopy for surface-science research in ultrahigh vacuum. These people were a natural constituency for the American Vacuum Society, which since the 1960s had been the home professional society of UHV surface scientists.⁸ But at the Oxnard meeting surface scientists began to give ground to STMers working in air or liquid, or to those looking at graphite, at high-temperature superconductors, and at other materials that weren't of interest to most surface scientists.

After 1987, the proportion of probe microscopists doing UHV surface science continued to decline, and the proportion doing AFM and other variants increased steadily. By 1990, it was no longer self-evident why all probe microscopists would be interested in attending the International Conference on *Scanning Tunneling Microscopy*, nor why that conference should be sponsored by the American *Vacuum Society*. The conference series and/or the professional society would soon have to choose whether to target a narrow segment of probe microscopists (UHV STMers) or to reorganize as a "big tent." Jim Murday and Rich Colton, the organizers for the 1990 meeting, were largely responsible for moving both the STM Conference and the AVS in the latter direction.

That choice sprang, in part, from Murday's bird's-eye view not only of the probe-microscopy community but also of the parallels and connections between that community and other research fields. On the one hand, he could understand probe microscopy from the perspective of a practicing surface scientist and a science administrator at the Naval Research Lab. He ran the NRL's Surface Chemistry program until 1988, when he was promoted to head the Chemistry Division. On the other hand, Murday also viewed the field from the vantage point of a grant officer at the Office of Naval Research, where he specialized in grants to surface scientists even though he had grantees in chemistry and surface engineering more generally.

In the early 1980s, through those two links to surface science, Murday became heavily involved with the American Vacuum Society. Through the 1980s, therefore, he sponsored grants to many semiconductor surface scientists, and he was a trustee of the society that published the most important journals of semiconductor surface science. Thus, it was natural that he, in turn, became the grant officer for many of the early STMers working on semiconductor surface science, and that he steered the AVS to become the primary professional society of the probe-microscopy community by sponsoring (and publishing the proceedings of) the STM Conferences. Murday's sponsorship of surface-science STMers, however, also led him to become a funder of (and a source of advice for) other kinds of probe microscopists, such as Calvin Quate's AFM group at Stanford.

At the same time, Murday and other surface scientists could see that their field was losing some of its appeal. As one surface scientist and STMer, Stan Williams, recalled, around 1990 "surface science had started to become a little bit blasé. The bloom was off."⁹ The late 1980s and the early 1990s were, therefore, an ambiguous time for the AVS. As the professional home for surface science, that discipline's decline could have negative implications for the AVS. Yet even if surface science as a whole was stagnating somewhat, it had been the conduit for bringing a steady stream of new members—including air STMers and AFMers—into the Vacuum Society.

In his capacity as an officer of the AVS, therefore, James Murday could see the benefits of the society's connection to probe microscopy. Maintaining that connection, however, would require some acknowledgment that the demographic center of the probe-microscopy community had drifted away from surface science. Thus, Murday devised a two-part strategy for cementing the AVS's connection to probe microscopy and then using that connection to refresh the Vacuum Society and add to its membership. First, he expanded the STM Conferences so that they no longer mapped solely onto the probe-microscopy community. Instead, he put probe microscopists at their core, but redrew the conferences' mission to encompass a much broader range of research fields, including those traditionally associated with the AVS. Second, he expanded the AVS itself, so that its purview now included a large zone shared in common with the STM Conferences. The boundaries of that zone were fuzzy, but roughly defined by a new term becoming popular in science policy circles in the United States, Japan, Switzerland, and elsewhere—nanotechnology.

Thus, when Murday and Colton chaired the 1990 STM Conference, they renamed it as the “Fifth International Conference on Scanning Tunneling Microscopy/Spectroscopy and the First International Conference on Nanometer Scale Science and Technology.”¹⁰ Murday’s plan was that from then on the STM Conference would dissolve and be replaced by a wider NANO Conference. A meeting based solely on one class of instrumentation could not, he thought, survive. But a conference on “nanometer scale science and technology,” with probe microscopy at its center, would solve the demographic and structural tensions of both the AVS and the probe-microscopy community, and cement the connection between the two.

It turned out that, with STM and AFM technology changing so rapidly, probe microscopists were (in Murday’s words) “not quite ready” to give up their own conference series.¹¹ However, when Murday was president of the AVS in 1992 he worked out a compromise—the NANO Conference would run in even years, and the STM Conference would run in odd years. The two would have overlapping attendance and content, but NANO would be slightly larger and broader. At the same time, Murday founded a Nanometer Scale Science and Technology Division of the AVS, and three years later he became the first chair of a new Nanometer Structures Division of the AVS’s international umbrella organization, the International Union for Vacuum Science, Technique, and Applications. Murday and the AVS could now—via the category of “nanometer scale science and technology”—give a more credible justification for offering a home to the full breadth of probe microscopy within the Vacuum Society.

More Conferences and Nano-ization

There could, of course, have been other solutions to probe microscopy’s demographic tensions. For instance, instead of broadening out to include an even wider class of attendees, the STM Conference could have splintered into a variety of different meetings each focused on one of probe microscopy’s niche sub-communities. Surface scientists, in particular, tended to favor that solution. As we have seen, some surface-science STMers objected to what they saw as the lowering of scientific standards brought on by the appearance of AFM and air STM. As they saw the STM Conferences open their doors first to non-UHV probe microscopy and then to an even more diffuse “nano” audience, some UHV STMers demanded a

return to a more focused, more surface-science-oriented meeting. In 1992, a group composed mainly of current and former IBM Yorktown and Bell Labs STMers petitioned the Gordon Research Conferences (an important conference-holding organization, primarily for natural-science disciplines, founded in 1931) to create a conference series in scanning tunneling microscopy.¹² The next spring, the GRC hosted the first such conference (the first of what the organizers hoped would be a series) at the Doubletree Hotel in Ventura, California—only 10 miles from Oxnard, the site of the 1987 STM meeting.

The location wasn't the only "retro" feature of this meeting. The talks, too, covered much the same territory as pre-1987 probe-microscopy meetings (semiconductors, UHV STM), and the speakers represented the same organizations (IBM, Bell Labs) that had dominated the early era of probe microscopy. The rear-guard character of the conference is clear in the report of a monitor sent by the Gordon organization:

This new conference is essentially a physics-physical chemistry splinter group of STM practitioners. They found the international STM conference too big and impersonal and wanted to distance themselves from the biological STM community whose work they do not hold in high regard. This resulted in a very small conference (59 total).¹³

Yet even these secessionists must have known that the world of surface science wasn't what it was when the STM was invented. After all, as the monitor noted, "the atmosphere was somewhat subdued because of the threat of job losses at e.g. IBM and AT and T."¹⁴

With IBM and Bell Labs unable to support as much classical surface science as they once did, some attendees could foresee that surface science would have to move in new directions and form new interdisciplinary collaborations. A significant number of respondents to a survey circulated by the GRC complained about the conference's constricted view, remarking that the talks "were very good but had too narrow a range of topics," and that "the conference was heavily biased toward semiconductors. Several participants would be positive to a somewhat wider scope."¹⁵ One respondent addressed the new realities of probe microscopy head on:

The topics covered represented only a portion of the frontiers in STM. Noticeably missing were presentations from biological STM as well as workers in the area of thin films. This conference was completely devoted to STM in UHV. Future conferences should include, not exclude, contributions from these areas.¹⁶

The monitor himself saw the narrow focus on UHV STM as detrimental not only to the breadth of the science presented but also to its quality:

[T]he field is rather narrow and could probably benefit from interaction with other areas of surface science. . . . On a chemical level, I found that some of the interpretations of the species involved in the various deposits [of a metal or semiconductor on a semiconductor substrate] to be very speculative. A complementary analytical tool is needed in this field.¹⁷

He recommended that the conference be shut down and its topics folded into one of the Gordon organization's other surface-science-oriented meetings, such as Inorganic Thin Films or Chemical Reactions at Surfaces.

In fact, the GRC's Scanning Tunneling Microscopy conference, and the group that organized it, disintegrated after the 1993 meeting. Some UHV surface-science STMers retired early or were fired in the mid 1990s because of job cuts at IBM and at Bell Labs. A few left probe microscopy for new instrumental communities, such as low-energy electron microscopy, that were still exploratory and immature.¹⁸ Many continued with STM in some form but relied on it less exclusively, using it as one tool among many rather than as the instrument that defined their research. Few returned often to the AVS-sponsored STM/NANO Conferences. When I interviewed members of this group in 2001, several commented that, although the STM/NANO meetings had been "really fun for a while because they were really small," they had become much less relevant and interesting around 1992 because they had no "common theme" uniting the "biologists, chemists, physicists" interested in probe microscopy.¹⁹ In the view of many of those who first applied the STM to surface science, the STM had become a mature tool, and therefore "an international meeting [dedicated to probe microscopy] isn't really needed" and had "largely outlived its usefulness."²⁰

For many probe microscopists, however, *nanotechnology* increasingly provided a powerful "common theme" that would allow all members of their increasingly diverse instrumental community to communicate—and even collaborate—with each other. In turn, the success of probe microscopists in forming interdisciplinary collaborations and commercializing their research conferred legitimacy on their adoption of nanotechnology as an organizing theme. In the early 1990s, probe microscopists began to serve as a model for other research communities that were facing similar demographic problems and were tentatively seizing on nanotechnology as a solution to those problems.

The evolution of another Gordon Conference series, this one on the Chemistry and Physics of Microstructure Fabrication, highlights probe microscopy's role in this diffusion of nanotechnology discourse. This conference series had begun in 1976 as a gathering place for specialists in photolithography, electron-beam lithography, molecular-beam epitaxy, and other techniques used to make microelectronic devices. In the beginning, many of the leaders of this conference series were employed at IBM and at Bell Labs and knew about ultrahigh-vacuum STM through colleagues in those organizations. Before 1992, though, probe microscopy made no appearance at all at the Gordon Microstructure Fabrication conferences.

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That is, the intellectual content of probe microscopy and microfabrication research was almost completely separate up until about 1990, even though members of both networks were often housed in the same organizations. Microfabrication specialists at IBM and at Bell Labs saw little need to adopt probe microscopy, which they knew primarily as a tool of basic surface-science research that would be of little use in making, or even visualizing, tiny structures composed of both insulating and non-insulating materials. In the 1980s, however, many microfabrication specialists left those corporate labs for academic positions at Stanford, Cornell, UC Santa Barbara, and other universities. There they took note of new kinds of probe microscopy, such as AFM, magnetic force microscopy, and STM lithography (i.e., using an STM to make tiny marks on a surface) that could be used in microfabrication. At Stanford, two doyens of microfabrication and probe microscopy, Fabian Pease and Calvin Quate, even started a collaboration on STM modification of polymers. Later, Pease helped Quate's students learn to make microfabricated AFM cantilevers.²¹

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The broadening of microfabrication research to accommodate probe microscopy wasn't, however, initially reflected in the content of the GRC Microfabrication Conferences.²² The 1988 and 1990 meetings remained so narrowly focused on traditional microfabrication techniques that the Gordon organization's home office worried that the series was becoming stagnant and involuted. Indeed, the Gordon organization seriously considered dissolving the series. As a Gordon monitor put it, "there might be an ingrowing clique forming which might be deleterious to branching out with significant representation in not-so-well-known areas."²³ To break the power of that clique, and to save the conference series from cancellation,

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the 1992 organizers opened the meeting to new techniques, especially STM and AFM.

Thus, at the 1992 meeting, probe microscopy garnered one full day (out of a four-and-a-half-day meeting), with talks such as “Nanofabrication with the STM” by Alex de Lozanne (a former Quate student).²⁴ The “nano” in his title jibed well with other talks by leading microfabrication specialists, such as Fabian Pease’s “Nanonatural lithography” and Henry Smith’s “Nanolithography: Some Paths Less Well Traveled.” At the next meeting, in 1994, the program was even more slanted toward probe microscopy and nanotechnology. For instance, Don Eigler (from IBM Almaden) was the only speaker to have an entire session devoted to a single paper (on “Atoms Where You Want Them: Exploiting the STM as a Fabrication Tool”).

The 1994 GRC Microfabrication meeting was also notably more disciplinarily diverse than its predecessors. In particular, speakers were recruited to lecture on biological topics previously unheard of in the world of microfabrication. Talks that year, for the first time, had titles like “Tracking Down Biological Motors Using Optical Tweezers,” “Microfabricated Arrays: DNA Electrophoresis and Cell Mobility,” and “Biocatalytic Synthesis of Polymers of Precisely Defined Structure.”²⁵ This broadening into biology was facilitated by the continuing infiltration of probe microscopy into the meeting. For instance, Dan Rugar (from IBM Almaden) presented a paper on his new idea for an AFM-based nuclear magnetic resonance microscope to decipher the structures of proteins and other biomolecules.

The transformation of the GRC Microfabrication meetings in 1992 and 1994 illustrates three intertwined trends. First, probe microscopy appeared on the scene suddenly and in force. This catalyzed an expansion beyond the conference’s traditionally limited set of techniques—the same kind of expansion seen in the STM Conferences after 1987. Second, the terrain of the GRC Microfabrication conference shifted away from an almost exclusive focus on semiconductors and (some) metals to include biological materials. Again, this paralleled a similar, earlier shift toward biology in the probe-microscopy community. Probe microscopists (such as Dan Rugar) who had moved from metals and semiconductors to molecular biology in the 1980s were one conduit for bringing biology into microfabrication in the early 1990s. Finally, GRC Microfabrication attendees began to identify themselves as working on “nano” rather than “micro.” In doing so, they received instruction from federal grant officers who—as Murday had for probe microscopy and surface science—articulated how nanotechnology

could be an organizing principle for research. On the final day of the 1992 meeting, for instance, Jane “Xan” Alexander, a grant officer from the Defense Advanced Research Projects Agency, gave a talk on “DARPA’s View of Nanotechnology.”

In 1994, the attendees examined the dramatic changes their conference had undergone and decided that the meeting’s name should be changed to match its new focus. At their business meeting they approved a motion to change the name to Chemistry and Physics of Nanostructure Fabrication²⁶—a small change, but one that connected the new GRC Nanofabrication conference to a host of other organizations, conferences, journals, and other institutions that were similarly “nano-izing” at the same time.

Relabeling, Aligning, Connecting

The STM/NANO Conference and the GRC Nanofabrication conference were not the first institutions to adopt the prefix “nano” as a way to overcome bureaucratic hurdles, attract new funding, or expand their membership. Such nano-ization goes back at least as far as 1986, when Cornell University’s National Submicron Facility (along with Stanford’s and MIT’s, one of the three leading academic microfabrication centers in the United States) was seeking to renew its grant from the National Science Foundation. To signal that it had made progress since its founding in 1978, the organization changed its name to National Nanofabrication Facility. Similarly, that same year, the Ultra-Small Structures Group at the University of Glasgow became the Nanoelectronics Research Group (and later, the Nanoelectronics Research Center).

In order to lay claim to the “nano” label, such institutions had to make a case that the objects of their members’ research were more or less between 1–100 nanometers large in at least one dimension. But the size scale of research was certainly not a sufficient condition for adoption of “nano”—plenty of institutions whose members were engaged in nanoscale research were very slow to adopt the “nano” label. What stimulated institutional and community leaders to steer their colleagues toward the “nano” label was often a demographic problem (e.g., declining or rapidly changing membership) or an administrative requirement (e.g., a grant renewal).

Probe microscopists—especially prominent ones—served as important allies for those who promoted the adoption of the “nano” label. The GRC Microfabrication Conference series is a good example of that. Perhaps an

even clearer instance is the Microcircuit Engineering Conference, which in 1994 changed its name to Micro and Nano Engineering International Conference. That year the meeting, which dated back to 1975, met in Davos, Switzerland, and was chaired by Heinrich Rohrer's IBM Zurich colleague Peter Vettiger. In planning the conference, Vettiger could see that the past few meetings had had declining attendance. That is, like the 1990 STM Conference, or the 1992 GRC Microfabrication Conference, the 1994 Microcircuit Engineering meeting was an institution in trouble. To reverse the meeting's decline, Vettiger, much like Murday or the GRC organizers before him, latched onto "nano" as a tool for broadening the conference's focus and thereby for building connections to communities such as probe microscopy.

To cement that bridge to probe microscopy, Vettiger not only gave the meeting its new "Micro and Nano" name; he also invited Rohrer to give the plenary talk on "The Nanometer Age: Chance and Challenge"—perhaps as a way to instruct the attendees in the opportunities afforded by being "Micro and Nano" rather than just "Micro." This "mere" relabeling seems to have had miraculous effects. As Vettiger put it in the introduction to the conference proceedings,

Traditionally the main thrust of this series of conferences has been microlithography in its broadest sense. In the past, however, we have observed a certain maturing and saturation effect which manifested itself in a slow but steady decrease in the numbers of submitted papers and participants. When it became clear that the next conference should again take place in Switzerland, the organizing team took the initiative to broaden the scope and also to give the conference an appropriate new name. The very favorable response to our initiative indicates that we took a step in the right direction. Not only did the number of submitted papers increase from slightly over 100 in 1993 to 172 in 1994, but the number of registered participants jumped from 222 in 1993 to 333 in 1994.²⁷

A significant part of that reinvigorated attendance came from opening up the meeting to probe microscopists—a link made possible, or at least legitimized, by adoption of the "nano" label. A conference once almost bereft of probe microscopists now saw a quarter of its talks focus on STM and AFM. By the next meeting, in 1996, "the number of abstracts proposed on nano-scale engineering . . . was nearly twice as large as that of the main topics, essentially the lithography techniques, resist and pattern transfer."²⁸

Once institutions like the Submicron Facility and the Microcircuit Engineering Conference began to achieve positive results by adopting the

“nano” label, closely related institutions quickly followed suit. Here are a few examples: In 1997, the International Microprocess Conference became the International Microprocesses and Nanotechnology meeting; in 2001, the University of Illinois’ Microelectronics Laboratory became the Micro and Nanotechnology Laboratory; in 2005, the University of Michigan’s Solid-State Fabrication Facility became the Michigan Nanofabrication Facility.

In many cases, adoption of the “nano” label formalized connections between research communities that had sprung up informally somewhat earlier. For example, in 1995, the International Symposium on Electron, Ion and Photon Beams (known colloquially as the Three Beams Conference) became the International Conference on Electron, Ion, and Photon Technology and Nanofabrication. Proceedings of the Three Beams Conference were, like those of the STM Conference, published by the American Vacuum Society. Before 1990, however, there would have been very few people who attended both the Three Beams Conference and the STM Conference. After 1990, participants in the two conferences would have begun to meet each other at the AVS-sponsored biannual NANO Conference. Thus, the NANO Conference exposed the attendees of the other two conferences to each other’s work. Through that exposure to probe microscopy, attendees of the Three Beams Conference came to see that the fabrication of tiny structures could be accomplished with an STM or an AFM rather than with an electron, photon, or ion beam. Renaming their conference to include the “nano” label allowed specialists in the “three beams” to build on the interests they had in common with probe microscopists—interests that they came to understand as having in common through their affiliation with other institutions that had already adopted the “nano” label.

Thus, the field of nanotechnology is composed of many organizations, disciplines, and instrumental communities that existed long before they adopted the “nano” label. The speed and timing of the renaming of these institutions has given rise to some skepticism about the content of nanotechnology. As some crystallographers interviewed by the historians Christian Kehrt and Peter Schüssler insisted in the early 2000s, “nanotechnology is 100 years old”. For those crystallographers, nanotechnology is not a new area of research, even if the widespread adoption of the label is new.²⁹

In the early 1990s, many scientists viewed nanotechnology discourse as a fad that probably would disappear soon, leaving little behind. Even some

people whose names have become synonymous with nanotechnology held this view in the early 1990s. Some early promoters of nanotechnology were well aware of this view, and even subscribed to it in part themselves. Yet the accusation that nanotechnology is “mere” relabeling of communities and institutions that have long been concerned with nanoscale phenomena misses the point. Any individual case of relabeling might be opportunistic, or may have been used to resolve very contingent bureaucratic, demographic, or structural tensions—but the ricocheting of the “nano” prefix across such a variety of communities and institutions created new opportunities for exchanging personnel and ideas. By adopting the “nano” label, institutions and communities made it possible for other adopters of that label to notice them, and legitimized greater overlaps in their membership. Looked at in this light, the contribution of probe microscopy to the formation of nanotechnology is both more and less than is popularly claimed.

On the one hand, many popular and official accounts of the history of nanotechnology rather inaccurately date the origin of the field to the invention of the scanning tunneling microscope. Philosophers Davis Baird and Ashley Shew call this the “standard story” of the emergence of nanotechnology.³⁰ In their view, this standard story gestures to a myth that the Nobel laureate Richard Feynman forecast nanotechnology in a 1959 speech titled “There’s Plenty of Room at the Bottom.” Among other things, Feynman called for microscopes that could inspect cellular processes and perform nanoscale lithography. Later, justifications for a US National Nanotechnology Initiative pointed to the scanning tunneling microscope and the atomic force microscope as fulfilling Feynman’s call:

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 [scanning optical] microscopes, provide the “eyes” and “fingers” required for nanostructure measurement and manipulation.³¹

This standard story is misleading in a number of ways. It obscures the importance to nanotechnology of continuing improvements to older technologies such as optical microscopy, field ion microscopy, and electron microscopy.³² It also makes it seem self-evident that probe microscopy is nanotechnology, whereas in fact many probe microscopists were ambivalent about the “nano” label. Finally, it cannot account for the fact that a great deal of today’s nanotechnology doesn’t make use of STM, AFM, or

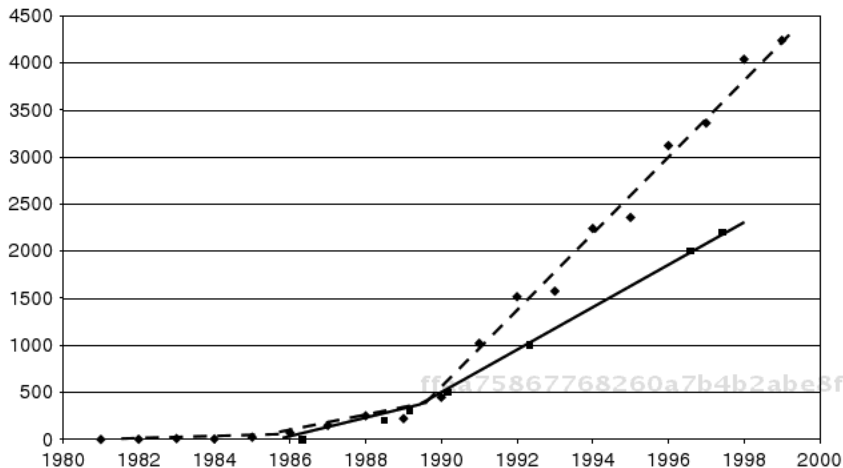


Figure 6.1

Numbers of probe-microscope articles published per year (◆) as measured by the Science Citation Index, and total number of NanoScopes sold by Digital Instruments (■). Trend lines are a best linear fit for three periodizations: 1981–1986, 1986–1990, and 1990–1999. Although somewhat arbitrary, this periodization indicates the modest increase in articles published after the start of the annual international STM Conferences and the introduction of commercial STMs around 1986. A much more dramatic increase in article production coincides with the start of the biannual NANO Conferences and the introduction of commercial AFMs around 1990.

their variants. Certainly, for most of the 1990s, most of the work being done at the National Nanofabrication Facility, or reported on at the International Microprocesses and Nanotechnology Conference, had nothing to do with STM or AFM.

On the other hand, probe microscopists did contribute to the formation of nanotechnology, in that the network of people strongly or weakly tied to their technology was very large and provided connections to a variety of other communities, organizations, and disciplines that could also be brought under the “nano” umbrella. On this account, even if STM and AFM were not the primary tools of all the participants in the nanotechnology enterprise, the probe-microscopy community was still instrumental in helping those participants view an affiliation with nanotechnology as desirable.

One influential probe microscopist who clearly articulated this specific vision of nanotechnology as a field defined by connections among other

fields was Heinrich Rohrer. At least as early as the 1991 STM Conference in Interlaken, Switzerland (the first one after the joint 1990 STM/NANO meeting), Rohrer was publicly advocating for nanotechnology as a way of organizing science more efficiently and creatively. At that 1991 meeting, he summarized the first ten years of STM and looked forward to a new “nano” era:

Ten years ago, the term “nanometer” was not yet familiar in solid-state physics and technology. Whether the progress and achievements made with local probe methods created the current nanometer “fashion” or were carried by it is a moot speculation. Whatever the case, local probe methods have given considerable impetus to nanometer-scale science and technology. They have created something like a casual relationship between us and individual atoms, molecules, clusters and nanometer-sized parts of macroscopic objects.³³

Rohrer argued that nanotechnology represented the intersection of two trends. On the one hand, for decades microfabrication specialists had been steadily crafting smaller and smaller objects out of bits of metal and semiconductor. This trend was pushed by the microelectronics industry, and is described by Moore's Law—the observation, first made in 1965 by Gordon Moore, that the number of transistors that could be placed on a silicon wafer doubled every 18 to 24 months.³⁴ On the other hand, the same period saw molecular biologists and chemists steadily becoming accustomed to working with larger and larger molecules—proteins, enzymes, long strands of DNA, and so on. Thus, Rohrer claimed, the nanometer age began when electrical engineers (and others moving *down* from the microscale) began working on objects of the same size as objects being worked on by biologists and chemists moving *up* from the atomic scale. In Rohrer's view, this convergence in size scale created an opportunity for electrical engineers, biologists, chemists, and others to collaborate in new ways. For Rohrer, then, “nanotechnology” is interdisciplinarity. Note, however, that Rohrer bracketed the role of probe microscopy in the emergence of interdisciplinary nanotechnology—it would be “moot speculation” to debate whether SPMs caused that emergence. As figure 6.2 shows, Rohrer clearly saw the trends that led to nanotechnology as long predating the STM. Probe microscopy facilitated the intersection of those trends, or at least made that intersection easier to see, but in Rohrer's view it would be too simplistic to say that probe microscopy caused, or was necessary for, the emergence of nanotechnology.

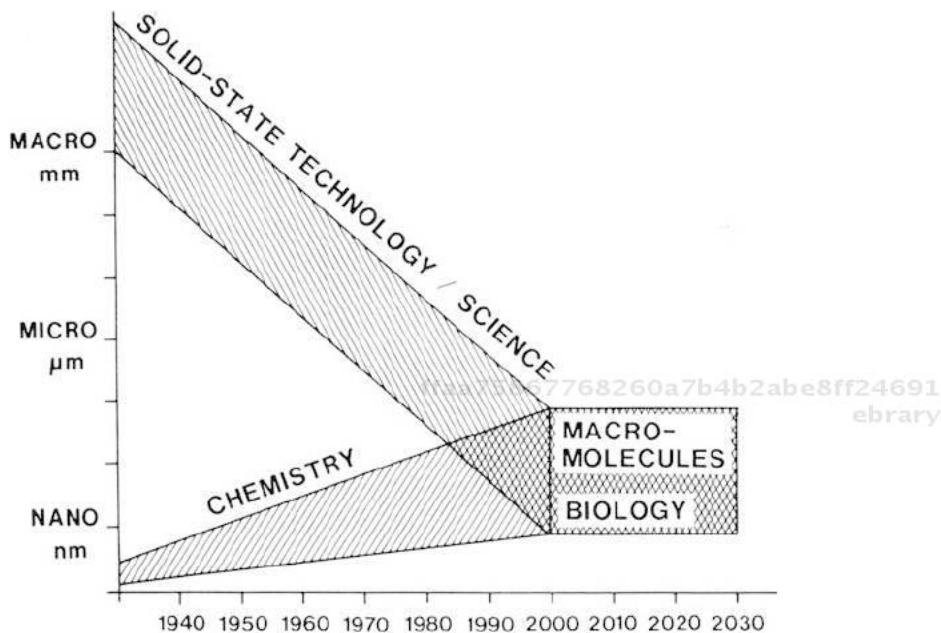


Figure 6.2

Heinrich Rohrer's depiction of nanotechnology as a product of the convergence of electrical engineers' ability to create ever smaller structures and chemists' and biologists' ability to manipulate ever larger macromolecules. Source: Heinrich Rohrer, "STM: 10 Years After," *Ultramicroscopy* 42 (1992): 1–6 (copyright 1992), reprinted with permission from Elsevier.

For Rohrer, probe microscopy was only one of many enablers of nanotechnology. Just as important as new instrumentation, he declared, were new forms of scientific governance. As Rohrer put it in a plenary speech for a conference in Japan on nanoscale science in 1992,

The nanometer age, or the "age of interdisciplinarity" poses formidable challenges beyond issues of purely scientific and technical nature. . . . [S]cientific bodies have to rethink their objectives and practices seriously and to find ways and means for an effective promotion of interdisciplinary science.³⁵

As we have seen, Rohrer—a physicist who supervised one of the first groups to open STM up to biological applications—exemplified "the promotion of interdisciplinary science." Now, he declared, it was up to "scientific bodies" to do the same.

National and International Isomorphism

According to the picture offered by Rohrer, although practitioners of any single field such as crystallography could reasonably claim to have been doing nanoscale research for “100 years,” that cannot be the full story. Nanotechnology as a distinct enterprise formed not within fields, but at their intersections. Probe microscopy, in Rohrer’s view, could “give considerable impetus” to the formation of those intersections. But sustaining that impetus would require scientific institutions, communities, and policy-making bodies to “rethink their objectives and practices.”

The rethinking of objectives and practices was necessarily both a transnational process and a nationally delimited one. Many of the institutions that were influential in both probe microscopy and the emergence of nanotechnology were at least somewhat transnational in scope. The American Vacuum Society, for instance, was connected to peer organizations around the world through its membership in the International Union for Vacuum Science, Technique, and Applications, and the society’s annual conferences usually had a significant international presence. Prominent university groups such as Paul Hansma’s or Calvin Quate’s were primarily funded by nationally delimited agencies, but also hosted and collaborated with scholars from around the world. Digital Instruments and other start-up companies usually concluded agreements with local distributors in multiple countries (and/or opened their own international offices and applications labs) within a few years of their founding. Yet nationally bounded organizations and policies were also indispensable to the spread of nanotechnology discourse. In the same way that probe microscopists’ memberships in many different fields allowed them to connect those fields under the “nano” label, SPMers’ positions in various national bodies allowed them to use nanotechnology to connect together many different national resources.

Again, Heinrich Rohrer provides one of the clearest examples of probe microscopists’ role in steering national institutions toward nanotechnology. In 1993, Rohrer helped create, and became president of, the Schweizerische Gesellschaft für Nanowissenschaften und Nanotechnik (Swiss Society for Nanoscience). At the end of that year, he and Iris Zschokke-Gränacher (a physicist from the University of Basel) convinced the Swiss Federal Council to mandate the Swiss National Science Foundation to begin a National

Research Program in nanoscience.³⁶ This led, in 1996, to an outlay of 56 million Swiss francs (matched by contributions from Swiss industry) for a Priority Program on Micro- and Nanosystems Technologies (MINAST).³⁷ In 1999, MINAST evolved into a National Center of Competence in Research (NCCR) in Nanoscale Science led by Hans-Joachim Güntherodt at the University of Basel, one of the earliest adopters of the STM and the AFM. Later, with the aid of Güntherodt, Christoph Gerber, and others, this NCCR evolved into the Swiss Nanoscience Institute. Today that institute, with its headquarters in Basel, coordinates nanotechnology research at eleven laboratories around Switzerland, in cooperation with nine industrial partners.

In the United States, James Murday played a role similar to Rohrer's and Güntherodt's in making probe microscopy central to the formation of American national institutions for nanotechnology. Murday's first venture along those lines was the Microstructure and Atomistic Processes on Surfaces (MAPS) initiative, which he developed in 1985 in collaboration with Larry Cooper, an ONR grant officer overseeing funding in microelectronics. MAPS funded STM work on surface structures, but with an orientation to microelectronics applications for STM that were, at the time, largely speculative. Five years later, with MAPS winding down, Cooper and Murday looked for a way to reinvent it. The new incarnation of MAPS, called the Properties of Interfacial Nanostructures (PIN) Initiative, expanded from STM to AFM, and thereby shifted from flat, pristine semiconductor surfaces to the three-dimensional semiconductor and oxide structures found in real-world microelectronic devices.

By 1990, Murday saw nanoscale research as a "freight train" that would inevitably impact high-tech industries, and therefore as an area in which the United States should take the lead.³⁸ He began pushing his vision of nanoscale research in various venues. In 1988 and 1989, Murday and Rich Colton organized topical conferences on Nanometer Scale Properties of Surfaces and Interfaces for the American Vacuum Society. In 1990, they teamed up to organize the STM/NANO meeting in Baltimore. And in 1992, Murday and Phaedon Avouris (an IBM STMer) organized a conference on Atomic and Nanoscale Modification of Materials for the Engineering Foundation.

The conference on Atomic and Nanoscale Modification of Materials for the Engineering Foundation exemplified Murday's strategy for nanotechnology. The first two days of the meeting exclusively featured leading

probe microscopists. On the third day, Fabian Pease and other leading microfabrication specialists took part in panel discussions moderated by people involved with STM. On the fourth day, a smorgasbord of topics, including buckminsterfullerenes, semiconductor nanocrystals, and molecular self-assembly, were presented, again in panel discussions moderated by probe microscopists. The meeting concluded with a report by Xan Alexander of DARPA summarizing an earlier workshop on Nanometer Science and Technology held by DARPA and the US Department of Energy.³⁹ As this lineup shows, Murday encouraged probe microscopists to lead, by precept and example, whatever group of research fields “nanometer science and technology” would eventually refer to. At that moment in the early 1990s, very little bound people working on buckyballs together with microfabrication specialists and dendrimer chemists. Coherence and cross-membership among those fields was partly an emergent product of a series of conferences drawing attention to the “nano” label. Organizing those conferences, and explaining their lessons to attendees, fell to a close-knit community of federal grant officers and high-profile scientists committed to shepherding nanotechnology. Some of the scientists and grant officers in that early elite—including Heinrich Rohrer and James Murday—had longstanding ties to probe microscopy. Others drew on probe microscopy in trying to bring coherence to a disparate mass of research fields, instrumental communities, and industries.

An incident from 1991 shows how close-knit this shepherding elite of grant officers and high-profile scientists was. That year, James Murday and Dick Brandt (another ONR officer) suggested to Xan Alexander that she develop a funding program in “nanostructured electronics” with AFM lithography as a central focus.⁴⁰ As it turned out, Alexander was already working with Larry Cooper on ULTRA, a program funding “ultrafast, ultrasmall” electronics research covering a spectrum of grantees from traditional silicon microelectronics to the highly speculative “molecular electronics” (i.e., using single molecules to replace electronic components such as transistors and capacitors).⁴¹ The confluence of interests among Murday, Brandt, Alexander, and Cooper allowed each officer to draw on the others’ portfolios in ways that gradually fleshed out the constitution of “nano.” For instance, ULTRA grantees were soon drafted into Murday’s series of nanoscale research conferences, while STMers became influential members of the ULTRA group.

As grant officers in the various military research funding agencies began to talk about ways of connecting or even coordinating their portfolios under the umbrella of “nanoscale science and technology,” James Murday emerged as the unofficial point man for the Department of Defense’s nanoscale initiatives. This proved useful in 1994 when Admiral David Jeremiah asked Murday to present his vision for nanotechnology to the DoD’s Joint Requirements Oversight Council (JROC), and later to the Secretary of Defense. This presentation greatly enhanced the visibility of nanotechnology within the DoD. As Murday puts it, “shortly after Jeremiah having me in front of the JROC, I was briefing three-stars all through the Pentagon who were basically trying to figure out, ‘Oh my god, what’s going on here? What should I be aware of?’”⁴² That heightened awareness of nanotechnology at the Pentagon led to the creation, in 1997, of a DoD Strategic Research Area committee on nanoscience, with Murday as its chair.⁴³

Not long after briefing the JROC, Murday heard about Mihail “Mike” Roco, a grant officer at the National Science Foundation, who was independently lobbying for a nanotechnology initiative that would unite the nanotechnology efforts of all federal funding agencies. Murday introduced himself to Roco and promised to bring the Pentagon’s nanotechnology projects on board Roco’s initiative. From then on, Murday was Roco’s junior partner in the creation of the National Nanotechnology Initiative.⁴⁴ When Roco persuaded the Clinton administration to let him chair an Interagency Working Group on Nanoscience, Engineering, and Technology within the National Science and Technology Council, Murday served as the working group’s executive secretary. When the National Nanotechnology Initiative came into being, Murday remained executive secretary of the NSTC’s new Subcommittee on Nanometer Science, Engineering, and Technology (which Roco continued to chair). Murday also became the first director of the National Nanotechnology Coordination Office, which managed the day-to-day activities of the National Nanotechnology Initiative, leaving Roco free to concentrate on spreading the word.

Thus, in James Murday, probe microscopy had a powerful advocate at the table in the creation of national institutions for nanotechnology in the United States. Murday himself sees his enthusiasm for nanotechnology as having been inspired directly by his association with probe microscopy. But probe microscopists were hardly pushing Murday to embed their work in a wider vision for nanotechnology. Indeed, many of them were

reluctant to go along with his plan to dissolve the STM Conference in favor of the NANO Conference. Nor did Murday's role in the institutionalization of nanotechnology stem solely from his work with probe microscopists. Rather, his advocacy for probe microscopy allowed him to form alliances with other grant officers (including Xan Alexander, Larry Cooper, and Mike Roco) who oversaw complementary funding portfolios. Those other portfolios were sometimes only tangentially related to probe microscopy, but when added to Murday's portfolio they covered a wide swathe of nanoscale research.

In the late 1980s and the early 1990s, many governments turned to nanotechnology discourse as a way to coordinate national research funding. Often that was done by gradually building on earlier efforts, much as Murday and Cooper's PIN initiative grew from the earlier MAPS. The Japanese government, for instance, began exploratory efforts with funding for projects on "Nano-Mechanisms" in 1985, which led to the "Atom-Craft" (atomic manipulation with STM) project in 1989, and the "Atom Technology" program in 1992. In many countries, such state-sponsored nanoscience initiatives aimed to bring basic researchers into greater contact with high-tech manufacturing, particularly in microelectronics. For instance, in 1987 the British government established a National Initiative of Nanotechnology, which was followed in 1988 by a so-called LINK effort in nanotechnology to "bridge the gap between science and the market place."⁴⁵ By 1994 the LINK program was giving a total of £23.6 million to 28 nanotechnology groups. Likewise, in Belgium, the European Community began a program in 1992 on the Physics AND Technology of Mesoscale Systems (PHANTOMS) that tied together traditional silicon microelectronics with molecular electronics, much like DARPA's ULTRA program.⁴⁶

Leaders of each of these national programs looked to the other countries' programs for models. For instance, the United Kingdom's NION and LINK programs were set up on the recommendation of Albert Franks, a physicist and metrologist who had recently learned about the Japanese government's initiatives in nanotechnology and "atom" technology. Franks' recommendations were further bolstered by a report comparing the UK's efforts in nanotechnology with those in the United States, Japan, and Europe.⁴⁷ Later, "Franks is also reputed to have inspired the Germans to develop their own nanotechnology strategy, after a chance discussion in a railway station with Gerd Bachmann, then a senior scientist with the German Research

Ministry.”⁴⁸ Similarly, Jim Murday’s former boss at the Naval Research Laboratory, William Tolles, spent six months in 1994 touring European nanotechnology laboratories and reporting his findings to American grant officers and scientists.⁴⁹

As various national programs in nanotechnology grew, advocates in other countries—such as Murday or Rohrer—could point to them as evidence of the need for their own institutions to keep up in an area that was critical to national security and economic competitiveness. As the Nobel laureate Richard Smalley put it to the US Presidential Council of Advisors on Science and Technology in 1999,

[T]he prefix ‘nano’ is heard in virtually every scientific and technical meeting throughout the world. Nano is in the ‘buzz’. Without the [National Nanotechnology Initiative], by 2010 it may be too late to insure that the US is in the lead, and vast nanotech-dominated markets in virtually every sector of the economy may be lost to foreign competition.⁵⁰

National coordination of budding nanotechnology institutions and international competition for the commercialization of nanotechnology were mutually reinforcing dynamics. The eventual products of the interplay of those dynamics were the national nanotechnology initiatives that came into being at the turn of the millennium.

The Current Nanometer Fashion

It took about a decade for the “buzz” about nanotechnology that Richard Smalley cited to result in the formation of the National Nanotechnology Initiative in the U.S. and peer initiatives in other countries. In the early 1990s, probe microscopists were sometimes the topic, sometimes the promoters, and sometimes the audience for “buzz” about nanotechnology. A few, including James Murday and Heinrich Rohrer, actively championed nanotechnology as a way of reorganizing (and connecting) various instrumental communities, and also revitalizing their nations’ science establishments. Most probe microscopists, however, didn’t promote nanotechnology as a research agenda, but neither did they reject it. Probe microscopists who attended the NANO Conference every other year, for instance, probably didn’t identify as a “nanotechnologists,” but most of them had few qualms about the expansion of the probe-microscopy community into areas such as biology. Such probe microscopists would no doubt recognize

the “NANO” in the conference’s name as a gesture intended to plausibly tie together research in physical and life sciences through the common use of the AFM and other instruments.

In general, probe microscopists hedged their bets on nanotechnology. When offered chances to align with a nanotechnology agenda, they did so opportunistically but lightly. They could see that if nanotechnology were to be institutionalized in a major way, their association with it could prove useful. Yet they took care to avoid committing too enthusiastically. William Tolles—despite his strong support for nanotechnology—made the case for such caution in a report of his 1994 tour of European nanotechnology research sites:

Opportunities [in nanotechnology] appear so evident that hype about the subject could attract practitioners bent on hypothetical postulates or excessive “salesmanship” without a realistic appraisal of the products of experimental research. Overselling a field can be as detrimental as overly criticizing a field. . . . Some individuals professing the visionary aspects of the field could lead to problems for the field overall.⁵¹

Likewise, Heinrich Rohrer strongly promoted a certain vision of nanotechnology, yet he also attempted to differentiate that vision from “the current nanometer ‘fashion.’”

The “visionary” individual who built a career on making nanotechnology “salesmanship” “fashionable” in the late 1980s and the early 1990s—and whom Tolles and Rohrer may have been warning against—was the Silicon Valley-based nanotechnology enthusiast K. Eric Drexler. In 1986, Drexler published *Engines of Creation: The Coming Age of Nanotechnology*, the book that introduced the word “nanotechnology” into the popular lexicon through a radical vision of programmable molecular “assemblers” creating a new society of immortal, shape-changing humans free from want and able to roam the universe. The same year, Drexler and his wife, Christine Peterson, founded the Foresight Institute near Stanford, where they could draw on a local network of futurists, entrepreneurs, venture capitalists, science-fiction writers, and space enthusiasts to promote their vision of nanotechnology. Drexler and Peterson embarked on an impressive public-relations campaign in which Drexler’s name and mentions of “molecular nanotechnology” (i.e., the assemblers) appeared in hundreds of newspapers, airline magazines, trade journals, television shows, science-fiction novels, and a few peer-reviewed scientific publications.⁵²

Drexler wasn't the first to use the term "nanotechnology." As we have seen, Norio Taniguchi introduced it to the precision engineering community as early as 1974. Even after Drexler's book appeared, scientists could, for a time, use the term without evoking futurist visions. By the early 1990s, however, Drexler's popularizing of the term had permanently linked it, for better or worse, to futurist tropes of immortality, space travel, and trans-humanism. William Tolles and others began to worry about "the possibility of overselling the term 'nanotechnology,' which is considered by some to be an unfortunate name to some extent."⁵³ "When [in 1994] I founded my research group at Hewlett-Packard," the surface scientist and STMer Stan Williams has said, "we called it 'Quantum Science Research' to avoid any connection with the negative connotations of 'nanotechnology.'"⁵⁴

Dangerous as its overselling might be, though, many scientists found the term "nanotechnology" quite attractive. By the early 1990s, Eric Drexler had succeeded in interesting venture-capital firms (such as Stewart Brand's Global Business Network) and influential figures (Al Gore, Newt Gingrich, Admiral Jeremiah) in supporting nanotechnology.⁵⁵ As Jeremiah's patronage of Murday shows, people who had learned about nanotechnology through Drexler could then be convinced to provide resources to scientists working on current technologies such as probe microscopy.

Embracing nanotechnology was, therefore, a risky but potentially rewarding move. As Mark Welland says of government support for nanotechnology in the UK in the early 1990s,

[T]he work of Drexler and [Drexler's colleague] Ralph Merkle at the time had a lot of currency in the UK, a lot of press. . . . In some senses, it helped because from a governmental/commercial perspective it painted a picture which had lots more promise rather than being a sort of surface science-based approach to life. . . . But it had significant negative effects, because the claims were unfounded, in my view. . . . It was really Drexler's work in the early 1990s that really got the "nano" term to become more prevalent. . . . It's been a good thing for nano to some extent. It's also been a bad thing. I think certainly in this country a lot of expectations [were] raised which were unrealistic.⁵⁶

Some probe microscopists were less willing than Mark Welland to grant that Drexler's advocacy had had any positive outcomes. Calvin Quate, for instance, told reporters in 1991: "I don't think [Drexler] should be taken seriously. He's too far out."⁵⁷ Don Eigler, the IBM surface scientist famous for manipulating single atoms, put it even more strongly in 2004: "To a person, everyone I know who is a practicing scientist thinks of Drexler's

contributions as wrong at best, dangerous at worst. There may be scientists who feel otherwise, I just haven't run into them."⁵⁸

Remarkably, earlier in his career Don Eigler wouldn't even have had to leave his workplace to meet a scientist who viewed Drexler more favorably than "wrong at best, dangerous at worst." John Foster, the former Quate student and IBM Almaden researcher who had used an STM to "herd" and "dissect" single molecules, occasionally interacted with Drexler's circle, and even gave a talk on his research at the First Foresight Conference on Nanotechnology in 1989. Because Drexler's version of molecular nanotechnology pivoted on the ability to manipulate individual atoms and molecules to build up complicated structures, the Foresight group often pointed to Foster's work as an important stepping stone to the realization of molecular assemblers. Nor was Drexler alone in the view that John Foster's work was at least a rudimentary demonstration proof for nanotechnology. An article in *IBM Research Magazine* from 1988, for instance, made the connection between molecular manipulation and Drexler's nanotechnology directly, if ambivalently:

Drexler claims that today's technology is hampered by the need to deal with "atoms in unruly herds." As it becomes possible to manipulate single atoms and molecules, he predicts, we will be on the threshold of a technological revolution. While Drexler's amoeba-size computers remain the stuff of science fiction, scientists at the Almaden Research Center may have taken one step on the long road to such atomic-level control. Patrick C. Arnett, John S. Foster, and Jane E. Frommer . . . have been using a scanning tunneling microscope (STM) to create images of organic molecules and then modify the arrangement and possibly even the shape of the molecules.⁵⁹

Similarly, John Pethica, who co-built the first STM in Britain, described Foster and Frommer's work as "one of the principal *gedanken* tools for nanotechnology—the proposed direct manipulation of matter, especially biological, on the atomic scale."⁶⁰

John Foster himself was happy to participate in some Foresight events, and at least mildly inspired by the enthusiasm of Drexler's group. He and Drexler even co-wrote a letter that appeared in *Nature*—a boon for Drexler's desire to appear in a prestigious peer-reviewed publication (even if the letter itself wasn't peer-reviewed).⁶¹ Foster enjoyed Drexler's approach partly because he saw in it an extension of the open-ended form of experimentation that typified the Quate group. At the same time, he was wary because Drexler and the Foresight group had so little familiarity with what was actually possible in the laboratory:

There have to be people that are hard-core, show-me [the data] scientists. On the other hand you have to have the visionaries, because if you don't you won't make some leaps of faith, you won't ever get over any hurdles. You probably wouldn't have ever made the tunneling microscope in the first place. There's value in both camps. Drexler, of course, was so hard over in this other [i.e., the visionaries'] camp that it was difficult for some people to take him seriously at all.⁶²

Foster remembers going to Foresight events and being "the only person actually doing experiments. Everyone else was just talking."⁶³ Whatever his sympathies for Drexler's vision, Foster's laboratory experience often led him to be, as he puts it, a "curmudgeon" raining on the Foresight group's parade.⁶⁴

Foster wasn't the only probe microscopist in the early 1990s to see Drexler's Foresight Institute as an important forum for discussion and networking, even if they found Drexler's own ideas about molecular assemblers implausible. John Baldeschwieler, for instance, attended the First Foresight Conference on Molecular Nanotechnology in 1989.⁶⁵ At the Second Foresight Conference in 1991, all three of the leading California academic groups mentioned in chapter 4 were represented. Paul Hansma and his postdoc Jan Hoh gave a paper (with Hoh's doctoral adviser, J. P. Revel, as a third author); Doug Smith, Quate's former student and Binnig's postdoc, presented a paper with Masakazu Aono of the Atom-Craft Project; and William Goddard, a close, senior collaborator of Baldeschwieler's at Caltech, gave a paper co-authored with Ralph Merkle, one of Drexler's main surrogates and the coiner of the term "computational nanotechnology."

In the late 1980s and the early 1990s, all three of the start-up companies associated with the leading California academic probe-microscopy groups also ventured to see what Drexler and nanotechnology discourse could offer them. Some of Digital Instruments' earliest advertisements, for instance, were published in the NT [Nanotechnology] Resources Catalog published by NANOPIRG (Nanotechnology Public Interest Group), a club loosely affiliated with Eric Drexler. NANOPIRG even claimed to be DI's representative in Northern California:

NANOPIRG brokers STM's [sic] in order to promote development of NT. . . . Let NANOPIRG introduce you to Digital Instruments' fine STM equipment, and upon purchase from DI mention NANOPIRG and receive a BONUS of 20 FREE NanoTips over and above those included with the complete systems.⁶⁶

Park Scientific Instruments hosted field trips to its offices for attendees of the Fourth Foresight Conference in 1995, and the Baldeschwieler-affiliated

start-up QuanScan declared its employees “Innovators in Nanotechnology” in every advertisement.

Yet these companies remained cautious about nanotechnology. As late as 2002, for instance, a financial news service reported that Veeco (with which Digital Instruments had merged) was *not* “touting its focus on nanotech.” Quoting Veeco spokesman John Brennan, the article noted that Veeco preferred “to describe its acquisition strategy as a way to ‘add enabling technologies in core target markets.’” The news service went on to quote Debra Wasser, vice president of investor relations for Veeco, as saying “The “story is already complicated enough without confusing Wall Street with talk of nanotechnology.”⁶⁷

Owning the Label

The perception that Eric Drexler's program carried significant risks may explain why the nano-ization of scientific institutions began gradually with the adoption of “nano” as a prefix, rather than with use of the full word “nanotechnology.” Even people who spent the late 1990s pushing for a National Nanotechnology Initiative in the United States often used circumlocutions for “nanotechnology” in the early 1990s. Jim Murday's NANO Conference, for instance, was officially the Conference on Nanometer Scale Science and Technology. Rick Smalley, who pushed his colleagues to adopt “nanotechnology” as one of the Rice University chemistry department's two main foci as early as 1992, chose the more anodyne name “Center for Nanoscale Science and Technology” for a center he founded in 1993.⁶⁸ Yet Smalley also sent copies of Drexler's *Engines of Creation* to Rice University's trustees in order to stir up interest in the CNST.⁶⁹ Thus, whatever risks proponents of nanotechnology saw in Drexler's vision, they were also sometimes willing to operate in the same intellectual space as Drexler. Some of the first institutions to adopt the full word “nanotechnology” (rather than the prefix “nano”) were, therefore, meeting places for influential academic scientists like Smalley, government administrators like Jim Murday, and members of Drexler's circle. One important illustration of this is the journal *Nanotechnology*, founded in 1990 by David Whitehouse, a British engineer.

Perhaps one reason Whitehouse was more receptive to the word “nanotechnology” was that he came out of the same surface engineering and

metrology community as the word's inventor, Norio Taniguchi. Whitehouse's description of the new journal in its first issue reinforced this lineage, declaring up front that "nanotechnology is not a new word" and that it "originated from ultra-precision engineering but has now developed such that it epitomizes the drawing together of advanced engineering technology and applied modern physics."⁷⁰ Recall that precision engineers were the same community to which Russell Young had tried to sell the Topografiner in the early 1970s. In fact, one of Whitehouse's four regional editors was Young's protégé, Clayton Teague, who in 1990 was group leader of the Micrometrology Group in NIST's Precision Engineering Division. After the journal's first volume, Teague replaced Whitehouse as editor in chief. That tied *Nanotechnology* more strongly to probe microscopy, both through Teague's association with Young and through his leadership of NIST's probe-based metrology project, the Molecular Measuring Machine. The editorial board also included several other influential probe microscopists of the sort who promoted wide-ranging links between their instrumental community and a variety of disciplines: Jim Murday, Paul Hansma, Kumar Wickramasinghe, Christian Joachim. Digital Instruments was one of the journal's first advertisers, and an STM image of the iconic silicon 7x7 graced the cover of each of the two issues in the journal's first volume.

Thus, probe microscopy was central to whatever David Whitehouse meant by "nanotechnology." It is important to note, however, that only about a quarter of the journal's articles had anything whatsoever to do with probe microscopy. Indeed, Whitehouse routinely accepted papers by British colleagues who used surface profilometers, a technology that probe microscopists claimed to have superseded with the AFM. The reason probe microscopists were central to the journal was *not* (as the "standard story" of nanotechnology's history claims) that nanotechnology couldn't have existed before the STM and the AFM. Rather, Whitehouse brought probe microscopists in because the community they had created operated in the interdisciplinary manner he envisioned for all of nanotechnology.

That is, Whitehouse defined nanotechnology as "a new way of thinking . . . concerned with bringing together disciplines at the atomic and molecular level."⁷¹ Probe microscopy was instrumental in the emergence of nanotechnology as a "new way of thinking," therefore, insofar as it brought disciplines together. Thus, when listing the topics covered in the journal *Nanotechnology*, Whitehouse mentioned probe microscopy only in

calling for articles on “the application of nanometer level instruments such as scanning tunneling microscopes and atomic force microscopes to biology, medicine, and materials science.”⁷²

In *Nanotechnology's* first years, even after Clayton Teague succeeded Whitehouse as editor in chief, members of the network centered on Drexler and the Foresight Institute found a home for their ideas in the journal. Teague attended the First Foresight Conference in 1989, and published the proceedings of the Second Foresight Conference in *Nanotechnology* in 1991. Over time, however, Drexler's and Foresight's presence in this journal, and in other government- and university-sponsored nanotechnology institutions, began to decline. As various institutions adopted the prefix “nano,” the term steadily became less speculative. Scientists could point to the family of “nano” institutions as constituting an extant, practical vision of nanotechnology opposed to (or at least separate from) Drexler's.

By the end of the 1990s, even those who had earlier backed away from the term now began to adopt the label “nanotechnology.” For instance, Stan Williams, who in 1994 explicitly rejected calling his work “nanoscience” because of the Drexlerian connotations, realized “eventually [that], because the word had found such widespread use in the public, we in the field essentially had to adopt it.”⁷³ Thus, in 1999 Williams co-edited, with Mike Roco and Paul Alivisatos (a Berkeley semiconductor nanocrystal specialist), an influential report on “Nanotechnology Research Directions” that established an agenda not only for the coming National Nanotechnology Initiative in the United States but also for parallel efforts as far away as Russia.⁷⁴

With the founding of various national nanotechnology initiatives at the turn of the millennium, most probe microscopists dropped any qualms about nanotechnology. Today, many probe microscopists participate in—and some lead—the institutions of nanotechnology. In 2006, the STM Conference was abolished in favor of an annual International Conference on Nanoscience + Technology. Many of the probe-microscopy companies founded circa 2000 had “nano” in their names (Nanosurf, Pacific Nanotechnology, NanoInk, etc.). By 2007, even Veeco proudly described its China Nanotechnology Center and its India Nanotechnology Laboratory to investors and told them its “metrology instruments are used by nanotechnology researchers, and we currently sell to most major scientific and research organizations engaged in the field of nanotechnology.”⁷⁵

To understand what probe microscopists gain from today's institutionalized nanotechnology, we must return to our three axes of analysis: disciplines, organizations, and instrumental communities. We have seen how one discipline, surface science, was perceived as in decline in the early 1990s, and how it used its association with probe microscopy to revive some of the institutions associated with it—especially the American Vacuum Society and its *Journal of Vacuum Science and Technology*. Yet the connection to probe microscopy *per se* was difficult to maintain. Initially, some surface scientists took this to mean they should sever themselves from the rest of the (increasingly diverse or cacophonous—depending on your viewpoint) probe-microscopy community.

Jim Murday and other nanotechnology proponents, however, drew a picture of nanotechnology in which surface science was not only an important subfield, but also a forerunner and model: "The scientific foundation of nanostructures enjoyed a renaissance starting in the 1960s. Surface science constrained one material dimension into the nanometer size scale. . . . The 1990s nanoscience renaissance has close parallels to the 1960s surface science renaissance."⁷⁶ Over time, many surface scientists came to believe that nanotechnology provided the best way for them to revive or transform their discipline while retaining much of their knowledge base. As Jun Nogami puts it,

strictly classical surface structure determination is dead as a field. Or extremely mature, and not very fundable. So what I do now I can honestly bill as being related to nanotechnology. But when you look at the actual kinds of materials I'm working with, I'm still working with metals and I'm still working with semiconductors.⁷⁷

As we saw at the end of chapter 3, many surface scientists have been extraordinarily successful at recasting themselves as leaders of nanotechnology.

This use of nanotechnology to revive perceived declines in the disciplines wasn't unique to surface science. As Peter Galison has shown for physics, for instance, the end of the Cold War brought on an existential crisis to which nanotechnology was one solution.⁷⁸ During the Cold War, many physicists thought of the sciences as a strict hierarchy, with high-energy research plumbing nature's most fundamental characteristics and therefore deserving of more prestige than applied areas such as condensed-matter physics (and with non-physics specialties even lower in the hierarchy).⁷⁹ But in the early 1990s, with the removal of Cold War's justifications for basic research funding (and with the onset of an economic recession), high-energy

physicists lost their place in that hierarchy. The turning point was Congress' denial of funding for the most high-profile high-energy project of the 1990s, the Superconducting Supercollider, in 1993.⁸⁰

In the next ten years, the physicists' hierarchy disintegrated. Applied areas such as semiconductor materials science and molecular biology now came to be seen as the major drivers of national high-tech competitiveness. In the United States, budgets for the National Institutes of Health doubled in the 1990s, and talk of aiding the American semiconductor industry became the main justification in winning the Clinton administration's support for a National Nanotechnology Initiative.⁸¹ Galison shows that a large segment of the physics community responded by fleeing formerly prestigious subfields and forging alliances with these newly valued disciplines. It now became common for physicists to collaborate with biologists, electrical engineers, and materials scientists. Many of these collaborations fell under the umbrella of nanotechnology, and physicists became some of the most outspoken proponents of new nanotechnology institutions that would foster such interdisciplinary ventures.⁸²

The end of the Cold War also precipitated an existential crisis for some of the organizations that had dominated early probe microscopy, thereby opening a space for other organizations to use the "nano" label in order to take their place. As we have seen, corporate labs, starting in the late 1980s and increasingly in the 1990s, trimmed their budgets, outsourced more work to universities, and focused attention much closer on product development. IBM cut back so heavily in the early 1990s that some of its researchers reportedly had to work part-time for IBM's sales force. The greatest corporate lab of them all, Bell Labs, was by the turn of the millennium only a shell of its former self. Some policy makers explicitly saw nanotechnology as a way to make up for the loss of so much corporate research infrastructure. Sometimes this substitution was direct—in Switzerland, for instance, the government bought up RCA's former laboratory in Zurich and merged it with a nanotechnology group founded in 1993 at the federally supported Paul Scherrer Institute.⁸³ Usually, though, governments created new academic nanotechnology institutions to occupy the niche once held by the big corporate labs. As Jim Murday puts it,

Bell Labs is just a shadow of what it once was, and IBM has had to scale back much of its operation as well. So they are not the dominant force they used to be globally across surface science or nano. . . . If they go away, we still have very good people,

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 good equipment. An industrial lab had stuff universities would drool over. . . . If it looks like [the decline of corporate research] is slowing the rate of progress, then maybe at the federal level we need to think a little differently about how we fund work. In the [National Nanotechnology] Initiative you already see some evidence of that awareness [in the effort] to go after and create centers. That's in some sense what 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.⁸⁴

As Murday's final point indicates, nanotechnology's institutionalization has largely taken place through the founding of dozens if not hundreds of academic nanotechnology centers, institutes, consortia, and laboratories.

Note, too, Murday's explanation of one of the primary features of a corporate lab that an academic nanotechnology center can mimic: the provision of expensive equipment to its members. Yet because a government-funded academic center of today is far more fiscally constrained than Bell Labs in the early 1980s, that provisioning of equipment has to be spread across a much larger constituency. In order to make up costs, many academic nanotechnology centers allow—really, require—researchers to pool their resources and buy instruments that they can share. Sharing tools not only saves money; it also welds together different disciplines, one of the main objectives of nanotechnology's proponents.

Thus, these centers depend on instrumental communities to contribute technologies to share, and to figure out how to apply them across disciplines. Probe microscopes are by no means the only such shared instrument, yet proponents of academic nanotechnology centers often cite the STM and the AFM as if they were *the* instruments of nanotechnology. Perhaps this is simply because the timing of their invention closely matches the emergence of nanotechnology. Equally important, though, is the fact that probe microscopists moved quickly toward interdisciplinary collaborations of the kind that these centers are supposed to foster. Probe microscopy, as we have seen, has long been the model for how an instrumental community can tie disciplines together under the rubric of nanotechnology.

Consider a typical interdisciplinary collaboration among members of an academic nanotechnology center: a materials scientist fabricates a substrate, characterizes it with an AFM, and takes it to a biochemist, who

deposits a particular antibody or DNA sequence on the substrate, characterizes it again with the AFM, and takes it to an electrical engineer, who deposits interconnects between the features on the substrate, characterizes it again with the AFM, and takes it to Such a collaboration requires contributions from disciplines, organizations, *and* instrumental communities: an organization (nanotechnology center) to house members and promote their interaction, disciplines to give those members the tools to prepare specimens and interpret/report knowledge generated about them, and instruments to generate that knowledge.

These are exactly the type of interdisciplinary interactions envisioned by most promoters of nanotechnology. These interactions could be, and are, made through tools other than SPMs—probe microscopy is only one of the instrumental communities knitting together the constituent disciplines of nanotechnology. Yet for all the interdisciplinary ligation provided by tools like the AFM, *disciplinary* forms, questions, values, and career tracks were indispensable to the development of those tools. We have seen how surface science provided the threshold-clearing puzzle (the atomic structure of the silicon 7×7 surface reconstruction) that focused widespread attention on the STM. We have seen, too, how the Zurich-California groups that committed to variation and selection of microscope design depended on various disciplinary canons when considering which variations they should pursue and select.

That is, the disciplines provided some of the certainty-in-the-moment needed to guide experimental action. They were not the only guiding framework—as we have seen, a more charismatic, personalized, *ad hoc* frame was sometimes employed. Either way, without some degree of certainty-in-the-moment, probe microscopy couldn't have transformed from an instrument into an instrumentality. After all, an AFM on a factory floor or in a quality-control lab must answer, with dependable consistency, simple, straightforward questions: What is the surface roughness of this wafer? Did this new process make grain sizes larger or smaller? And so on.

Yet the probe-microscopy community's success has also derived from its members' ability to recognize and exploit *uncertainty-in-the-moment*. Probe microscopists have made great gains by knowing that a paper that is "wrong" can still be a valuable contribution, and by being able to be surprised by an instrument's capabilities or potential. But making something from such surprises wasn't a private endeavor. Rather, it was made possible

by the existence of a network of competitors and collaborators with whom to share and build on those surprises—an instrumental community.

The dynamic tensions of certainty and uncertainty, and the amortizing of uncertainty across a large and differentiated network, go a long way to explaining probe microscopy's instrumental contribution to nanotechnology. Surprises about what a probe microscope could do were steadily transformed into connections to new disciplines and industries that could make use of those surprising capabilities. Anxieties about what a probe-microscopy image meant were skillfully ameliorated by consigning interpretation and elaboration of those images to communities of disciplined experts. The task of creating certain knowledge about and from probe microscopes pulled disciplines and industries into contact with this instrumental community one by one. Yet the residual uncertainty about how a microscope should be designed and used left open the possibility for new disciplines and industries to be drawn in, and thereby to create new connections to the disciplines and industries already employing SPMs.

Thus, no discipline, industry, or organization remained intact upon encountering probe microscopy. As new members joined the probe-microscopy community, the new connections they could draw changed what it meant to be a surface scientist, an IBMer, an engineer at a data storage company, a member of the American Vacuum Society, or a DARPA grant officer. Along the way, as we have seen, probe microscopy too changed dramatically. Some of the changes were intended, some weren't; some were predictable, some weren't. For 25 years or more, nanotechnology has functioned as a rubric for containing, and also for exploiting, the dynamism within, and engendered by, the probe-microscopy community. Nanotechnology offers a bureaucratic form, promotional labels, and visibility to potential collaborators for probe microscopy and the other constantly evolving networks it contains. In return, by constantly (if unpredictably) evolving and interlinking, those networks have given nanotechnology its claim to instrumentality.

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Appendix A: Abbreviations

AES	Auger electron spectroscopy
AFM	atomic force microscope (or microscopy)
APS	American Physical Society
ASU	Arizona State University
AT&T	American Telephone and Telegraph
AVS	American Vacuum Society
BEEM	ballistic electron emission microscope (or microscopy)
CNSI	California Nanosystems Institute
CNST	Center for Nanoscale Science and Technology (Rice University)
CSS	Central Scientific Services (IBM)
DARPA	Defense Advanced Research Projects Agency (US)
DI	Digital Instruments
ESCA	Electron Spectroscopy for Chemical Analysis (a.k.a XPS)
ETH	Eidgenössische Technische Hochschule (Swiss Federal Institute of Technology)
FEM	field emission microscope (or microscopy)
FIM	field ion microscope (or microscopy)
HGP	Human Genome Project (US)
HOPG	Highly oriented pyrolytic graphite
IBM	International Business Machines
IETS	inelastic electron tunneling spectroscopy
JROC	Joint Requirements Oversight Council (US)
JVST	Journal of Vacuum Science and Technology
LEED	low-energy electron diffraction
LFM	lateral force microscope (or microscopy)
MAPS	Microstructure and Atomistic Processes on Surfaces (US)
MFM	magnetic force microscope (or microscopy)

MI	Molecular Imaging
MINAST	Micro- and Nanosystems Technologies program (Switzerland)
MSI	Master's in Scientific Instrumentation
MTS	Member of the Technical Staff (at Bell Labs)
NBS	National Bureau of Standards (US)
NCCR	National Center of Competence in Research (Switzerland)
NDT	non-destructive testing
NIH	National Institutes of Health (US)
NIST	National Institute of Standards and Technology (US)
NNI	National Nanotechnology Initiative (US)
NRL	Naval Research Laboratory (US)
NSOM	near-field scanning optical microscope (or microscopy; SNOM in Europe)
ONR	Office of Naval Research (US)
PCR	polymerase chain reaction
PIN	Properties of Interfacial Nanostructures (US)
PSI	Park Scientific Instruments
RHEED	reflection high-energy electron diffraction
SBIR	small-business innovation research
SCM	scanning capacitance microscope (or microscopy)
SecM	scanning electrochemical microscope (or microscopy)
SICM	scanning ion conductance microscope (or microscopy)
SKM	scanning Kelvin probe microscope (or microscopy)
SNOM	scanning near-field optical microscope (or microscopy) (NSOM in US)
SPM	scanning probe microscope (or microscopy)
SThM	scanning thermal microscope (or microscopy)
STM	scanning tunneling microscope (or microscopy)
STS	Science and Technology Studies (a.k.a. Science, Technology and Society)
STS	scanning tunneling spectroscopy
TEM	transmission electron microscope (or microscopy)
TunA	tunneling AFM
UCSB	University of California, Santa Barbara
UHV	ultra-high vacuum
UPS	ultraviolet photoelectron spectroscopy
UT	University of Texas
UVA	University of Virginia
XPS	X-ray photoelectron spectroscopy(a.k.a. ESCA)

Appendix B: Interviews Conducted by the Author

Lowell Howard, Gaithersburg, Maryland, June 28, 2000
Joe Stroschio, Gaithersburg, Maryland, June 28, 2000
John Villarrubia, Gaithersburg, Maryland, June 28, 2000
Ronald Dixson, Gaithersburg, Maryland, June 29, 2000
Ted Vorburger, Gaithersburg, Maryland, June 29, 2000
Russell Young, Gaithersburg, Maryland, June 29, 2000
John Kopanski, Gaithersburg, Maryland, June 30, 2000
Rick Silver, Gaithersburg, Maryland, June 30, 2000
James Murday, Washington, DC, July 6, 2000
Gerd Binnig, Rüschnikon, Switzerland, September 26, 2000
Fred Leibsle, Kansas City, Missouri, January 1, 2001
Bob Dunn, Lawrence, Kansas, January 2, 2001
Dongming Chen, Cambridge, Massachusetts, February 18, 2001
Nancy Burnham, Worcester, Massachusetts, February 20, 2001
Jene Golovchenko, Cambridge, Massachusetts, February 20, 2001
Sunney Xie, Cambridge, Massachusetts, February 21, 2001
Phaedon Avouris, Yorktown Heights, New York, February 22, 2001
Julian Chen, Yorktown Heights, New York, February 22, 2001
Joe Demuth, Yorktown Heights, New York, February 22, 2001
John Kirtley, Yorktown Heights, New York, February 22, 2001
Norton Lang, Yorktown Heights, New York, February 23, 2001
Rudolph Tromp, Yorktown Heights, New York, February 23, 2001
Kumar Wickramasinghe, Yorktown Heights, New York, February 23, 2001
Dawn Bonnell, Philadelphia, Pennsylvania, February 26, 2001
Walter Smith, Haverford, Pennsylvania, February 26, 2001
Matt Thompson, Chadd's Ford, Pennsylvania, February 26, 2001
Eric Rufe, Chadd's Ford, Pennsylvania, February 26, 2001

Ray Eby, Rosemont, New Jersey, February 28, 2001
Nick Guilbert, Hightstown, New Jersey, February 28, 2001
Don Hamann, Berkeley Heights, New Jersey, February 28, 2001
Joe Griffith, Berkeley Heights, New Jersey, February 28, 2001
Shirley Chiang, Davis, California, March 8, 2001
Mike Crommie, Berkeley, California, March 9, 2001
Arun Majumdar, Berkeley, California, March 9, 2001
Miquel Salmeron, Berkeley, California, March 9, 2001
Gary Aden, Sunnyvale, California, March 12, 2001
Gordon Kino, Palo Alto, California, March 12, 2001
Andreas Berghaus, Sunnyvale, California, March 13, 2001
Chuck Bryson, Sunnyvale, California, March 13, 2001
Bob Jaynes, Sunnyvale, California, March 13, 2001
Steve Minne, Palo Alto, California, March 13, 2001
Kelvin Walsh, Sunnyvale, California, March 13, 2001
Tom Albrecht, phone interview, March 14, 2001
Jane Frommer, San Jose, California, March 14, 2001
Dan Rugar, San Jose, California, March 14, 2001
Jonathon Mamin, San Jose, California, March 15, 2001
Matthew Mate, San Jose, California, March 15, 2001
Bob Wilson, San Jose, California, March 15, 2001
Gary McClelland, San Jose, California, March 16, 2001
Bruce Terris, San Jose, California, March 16, 2001
Helen Hansma, Santa Barbara, California, March 19, 2001
Paul Hansma, Santa Barbara, California, March 19, 2001
Craig Prater, Santa Barbara, California, March 19, 2001
Jason Cleveland, Santa Barbara, California, March 20, 2001
Virgil Elings, Santa Barbara, California, March 20, 2001
Terry Mehr, Santa Barbara, California, March 20, 2001
Joe Zasadzinski, Santa Barbara, California, March 20, 2001
Sergei Magonov, Santa Barbara, California, March 21, 2001
Steve Buratto, Santa Barbara, California, March 22, 2001
Monte Heaton, Santa Barbara, California, March 22, 2001
Dennis Adderton, Santa Barbara, California, March 23, 2001
Ken Babcock, Santa Barbara, California, March 23, 2001
Dan Bocek, Santa Barbara, California, March 23, 2001
Kevin Kjoller, Santa Barbara, California, March 23, 2001

Scot Gould, Claremont, California, March 27, 2001
Ari Requicha, Los Angeles, California, March 27, 2001
John Baldeschwieler, Pasadena, California, March 28, 2001
Nathan Lewis, Pasadena, California, March 28, 2001
Paul West, Irvine, California, March 30, 2001
Kathryn Moler, Palo Alto, California, April 2, 2001
David Braunstein, San Jose, California, April 3, 2001
Tom Kenny, Palo Alto, California, April 3, 2001
Grant Henderson, Toronto, Ontario, April 24, 2001
Cynthia Goh, Toronto, Ontario, April 25, 2001
Lukas Novotny, Ithaca, New York, April 26, 2001
Randy Feenstra, Pittsburgh, Pennsylvania, May 2, 2001
Paul Cutler, State College, Pennsylvania, May 3, 2001
Paul Weiss, State College, Pennsylvania, May 3, 2001
Max Lagally, Madison, Wisconsin, May 7, 2001
Katerina Moloni, Madison, Wisconsin, May 7, 2001
Bob Hamers, Madison, Wisconsin, May 9, 2001
Franz Himpfel, Madison, Wisconsin, May 9, 2001
Mark Hersam, Evanston, Illinois, May 10, 2001
Mike Ward, Minneapolis, Minnesota, May 14, 2001
Dan Frisbie, Minneapolis, Minnesota, May 16, 2001
Wayne Gladfelter, Minneapolis, Minnesota, May 16, 2001
Oden Warren, Minneapolis, Minnesota, May 16, 2001
Robert Wolkow, Ottawa, Ontario, May 22, 2001
Dave Farrell, Rochester, New York, May 29, 2001
Paul Bryant, Kansas City, Kansas, June 20, 2001
Marc Porter, Ames, Iowa, June 21, 2001
Eric Henderson, Ames, Iowa, June 22, 2001
Curtis Mosher, Ames, Iowa, June 22, 2001
Andy Gewirth, Urbana-Champaign, Illinois, June 25, 2001
Joe Lyding, Urbana-Champaign, Illinois, June 25, 2001
TC Chiang, Urbana-Champaign, Illinois, June 26, 2001
Ali Yazdani, Urbana-Champaign, Illinois, June 26, 2001
Bob Jaklevic, Farmington Hills, Michigan, June 27, 2001
Jun Nogami, East Lansing, Michigan, June 28, 2001
Stuart Tessmer, East Lansing, Michigan, June 28, 2001
John Green, Troy, Michigan, June 29, 2001

Mike Pashley, Briarcliff Manor, New York, July 13, 2001
Don Chernoff, Indianapolis, Indiana, September 5, 2001
Don Eigler, San Jose, California, October 11, 2001
Barbara Jones, San Jose, California, October 11, 2001
Vic Kley, Berkeley, California, October 11, 2001
Mike Allen, Alameda, California, October 12, 2001
Mike Kirk, Milpitas, California, October 12, 2001
John Alexander, Milpitas, California, October 15, 2001
Stefan Kaemmer, Milpitas, California, October 15, 2001
Marco Tortonese, Milpitas, California, October 15, 2001
Carlos Bustamante, Berkeley, California, October 17, 2001
Barney Drake, Santa Barbara, California, October 18, 2001
Pete Maivald, Santa Barbara, California, October 18, 2001
James Massie, Santa Barbara, California, October 18, 2001
Jerome Wiedmann, Santa Barbara, California, October 18, 2001
John Foster, Santa Barbara, California, October 19, 2001
Jim Gimzewski, Los Angeles, California, October 22, 2001
George McMurtry, Agoura Hills, California, October 22, 2001
Bill Kaiser, Pasadena, California, October 23, 2001
Frank Ogletree, Berkeley, California, October 24, 2001
Alexis Baratoff, Basel, Switzerland, November 7, 2001
H.-J. Hug, Basel, Switzerland, November 7, 2001
Dieter Pohl, Basel, Switzerland, November 7, 2001
H.-J. Güntherodt, Basel, Switzerland, November 8, 2001
Ernst Meyer, Basel, Switzerland, November 8, 2001
H.-P. Lang, Basel, Switzerland, November 9, 2001
Robert Sum, Liestal, Switzerland, November 9, 2001
Urs Dürig, Rüschtikon, Switzerland, November 12, 2001
Christoph Gerber, Rüschtikon, Switzerland, November 12, 2001
Bruno Michel, Rüschtikon, Switzerland, November 12, 2001
S. F. Alvarado, Rüschtikon, Switzerland, November 13, 2001
Heinrich Rohrer, Rüschtikon, Switzerland, November 13, 2001
Hermann Gaub, Munich, Germany, November 14, 2001
Reinhard Guckenberger, Munich, Germany, November 14, 2001
Wolfgang Heckl, Munich, Germany, November 14, 2001
Khaled Karrai, Munich, Germany, November 14, 2001

Dieter Kolb, Ulm, Germany, November 15, 2001
Klaus Weishaupt, Ulm, Germany, November 15, 2001
Othmar Marti, Ulm, Germany, November 16, 2001
Franz Giessibl, Augsburg, Germany, November 16, 2001
Thomas Berghaus, Taunusstein, Germany, November 19, 2001
Eric Heller, Cambridge, Massachusetts, January 24, 2002
Jan Hoh, Baltimore, Maryland, June 10, 2002
Bob Celotta, Gaithersburg, Maryland, June 11, 2002
John Dagata, Gaithersburg, Maryland, June 11, 2002
Bill Gadzuk, Gaithersburg, Maryland, June 11, 2002
Rich Colton, Washington, DC, June 27, 2002
Lloyd Whitman, Washington, DC, June 27, 2002
Clayton Teague, Gaithersburg, Maryland, June 28, 2002
James Murday, Washington, DC, July 8, 2002
Hollis Wickman, Arlington, Virginia, July 9, 2002
Stuart Lindsay, Tempe, Arizona, January 6, 2003
Ig Tsong, Tempe, Arizona, January 6, 2003
Tianwei Jing, Tempe, Arizona, January 7, 2003
Daphna Yaniv, Tempe, Arizona, January 7, 2003
Vance Nau, Tempe, Arizona, January 8, 2003
Dror Sarid, Tucson, Arizona, January 9, 2003
Brian Swartzentruber, Albuquerque, New Mexico, January 10, 2003
John Kramar, Gaithersburg, Maryland, July 23, 2003
Joel Kubby, Webster, New York, October 23, 2003
Howard Mizes, Webster, New York, October 23, 2003
Charles Duke, Webster, New York, October 30, 2003
Becky Pinto, Milpitas, California, February 3, 2004
Brian Trafas, Milpitas, California, February 3, 2004
Don Tennant, Berkeley Heights, New Jersey, April 29, 2005
Ted Madey, New Brunswick, New Jersey, May 5, 2005
Tom Beebe, Newark, Delaware, May 17, 2005
Hank Wohltjen, phone interview, September 13, 2005
Hank Smith, Cambridge, Massachusetts, October 25, 2005
William Tolles, Alexandria, Virginia, December 2, 2005
Aaron Fein, Gaithersburg, Maryland, December 3, 2005
Stan Williams, Palo Alto, California, March 14, 2006

Bob Buhrman, Ithaca, New York, April 24, 2006
Paul Hansma, Santa Barbara, California, May 2, 2006
Allan Melmed, Terra Alta, West Virginia, August 3, 2006
Paul Hansma, Santa Barbara, California, August 7, 2006
James Murday, Washington, DC, May 29, 2007
John Foster, Santa Barbara, California, May 15, 2009
Alan Kleinsasser, Pasadena, California, May 3, 2010

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Notes

Chapter 1

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1. See Markoff 2000.
2. Interview with Tom Kalil conducted by Patrick McCray, Berkeley, June 12, 2006; interview with James Murday conducted by C. C. M. Mody, Washington, DC, May 29, 2007. (In subsequent notes, unless another interviewer is named, the interviewer is C. C. M. Mody and the details of the interview are listed in appendix B.)
3. See Feynman 1999. For an analysis of the uses and abuses of Feynman's speech, see Toumey 2005.
4. See Etzkowitz 2008.
5. That is, a map where New York and Los Angeles are about 3 feet apart, and where an inch on the map represents about 70 miles in the real world.
6. For the original published version of this image, see Mamin et al. 1991.
7. For instance, Regis (1995, pp. 197–198) asserts that the STM's ability to image and manipulate atoms confirmed "Feynman's 'great future'" for nanotechnology predicted in the "Plenty of Room at the Bottom" speech. In the Science section of the *New York Times*, Andrew Pollack (1991), quoting several STMers, claimed that "perhaps the most exciting developments [in nanotechnology] relate to scanning probe microscopes, the best known of which are the scanning tunneling microscope . . . [which] has allowed scientists to see individual atoms, a prerequisite if they are going to attempt to build molecular structures."
8. See, for instance, Committee for the Review of the National Nanotechnology Initiative 2002, which features an STM image on the cover and begins its text with the statement that nanotechnology "is the result of many developments in the last two decades of the 20th century, including inventions of scientific instruments like the scanning tunneling microscope."

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9. See Hacking 1983. Even some philosophers who are relatively uninterested in nanotechnology, including Hacking (1992) and Barad (1999), have made extensive use of the STM.

10. See, e.g., Pitt 2006, Ruivenkamp and Rip 2010, or Toumey 2009.

11. The quotations are from Darby and Zucker 2003 and from Rothaermel and Thursby 2007.

12. The quotation is from Jansen, Görtz, and Heidler 2009.

13. This estimate and the following estimate of economic value of nano products are from President's Council of Advisers on Science and Technology 2010.

14. Among the works that established the basis for this consensus were Layton 1971, Kline 1992, Vincenti 1990, Hacking 1983, Galison 1987, and Latour 1987. ebruary

15. The quotation is from p. 396 of Galison 1997. Other historical studies of Big Science include Galison and Hevly 1992, McCray 2004, and Pickering 1984. For an interesting attempt to contrast big and small science styles, see Knorr-Cetina 1999. For an exhaustive look at the evolution of an instrument (gravitational radiation flux detectors) from small to big science, see Collins 2004.

16. See Rhodes 1986, 1995. Galison and Bernstein (1989) show that some physicists who joined the hydrogen bomb project were attracted by its use as a new instrument (for studying fusion reactions) rather than any commitment to it as an instrumentality (i.e., as a thermonuclear weapon).

17. See McCray 2005 and forthcoming.

18. See, e.g., Kaiser 2005; Kohler 1994; Creager 2002; Rader 2004.

19. I am especially indebted to those who have recently published work on the history of microelectronics, including Holbrook et al. (2000), Lécuyer (2006), Lécuyer and Brock (2006), Choi (2007), Leslie (2001), Riordan and Hoddeson (1997), Saxe-nian (1994), and Kenney (2000). ebruary

20. Such studies include Elzen 1986, Reinhardt 2006, Grayson 2002, Jordan and Lynch 1992, Kunkle 1995, Lenoir and Lécuyer 1997, Pantalony 2009, Strick 1998, Bromberg 1991, Rasmussen 1997, Blume 1992, and Johnston 2006.

21. Something similar occurred with the invention of the polymerase chain reaction at the Cetus Corporation (Rabinow 1996).

22. See, e.g., Etkowitz 1994; Henderson, Jaffe, and Trajtenberg 1998; Mowery, Nelson, and Sampat 2004; Nelson 2005; Kenney and Patton 2009; Hughes 2001; Colyvas 2007; Yi 2008; Murray 2004; Newfield 2003; Cortright and Mayer 2002; Slaughter and Leslie 1997; Geiger and Sá 2008.

23. I thank an anonymous reviewer of a draft of this book for pushing me to define instrumental community more explicitly, and to do so in network terms.

24. See Joerges and Shinn 2001. Reinhardt and Steinhauser (2008) extend Joerges and Shinn's framework to include a wider variety of actors. Their concept of a "scientific-technological community" closely parallels my "instrumental community."

25. See Shah 2005. In the present book, I draw heavily on works in the "user innovation" subfield in economics and management, such as von Hippel 2005, Shah and Tripsas 2007, and Lüthje, Herstatt, and von Hippel 2005. Some of these authors focus specifically on scientific instruments—see Riggs and von Hippel 1994. One of my aims in writing this book is to add to the growing nexus between user innovation studies and the literature on users in the social construction of technology (SCOT) field. For the latter, see Bijker 1995, Kline and Pinch 1996, Oudshoorn and Pinch 2003, Haring 2007, and Rosen 1993.

26. Beaulieu 2010.

27. Gieryn 1999.

28. Though this book is agnostic about how to include non-human actants in an analysis of such networks, my thinking about the networks through which STM and AFM traveled owes a great debt to Latour 1987 and to other works in the tradition of actor-network theory. Readers who are so inclined should be able to read what follows as though probe microscopes were actants moving, and being moved, through ever-evolving networks, usually with the aid of human actors who claimed to speak on behalf of the microscopes.

29. Aker 2007; Rosenberg 1997.

30. Some New Institutionalists have extensively discussed science, technology, and network forms very similar to my "instrumental community." See, e.g., Owen-Smith and Powell 2004; Liebeskind et al. 1996; Wry et al. 2010. Historians, and even sociologists, of science and technology, however, have not paid much attention to the New Institutionalists' ideas. This may be beginning to change, though—see Kaplan and Radin, forthcoming.

31. See DiMaggio and Powell 1983 and DiMaggio 1991.

32. See Abbott 1988 and Perrow 1986.

33. Interview with James Gimzewski. For more on Gimzewski's artwork, see Roosth 2009.

34. This book is particularly indebted to the sociology of scientific knowledge strand of STS. See Bloor 1976; Barnes, Bloor, and Henry 1996; Collins 1985; Pinch 1986.

35. For influential examples of such studies, see Shapin and Schaffer 1985; Shapin 1994; Dear 1995.

36. Donald Mackenzie's interview-based studies (1990, 1996, 2006) provided an excellent model.
37. In STS this is sometimes called the "lab studies" genre. See Latour and Woolgar 1986; Traweek 1988; Knorr-Cetina 1981; Lynch 1985; Gusterson 1996.
38. Collins (2004) offers a model of how to combine laboratory ethnography and historical interviews with scientists.
39. Baird and Shew (2004) offer some interesting comments from one such "ghost member" of the probe-microscopy community.
40. As will be especially evident in chapters 4 and 5, my discussion of trust owes much to the work of Steve Shapin (1994, 2001, 2008).

Chapter 2

1. For further details on the twentieth-century evolution in vacuum technology and the early history of surface science, see Madey and Brown 1984.
2. For histories of pre-surface-science "tubes," see Brock 2008; Reich 1985; Wise 1985; Lécuyer 2006, chapter 1.
3. See Duke 1984.
4. Source: interview with Charles Duke.
5. See Cochrane 1976; Passaglia 1999; Schooley 2000.
6. The background for this story comes from my interview with Russell Young. See also Villarrubia 2001.
7. Melmed 1996.
8. See Rasmussen 1997, pp. 30–46 and 60–66.
9. Source: videotape (published by American Vacuum Society) of presentation by Ted Madey and Bruce Kendall at special session on NBS/NIST Centennial. I found the tape in the AIP's Niels Bohr Library and Archives.
10. National Academies of Science/National Research Council advisory panel to the NBS, summary of October 28–29, 1968 meeting, National Bureau of Standards collection, National Archives and Records Administration, Accession 167–71a-6103, Box 15.
11. Letter from Karl Kessler [chief of the Optical Physics Division, Simpson's direct boss] to Ernie Ambler [Director of the Institute for Basic Standards, Kessler's direct boss], June 26, 1970, Optical Physics Division 1970 folder, National Bureau of Standards collection, National Archives and Records Administration, Accession 167-74-0039, Box 1 (Ambler 1970 Correspondence).

12. See Mody 2005.
13. Electrical current is just a measure of how many electrons are moving through a region per unit time.
14. See Young 1966.
15. Articles targeted at precision engineers include Young and Scire 1972 and Anonymous 1967. Conference talks include Young 1976 (an abstract of a talk at the annual meeting of the American Society of Lubrication Engineers). Though this talk was given 5 years after the Topografiner project was canceled, Young still announced that "in this talk, some high resolution surface topography techniques such as interference microscopy and the NBS Topografiner will be described."
16. Letter from US Patent and Trademark Office, Examiner B. Anderson, to IBM Patent Dept., November 9, 1981 (copy provided to the author by Russell Young). The patent referenced is Scanning Tunneling Microscope, 4,343,993, filed September 12, 1980, granted August 10, 1982. See also Gadzuk 1987.
17. See Wisnioski 2003; Vettel 2006; Leslie 1993, chapter 9.
18. Ernest Ambler oral history interview, conducted by Karma Beal and Frederick Fellows, November 7, 1986, NIST Archives, Information Services Division, National Institute of Standards and Technology.
19. Ernest Ambler oral history interview, conducted by Karma Beal, July 27, 1988, NIST Archives.
20. John A. Simpson, memo to R. D. Young, "Status of Topografiner Program," July 28, 1971, copy given to author by Russell Young.
21. Kolata 1977.
22. Russell Young, Ultra μ [micrometer] notebook 2, entry for July 17, 1969, copy given to author by R. Young.
23. Russell Young, notebook 6 [i.e., the sixth ultramicrometer notebook], entry for May 20, 1971, copy given to author by Russell Young.
24. Ibid.
25. Taniguchi 1974.
26. Ibid.
27. Ibid.
28. Source: Young 1971. Young (1980) later invoked the mantra of "small is beautiful" as a way to encourage precision engineers to take miniaturization more seriously.
29. Ibid.

30. Letter from John A. Simpson to unknown, April 15, 1986, copy of letter forwarded from Bill Gadzuk to Barbara Hope Cooper on September 15, 1994, in Barbara Hope Cooper papers, 1969–1999, collection 14-22-3072, Division of Rare and Manuscript Collections, Cornell University Library, Box 1, Folder 7.

31. Young, Ward, and Scire 1972.

32. Simpson, memo to Young (see note 20 above).

33. Young, Ward, and Scire 1971.

34. Memo, Simpson to unknown (see note 30 above).

35. Letter from Ernest Ambler to Chairman and members of Atomic and Molecular Physics Panel, re: Forthcoming meeting of December 14–15, 1970, dated November 16, 1970, Dr. Ambler's office 1970 folder, National Bureau of Standards collection, National Archives and Records Administration, Accession 167-74-0039, Box 1 (Ambler 1970 Correspondence).

36. Simpson, memo to Young (see note 20 above).

37. Interview with Robert Jaklevic.

38. See Teague 1978.

39. Teague 1986.

40. Consider how counterintuitive the Josephson Effect is—it's as if the lights in your home are on, even though no electricity is flowing into your house (i.e., the power lines are down, but electrons are still moving in the wires in your home).

41. For information on IBM's foray into molecular electronics (another radical alternative to silicon), see Choi and Mody 2009.

42. Keyes 1969.

43. Robinson 1983.

44. The estimate of the number of personnel is from van Duzer 2008. For a contemporary evaluation by NSA program officers, see Welker and Bedard 1978.

45. The quotation is from Fisher 1989. The general story line of the rest of the chapter is summarized in Binnig and Rohrer 1987.

46. Russell Young appears to have anticipated something like this—there are several entries in his notebooks suggesting that the Topografiner might eventually be used to measure the thickness of oxide films.

47. Fisher 1989.

48. See, e.g., Gadzuk 1987.

49. Loge 1998.

50. Robinson 1982. This section is also informed by my interview with Alan Kleinsasser.
51. Kehoe 1983.
52. Binnig and Rohrer 1999.
53. Speiser 1998.
54. Interview with Othmar Marti.
55. Christoph Gerber, personal communication.
56. Interview with Don Hamann.
57. Single atomic steps of the CaIrSn_4 sample were being imaged by June of 1981 (Rohrer 1990).
58. Russell Young briefly considered this possibility in his notebooks though he never envisioned that atomic resolution could be achieved.
59. Casually referred to as the "silicon seven-by-seven." For more on the history of the 7×7 , see Mody and Lynch 2010.
60. Duke 1984.
61. Because of the acronyms for all the instruments: XPS, AES, LEED, RHEED, UPS, and so on.
62. Duke 1984.
63. Duke interview.
64. Interview with R. Stanley Williams by C. C. M. Mody, Palo Alto, March 14, 2006 (available in Chemical Heritage Foundation oral history collection); Stanley Williams, personal communication.
65. Interview with Rudolph Tromp.
66. Binnig et al. 1982.
67. Hamann interview.
68. Binnig et al. (1983) thank Himpfel for "hints concerning sample preparation"; they also cite "private communication" with him in describing how they prepared the 7×7 .
69. Gerber, personal communication.
70. Edwards 2006, p. 57.
71. On electron microscopy and atomic resolution, see Isaacson et al. 1977.
72. Binnig et al. 1982.

73. Takayanagi et al. 1985. For a review of resolution of the 7x7 debate, see Duke 1996.

74. Jun Nogami, personal communication.

Chapter 3

1. Murday interview, 2007.

2. Binnig and Rohrer 1986.

3. See especially Collins 1985, Kaiser 2005, and chapter 6 of Shapin and Schaffer 1985.

4. How antitrust worries affected corporate research is a recurring theme in histories of Cold War science. For an overview, see Mirowski and Sent 2008. For some specific examples in the semiconductor industry, see Choi 2007 and Misa 1985.

5. Sweet 1991.

6. Lubkin 1984.

7. Sobel 1981, p. 335.

8. Sweet 1991.

9. For an overview of the rise and fall of basic research as a privileged category in American corporate labs, see Asner 2006.

10. Patel 1984.

11. Theberge 1978. The uncertainty of research is the crux of Shapin 2008.

12. Theberge 1978.

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13. Vesio 1982.

14. Ibid.

15. Ibid.

16. Logue 1998.

17. Arno Penzias, quoted in Sweet 1991.

18. Hamann interview.

19. The Tersoff-Hamann approximation says that, for many simple, well-defined metal and semiconductor surfaces, the STM image approximates to the "density of states"—i.e., each point in the STM image measures how closely packed the energy levels of bound electrons are at that point.

20. Interview with Charles Duke.

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21. The quotations are from McRae and Caldwell 1981 and Bennett et al. 1983, respectively.
22. Interview with Jene Golovchenko.
23. Ibid.
24. Ibid.
25. Ibid.
26. John R. Arthur, oral history interviews by David C. Brock, February 22, March 8, March 30, and April 28, 2010, oral history collection, Chemical Heritage Foundation, Philadelphia.
27. Golovchenko interview.
28. Interview with Randy Feenstra.
29. Ibid.
30. Interview with Jim Murday, July 6, 2000.
31. Feenstra interview.
32. Interview with Shirley Chiang.
33. Binnig and Rohrer seem to have applied a slightly lower standard for replication. In their 1984 article they cite several groups as having already achieved "successful operation of STM." Some of the groups they cite (e.g., Pashley, Pethica, and Coombs 1985) had generated STM images "showing features of atomic size" by that point, but not resolution of individual atoms in a surface reconstruction.
34. Interview with Joe Griffith.
35. Interview with Jonathon Mamin.
36. Feenstra interview; Golovchenko interview; interview with Paul Hansma conducted by the author, Santa Barbara, California, May 2, 2006 (available in Chemical Heritage Foundation oral history collection).
37. Interview with Bob Hamers.
38. Interview with Rudolph Tromp.
39. Interview with Brian Swartzentruber.
40. Interview with John Kirtley.
41. Tromp interview.
42. Griffith interview.
43. Interview with Mark Welland conducted by Patrick McCray, Cambridge, UK, August 6, 2009.

44. Interview with R. Stanley Williams.
45. Ibid.
46. Griffith interview. The review is Griffith and Kochanski 1990.
47. Tromp interview; Williams interview; S. Chiang interview. The two articles are Wilson and Chiang 1987 and van Loenen et al. 1987.
48. Interview with Dawn Bonnell.
49. Interview with John Villarrubia.
50. Golovchenko interview. See also the faux-LEED image in Demuth, Koehler, and Hamers 1988.
51. Interview with Craig Prater.
52. Griffith interview.
53. Hamers interview.
54. See the cover of *Physics Today* for January 1987. The image was also reported (this time with gallium in green and arsenic in red) in Feenstra et al. 1987.
55. Hamers interview.
56. See Regan 1989 and Anderson and Yuce 1990.
57. Sweet 1991.
58. Griffith interview.
59. Sweet 1993.
60. This is the argument of Bassett (2002).
61. Griffith interview.
62. Patel 1984.
63. Swartzentruber interview; Bonnell interview; interview with Paul Weiss.
64. See Lagally 2003.
65. Williams interview.
66. Ibid.

Chapter 4

1. See Baro et al. 1985.
2. Binnig 1992.

3. Marti interview and personal communication.
4. Jaklevic and Kaiser also had the support of Ford's Vice President of Research, Dale Compton, a physicist. Much like IBM and Bell Labs, Ford was then strongly committed to basic research. As at many other companies, that commitment waned in the 1990s. In Jaklevic's view, support for a project like his and Kaiser's STM would be very scarce in today's corporate research environment (sources: Jaklevic interview and personal communication).
5. Christoph Gerber, personal communication.
6. Hansma 1982. In traditional IETS, molecules are adsorbed onto the insulating layer between the two electrodes of a sandwich tunnel junction. As electrons tunnel from one electrode to the other, they interact with, and reveal spectroscopic information about, the adsorbed molecules.
7. P. Hansma interview, May 2, 2006.
8. Quate 1986. The original article Quate read was Schwarzschild 1982.
9. Edward Ginzton, a professor of electrical engineering who later would become chairman of Varian Associates, was one of Quate's mentors. Ginzton and Frederick Terman, Stanford University's longtime provost, skillfully cultivated ties between Stanford and Bay Area microwave companies in the 1940s and the 1950s. See Leslie and Kargon 1996 and Lowen 1997.
10. H. Smith interview.
11. Mody 2011.
12. Interview with John Foster, May 15, 2009.
13. Laboratory notebook, April 1983, page 20, Calvin Quate papers, Stanford University Archives, collection SC 347 (04-117), 2004 accession, Box 2.
14. Interview with Franz Giessibl; letter from Marti to Quate, August 11, 1992, Calvin Quate papers, Stanford University Archives, collection SC 347 (04-117), 2004 accession, Box 1, Folder 6.
15. Mamin interview; personal communication.
16. E.g., Mamin et al. 1986 and Tomanek et al. 1987.
17. Interview with John Baldeschwieler conducted by David Brock and Arthur Daemmrich, June 13, 2003, Philadelphia (accessible in Chemical Heritage Foundation's oral history collection).
18. Gerber et al. 1986.
19. S. Chiang interview; Gimzewski interview; Gerber, personal communication.
20. Interview with Jun Nogami; Mamin interview.

21. Interview with Miquel Salmeron. Salmeron, in a personal communication, notes, however, that there were a few exceptions to this rule, particularly after Quate, Hansma, and Binnig forged ahead with air STM, and even more so after air STMs were commercialized. See, e.g., Hess et al. 1989 or Hawley et al. 1991.

22. P. Hansma interview, May 2, 2006.

23. Ibid.

24. Weiss interview; Paul Weiss, personal communication.

25. Salmeron interview.

26. Interview with Helen Hansma; P. Hansma interview, August 7, 2006.

27. Interview with Stuart Lindsay.

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28. Marti interview.

29. Quate 1994.

30. Interview with Mike Kirk. The transition in gravitational radiation physics from a small science to a big science is wonderfully and exhaustively described in Collins 2004.

31. Coombs and Pethica 1986.

32. Pethica's presentation at the Oberlech conference is usually mentioned by the AFM's inventors as the inspiration for force microscopy (Binnig, Quate, and Gerber 1986; Quate 1994; Gerber, personal communication). In the original AFM article, however, Teague's thesis is cited alongside Pethica's, as are other articles by Teague, Richard Deslattes, and other Bureau personnel. From its citation history, it is clear that Teague's dissertation (much like Othmar Marti's thesis) circulated among early STM groups at Stanford, UC Santa Barbara, and Penn State well before the invention of the AFM.

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33. Laboratory notebook, April 1983, page 17, Calvin Quate papers, Stanford University Archives, collection SC 347 (04-117), 2004 accession, Box 2.

34. Interview with Paul Hansma, August 7, 2006; interview with Scot Gould.

35. Gould interview.

36. P. Hansma interview, August 7, 2006; Gould interview; interview with Barney Drake.

37. Anyone who has seen a bright, dancing spot on a wall caused by sunlight bouncing off a wristwatch is familiar with an optical lever. A very slight flick of the wrist sends the spot flying several feet away on the wall—hence the analogy to a mechanical lever, where a force applied over a small distance on one side of the fulcrum yields a movement over a long distance on the other side.

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38. Meyer and Amer 1988. Hansma's first published announcement of the optical lever came a few months later in Alexander et al. 1989. It should be noted that Meyer and Amer were awarded the patent for this concept: US patent 5,144,833, for "atomic force microscopy."
39. See Quate 1994.
40. P. Hanmsa interview, August 7, 2006; Drake interview; Gould interview.
41. Scot Gould, personal communication.
42. P. Hanmsa interview, August 7, 2006; Gould interview; Nogami interview.
43. H. Smith interview.
44. Quate 1994.
45. Piezoresistive cantilevers have generally turned out to be less sensitive than the optical lever. Though widely used in some applications, they have not supplanted laser detection.
46. Prater, personal communication.
47. Quate 1994. Of course, Albrecht contributed much more to AFM than just the microfabricated cantilever, including the first AFM images of the atomic structure of boron nitride and construction of the prototype for one of the first commercial AFMs (sold by Park Scientific Instruments).
48. In Albrecht et al. 1990, one of the Quate group's first publications on microfabricated cantilevers, the group thanks "P. Hansma and co-workers for evaluating cantilevers and suggesting improvements."
49. Kirk interview.
50. Vincenti 1990.
51. Villarrubia interview.
52. P. Hanmsa interview, August 7, 2006; Drake interview; Gould interview.
53. Nogami interview.
54. Interview with Andy Gewirth.
55. Nogami interview.
56. Granovetter 1973.
57. Barabási and Albert 1999.
58. The emphasis on epistemic and normative uncertainties in this section is inspired by Shapin 2008.
59. Drake interview.

60. Nogami interview; interview with Christoph Gerber; interview with John Foster, October 19, 2001.
61. Interview with Paul Hansma, March 19, 2001.
62. P. Hansma interview, May 2, 2006; Shapin 2001.
63. Gould interview.
64. Drake interview.
65. Nogami interview.
66. Smith, Kirk, and Quate 1987.
67. Hamers 1989.
68. Hamers 1996.
69. Katayama et al. 1991. See also Wan, Lin, and Nogami 1992.
70. Williams interview; Nogami interview.
71. Kirk 1989.
72. Interview with Jan Hoh; H. Hansma interview; Gould interview.
73. Hoh interview.
74. P. Hansma interview, 2001.
75. Gewirth interview. Acknowledgments to Moore for providing HOPG samples can be found in many STM and AFM publications from this era. Early examples include Binnig et al. 1987, Gewirth and Bard 1988, and Mate et al. 1989.
76. Interview with Howard Mizes; Murday interview, 2000. See also Albrecht et al. 1988a.
77. Hamers interview.
78. Gimzewski interview.
79. Kevles 1997.
80. P. Hansma interview, August 7, 2006.
81. Driscoll, Youngquist, and Baldeschwieler 1990.
82. Stuart Lindsay, "AFM and STM in Novel Approaches to Sequencing," NIH Grant No. 1R21HG000818-01A1 (1993), granted September 1, 1993; Thomas Beebe, "Feasibility Studies for STM/AFM-Based DNA Sequencing," NIH Grant No. 1R21HG000613-01 (1992), granted June 1, 1992.
83. Beebe et al. 1989; Lindsay et al. 1989.

84. Binnig and Rohrer 1984. See also Travaglini et al. 1987.
85. Lindsay interview.
86. Interview with Tom Beebe.
87. Clemmer and Beebe 1991; Heckl and Binnig 1992.
88. P. Hansma interview, 2001; Beebe interview.
89. Guckenberger et al. 1994; Kanno et al. 1999.
90. Lindsay interview.

Chapter 5

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1. Geiger and Sá (2008) nicely summarize the long history of commercial justifications for American universities. For further citations, see note 22 to chapter 1.
2. Mowery, Nelson, and Sampat 2004.
3. Mazzola 2003; Smaglik 2002; Uldrich 2006.
4. Purvis 2004.
5. Marti interview; *Physics Today* Buyers' Guide, August 1988, p. 101.
6. From Calvin Quate papers, Stanford University Archives, collection SC 347 83-033 (1987 accession), Box 1: letter from Quate to John Dimmock, Director, Electronic and Solid-State Sciences Program, ONR, February 9, 1977; Addenda to Renewal Proposal for the Joint Services Electronics Program at Stanford University Ginzton Laboratory—Research Unit 7, C.F. Quate, January 13, 1977—both in Folder Contract N000014-75-C-0632 ONR (JSEP) correspondence 1974–77; “Improved Resolution in the Acoustic Microscope, Final Report for the National Science Foundation, covering the period March 1 1975—February 28 1977,” GL [Ginzton Lab] Report No. 2697 April 1977, PI CF Quate, in Folder Contract ENG75-02028 NSF
7. Letter from Gordon Kino and Calvin Quate to Dr. James McGroddy, Director of Organizational Planning, IBM, August 9, 1983, in Calvin Quate papers, Stanford University Archives, collection SC 347 83-033 (1987 accession), Box 3, Folder Correspondence 1983 July–December.
8. Final Report for the period 1 July 1978–31 December 1981, “Acoustic Microscopy for Nondestructive Evaluation of Materials,” sponsored by AFOSR and ARPA under contract F49620-78-C-0098, G.L. Report No. 3365, December 1981, by C.F. Quate, in Calvin Quate papers, Stanford University Archives, collection SC 347 83-033 (1987 accession), Box 2, Folder AFOSR Final Report (Dec. 1981).
9. This instrumental motivation should not be seen as mutually exclusive of more altruistic reasons for these gifts. As Stuart Lindsay put it, “Paul Hansma was *incredibly*

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helpful. . . . Paul is an *incredibly nice* guy. . . . He could see that I couldn't bear the thought of having to fly out to Santa Barbara, and *bless him*, Paul offered to give me a microscope." (Lindsay interview)

10. Swartzentruber interview.

11. Kirk interview.

12. Foster interview, 2001.

13. Wandass, Murday, and Colton 1989.

14. Lang et al. 1988.

15. One freelance instrument builder, Bob McAllister, claimed to have been "the first to exhibit a commercially available UHV Scanning Tunneling Microscope in 1986." It is certainly true that McAllister and RHK Technology were advertising a jointly built STM by 1987. See *Journal of Vacuum Science and Technology A* 5 (1987), no. 9: xxxi. McAllister's STMs were custom instruments, built in very small numbers, based largely on a design developed at the Lawrence Berkeley Laboratory.

16. P. Hansma interview, May 2, 2006.

17. *Physics Today*, April 1987: 113.

18. *Physics Today*, July 1988: 93.

19. Interview with Virgil Elings.

20. *FASEB Journal* 4 (1990), no. 13: 1.

21. Elings' harrowing description of his role in the CEA fire can be found at <http://web.mit.edu/physics/giving/profiles/elings.html>. On the consequences of the CEA explosion for high-energy physics, see pp. 353–362 of Galison 1997.

22. See Elings 1995. See also Nicoli, Barrett, and Elings 1978. Jaccarino was the condensed-matter physicist with whom Rohrer worked during his sabbatical at UC Santa Barbara in the 1970s.

23. Nicoli, Barrett, and Elings 1978.

24. Ibid.

25. Elings 1995.

26. Interview with Dennis Adderton.

27. Elings 1995.

28. Elings interview.

29. Nicoli, Barrett, and Elings 1978.

30. Ibid.

31. Elings 1995.
32. Virgil B. Elings and Caliste J. Landry, "Optical Display Device," US Patent No. 3,647,284 (1970), filed November 30, 1970, granted March 7, 1972.
33. Shapin 1994 and 2008; Thorpe and Shapin 2000.
34. Anonymous 2007a.
35. Ibid.
36. Interview with Jason Cleveland.
37. Interview with Jerome Wiedmann.
38. Ibid.
39. Interview with Ken Babcock.
40. Ibid.
41. Lindsay interview.
42. Hoh interview.
43. P. Hansma interview, August 7, 2006.
44. Prater interview.
45. Ibid.
46. *Physics Today*, June 1988: 112; July 1988: 93; September 1988: 147; January 1989: 115; August 1989: 80; March 1990: 106.
47. Elings interview; interview with Don Chernoff.
48. Elings interview.
49. Hamers interview.
50. Nogami interview.
51. Ibid.
52. Elings interview.
53. Elings, personal communication.
54. Hallmark et al. 1987.
55. Lindsay interview.
56. Elings, personal communication.
57. *Physics Today*, September 1989: 80. The same ad ran on p. 30 of the October issue of *American Laboratory*.

58. *Physics Today*, April 1989: 99.
59. *Physics Today*, November 1989: 126.
60. *Physics Today*, September 1989: 80.
61. *Physics Today*, September 1990: 110.
62. *Physics Today*, January 1990: 102.
63. *Physics Today*, February 1991: 57.
64. P. Hansma interview, 2001.
65. Exclusive license agreement for atomic force microscopes, University of California Agreement Control Number 89-04-0016, July 14, 1989, US Patent and Trade-mark Office file for Patent No. 4935634.
66. Manalis 1998.
67. Hoh interview.
68. Ibid.
69. E.g., *American Laboratory*, April 1991: 29; *Physics Today*, April 1991: 21; *Journal of Vacuum Science and Technology A* 9 (1991), no. 4: x. The new tag line was introduced in an ad in *Journal of Vacuum Science and Technology A* (10, 1992, no. 5: lxxx).
70. The articles corresponding to those covers are Zasadzinski et al. 1988; Hansma et al. 1988; Schardt, Yau, and Rinaldi 1989; Manne et al. 1991; Hawley et al. 1991; and Hansma et al. 1992. Elings was a co-author of four of these articles.
71. *Physics Today*, May 1993: 16.
72. *Physics Today*, August 1993: 8.
73. *Physics Today*, February 1992: 69.
74. Lindsay interview.
75. *Physics Today*, February 1990: 103.
76. Nogami interview.
77. *Physics Today*, February 1991: 51.
78. The website of Sang-il Park's spin-off company, Park Systems, claims that Park Scientific Instruments introduced "the first commercial AFM" in 1989 (source: <http://www.parkafm.com/company/02history.php>). It's possible that PSI had some version of its commercial AFM ready before DI did. However, the earliest advertisement for a Park AFM that I have found is in *Physics Today* (February 1990: 103). That ad is for Park's UHV STM, "with AFM compatibility just on the horizon."
79. Interview with David Braunstein.

80. Saxenian 2000.
81. *Physics Today*, February 1990: 103.
82. Prater interview.
83. Interview with Mike Allen.
84. Friedbacher and Fuchs 1999.
85. Griffith interview.
86. Interview with Kumar Wickramasinghe.
87. Adderton interview; Babcock interview.
88. Dennis Adderton, personal communication.
89. Wickramasinghe, personal communication.
90. Griffith interview.
91. Ibid.
92. Anonymous 1995.
93. Wickramasinghe interview.
94. Chiang, personal communication. See Mate et al. 1987. As Chiang notes today, that work garnered more than three times as many citations as any of the STM articles she published while at IBM.
95. Y. E. Strausser and M. G. Heaton, "IC Failure Analysis and Defect Inspection with Scanning Probe Microscopy," Digital Instruments application note, 1997; F. M. Serry, I. Revenko, and M. J. Allen, "Applications of Atomic Force Microscopy for Contact Lens Manufacturing," Digital Instruments application note, 1998 (author's collection).
96. Proksch, Babcock, and Cleveland 1999; Revenko and Proksch 2000.
97. *Digital Instruments v. Topometrix*, US District Court, Northern District of California, Docket No. C93-20900, 1993.
98. Anonymous 1999.
99. Fasca 1998.
100. Ibid.
101. Source: <http://www.ia.ucsb.edu>.
102. Source: <http://www.veeco.com>. See also Anonymous 2000.
103. Interview with Dan Bocek.

104. Ibid.
105. Cleveland interview; Bocek interview.
106. See Lécuyer 2006 and Bassett 2002.

Chapter 6

1. This measure was generated by searching the Science Citation Index. The search string "scanning tunneling microscope* OR atomic force microscope* OR magnetic force microscope*" was entered in the "Topic" category. Obviously, many articles are left out of such a crude search, but the resulting estimate of numbers of probe microscope articles should suffice for the qualitative argument here. Note that Murday et al. 2005 and other semi-official descriptions of nanotechnology have used exactly the same metric.

2. Sources of data points for NanoScope sales: 200 sales—*Physics Today*, June 1989: 21; 300 sales—*Physics Today*, February 1990, inside back cover; 500 sales—*Physics Today*, February 1991: 57; 1,000 sales—*Journal of Vacuum Science and Technology A* 11 (1993), no. 4: A7; 2,000 sales—*Nanovations*, summer 1997, found in Richard Smalley papers, Chemical Heritage Foundation collection, Box 57, Folder 1; 2200 sales—Anonymous 1998.

3. Gross, Smith, and Carey 1993.

4. By "nanotechnology discourse" I mean something like the body of representations of different activities that could count as nanotechnology, and the iterative shaping of those activities by that talk and by institutions built to disseminate that talk. For a fuller and more nuanced explication of the type of process I'm gesturing to, see Edwards 1996. Edwards' idea of "mutual orientation" of different actors toward the elements of a discourse is quite similar to the way I am describing nanotechnology.

5. The other members of the committee were Randy Feenstra of IBM Yorktown and Hans-Joachim Güntherodt of the University of Basel. Güntherodt's group was very closely tied to IBM Zurich, so he and Feenstra can be seen as representing the American and European camps of IBM's STM program.

6. Feenstra interview.

7. Most of this section draws on the 2007 Murday interview.

8. Duke interview.

9. Williams interview.

10. See the contents page of *Journal of Vacuum Science and Technology B* 9 (1991), no. 2.

11. Murday interview, 2007.

12. For more on the history of the Gordon Research Conferences, see Daemmrich, Gray, and Shaper 2006.
13. Alan Cowley, "1993 Conference Evaluation, Scanning Tunneling Microscopy," Series VI (Evaluations), Box 131, Folder 2 (1993–4), Gordon Research Conference papers, Chemical Heritage Foundation.
14. Ibid.
15. Ibid.
16. Ibid.
17. Ibid.
18. Tromp interview.
19. Nogami interview.
20. Quotations are from Nogami and Hamers interviews. See also Feenstra interview.
21. Albrecht et al. 1988b; Tortonese 1993.
22. For more on the evolution of the microfabrication community, see Mody, forthcoming.
23. "Written Comments Made by Conferees 1988," Series VI (Evaluations), Box 129, Folder 4 (1983–1989), Gordon Research Conference papers, Chemical Heritage Foundation.
24. Conference agenda, Series V (Programs), Box 123, Folder 11 (1992), Gordon Research Conference papers, Chemical Heritage Foundation.
25. Conference agenda, Series V (Programs), Box 124b, Folder 11 (1994), Gordon Research Conference papers, Chemical Heritage Foundation.
26. Interview with Don Tennant.
27. Lehmann, Stauffer, and Vettiger 1995.
28. Perrocheau 1996.
29. Kehrt and Schüssler 2009.
30. Baird and Shew 2004.
31. Interagency Working Group 2000. The near-field scanning optical microscope (NSOM) combines elements of AFM technology and traditional light microscopy.
32. Ignoring the importance of older technologies in order to emphasize new ones is a common feature of both popular and professional histories of technology. See Edgerton 2006.
33. Rohrer 1992.

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34. Mollick 2006; Brock 2006.
35. Rohrer 1993.
36. Trueb 2000.
37. Fichtner et al. 2004.
38. Murday interview, 2007.
39. "Program from Meeting," Richard Smalley papers, Woodson Research Center, Fondren Library, Rice University, Box 2, Folder 24.
40. Brandt also oversaw grants for a number of probe microscopists, including Quate. See Calvin Quate and Steve Minne, "Annual Report," Grant N00014-96-1-0771 (April 7, 1988).
41. Choi and Mody 2009.
42. Murday interview, 2007.
43. See Murday et al. 2005.
44. For a description of the political process by which the National Nanotechnology Initiative was created, see McCray 2005.
45. Wienroth and Kearnes, forthcoming.
46. "Mesoscale" is roughly equivalent to "nanoscale," in that both terms focus on a size regime (around 1 to 100 nanometers) in which quantum and classical properties intersect.
47. My discussion of Franks is taken from Wienroth and Kearnes (forthcoming), but see also Franks 1987, Raven et al. 1986, and Franks 1993.
48. Garland 2010.
49. I have found Tolles' report in at least three forms—the one cited above; W. Tolles, email to European Nano Newsletter list, Richard Smalley papers, Woodson Research Center, Fondren Library, Rice University, Box 3, Folder 5; Tolles 1994; and Tolles 1996. It is also cited in a Foresight Institute newsletter—see Phelps 1996). It seems to have circulated widely among people interested in building nanotechnology institutions in the United States.
50. R. E. Smalley, "Some Thoughts on the WHY of the National Nanotechnology Initiative," November 4, 1999, Richard Smalley papers, Chemical Heritage Foundation archives, Box 57, Folder 7.
51. Tolles 1994, pp. 71 and 124.
52. McCray 2009.
53. Tolles 1994, p. 124.

54. Toumey 2008. See also Williams interview.
55. Brand and the Global Business Network's co-founder, Peter Schwartz, both sat on the board of the Foresight Institute, and the GBN funded some of the Foresight Conferences. For an excellent history of Brand's circle, see Turner 2006. Turner lays out the connections between futurists like Brand and New Right figures such as Gingrich. For a description of Drexler's ties to Gore and Jeremiah, see Regis 1995. See also Drexler's testimony before an enthusiastic Al Gore in Senate Subcommittee on Science, Technology and Space, Committee on Commerce, Science, and Transportation, *New Technologies for a Sustainable World*, 102nd Congress, 1992.
56. Welland interview with McCray.
57. Pollack 1991.
58. Toumey 2008, pp. 146 and 149.
59. Gan 1988.
60. Foster, Frommer, Arnett 1988; Pethica 1988.
61. Drexler and Foster 1990. This letter is referenced repeatedly in the Foresight Institute's newsletter—see *Foresight Update* issues 9 (two mentions), 10, and 52.
62. Foster interview, 2001.
63. Ibid.
64. Ibid.
65. "The First Foresight Conference on Nanotechnology, List of Participants," Jim Bennett papers, Patrick McCray's nanotechnology working files.
66. "NT Resources Catalog, NANOPiRG, Fall 1987," Conrad Schneiker papers, Patrick McCray's nanotechnology working files.
67. Anonymous 2002.
68. Mody 2010; 1992 Rice Chemistry department retreat recording, copy given to the author by Bob Curl.
69. Regis 1995.
70. David Whitehouse, in frontmatter of first issue of *Nanotechnology*, 1990.
71. Ibid.
72. Ibid.
73. Toumey 2008.
74. Roco, Williams, and Alivisatos 1999. On the importance of this report, including its translation into Russian, see Lane 2001.

75. Anonymous 2007b.
76. Murday et al. 2005.
77. Nogami interview.
78. Galison 2004a, 2007a, 2007b.
79. In some ways, surface scientists saw the patronizing attitude of high-energy physicists toward condensed-matter physics as replicated in the views of condensed-matter physicists toward surface science. Indeed, the major reason surface scientists sought a professional home in the AVS in the 1960s is reportedly that condensed-matter physicists wouldn't let them have stand-alone topical sessions at the annual March meeting of the American Physical Society. Sources: Duke interview; Murday interview, 2007.
80. Kevles 1997.
81. On the importance of rhetoric about the semiconductor industry in promoting nanotech, see McCray 2009.
82. Galison (2004b) shows that the same existential crisis at the end of the Cold War pushed some physicists in the opposite direction—toward mathematics and away from experimentation.
83. Tolles 1994, p. 115.
84. Murday interview, 2007.

Bibliography

Abbott, Andrew Delano. 1988. *The System of Professions: An Essay on the Division of Expert Labor*. University of Chicago Press.

Akera, Atsushi. 2007. *Calculating a Natural World: Scientists, Engineers, and Computers during the Rise of U.S. Cold War Research*. MIT Press.

Albrecht, T. R., H. A. Mizes, J. Nogami, Sang-il Park, and C. F. Quate. 1988a. Observation of Tilt Boundaries in Graphite by Scanning Tunneling Microscopy and Associated Multiple Tip Effects. *Applied Physics Letters* 52 (5): 362.

Albrecht, T. R., M. M. Dovek, C. A. Lang, P. Grütter, C. F. Quate, S. W. J. Kuan, C. W. Frank, and R. F. W. Pease. 1988b. Imaging and Modification of Polymers by Scanning Tunneling Microscopy and Atomic Force Microscopy. *Journal of Applied Physics* 64 (3): 1178–1184.

Albrecht, T. R., S. Akamine, T. E. Carver, and C. F. Quate. 1990. Microfabrication of Cantilever Stylus for the Atomic Force Microscope. *Journal of Vacuum Science & Technology A* 8 (4): 3386–3396.

Alexander, S., L. Hellemans, O. Marti, J. Schneir, V. Elings, P. K. Hansma, M. Longmire, and J. Gurley. 1989. An Atomic-Resolution Atomic-Force Microscope Implemented Using an Optical Lever. *Journal of Applied Physics* 65 (1): 164.

Anderson, Bill, and Hakan Yuce. 1990. Optical-Fiber Reliability. *Bellcore Exchange*, October: 8–13.

Anonymous. 1967. The Ultramicrometer: New Measuring Tool. *Machinery* 73 (12): 102–104.

Anonymous. 1995. Veeco SXM® Atomic Force Microscope Used for Advanced Sub-micron Applications. *Business Wire*, February 6.

Anonymous. 1998. Veeco Completes Merger with Digital Instruments; Appoints New Members to Board of Directors. *Business Wire*, May 29.

Anonymous. 1999. Zygo and IBM Sign Additional Worldwide Distribution Agreement for IBM Production Line Atomic Force Microscope. *Business Wire*, January 12.

Anonymous. 2000. Veeco Instruments Purchases IBM's Atomic Force Microscope Assets. *Business Wire*, March 27.

Anonymous. 2002. Veeco Came, Saw, Acquired Majority of the AFM Market. *Small Times*, October 8.

Anonymous. 2007a. Question and Answer: Virgil Elings. *Convergence* (<http://convergence.ucsb.edu/article/question-and-answer-virgil-elings>).

Anonymous. 2007b. *2007 Annual Report on Form 10-K*. Veeco.

Asner, Glen Ross. 2006. *The Cold War and American Industrial Research*. Ph.D. dissertation, Carnegie Mellon University.

Baird, Davis, and Ashley Shew. 2004. Probing the History of Scanning Tunneling Microscopy. In *Discovering the Nanoscale*, ed. D. Baird, A. Nordmann, and J. Schummer. IOS Press.

Barabási, Albert-László, and Réka Albert. 1999. Emergence of Scaling in Random Networks. *Science* 286 (5439) (October 15): 509–512.

Barad, Karen. 1999. Agential Realism: Feminist Interventions in Understanding Scientific Practices. In *The Science Studies Reader*, ed. M. Biagioli. Routledge.

ffaa75867768260a7b4b2abe8ff24691
ebrary Barnes, Barry, David Bloor, and John Henry. 1996. *Scientific Knowledge: A Sociological Analysis*. University of Chicago Press.

Baro, A. M., R. Miranda, J. Alaman, N. Garcia, G. Binnig, H. Rohrer, C. Gerber, and J. L. Carrascosa. 1985. Determination of Surface Topography of Biological Specimens at High Resolution by Scanning Tunneling Microscopy. *Nature* 315 (6016) (May 16): 253–254.

Bassett, Ross Knox. 2002. *To the Digital Age: Research Labs, Start-up Companies, and the Rise of MOS Technology*. Johns Hopkins University Press.

Beaulieu, Anne. 2010. From Co-Location to Co-Presence: Shifts in the Use of Ethnography for the Study of Knowledge. *Social Studies of Science* 40: 453–470.

Beebe, T. P., T. E. Wilson, D. F. Ogletree, J. E. Katz, R. Balhorn, M. B. Salmeron, and W. J. Siekhaus. 1989. Direct Observation of Native DNA Structures with the Scanning Tunneling Microscope. *Science* 243 (4889) (January 20): 370–372.

Bennett, P. A., L. C. Feldman, Y. Kuk, E. G. McRae, and J. E. Rowe. 1983. Stacking-Fault Model for the Si(111)-(7×7) surface. *Physical Review B* 28 (6): 3656.

Bijker, Wiebe E. 1995. *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change*. MIT Press.

Binnig, G. 1992. Force Microscopy. *Ultramicroscopy* 42–44 (1): 7–15.

Binnig, G., H. Rohrer, C. Gerber, and E. Weibel. 1982. Surface Studies by Scanning Tunneling Microscopy. *Physical Review Letters* 49 (1) (July 5): 57.

Binnig, G., H. Rohrer, C. Gerber, and E. Weibel. 1983. 7×7 Reconstruction on Si(111) Resolved in Real Space. *Physical Review Letters* 50 (2): 120.

Binnig, Gerd, and Heinrich Rohrer. 1984. Scanning Tunneling Microscopy. In *Trends in Physics*, ed. J. Jant and J. Pantolicek. European Physical Society.

Binnig, Gerd, and Heinrich Rohrer. 1986. Scanning Tunneling Microscopy. *IBM Journal of Research and Development* 30 (4): 355–369.

Binnig, Gerd, and Heinrich Rohrer. 1987. Scanning Tunneling Microscopy—From Birth to Adolescence. *Reviews of Modern Physics* 59 (3): 615–625.

Binnig, Gerd, and Heinrich Rohrer. 1999. In Touch with Atoms. *Reviews of Modern Physics* 71 (2): S324–S330.

ffaa75867768260a7b4b2abe8ff24691
ebrary Binnig, G., C. F. Quate, and C. Gerber. 1986. Atomic Force Microscope. *Physical Review Letters* 56 (9): 930–933.

Binnig, G., C. Gerber, E. Stoll, T. R. Albrecht, and C. F. Quate. 1987. Atomic Resolution with Atomic Force Microscope. *Surface Science* 189–190: 1–6.

Bloor, David. 1976. *Knowledge and Social Imagery*. Routledge & Keegan Paul.

Blume, Stuart S. 1992. *Insight and Industry: On the Dynamics of Technological Change in Medicine*. MIT Press.

Brock, David C., ed. 2006. *Understanding Moore's Law: Four Decades of Innovation*. Chemical Heritage Foundation.

Brock, W. H. 2008. *William Crookes (1832–1919) and the Commercialization of Science*. Ashgate.

Bromberg, Joan Lisa. 1991. *The Laser in America, 1950–1970*. MIT Press.

Choi, Hyungsub. 2007. The Boundaries of Industrial Research—Making Transistors at RCA, 1948–1960. *Technology and Culture* 48 (4): 758–782.

Choi, Hyungsub, and Cyrus C. M. Mody. 2009. The Long History of Molecular Electronics: Microelectronics Origins of Nanotechnology. *Social Studies of Science* 39 (1): 11–50.

Clemmer, C. R., and T. P. Beebe. 1991. Graphite: A Mimic for DNA and Other Biomolecules in Scanning Tunneling Microscope Studies. *Science* 251 (4994) (February 8): 640–642.

Cochrane, Rexmond Canning. 1976. *Measures for Progress: A History of the National Bureau of Standards*. Arno.

Collins, H. M. 1985. *Changing Order: Replication and Induction in Scientific Practice*. Sage.

Collins, H. M. 2004. *Gravity's Shadow: The Search for Gravitational Waves*. University of Chicago Press.

Colyvas, J. 2007. From divergent meanings to common practices: Institutionalization processes and the commercialization of university research. Ph.D. dissertation, Stanford University.

Committee for the Review of the National Nanotechnology Initiative. 2002. *Small Wonders, Endless Frontiers: A Review of the National Nanotechnology Initiative*. National Research Council.

Coombs, J. H., and J. B. Pethica. 1986. Properties of Vacuum Tunneling Currents—Anomalous Barrier Heights. *IBM Journal of Research and Development* 30 (5): 455–459.

Cortright, Joseph, and Heike Mayer. 2002. *Signs of Life: The Growth of Biotechnology Centers in the U.S.* Brookings Institution.

Creager, Angela N. H. 2002. *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965*. University of Chicago Press.

Daemmrich, Arthur A., Nancy Ryan Gray, and Leah Shaper, eds. 2006. *Reflections from the Frontiers: Explorations for the Future: Gordon Research Conferences, 1931–2006*. Chemical Heritage Foundation.

Darby, Michael R., and Lynne G. Zucker. 2003. Grilichesian Breakthroughs: Inventions of Methods of Inventing and Firm Entry in Nanotechnology. SSRN eLibrary (July). http://papers.ssrn.com/sol3/papers.cfm?abstract_id=421786.

Dear, Peter. 1995. *Discipline & Experience: The Mathematical Way in the Scientific Revolution*. University of Chicago Press.

Demuth, J. E., U. Koehler, and R. J. Hamers. 1988. The STM Learning Curve and Where It May Take Us. *Journal of Microscopy—Oxford* 152: 299–316.

DiMaggio, Paul J. 1991. Constructing an Organizational Field as a Professional Project: U.S. Art Museums, 1920–1940. In *The New Institutionalism in Organizational Analysis*, ed. Walter W. Powell and Paul J. DiMaggio. University of Chicago Press.

DiMaggio, Paul J., and Walter W. Powell. 1983. The Iron Cage Revisited: Institutional Isomorphism and Collective Rationality in Organizational Fields. *American Sociological Review* 48 (2): 147–160.

Drexler, K. Eric, and John S. Foster. 1990. Synthetic Tips. *Nature* 343 (6259): 600.

Driscoll, Robert J., Michael G. Youngquist, and John D. Baldeschwieler. 1990. Atomic-Scale Imaging of DNA Using Scanning Tunnelling Microscopy. *Nature* 346 (6281) (July 19): 294–296.

Duke, C. B. 1984. Atoms and Electrons at Surfaces: A Modern Scientific Revolution. In *History of Vacuum Science and Technology: A Special Volume Commemorating the 30th Anniversary of the American Vacuum Society, 1953–1983*, ed. T. Madey and W. Brown. American Institute of Physics.

ffaa75867768260a7b4b2abe8ff24691
ebruary

Duke, C. B. 1996. Semiconductor Surface Reconstructions: The Structural Chemistry of Two Dimensional Surface Compounds. *Chemical Reviews* 96: 1237–1259.

van Duzer, Theodore. 2008. Superconductor Digital Electronics Past, Present, and Future. *IEICE Transactions on Electronics E* 91-C (3): 260–271.

Edgerton, David. 2006. *The Shock of the Old: Technology and Global History since 1900*. Profile Books.

Edwards, Paul N. 1996. *The Closed World: Computers and the Politics of Discourse in Cold War America*. MIT Press.

Edwards, Steven A. 2006. *The Nanotech Pioneers: Where Are They Taking Us?* Wiley-VCH Verlag.

ffaa75867768260a7b4b2abe8ff24691
ebruary

Elings, Virgil. 1995. "Invent or Die" Is the Key to Success in Science. *R&D Magazine*, 21.

Elzen, Boelie. 1986. Two Ultracentrifuges: A Comparative Study of the Social Construction of Artefacts. *Social Studies of Science* 16 (4): 621–662.

Etzkowitz, Henry. 1994. Knowledge as Property: The Massachusetts Institute of Technology and the Debate over Academic Patent Policy. *Minerva* 32 (4): 383–421.

Etzkowitz, Henry. 2008. *The Triple Helix: University-Industry-Government Innovation in Action*. Routledge.

Fasca, Chad. 1998. Veeco Deals for Digital Instruments. *Electronic News*, February 16. http://findarticles.com/p/articles/mi_m0EKF/is_n2206_v44/ai_20323832/pg_2/?tag=content;coll. ebrary

Feenstra, R. M., Joseph A. Stroschio, J. Tersoff, and A. P. Fein. 1987. Atom-Selective Imaging of the GaAs(110) Surface. *Physical Review Letters* 58 (12) (March 23): 1192.

Feynman, Richard P. 1999. There's Plenty of Room at the Bottom: An Invitation to Enter a New Field of Physics. In Feynman, *The Pleasure of Finding Things Out*. Perseus.

Fichtner, Wolfgang, Qiuting Huang, Bernd Witzigmann, Hubert Kaeslin, Norbert Felber, and Dölf Aemmer. 2004. *Research Review 2004: Integrated Systems Laboratory, Microelectronics Research Center*. Eidgenössische Technische Hochschule Zürich.

Fisher, Arthur. 1989. Seeing Atoms. *Popular Science* (April): 102–107.

Foster, J. S., J. E. Frommer, and P. C. Arnett. 1988. Molecular Manipulation Using a Tunneling Microscope. *Nature* 331 (6154): 324–326. ebrary

Franks, Albert. 1987. Nanotechnology. *Journal of Physics. E, Scientific Instruments* 20: 1442–1451.

Franks, Albert. 1993. The UK National Initiative on Nanotechnology. In *International Progress in Precision Engineering*, ed. N. Ikawa. Butterworth-Heinemann.

Friedbacher, G., and H. Fuchs. 1999. Classification of Scanning Probe Microscopies (Technical Report). *Pure and Applied Chemistry* 71: 1337–1357.

Gadzuk, J. William. 1987. STM Not Developed in a Vacuum. *Physics Today* 40 (11): 11.

Galison, Peter. 1987. *How Experiments End*. University of Chicago Press.

Galison, Peter. 1997. *Image and Logic: A Material Culture of Microphysics*. University of Chicago Press.

Galison, Peter. 2004a. Paper presented at Imaging and Imagining Nanoscience & Engineering conference, Columbia, SC, March 4.

Galison, Peter. 2004b. Mirror Symmetry: Persons, Values, and Objects. In *Growing Explanations: Historical Perspectives on Recent Science*, ed. N. Wise. Duke University Press.

Galison, Peter. 2007a. Presentation, Not Representation: Nanoimages after Epistemology. Paper presented at Images of the Nanoscale conference, Columbia, SC, October 25.

Galison, Peter. 2007b. Paper presented at Joint Wharton-CHF Symposium on the Social Studies of Nanotechnology, Philadelphia, June 7.

Galison, Peter, and Barton Bernstein. 1989. In Any Light: Scientists and the Decision to Build the Hydrogen Bomb. *Historical Studies in the Physical and Biological Sciences* 19: 267–347.

Galison, Peter, and Bruce Hevly, eds. 1992. *Big Science: The Growth of Large-Scale Research*. Stanford University Press.

Gan, Judith B. 1988. Scanning New Terrains: Almaden Scientists Break New Ground in Imaging and Manipulating Organic Molecules. *IBM Research Magazine* (Fall).

Garland, Andrew. 2010. Pioneer Spirit. *Nano: The Magazine for Small Science*, Issue 14.

Geiger, Roger L., and Creso M. Sá. 2008. *Tapping the Riches of Science: Universities and the Promise of Economic Growth*. Harvard University Press.

Gewirth, Andrew A., and Allen J. Bard. 1988. In Situ Scanning Tunneling Microscopy of the Anodic Oxidation of Highly Oriented Pyrolytic Graphite Surfaces. *Journal of Physical Chemistry* 92 (20): 5563–5566.

Gerber, C., G. Binnig, H. Fuchs, O. Marti, and H. Rohrer. 1986. Scanning Tunneling Microscope Combined with a Scanning Electron Microscope. *Review of Scientific Instruments* 57 (2): 221.

Gieryn, Thomas F. 1999. *Cultural Boundaries of Science*. University of Chicago Press.

Granovetter, Mark S. 1973. The Strength of Weak Ties. *American Journal of Sociology* 78 (6): 1360–1380.

Grayson, Michael A. 2002. *Measuring Mass: From Positive Rays to Proteins*. Chemical Heritage Press.

Griffith, J. E., and G. P. Kochanski. 1990. Scanning Tunneling Microscopy. *Annual Review of Materials Science* 20: 219–244.

Gross, Neil, Emily Smith, and John Carey. 1993. Windows on the World of Atoms. *BusinessWeek* online (<http://www.businessweek.com>), August 30.

Guckenberger, Reinhard, Manfred Heim, Gregor Cevc, Helmut F Knapp, Winfried Wiegräbe, and Anton Hillebrand. 1994. Scanning Tunneling Microscopy of Insulators and Biological Specimens Based on Lateral Conductivity of Ultrathin Water Films. *Science* 266 (5190) (December 2): 1538–1540.

Gusterson, Hugh. 1996. *Nuclear Rites: A Weapons Laboratory at the End of the Cold War*. University of California Press.

Hacking, Ian. 1983. *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge University Press.

Hacking, Ian. 1992. The Self-Vindication of the Laboratory Sciences. In *Science as Practice and Culture*, ed. A. Pickering. University of Chicago Press.

Hallmark, V. M., S. Chiang, J. F. Rabolt, J. D. Swalen, and R. J. Wilson. 1987. Observation of Atomic Corrugation on Au(111) by Scanning Tunneling Microscopy. *Physical Review Letters* 59 (25) (December 21): 2879.

Hamers, R. J. 1989. Atomic-Resolution Surface Spectroscopy with the Scanning Tunneling Microscope. *Annual Review of Physical Chemistry* 40 (1): 531–559.

Hamers, R. J. 1996. Scanned Probe Microscopies in Chemistry. *Journal of Physical Chemistry* 100 (31) (January 1): 13103–13120.

Hansma, H. G., J. Vesenska, C. Siegerist, G. Kelderman, H. Morrett, R. L. Sinsheimer, V. Elings, C. Bustamante, and P. K. Hansma. 1992. Reproducible Imaging and Dissection of Plasmid DNA under Liquid with the Atomic Force Microscope. *Science* 256 (5060) (May 22): 1180–1184.

Hansma, Paul K., ed. 1982. *Tunneling Spectroscopy: Capabilities, Applications, and New Techniques*. Plenum.

Hansma, P. K., V. B. Elings, O. Marti, and C. E. Bracker. 1988. Scanning Tunneling Microscopy and Atomic Force Microscopy: Application to Biology and Technology. *Science* 242 (4876) (October 14): 209–216.

- Haring, Kristen. 2007. *Ham Radio's Technical Culture. Inside technology*. MIT Press.
- Hawley, Marilyn, Ian D. Raistrick, Jerome G. Beery, and Robert J. Houlton. 1991. Growth Mechanism of Sputtered Films of YBa₂Cu₃O₇ Studied by Scanning Tunneling Microscopy. *Science* 251 (5001) (March 29): 1587–1589.
- Heckl, W. M., and G. Binnig. 1992. Domain Walls on Graphite Mimic DNA. *Ultra-microscopy* 42–44 (2): 1073–1078.
- Henderson, Rebecca, Adam B. Jaffe, and Manuel Trajtenberg. 1998. Universities as a Source of Commercial Technology: A Detailed Analysis of University Patenting, 1965–1988. *Review of Economics and Statistics* 80 (1): 119–127.
- Hess, H. F., R. B. Robinson, R. C. Dynes, J. M. Valles Jr., and J. V. Waszczak. 1989. Scanning-Tunneling-Microscope Observation of the Abrikosov Flux Lattice and the Density of States near and inside a Fluxoid. *Physical Review Letters* 62 (2): 214–216.
- von Hippel, Eric. 2005. *Democratizing Innovation*. MIT Press.
- Holbrook, Daniel, Wesley M. Cohen, David A. Hounshell, and Steven Klepper. 2000. The Nature, Sources, and Consequences of Firm Differences in the Early History of the Semiconductor Industry. *Strategic Management Journal* 21 (10): 1017–1041.
- Hughes, Sally Smith. 2001. Making Dollars out of DNA: The First Major Patent in Biotechnology and the Commercialization of Molecular Biology, 1974–1980. *Isis* 92 (3): 541–575.
- Interagency Working Group on Nanoscience, Engineering, and Technology. 2000. *National Nanotechnology Initiative: Leading to the Next Industrial Revolution*. National Science and Technology Council.
- Isaacson, M., D. Kopf, M. Utlaut, N. W. Parker, and A. V. Crewe. 1977. Direct Observations of Atomic Diffusion by Scanning Transmission Electron Microscopy. *Proceedings of the National Academy of Sciences of the United States of America* 74 (5): 1802–1806.
- Jansen, Dorothea, Regina Görtz, and Richard Heidler. 2009. Knowledge Production and the Structure of Collaboration Networks in Two Scientific Fields. *Scientometrics* 83: 219–241.
- Joerges, Bernward, and Terry Shinn. 2001. A Fresh Look at Instrumentation: An Introduction. In *Instrumentation between Science, State and Industry*, ed. B. Joerges and T. Shinn. Kluwer.

Johnston, Sean. 2006. *Holographic Visions: A History of New Science*. Oxford University Press.

Jordan, Kathleen, and Michael Lynch. 1992. The Sociology of a Genetic Engineering Technique: Ritual and Rationality in the Performance of a 'Plasmid Prep'. In *The Right Tools for the Job: at Work in the Twentieth-Century Life Sciences*, ed. Adele E. Clarke and Joan H. Fujimora. Princeton University Press.

Kaiser, David. 2005. *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*. University of Chicago Press.

Kanno, Takashi, Hiroyuki Tanaka, Tomohiko Nakamura, Hitoshi Tabata, and Tomoji Kawai. 1999. Real Space Observation of Double-Helix DNA Structure Using a Low Temperature Scanning Tunneling Microscopy. *Japanese Journal of Applied Physics* 38 (2): L606–L607.

Kaplan, Sarah, and Joanna Radin. Forthcoming. Bounding an Emerging Technology: Para-Scientific Media and the Drexler-Smalley Debate about Nanotech. *Social Studies of Science*.

Katayama, M., R. S. Williams, M. Kato, E. Nomura, and M. Aono. 1991. Structure Analysis of the Si(111) $\sqrt{3} \times \sqrt{3}$ R30°-Ag surface. *Physical Review Letters* 66 (21) (May 27): 2762.

Kehoe, Louise. 1983. Death Knell for Josephson? Why the Giant Turned Back from the Junction. *Financial Times*, November 22.

Kehrt, Christian, and Peter Schussler. 2010. Nanoscience Is 100 Years Old. In *Governing Future Technologies: Nanotechnology and the Rise of an Assessment Regime*, ed. M. Kaiser et al. Springer.

Kenney, Martin, ed. 2000. *Understanding Silicon Valley: The Anatomy of an Entrepreneurial Region*. Stanford University Press.

Kenney, Martin, and Donald Patton. 2009. Reconsidering the Bayh-Dole Act and the Current University Invention Ownership Model. *Research Policy* 38 (9): 1407–1422.

Kevles, Daniel J. 1997. Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project. *Historical Studies in the Physical and Biological Sciences* 27: 269–297.

Keyes, Robert W. 1969. Physical Problems and Limits in Computer Logic. *IEEE Spectrum* 6: 36–45.

Kirk, Michael Dominic. 1989. Low-temperature scanning tunneling spectroscopy. Ph.D. dissertation, Stanford University.

Kline, Ronald R. 1992. *Steinmetz: Engineer and Socialist*. Johns Hopkins University Press.

Kline, Ronald R., and Trevor Pinch. 1996. Users as Agents of Technological Change: The Social Construction of the Automobile in the Rural United States. *Technology and Culture* 37 (4): 763–795.

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

Knorr-Cetina, K. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Harvard University Press. ffaa75867768260a7b4b2abe8ff24691 ebrary

Kohler, Robert E. 1994. *Lords of the Fly: Drosophila Genetics and the Experimental Life*. University of Chicago Press.

Kolata, Gina Bari. 1977. National Bureau of Standards: A Fall from Grace. *Science* 197 (4307) (September 2): 968–970.

Kunkle, Gregory C. 1995. Technology in the Seamless Web: 'Success' and 'Failure' in the History of the Electron Microscope. *Technology and Culture* 36: 80–103.

Lagally, Max G. 2003. Transition from Reciprocal-Space to Real-Space Surface Science—Advent of the Scanning Tunneling Microscope. *Journal of Vacuum Science & Technology A* 21 (5): S54–S63.

ffaa75867768260a7b4b2abe8ff24691 ebrary
Lane, Neal. 2001. The Grand Challenges of Nanotechnology. *Journal of Nanoparticle Research* 3: 95–103.

Lang, C. A., J. K. H. Horber, T. W. Hansch, W. M. Heckl, and H. Mohwald. 1988. Scanning Tunneling Microscopy of Langmuir—Blodgett Films on Graphite. *Journal of Vacuum Science & Technology A* 6 (2): 368–370.

Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers through Society*. Harvard University Press.

Latour, Bruno, and Steve Woolgar. 1986. *Laboratory Life: The Construction of Scientific Facts*. Princeton University Press.

Layton, Edwin T., Jr. 1971. Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America. *Technology and Culture* 12: 562–580.

Lécuyer, Christophe. 2006. *Making Silicon Valley: Innovation and the Growth of High Tech, 1930–1970. Inside technology*. MIT Press.

Lécuyer, Christophe, and David C. Brock. 2006. The Materiality of Microelectronics. *History and Technology: an International Journal* 22 (3): 301.

Lehmann, H. W., U. Staufer, and P. Vettiger. 1995. Preface. *Microelectronic Engineering* 27: vii–viii.

Lenoir, Timothy, and Christophe Lécuyer. 1997. Instrument Makers and Discipline Builders: The Case of Nuclear Magnetic Resonance. In T. Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*. Stanford University Press.

Leslie, Stuart W. 1993. *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*. Columbia University Press.

Leslie, Stuart W. 2001. Blue Collar Science: Bringing the Transistor to Life in the Lehigh Valley. *Historical Studies in the Physical and Biological Sciences* 32 (1): 71–113.

Leslie, Stuart W., and Robert H. Kargon. 1996. Selling Silicon Valley: Frederick Terman's Model for Regional Advantage. *Business History Review* 70 (4): 435–472.

Liebeskind, Julia Porter, Amalya Lumerman Oliver, Lynne Zucker, and Marilyn Brewer. 1996. Social Networks, Learning, and Flexibility: Sourcing Scientific Knowledge in New Biotechnology Firms. *Organization Science* 7 (4): 428–443.

Lindsay, S. M., T. Thundat, L. Nagahara, U. Knipping, and R. L. Rill. 1989. Images of the DNA Double Helix in Water. *Science* 244 (4908) (June 2): 1063–1064.

van Loenen, E. J., J. E. Demuth, R. M. Tromp, and R. J. Hamers. 1987. Local Electron States and Surface Geometry of Si(111)-sqrt 3 sqrt 3 Ag. *Physical Review Letters* 58 (4) (January 26): 373.

Logue, Joseph C. 1998. From Vacuum Tubes to Very Large Scale Integration: A Personal Memoir. *Annals of the History of Computing, IEEE* 20 (3): 55–68.

Lowen, Rebecca S. 1997. *Creating the Cold War University: The Transformation of Stanford*. University of California Press.

Lubkin, Gloria B. 1984. Bell Labs Fissions, Yielding AT&T Bell Labs and Bellcore. *Physics Today* 36 (5): 77–80.

Lüthje, Christian, Cornelius Herstatt, and Eric von Hippel. 2005. User-Innovators and "Local" Information: The Case of Mountain Biking. *Research Policy* 34 (6): 951–965.

Lynch, Michael. 1985. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*. Routledge & Kegan Paul.

MacKenzie, Donald A. 1990. *Inventing Accuracy: An Historical Sociology of Nuclear Missile Guidance*. MIT Press.

MacKenzie, Donald A. 1996. *Knowing Machines: Essays on Technical Change*. MIT Press.

MacKenzie, Donald A. 2006. *An Engine, Not a Camera: How Financial Models Shape Markets*. MIT Press.

Madey, Theodore E., and William C. Brown, eds. 1984. *History of Vacuum Science and Technology: A Special Volume Commemorating the 30th Anniversary of the American Vacuum Society, 1953–1983*. American Institute of Physics.

Mamin, H. Jonathon, Eric Ganz, David W. Abraham, Ruth Ellen Thomson, and John Clarke. 1986. Contamination-Mediated Deformation of Graphite by the Scanning Tunneling Microscope. *Physical Review B: Condensed Matter and Materials Physics* 34: 9015–9018.

Mamin, H. J., S. Chiang, H. Birk, P. H. Guethner, and D. Rugar. 1991. Gold Deposition from a Scanning Tunneling Microscope Tip. *Journal of Vacuum Science & Technology B* 9: 1398–1402.

Manalis, Scott Robert. 1998. Optical detection for microfabricated cantilever arrays. Ph.D. dissertation, Stanford University.

Manne, S., P. K. Hansma, J. Massie, V. B. Elings, and A. A. Gewirth. 1991. Atomic-Resolution Electrochemistry with the Atomic Force Microscope: Copper Deposition on Gold. *Science* 251 (4990) (January 11): 183–186.

Markoff, John. 2000. A Clinton Initiative in a Science of Smallness. *New York Times*, January 21.

Mate, C. Mathew, Gary M. McClelland, Ragnar Erlandsson, and Shirley Chiang. 1987. Atomic-Scale Friction of a Tungsten Tip on a Graphite Surface. *Physical Review Letters* 59 (17): 1942–1945.

Mate, C. Mathew, Ragnar Erlandsson, Gary M. McClelland, and Shirley Chiang. 1989. Direct Measurement of Forces during Scanning Tunneling Microscope Imaging of Graphite. *Surface Science* 208 (3): 473–486.

Mazzola, Laura. 2003. Commercializing Nanotechnology. *Nature Biotechnology* 21 (10): 1137–1143.

McCray, W. Patrick. 2004. *Giant Telescopes: Astronomical Ambition and the Promise of Technology*. Harvard University Press.

McCray, W. Patrick. 2005. Will Small Be Beautiful? Making Policies for Our Nanotech Future. *History and Technology* 21: 177–203.

McCray, W. Patrick. 2009. From Lab to iPod: A Story of Discovery and Commercialization in the Post–Cold War Era. *Technology and Culture* 50 (1): 58–81.

McCray, W. Patrick. Forthcoming. California Dreamin': Visioneering the Technological Future. In *Minds and Matters: Technology in California and the West*, ed. V. Janssen. University of California Press.

McRae, E. G., and C. W. Caldwell. 1981. Structure of Si(111)-(7 × 7)H. *Physical Review Letters* 46 (25) (June 22): 1632.

Melmed, Allan J. 1996. Recollections of Erwin Müller's Laboratory: The Development of FIM (1951–1956). *Applied Surface Science* 94–5: 17–25.

Meyer, Gerhard, and Nabil M. Amer. 1988. Novel Optical Approach to Atomic Force Microscopy. *Applied Physics Letters* 53 (12): 1045.

Mirowski, Philip, and Esther-Mirjam Sent. 2008. The Commercialization of Science and the Response of STS. In *The Handbook of Science and Technology Studies*, ed. E. Hackett, O. Amsterdamska, M. Lynch, and J. Wajcman. MIT Press.

Misa, Thomas J. 1985. Military Needs, Commercial Realities, and the Development of the Transistor, 1948–1958. In *Military Enterprise and Technological Change: Perspectives on the American Experience*, ed. M. R. Smith. MIT Press.

Mody, Cyrus C. M. 2005. The Sounds of Science: Listening to Laboratory Practice. *Science, Technology & Human Values* 30: 175–198.

Mody, Cyrus C. M. 2010. *Institutions as Stepping Stones: Rick Smalley and the Commercialization of Nanotubes*. *Studies in Materials Innovation series*. Chemical Heritage Foundation.

Mody, Cyrus C. M. 2011. Conversions: Sound and Sight, Military and Civilian. In *Oxford Handbook of Sound Studies*, ed. T. Pinch and K. Bijsterveld. Oxford University Press.

Mody, Cyrus C. M. Forthcoming. Conferences and the Emergence of Nanoscience. In *The Social Life of Nanotechnology*, ed. B. Harthorn and J. Mohr. Routledge.

Mody, Cyrus C. M., and Michael Lynch. 2010. Test Objects and Other Epistemic Things: A History of a Nanoscale Object. *British Journal for the History of Science* 43 (3): 423–458.

Mollick, Ethan. 2006. Establishing Moore's Law. *IEEE Annals of the History of Computing* 28 (3): 62–75.

Mowery, David C, Richard R. Nelson, and Bhavan N. Sampat. 2004. *Ivory Tower and Industrial Innovation: University-Industry Technology Transfer Before and After the Bayh-Dole Act in the United States*. Stanford Business Books.

Murday, J. S., B. D. Guenther, C. G. Lau, C. R. K. Marrian, J. C. Pazik, and G. S. Pomrenke. 2005. Overview of the Nanoscale Science and Technology Program in the Department of Defense. In *Defense Applications of Nanomaterials*, ed. A. Miziolek et al. American Chemical Society.

Murray, Fiona. 2004. The Role of Academic Inventors in Entrepreneurial Firms: Sharing the Laboratory Life. *Research Policy* 33 (4): 643–659.

Nelson, Andrew J. 2005. Cacophony Or Harmony? Multivocal Logics and Technology Licensing by the Stanford University Department of Music. *Industrial and Corporate Change* 14 (1): 93–118.

Newfield, Christopher. 2003. *Ivy and Industry: Business and the Making of the American University, 1880–1980*. Duke University Press.

Nicoli, David F, Paul H. Barrett, and Virgil B. Elings. 1978. Masters in Instrumentation. *Physics Today* 31 (9): 9.

ffaa75867768260a7b4b2abe8ff24691
ebrary

Oudshoorn, Nelly, and T. J. Pinch, eds. 2003. *How Users Matter: The Co-Construction of Users and Technologies*. MIT Press.

Owen-Smith, Jason, and Walter W. Powell. 2004. Knowledge Networks as Channels and Conduits: The Effects of Spillovers in the Boston Biotechnology Community. *Organization Science* 15 (1): 5–21.

Pantalony, David. 2009. *Altered Sensations: Rudolph Koenig's Acoustical Workshop in Nineteenth-Century Paris*. Springer.

Pashley, M. D., J. B. Pethica, and J. Coombs. 1985. Scanning Tunnelling Microscope Studies. *Surface Science* 152–153 (1): 27–32.

Passaglia, Elio. 1999. *A Unique Institution: The National Bureau of Standards, 1950–1969*. National Institute of Standards and Technology.

ffaa75867768260a7b4b2abe8ff24691
ebrary

Patel, C. Kumar N. 1984. Physics Research: Seeking Tomorrow's Technologies. *Bell Laboratories Record* 62 (April): 21–25.

Perrocheau, Jacques. 1996. Preface. *Microelectronic Engineering* 30: vii–viii.

Perrow, Charles. 1986. *Complex Organizations: A Critical Essay*. McGraw-Hill.

Pethica, J. B. 1988. Scanning Tunneling Microscopes: Atomic-Scale Engineering. *Nature* 331 (6154): 301.

Phelps, Lew. 1996. Naval Research Laboratory Surveys European Nanotechnology. *Foresight Update* 24: 2–4.

Pickering, Andrew. 1984. *Constructing Quarks: A Sociological History of Particle Physics*. University of Chicago Press.

Pinch, T. J. 1986. *Confronting Nature: The Sociology of Solar-Neutrino Detection*. Reidel.

Pitt, Joseph. 2006. When Is an Image Not an Image. In *Nanotechnology Challenges: Implications for Philosophy, Ethics, and Society*, ed. J. Schummer and D. Baird. World Scientific.

Pollack, Andrew. 1991. Atom by Atom, Scientists Build 'Invisible' Machines of the Future. *New York Times*, November 26.

President's Council of Advisers on Science and Technology. 2010. Report to the President and Congress on the Third Assessment of the National Nanotechnology Initiative. Washington, DC, March 12.

Proksch, R., K. Babcock, and J. Cleveland. 1999. Magnetic Dissipation Microscopy in Ambient Conditions. *Applied Physics Letters* 74 (3): 419–421.

Purvis, Gail. 2004. The Compound Weathervanes of Nanotechnology. *III-Vs Review* 17 (4): 42–46.

Quate, Calvin F. 1986. Vacuum Tunneling: A New Technique for Microscopy. *Physics Today* 39 (8): 26.

Quate, Calvin F. 1994. The AFM as a Tool for Surface Imaging. *Surface Science* 299–300: 980–995.

Rabinow, Paul. 1996. *Making PCR: A Story of Biotechnology*. University of Chicago Press.

Rader, Karen A. 2004. *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955*. Princeton University Press.

Rasmussen, Nicolas. 1997. *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940–1960*. Stanford University Press.

Raven, T., C. G. Sawyers, D. G. Jenkins, and D. J. Coates. 1986. Review of the UK Position and Expertise in Nanotechnology (Report to Department of Trade and Industry, public release version).

Regan, Patrick S. 1989. Profile: Scanning Tunneling Microscope. *Bellcore Exchange* 5 (4): 21–23.

Regis, Edward. 1995. *Nano: The Emerging Science of Nanotechnology: Remaking the World-Molecule by Molecule*. Little, Brown.

Reich, Leonard S. 1985. *The Making of American Industrial Research: Science and Business at GE and Bell, 1876–1926*. Cambridge University Press.

Reinhardt, Carsten. 2006. *Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry*. Science History Publications.

Reinhardt, Carsten, and Thomas Steinhauser. 2008. Formierung einer Wissenschaftlich-technischen Gemeinschaft: NMR-Spektroskopie in der Bundesrepublik Deutschland. *NTM: International Journal of History and Ethics of Natural Science* 16 (1): 73–101.

Revenko, I., and R. Proksch. 2000. Magnetic and Acoustic Tapping Mode Microscopy of Liquid Phase Phospholipid Bilayers and DNA Molecules. *Journal of Applied Physics* 87 (1): 526–533.

Rhodes, Richard. 1986. *The Making of the Atomic Bomb*. Simon & Schuster.

Rhodes, Richard. 1995. *Dark Sun: The Making of the Hydrogen Bomb*. Simon & Schuster.

Riggs, William, and Eric von Hippel. 1994. Incentives to Innovate and the Sources of Innovation: The Case of Scientific Instruments. *Research Policy* 23 (4): 459–469.

Riordan, Michael, and Lillian Hoddeson. 1997. *Crystal Fire: The Birth of the Information Age*. Norton.

Robinson, Arthur L. 1982. New Superconductors for a Supercomputer. *Science* 215 (4528) (January 1): 40–43.

Robinson, Arthur L. 1983. IBM Drops Superconducting Computer Project. *Science* 222 (4623) (November 4): 492–494.

Roco, M. C., R. S. Williams, and P. Alivisatos, eds. 1999. *Nanotechnology Research Directions: IWGN Workshop Report*. National Science and Technology Council.

Rohrer, H. 1990. Preface. In *Scanning Tunneling Microscopy and Related Methods*, ed. R. Behm, N. García, and H. Rohrer. Kluwer.

Rohrer, H. 1992. STM: 10 Years after. *Ultramicroscopy* 42: 1–6.

Rohrer, H. 1993. Limits and Possibilities of Miniaturization. *Japanese Journal of Applied Physics*, Part 1 32.3B: 1335–1341.

Roosth, Sophia. 2009. Screaming Yeast: Sonocytology, Cytoplasmic Milieus, and Cellular Subjectivities. *Critical Inquiry* 35 (2): 332–350.

Rosen, Paul. 1993. The Social Construction of Mountain Bikes: Technology and Post-modernity in the Cycle Industry. *Social Studies of Science* 23: 479–513.

Rosenberg, Charles. 1997. Toward an Ecology of Knowledge: On Discipline, Context, and History. In Rosenberg, *No Other Gods: On Science and American Social Thought*. Johns Hopkins University Press.

Rothaermel, Frank T., and Marie Thursby. 2007. The Nanotech Versus the Biotech Revolution: Sources of Productivity in Incumbent Firm Research. *Research Policy* 36 (6): 832–849.

Ruivenkamp, Martin, and Arie Rip. 2010. Visualizing the Invisible Nanoscale Study: Visualization Practices in Nanotechnology Community of Practice. *Science Studies* 23 (1): 3–36.

Saxenian, AnnaLee. 1994. *Regional Advantage: Culture and Competition in Silicon Valley and Route 128*. Harvard University Press.

Saxenian, AnnaLee. 2000. The Origins and Dynamics of Production Networks in Silicon Valley. In *Understanding Silicon Valley: The Anatomy of an Entrepreneurial Region*, ed. M. Kenney. Stanford University Press.

Schardt, Bruce C., Shueh-Lin Yau, and Frank Rinaldi. 1989. Atomic Resolution Imaging of Adsorbates on Metal Surfaces in Air: Iodine Adsorption on Pt(111). *Science* 243 (4894) (February 24): 1050–1053.

Schooley, James F. 2000. *Responding to National Needs: The National Bureau of Standards Becomes the National Institute of Standards and Technology, 1969–1993*. National Institute of Standards and Technology.

Schwarzschild, Bertram M. 1982. Microscopy by Vacuum Tunneling. *Physics Today* 35 (4): 21.

Shah, Sonali. 2005. From Innovation to Firm Formation in the Windsurfing, Skateboarding, and Snowboarding Communities. Working Paper, University of Illinois.

Shah, Sonali K., and Mary Tripsas. 2007. The Accidental Entrepreneur: The Emergent and Collective Process of User Entrepreneurship. *Strategic Entrepreneurship Journal* 1 (1): 123–140.

Shapin, Steven. 1994. *A Social History of Truth: Civility and Science in Seventeenth-Century England*. University of Chicago Press.

Shapin, Steven. 2001. Proverbial Economies: How an Understanding of Some Linguistic and Social Features of Common Sense Can Throw Light on More Prestigious Bodies of Knowledge, Science for Example. *Social Studies of Science* 31 (5): 731–769.

Shapin, Steven. 2008. *The Scientific Life*. University of Chicago Press.

Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton University Press.

Slaughter, Sheila, and Larry L. Leslie. 1997. *Academic Capitalism: Politics, Policies, and the Entrepreneurial University*. Johns Hopkins University Press.

Smaglik, Paul. 2002. Small World, Big Opportunities. *Nature* 418 (6899): 4–6.

Smith, Douglas P. E., Michael D. Kirk, and Calvin F. Quate. 1987. Molecular Images and Vibrational Spectroscopy of Sorbic Acid with the Scanning Tunneling Microscope. *Journal of Chemical Physics* 86 (11): 6034.

Sobel, Robert. 1981. *IBM: Colossus in Transition*. Times Books.

Speiser, Ambrose P. 1998. IBM Research Laboratory Zurich: The Early Years. *Annals of the History of Computing, IEEE* 20 (1): 15–28.

Strick, James. 1998. Against the Tide: Adrianus Pijper and the Debate over Bacterial Flagella, 1946–1956. *Isis* 87: 274–305.

Sweet, William. 1991. Bell Labs Reorganizes Research for More Competitive Environment. *Physics Today* 44 (6): 97–102.

Sweet, William. 1993. IBM Cuts Research in Physical Sciences at Yorktown Heights and Almaden. *Physics Today* 46 (6): 75–79.

Takayanagi, Kunio, Yasumasa Tanishiro, Shigeki Takahashi, and Masaetsu Takahashi. 1985. Structure Analysis of Si(111)-7 × 7 Reconstructed Surface by Transmission Electron Diffraction. *Surface Science* 164 (2): 367–392.

Taniguchi, Norio. 1974. On the Basic Concept of 'Nano-Technology'. In *Proceedings of the International Conference on Production Engineering*, Part 2.

Teague, E. Clayton. 1978. Room Temperature Gold-Vacuum-Gold Tunneling Experiments. Ph.D. dissertation, North Texas State University.

Teague, E. 1986. Room Temperature Gold-Vacuum-Gold Tunneling Experiments. *Journal of Research of the National Bureau of Standards* 91 (4): 171–233.

Theberge, Jane C. 1978. Anderson on Science: An Interview with the Nobel Prize Laureate. *Bell Laboratories Record* 56 (January): 3–8.

Thorpe, Charles, and Steven Shapin. 2000. Who Was J. Robert Oppenheimer? *Social Studies of Science* 30 (4): 545–590.

Tolles, William M. 1994. *Nanoscience and Nanotechnology in Europe, Report #NRL/FR/1003-94-9755*. Naval Research Laboratory.

Tolles, William M. 1996. Nanoscience and Nanotechnology in Europe. *Nanotechnology* 7 (2): 59–105.

Tománek, David, Steven G. Louie, H. Jonathon Mamin, David W. Abraham, Ruth Ellen Thomson, Eric Ganz, and John Clarke. 1987. Theory and Observation of Highly Asymmetric Atomic Structure in Scanning-Tunneling-Microscopy Images of Graphite. *Physical Review B: Condensed Matter and Materials Physics* 35: 7790–7793.

Tortonese, Marco. 1993. Force sensors for scanning probe microscopy. Ph.D. dissertation, Stanford University.

Toumey, Chris. 2005. Apostolic Succession: Does Nanotechnology Descend from Richard Feynman's 1959 Talk? *Engineering and Science* 68 (1): 16–23.

Toumey, Chris. 2008. Reading Feynman into Nanotechnology: A Text for a New Science. *Techné* 12 (3): 133–168.

Toumey, Chris. 2009. Truth and Beauty at the Nanoscale. *Leonardo* 42 (2): 151–155.

Travaglini, G., H. Rohrer, M. Amrein, and H. Gross. 1987. Scanning Tunneling Microscopy on Biological Matter. *Surface Science* 181 (1): 380–390.

Traweek, Sharon. 1988. *Beamtimes and Lifetimes: The World of High Energy Physicists*. Harvard University Press.

Trueb, Lucien. 2000. *National Research Program 36 Nanosciences Final Summary 1996–2000*. Swiss National Science Foundation.

Turner, Fred. 2006. *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism*. University of Chicago Press.

Uldrich, Jack. 2006. *Investing in Nanotechnology: Think Small. Win Big*. Adams Media.

Vesio, Lenora M. 1982. Research in the Face of Change. *Bell Laboratories Record*, October: 214–218.

Vettel, Eric James. 2006. *Biotech: The Countercultural Origins of an Industry*. University of Pennsylvania Press.

Villarrubia, John. 2001. The Topografiner: An Instrument for Measuring Surface Microtopography. In *A Century of Excellence in Measurements, Standards, and Technology: A Chronicle of Selected NBS/NIST Publications, 1901–2000*, ed. D. Lide. Special publication 958, National Institute of Standards and Technology.

Vincenti, Walter. 1990. *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History*. Johns Hopkins University Press.

Wan, K. J., X. F. Lin, and J. Nogami. 1992. Reexamination of the Ag/Si (111)- $\sqrt{3}\times\sqrt{3}$ Surface by Scanning Tunneling Microscopy. *Physical Review B: Condensed Matter and Materials Physics* 45: 9509–9512.

Wandass, Joseph H., James S. Murday, and Richard J. Colton. 1989. Magnetic Field Sensing with Magnetostrictive Materials Using a Tunneling Tip Detector. *Sensors and Actuators* 19 (3): 211–225.

Welker, Nancy K., and Fernand D. Bedard. 1978. Digital Josephson Technology—Present and Future. *AIP Conference Proceedings* 44 (1): 425–436.

Wienroth, Matthias, and Matthew B. Kearnes. Forthcoming. *From Difficult Birth to Silent Decline: Nanotechnology Governance in the UK from 1986 to 2010*. Chemical Heritage Foundation.

Wilson, R. J., and S. Chiang. 1987. Structure of the Ag/Si(111) Surface by Scanning Tunneling Microscopy. *Physical Review Letters* 58 (4) (January 26): 369.

Wise, George. 1985. *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research*. Columbia University Press.

Wisnioski, Matt. 2003. Inside “the System”: Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s. *History and Technology: an International Journal* 19 (4): 313.

Wry, Tyler, Royston Greenwood, P. Devereaux Jennings, and Michael Lounsbury. 2010. Institutional Sources of Technological Knowledge: A Community Perspective on Nanotechnology Emergence. In *Technology and Organization: Essays in Honor of Joan Woodward*, ed. N. Phillips, G. Sewell, and D. Griffiths. Emerald Group.

Yi, D. 2008. The recombinant university: Genetic engineering and the emergence of biotechnology at Stanford, 1959–1980. Ph.D. dissertation, Princeton University.

Young, Russell, John Ward, and Frederic Scire. 1972. The Topografiner: An Instrument for Measuring Surface Microtopography. *Review of Scientific Instruments* 43 (7): 999–1011.

Young, Russell D. 1966. Field Emission Ultramicrometer. *Review of Scientific Instruments* 37 (3): 275–278.

Young, Russell D. 1971. Surface Microtopography. *Physics Today*, November: 42–49.

Young, Russell D. 1976. Microtopography. *Lubrication Engineering* 32 (8): 439.

Young, Russell D. 1980. Frontiers in Microtechnology. *Precision Engineering* 2: 3.

Young, Russell D., and Frederic Scire. 1972. Precision Reference Specimens of Surface-Roughness—Some Characteristics of Cali-Block. *Journal of Research of the National Bureau of Standards. C, Engineering and Instrumentation* 76: 21–23.

Young, Russell D., John Ward, and Frederic Scire. 1971. Observation of Metal-Vacuum-Metal Tunneling, Field Emission, and Transition Region. *Physical Review Letters* 27 (14): 922–924.

Zasadzinski, J. A. N., J. Schneir, J. Gurley, V. Elings, and P. K. Hansma. 1988. Scanning Tunneling Microscopy of Freeze-Fracture Replicas of Biomembranes. *Science* 239 (4843): 1013–1015.

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