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Instruments of Commerce and Knowledge
Probe Microscopy, 1980–2000

Cyrus C. M. Mody

9.1 Introduction

Universities have long struggled to define their relation to the business world (Geiger 2004). How much should professors be involved in commercial activities? How much influence should firms have on university policies? How much should universities themselves be run like for-profit businesses? Today, much of the U.S. science and engineering workforce is trained at institutions where professional start-up companies are the norm and where much corporate research has been outsourced to academic groups. This situation differs markedly from that of the period between 1945 and about 1970, when federal funding for basic research was so large that academics did not need to rely on corporate support, and companies like IBM and AT&T could run big laboratories doing high-quality basic research (Mirowski and Sent 2007).

As with any trend in higher education, emotions run high in discussions of university-industry relations. Critics see "academic capitalism" (Slaughter and Leslie 1997) as leading to the exploitation of students, the neglect of teaching, and the distortion of scientific knowledge to meet commercial patrons' needs (Bok 2003; Kirp 2003). Proponents see it as enticing professors out of their ivory tower and making universities more responsive to changing public (and market) demands (Etzkowitz 2002). Proponents call for more direct incentives for professors to commercialize their discoveries; critics call for a return to a (probably nonexistent) golden

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age when academic scientists did not need to justify every discovery in terms of its commercial applications.[1]

Most of these arguments are couched in very abstract terms. Where we do have detailed, empirical studies of academic capitalism, they have tended to be broad surveys or examinations of a small selection of regions and universities, especially Stanford, MIT, Silicon Valley, and Route 128 (Vettel 2006; Lécuyer 2006; Kenney 2000). These studies reveal much, but they neglect key aspects of the way science is actually conducted. Most researchers participate in networks that are geographically dispersed and that include colleagues in both academia and industry and from a variety of disciplines. To understand the commercialization of academic knowledge, we need a multiinstitutional, multidisciplinary, multiregional unit of analysis—what I will call an "instrumental community." By this I mean the porous group of people commonly oriented to building, developing, using, selling, and popularizing a particular technology of measurement.[2] Such communities are instrumental primarily in focusing on new research tools—microscopes (Rasmussen 1997), fruit flies (Kohler 1994), tobacco mosaic virus (Creager 2002), lab rats (Rader 2004), ultracentrifuges (Elzen 1986), and so forth. Because such communities usually include academic and commercial participants, though, they will often seek ways to morph those tools into industrially-relevant devices. Thus, such communities are also instrumental in focusing on new ways of doing or making things.

There are a number of excellent case studies of various instrumental communities, spanning from the seventeenth century to the 1960s (Shapin and Schaffer 1985; Jackson 2000; Pantalony 2004; Bromberg 1991; Lenoir and Lécuyer 1995). Yet there have been virtually no studies of instrumental communities that have arisen since the late 1970s. We know that there have been significant changes in legislation, federal funding, corporate research, and the demographics of science in the past three decades. We do not know how those changes have affected the operation of instrumental communities, nor how they have affected relationships between corporate and academic members of those communities. This chapter aims to bring these issues to the fore through a case study of the development and commercialization of the scanning tunneling microscope (STM) and its near-relatives, the atomic force microscope (AFM) and magnetic force microscope (MFM)—known collectively as probe microscopes.[3] In 1981, there

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[1] Shapin (2003) casts doubt on the assumptions underlying both these positions.

[2] An "instrumental community" bears a close resemblance to the "innovation communities" analyzed by Shah (2003). "Instrumental community" is—so far as I know—my own formulation, but others have covered very similar ground, especially Blume (1992) and Shinn (1997).

[3] The technical details of the microscopes are important to this story, but can be glossed for the purposes of this chapter. Basically, all scanning probe microscopes bring a small, solid probe very close (usually to within a nanometer—one billionth of a meter) to a sample and measure the strength of different kinds of interactions between probe and sample to deter-

was only one, homemade, unreliable STM at the IBM research lab in Zurich. Today, through the joint efforts of corporate and academic researchers, there are thousands of AFMs, MFMs, and STMs at universities, national labs, and industrial research and quality control facilities. High school students make STMs from Legos, while chip manufacturers use million-dollar AFMs on the factory floor. One AFM has even made it to the surface of Mars.

Using the instrumental community as a unit of analysis allows us to approach the same issues that motivate the other chapters in this volume, but from a different perspective and with a different methodology. Looking at the dynamics of an instrumental community can help us understand several things: some reasons why professors commercialize their research; some ways the training of graduate students and postdocs is linked to the needs of companies their supervisors are associated with (Davis, chapter's, this volume); ways to interpret regions' rates of retention of their science and engineering graduates (Sumell, Stephan, and Adams, chapter 8, this volume); and ways gender and ethnic diversity can make for more robust (and commercial) knowledge (Whittington, chapter 6, this volume).

Unfortunately, an instrumental community is a nebulous, intangible, unstable grouping that would be very difficult to study via the methodologies of the other chapters in this volume. Surveys of scientists and engineers, for instance, work well when there are institutions that map closely to the group being surveyed: universities offer an infrastructure for surveying recent Ph.D.s.; professional societies (and their journals) offer an infrastructure for surveying members of specific disciplines; funding agencies offer

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mine the height (and other characteristics) of the sample. The probe is then rastered much like the pixels on a TV screen and a matrix of values for the strength of the tip-sample interaction is converted into a visual "picture" of the surface. Different probe microscopes use different kinds of tip-sample interactions to generate their images. The earliest probe microscope, the STM, works by putting a voltage difference between the tip and a metal or semiconductor sample; when the tip is brought close to the sample, some electrons will quantum mechanically "tunnel" between them. The number of electrons that do so (the "tunnel current") is exponentially dependent on the distance between tip and sample; also, the stream of tunneling electrons is very narrow. Thus, an STM has ultrahigh resolution both vertically and laterally—most STMs can actually see individual atoms on many samples. Today, the STM's younger cousin, the atomic force microscope, is more commonly used. An AFM uses a very small but flexible cantilever as a probe. As the tip of the cantilever (usually weighted with a small pyramid of extra atoms) is brought close to the surface, the cantilever bends due to the attraction or repulsion of interatomic forces between tip and sample. The degree of bending is then a proxy for the height of the surface. Originally this bending was measured by putting an STM on the back of the cantilever; today the deflection is detected by bouncing a laser off the cantilever and measuring the movement of the reflected spot. Another common and industrially-relevant tool, the magnetic force microscope, works in a similar way, but uses a magnetic tip to map the strength of magnetic domains on a surface, rather than surface height. Both the AFM and MFM have slightly less resolution than the STM (i.e., they cannot usually see single atoms); yet because they (unlike the STM) can be used on insulators as well as conductors, and in air and fluids as well as vacuum, they have become much more popular.

an infrastructure for surveying their grantees. Often, no institution maps well to an instrumental community. Part (but only part) of the probe microscopy community was affiliated with a professional society—the American Vacuum Society—and probe microscopists tended to publish in many different journals and get funding from many different sources. The closest the community came to owning an institution were the annual (later biennial) STM Conferences. Yet even these only drew part of the community and, over the years, evolved into general nanotechnology conferences rather than meetings devoted to a specific class of instrumentation.

I have opted to study (and partially define) the instrumental community through extensive oral history interviewing. Interviews can be a problematic methodology: people misremember or mislead, interviewers ask leading questions, important people can often spare little or no time for an interview, one cannot interview everyone, or even know exactly who to talk to. Still, interviews allow scientists and engineers to map out their relevant communities for themselves, rather than having an institution do it for them. That is, they can tell the interviewer who they were working with, who they thought of as peers and competitors, with whom they were sharing ideas, and so forth. By then interviewing *those* people, the historian can delineate the network of relationships that make up an instrumental community.

In the end, interview and survey data should be complementary. My interviews, for instance, revealed some rather counterintuitive motivations for people to found start-up companies. Unfortunately, interviews cannot show how common such motivations are across the science and engineering community. Such motivations can, however, be folded into future survey questionnaires. Similarly, many of my interviews were informed by quantitative data from other studies. For instance, many interviewees in this study were people who were postdoctoral fellows when they first built or used a probe microscope. Those people could easily have been invisible in my study had I not seen quantitative data on the evolving nature of the postdoc as an institution of American science.

Letting the participants map out the boundaries of their instrumental community allows us to see the wide variety of relationships linking nodes in this network: student-teacher, buyer-seller, funder-grantee, supervisor-postdoc, inventor-early adopter, editor-author, and so forth. Indeed, it is the diversity of such linkages in the network that makes an instrumental community robust. Instrumental communities are also made more robust by the variety of the linkages connecting universities and corporations. Both proponents and critics of academic capitalism in science tend to focus on a small subset of corporate-academic relationships: professorial start-ups, patenting of academic research, and corporate sponsorship of academic research.

This chapter will explore a much wider range of relationships, however.

These include: researcher sabbaticals from firms to universities and vice versa, technology transfer through firms' hiring of former graduate students and universities' hiring of former corporate researchers, corporate sponsorship of community-building activities such as conferences, corporate influence over researchers' choices of materials to characterize with their microscopes, corporate supply of parts for building microscopes (and academic feedback to the design of those parts), and corporate sponsorship of intramural research to stimulate formation of an extramural academic market. Most of these kinds of relationships are invisible in the debate about academic capitalism. Yet we shall see that other forms of commercialization of academic research (e.g., professorial start-ups) actually have their roots in these less-noticed dynamics of an instrumental community.

9.2 Inventing and Community-Building

Invention, though often praised in the abstract, can be problematic for corporate scientists and engineers. Inventions can emerge from digressions from assigned tasks, and may not meet any commercial objective. Corporate inventors often need to defy their managers to promote their innovations. The STM was this kind of institutional orphan. Its inventors, Gerd Binnig and Heini Rohrer, had been tasked in 1978 with finding new ways to characterize thin films for an advanced supercomputer project on which IBM had staked much of its reputation. Yet by the time they came up with the STM, the supercomputer project had been canceled (Binnig and Rohrer 1985, 1987). Binnig and Rohrer's response was threefold. First, they temporarily hid the STM from managerial oversight. Second, they began querying IBM colleagues about new applications for their microscope, eventually attracting interest from the company's semiconductor surface scientists.

Their third critical strategy was to cultivate an extramural, academic community. By convincing colleagues at universities to replicate the instrument, they could point to extramural interest as a reason why their managers should let them continue developing the STM. Indeed, the interest in STM both inside and outside Big Blue convinced IBM's senior research managers that the STM—despite the absence of commercial relevance—should become a major corporate project. Multiple groups of scientists at the IBM laboratories in Zurich; Yorktown Heights, New York; and San Jose, California were recruited to build STMs and make discoveries that would bring credit to the instrument and to the company. In turn, IBM's research archrival, Bell Labs, saw a need to steal Big Blue's thunder and began recruiting its own cadre of STMers.

The dynamics of building the STM community show how the corporate and academic worlds are interpermeated much more than is noticed in debates about academic capitalism. Binnig and Rohrer could quickly find and convince academics to build their own STMs because of networks of personnel exchange between IBM and various universities. Some replicators were professors taking sabbaticals at IBM, some were academics Rohrer had known from his own sabbaticals at universities, and some were people who had been postdocs at IBM or currently had students serving postdoctoral appointments there.[4]

Similarly, interest in the STM grew within IBM and Bell Labs not because it could solve commercially-relevant problems, but because it could generate credible knowledge within academic disciplines such as physics and surface science.[5] Accolades from an academic audience—evidenced by standing-room-only crowds at American Physical Society meetings, the awarding of the Nobel Prize to Binnig and Rohrer in 1986, and the growth of academic STM—were largely the aim of IBM's STM program. Among other things, prestige within a hot new instrumental community like tunneling microscopy allowed IBM to recruit the best graduate students as postdocs and junior researchers. Some of those persons in turn built the second and third generations of IBM's tunneling microscopes.

9.3 Dynamics of Community

By 1986, the STM was no longer in any danger. An instrumental community was beginning to take shape, and was even beginning to organize itself through an annual conference series. In the first few years, those conferences (and the community as a whole) was dominated by corporate groups, especially from IBM and Bell Labs. Early academic STMers, such as Paul Hansma at the University of California at Santa Barbara (UCSB), Calvin Quate at Stanford, and John Baldeschwieler at Caltech, were important contributors to the community. Yet these academics struggled to compete with better-resourced corporate groups. The IBM and Bell Labs STMers also had the advantage of proximity to other STM groups housed in their same buildings. While there was often intense competition between groups that were working for the same organization, their copresence did allow the tacit knowledge (Polanyi 1962; Collins 1975) needed to build an STM to flow more quickly at IBM and Bell Labs than at more isolated locales such as Stanford and UCSB.

Bell Labs and IBM were both traditionally strong in computing and microelectronics research. Their early dominance of the STM community

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[4] The source material for this study is a collection of interviews with over 150 probe microscopists conducted between 2000 and 2004. I will reference specific oral histories using an alphanumeric code listed in the appendix to this chapter. Information about the corporate-academic network of sabbaticals and hires came from, among others, <TB1>, <JM1>, and <PH1>.

[5] There is rich historical material on the large, corporate labs of the twentieth century: Wise (1985); Riordan and Hoddeson (1997); Bassett (2002); Knowles and Leslie (2001).

meant, therefore, that early STMers largely used their microscopes to study materials used in microelectronics manufacturing. In particular, Binnig and Rohrer were most successful in enrolling colleagues interested in the surface structure of metals and semiconductors. A few semiconductor surfaces (especially of silicon) became yardsticks for measuring whether a group had a working STM or not—until a group's STM had resolved single atoms of silicon, its builders could not enter the top tier of STM builders.[6] Both corporate and academic STMers were evaluated in this way. However, there was considerably more local knowledge about preparing silicon specimens at Bell Labs and IBM than in academic groups such as Quate's or Baldeschwieler's.

Thus, the accreditation standards of the early STM community favored corporate groups. Binnig and Rohrer, however, were keen to undo their own company's lead in STM research. Thus, they began looking for new applications for the STM that would not interest IBM management, but where academic researchers could move forward quickly. In Europe, for instance, Binnig and Rohrer collaborated with academics to explore applications for the STM in electrochemistry and biophysics. In the United States, Binnig visited Stanford for more than a year to help Quate's group think of new uses for the STM, while traveling around to help other groups get their microscopes running. And Rohrer dispatched Binnig to Santa Barbara (where Rohrer had taken a sabbatical several years earlier) to convince Paul Hansma to adapt the STM to do vibrational spectroscopy of molecules.

Soon, the American STM community began to segregate into two moieties—surface science STMers, dominated by (but not exclusive to) corporate and national laboratories on the East Coast; and nonsurface scientists, dominated by (but not exclusive to) universities on the West Coast.[7] These two moieties continued to share a great deal. Members of each occasionally collaborated, and a few people moved from one to the other. More importantly, the basic design of the STM was—in the late 1980s—common to both, so design innovations in one moiety could be transported to the other. This meant that opportunities for copresence—conferences and visits and sabbaticals between labs—continued to be useful for both moieties until the early 1990s. Yet the two moieties did differ markedly on some points of STM design and use. In particular, surface science STMers built their microscopes for compatibility with ultrahigh vacuum (UHV) chambers, so as to keep their metal and semiconductor samples pristine. These chambers were, however, large, finicky, expensive, and time-consuming. Academics

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[6] <JD2>, <PW2>.

[7] Crucial corporate members of the latter, predominantly academic, moiety were Quate's allies within IBM: Dan Rugar, John Foster, and Tom Albrecht (former students who worked at IBM Almaden); Kumar Wickramasinghe (a former postdoc, later at IBM Yorktown); and Gerd Binnig (who took a sabbatical at Stanford from 1985 to 1986).

like Quate and Hansma, who were less interested in studying pristine semiconductor samples, therefore developed easier, cheaper variants of STM that did not require a vacuum chamber, such as doing tunneling microscopy of samples exposed to open air, or immersed in water, oil, or a variety of different gases.[8]

Surface science STMers had a well-defined set of questions to ask and materials to study. By branching into new uses of STM, Quate, Hansma, and Baldeschwieler freed themselves from the constraints of surface science, but they also forfeited the structure that a discipline like surface science can supply. They could better afford to temporarily put aside discipline than the younger STMers in the corporate labs because Hansma, and especially Quate and Baldeschwieler, were all tenured faculty with long track records of inventing instruments, getting grants, and winning acclaim from their colleagues. The STM was, for them, a chance to start *over,* rather than (as it was for the young corporate STMers) a chance to start *off.*

Still, by going down this road, they now had little idea what materials to look at, what questions to ask, how to interpret their data, or what audiences might be interested in their work. To answer those questions, they encouraged their students to quickly build a wide variety of microscopes and to playfully use them to characterize haphazard materials—leaves of houseplants, polaroids, bone from ribeye steaks, ice, the electrochemistry of Coke versus Pepsi, and so forth.[9] This undisciplined, shoestring bricolage extended even into microscope-building: the Baldeschwieler group made STM probes from pencil leads, for instance, while the Hansma group made AFM tips from hand-crushed pawn shop diamonds, glued to tin foil cantilevers with brushes made from their own eyebrow hairs.

Yet such indiscipline could damage STM's acceptance by new disciplinary audiences, since the STMers' ways of preparing samples and interpreting images might not be credible to biologists, electrochemists, materials scientists, geologists, and so forth. Thus, Quate, Hansma, and other academic STMers began bringing representatives (postdocs or young professors) from potential new disciplinary audiences in to work with their students, learn how to use the microscope, show the group how to prepare samples, and then proselytize for the technique within their home community. Quate tended to recruit postdocs himself and share them with other Stanford faculty.

Hansma actively sought collaborations with young faculty both at UCSB and elsewhere, but he was more haphazard about postdocs, taking in people who brought their own money and expertise but not seeking them out. Nevertheless, he had a long string of such visitors, since by the late 1980s graduating Ph.D.s in molecular biology or electrochemistry

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[8] <CP1>, <PH1>.

[9] <CP1>, <JN1>.

could see that if they learned to use STM or AFM they would be able to understand their discipline's canonical samples in a way that none of their disciplinary colleagues would. At both Stanford and UCSB, some of these people took a microscope with them when they left, some founded their own microscope-building groups at other universities, and some used their knowledge of probe microscopy as a tool for gaining acceptance among disciplinary colleagues and securing tenure from their universities.[10]

Thus, the differences between the two moieties were as much about pedagogy and career arc as they were about samples, designs, and audience. In groups such as Quate's and Hansma's, graduate students were trained to build instruments quickly and collaboratively, to think primarily about novel design rather than use. Postdocs in those groups, meanwhile, were trained to develop new uses for the microscopes, and to integrate them into various established disciplines—STM for biology, materials science, electrochemistry, and so forth.

In the corporate labs, postdocs and young staff scientists also underwent a kind of training. At the time, Bell Labs was considered one of the preeminent research institutions in the world in a variety of fields, especially solid state physical sciences. In a few areas, especially those relevant to the STM such as surface science and semiconductor physics, IBM ran neck-and-neck with Bell Labs. Thus, postdocs in these organizations had the opportunity to do exciting, cutting-edge work. But they also had to compete hard to remain in that rarefied world. To do so, they needed to convince the large numbers of senior managers and surface scientists/semiconductor physicists within these organizations that they could contribute rigorous knowledge to those disciplines. The young corporate STMers therefore learned to build and use microscopes geared specifically to the questions and materials of surface science. Indeed, the most helpful managers were those who directed postdocs to see the disciplinary apparatus of surface science as a way to define problem areas—a kind of functional equivalent of the research/career plans that Davis (chapter 3, this volume) discusses. After they had established themselves they could branch out somewhat, but early on the young corporate STMers all built relatively similar microscopes to look at the same handful of samples—though with enough variation to demonstrate their builders' individual initiative, creativity, and experimental ingenuity.[11]

In other words, the instrumental community growing around the STM included elements of pedagogy at all participating sites, rather than just in the academic groups—the STM was a technology for turning young researchers into full-fledged scientists as much as a new technique for char-

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[10] <AG1>, <HG1>, <JN1>. The propagation of a technique through the cascade of postdocs and collaborators away from one of the centers of an instrumental community is described in Kaiser (2005).

[11] <BW2>.

acterizing materials. Analysts of academic capitalism should keep this in mind—universities have no monopoly on scientific training. Moreover, in this particular case the pedagogical uses of the STM encouraged a wider division of labor in the instrumental community. Because young corporate STMers had such a monopoly on metal and semiconductor samples, graduate students building STMs were instead encouraged to expand the instrument's capabilities into new areas.

Critics of academic capitalism often complain that corporate influence can restrict academic researchers' focus too narrowly—that only those lines of research that might be profitable are pursued. In some cases, such influence clearly can be detrimental to the conduct of science. In other cases, such as the early STM community, corporate influence actually prompted academic research to adopt a diversity of approaches and a more expansive outlook.

9.4 Building and Buying

Until 1986, all probe microscopes (whether corporate or academic) were home-built, in that they were put together by the groups that were using them. Yet home-built instruments were not made entirely from scratch—some components were made by hand, but most were bought from commercial suppliers. The STM designs were strongly shaped by the commercial availability of components such as operational amplifiers, and high-grade materials such as platinum-iridium alloy. In some cases, STM builders simply ordered these items from catalogs. In other cases, they were active consumers, lobbying companies to modify products (vacuum chambers, piezoelectric crystals, video output devices, etc.) to suit their needs.[12]

Thus, STMers were both consumers and producers of equipment. This is important to note because most proponents of academic capitalism focus solely on academics as *producers* of marketable knowledge and goods. Their recommendations for achieving a more commercial university therefore center on stimulating professorial start-up companies. Yet universities may find that the best way to gain influence over an instrumental community is by encouraging professors to be savvy, active *consumers* who can trade their expertise for favorable deals from manufacturers.

In a number of indirect and often counterintuitive ways, commerce supplied the infrastructure needed to make the STM community grow. Information about commercial sources of reliable components and materials was

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[12] <DF1>. Much recent history of technology has focused on the active role of users. For consumers' adaptations of artifacts for uses that manufacturers were unaware of, or even opposed, see Kline and Pinch (1996). For users' pressure on companies (often—as in instrumental communities—through threats to form their own cooperatives or firms), see Fischer (1992). For an overview of different kinds of user activity, see the essays in Oudshoorn and Pinch (2003).

a major topic of gossip among early STM builders. Those who had built working instruments offered blueprints and recommended particular commercial suppliers of components to new members of the community. Those newcomers, anxious to make up for lost time, rarely questioned their predecessors' advice, so that STM-building came to resemble doing a project from *Popular Mechanics.* STM-building became standardized through STMers' nearly ritualistic allegiance to recommended suppliers, even when the technical rationale for their components disappeared. For instance, IBM's STMers used a trademarked rubber called Viton (from Dupont) to dampen vibration, because Viton could survive ultrahigh vacuum.[13] Later, as IBM's blueprints disseminated, Viton continued to be widely used even in academic STMs that operated in air or fluid, not vacuum.[14]

A commercial infrastructure also helped STMers standardize the materials they looked at with their microscopes. As Daniel Lee Kleinman (2003) has noted, corporate influence over the choice of research materials is pervasive but indirect. Tapping into the right commercial infrastructure can be crucial to growing an instrumental community. Reliable, cheap commercial sources of materials give newcomers easy access to research, and give the rest of the community a yardstick by which to measure newcomers' progress. Among corporate surface science STMers, this yardstick was provided by a few key semiconductor samples. Newcomers had to prove that their microscopes could resolve individual atoms of those canonical semiconductor samples to gain entry to the instrumental community.

Atoms can (ordinarily) only be seen on semiconductor samples when they are kept in ultrahigh vacuum. Thus, when academic STMers designed microscopes for use in air and water, they could no longer use the canonical semiconductor surfaces as a standard of microscope-building ability. Quate, Hansma, and others looked desperately for new yardstick materials. Gold, paraffin, and graphite vied for the job, but graphite won out partly because ultrapure samples could be obtained cheaply from commercial sources.[15] Union Carbide used graphite to make monochromators for neutrons, an application requiring extraordinarily pure samples; hence, they rejected large amounts of slightly imperfect graphite still pure enough for STMers. The Quate group heard about this and alerted other academic groups who then called Union Carbide's graphite man, Arthur Moore, to get cheap, more or less standardized samples. By 1989, the STM community was awash in graphite, such that talks about that material outnumbered talks on semiconductors at the annual STM conferences.

Sometimes, the industrial relevance of materials was a direct influence acting on academic microscopists, feeding back into the designs of their in-

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[13] <CG1>, <RT1>, <VE1>.

[14] For similar instances of practices spreading through an experimental community through transmission of knowledge about particular brands, see Jordan and Lynch (1998).

[15] <AG1>.

struments. Cal Quate, for instance, framed his STM work within Stanford's long tradition of industrial ties and his own involvement in developing acoustic microscopy in the 1970s as a nondestructive characterization tool for manufacturing.[16] Nondestructive testing held tremendous promise for microelectronics, where chips must be inspected throughout the manufacturing process, yet where traditional testing tools (especially electron microscopy) require breaking and discarding expensive silicon wafers. Quate moved into STM believing it could be the next generation nondestructive evaluation tool for the microelectronics industry. He was then able to inspire his former students and postdocs at microelectronics giants such as IBM to follow his lead and join the STM community.[17]

The STM, though, requires a conducting (metal or semiconductor) sample, whereas most microelectronic materials have an insulating oxide layer. This was unproblematic for corporate surface scientists tasked with generating basic knowledge about materials like silicon and gallium arsenide. Yet STM's restriction to conducting materials blocked its use in nondestructive testing or in industrial quality control more generally. So when IBM allowed Gerd Binnig to take a sabbatical at Stanford from 1985 to 1986, he and Quate adapted the STM so it could use interatomic forces to map insulating materials, calling their new invention the atomic force microscope (AFM). Thus, Quate positioned his research much further downstream in IBM's R&D cycle than most of IBM's own STMers and, together, IBM and Stanford dramatically shifted the world of academic and corporate probe microscopy.

9.5 Commercialization and Gray Markets

What we have seen so far, then, are the more intricate, unglamorous ways corporate and academic actors are linked within an instrumental community: through pedagogy, through institutional politics, through commercial infrastructures, and through tacit knowledge. These are not the relationships that exercise most analysts of academic capitalism. Instead, both proponents and critics tend to focus on large corporate buy-ins to academic departments, professors keeping research secret so they can patent it, and corporations and universities colluding to suppress unfavorable results.

One topic central to the academic capitalism debate will occupy the rest of this chapter—the commercialization of academic research and the founding of professorial start-up companies. The commercialization process was not sudden, dramatic, and profit-driven, but built slowly and qui-

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[16] <JF1>, <DR1>, <MK1>. "Nondestructive testing" means that the process of quality control testing does not damage the item being tested. Products can be taken off the assembly line while half-finished, inspected, then returned to the assembly line. See Quate (1985) for a brief description of scanning acoustic microscopy at Stanford.

[17] Quate's optimism for STM derived from its ultrahigh resolution and the fact that (ideally) the STM tip does not touch (and thereby mar) the sample surface.

etly from the kinds of practices I have described thus far. Commercialization of probe microscopy was driven less by profit-seeking and more by the desire of elite STMers to grow a larger instrumental community in which their groups would be centers of expertise. This desire was strong among both corporate and academic groups, though the motivations differed in the two moieties. At IBM and (to a lesser extent) Bell Labs, research managers wanted to increase the number of in-house STM groups, so as to keep the center of the STM community within the corporation. Academics like Quate and Hansma wanted to grow the STM (and AFM) community because they were looking for new applications and audiences, and because they wanted to build a critical mass of researchers committed to nonsurface science probe microscopy.

Both Bell Labs and IBM built something like an internal free market for tunneling microscopy, with multiple groups in different parts of the organization given similar tasks and competing for the attention of senior managers. Both companies also developed an infrastructure for STM research that allowed new lab groups to get up to speed very quickly. For instance, Bell Labs housed several (varying between two and four) STMs in an old tractor shed on the edge of its property. There, microscope builders could very quickly trade ideas, materials, blueprints, and software—very much in the same way that Quate's students worked on multiple microscopes at once and cannibalized parts and design ideas from one project to another.[18]

IBM took the internal STM market/infrastructure to even greater lengths. IBM had been first into STM, yet it took other IBM groups just as long—almost two years in some cases—as everyone else to replicate Binnig and Rohrer's microscope. Thus, senior management cast about for ways to package the tacit knowledge of instrument-building and reduce replication time. The preferred strategy was to make semistandardized, batch-produced (Scranton 1997) STM packages available to its researchers. The first was the "Blue Box" designed by Othmar Marti, a Swiss graduate student doing doctoral work at IBM Zurich.[19] The Blue Box was primarily an electronics package—researchers constructed the hardware themselves, often using Binnig and Rohrer's designs. The STM electronics presented a significant challenge; complicated feedback circuitry brings the probe to the surface, reads out and controls the tunnel current, and rasters the tip without crashing. The success of the Blue Box in allowing newcomers to work around these difficulties inspired a more ambitious effort at IBM Yorktown. There, Joe Demuth, manager of an STM group, assigned his postdocs to work with Yorktown's Central Scientific Services (CSS) shop to develop and batch-produce complete STMs to "sell" to other Yorktowners.[20]

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[18] <JG3>, <BS1>.

[19] <OM1>, <JG1>.

[20] <BH1>, <RT1>, <JD2>.

By 1990, about a dozen of these CSS STM's were in use at Yorktown and the nearby Hawthorne facility; some also accompanied former IBM postdocs when they left to become professors.[21] Yorktown management encouraged use of the CSS STM by making its purchase a zero-cost budget item for each research group. Still, groups had to invest labor—usually a postdoc—to make the microscope productive. This confronted its postdoc users with a dilemma. They needed to creatively solve technical problems and display initiative to managers to advance to staff positions. This meant they needed to radically reconstruct the CSS STM to show off their skills. Postdocs also found that the CSS STM pulled them into intense institutional politics. Postdocs using the CSS STM found that competing Yorktown groups viewed them as partisans of Demuth's style of microscopy. Thus, there was great institutional pressure on these postdocs to disavow the CSS STM by rebuilding and transforming it.[22] The culture of research at Yorktown made it impossible to view the CSS microscope as a ready-to-use black box (Latour and Woolgar 1986), and therefore precluded any possibility of its commercialization outside IBM.

In contrast, commercialization was more successful from academic STM and AFM groups largely because of the outward-looking, multidisciplinary style they had cultivated in order to avoid competing with the surface scientists at IBM and Bell Labs. People like Binnig, Rohrer, Quate, and Hansma were extraordinarily open with newcomers, freely offering blueprints and advice in order to build a critical mass of nonsurface science probe microscopists. Thus, the circulation of materials and ideas—a kind of "gray market"—became the norm in academic STM and AFM. Software (to control probe and display images) was particularly easy to distribute, and the groups that gave it away both won goodwill within the instrumental community, and ensured access to modifications to the software that their collaborators came up with.[23] Sometimes code was given for free, sometimes at nominal cost. Profit was not the motive for dissemination.

A well-traveled *hardware* innovation was the microfabricated AFM cantilever. One perceived defect of early AFMs was that probes were laboriously handmade from small strips of aluminum foil with a tiny sliver of diamond glued on one side and a tiny shard of glass on the other.[24] Although these cantilevers could yield exquisite AFM images, each required considerable time and training to make, and results were so particular to one cantilever and its maker that images taken with different cantilevers were diffi-

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[21] <DB1>.

[22] <JV1>, <BW2>.

[23] <MS1>.

[24] Diamonds were used as tips because their sharp points were less likely to wear down from repeated use than other materials. The glass on the back of the cantilever acted as a small mirror, bouncing laser light into a photodiode; the position of the reflected beam in the photodiode indicated how much the cantilever was bending (i.e., a proxy for how much the surface was pulling or pushing on the diamond tip).

cult to compare. Handmade cantilevers sufficed early on, when every image was new and spectacular. But as the technique matured, AFMers sought standardization. The Quate group delivered this by integrating itself with microlithography expertise at Stanford and around Silicon Valley. Over several years, Quate sent his students to other electrical engineering professors at Stanford to learn the microelectronics industry's techniques for patterning and etching silicon. The students adapted those techniques to make batches of small, standardized silicon cantilevers. By 1990, Quate was sending surplus probes to friends and collaborators, sometimes so he and his students could share authorship on those collaborators' papers. Quickly, Quate-type probes became essential to AFM research.[25]

Quate's and Hansma's multidisciplinary collaborations prepared the ground for commercialization in other ways. These collaborators would usually found their own STM or AFM groups at other universities and effectively advertise for the technique, building interest in probe microscopy among biologists, electrochemists, mineralogists, and so forth. Eventually interest from those disciplines would turn them into markets for commercial STMs and AFMs. At the same time, Quate's and Hansma's graduate students learned to deal with potential "customers" from other disciplines and to design microscopes with their needs in mind. The leap from these practices to outright commercialization was very small.

The first to make this leap was a Quate student, Doug Smith, who founded the Tunneling Microscope Company in 1986. Smith had only one employee, a fellow student who helped put together scanners, and he recruited customers by word of mouth. He viewed the company less as an ongoing enterprise than as a way to sweeten the hardships of graduate school—a well-circulated story is that he sold just enough microscopes to buy a BMW before taking a postdoctoral fellowship. Quate himself pushed Smith to separate scholarship and business more cleanly: "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 and move out."[26]

On the demand side, Smith's customers were in much the same position as the postdocs at IBM who were presented with the CSS STM. They saw a commercial STM as a way to quickly catch up and join a hot new instrumental community. Yet they knew that if they were to join the elite of that community, they would have to demonstrate instrument-building virtuosity on their own. Thus, like the batch-produced IBM instruments, Smith's commercial STMs were more starter kits than black-boxed devices. To use the instrument, customers needed to construct much of it themselves.[27] All

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[25] <TA1>, <MK1>, <BD2>.

[26] <MK1>.

[27] <JF1>, <NB1>, <RC1>.

Smith sold was the microscope “head”—the piezoelectric scanner, tip, base, and vibration-isolating stacks of Viton. Customers built the electronics themselves, customizing the microscope for their own applications.

Today, when customers buy an off-the-shelf AFM, they generally are buying all of the expertise of microscope-building so that they will not have to develop it themselves. Early on, that expertise was exactly what customers of commercial STMs did *not* want to buy. Instead, they were purchasing time, membership in an instrumental community, and a platform on which to demonstrate their own instrument-building expertise. Consumers wanted to be on an equal footing with producers, whether in a commercial or academic setting.

9.6 Digital Instruments

Commercialization of the STM was accomplished through a series of minute, unremarkable steps. Very little separated the home-built STM (made with parts ordered from catalogs, often using blueprints given by colleagues) from the STMs sold by Smith and (internally) IBM. The next step was only slightly more dramatic—the founding of organizations dedicated wholly to manufacturing and selling probe microscopes. Critics and proponents of academic capitalism both lay heavy emphasis on the founding of start-ups. It is seen both as the best means to extract profit from academic work, and as the ultimate distraction from the university’s pedagogical mission. Yet both these views neglect the realities of how start-ups operate within an instrumental community. Profit is often the least visible (and least successful) motivation for founding a start-up. Start-ups often extend and enhance the pedagogical culture of an instrumental community rather than despoiling it.

Digital Instruments (DI), the first true start-up in the probe microscopy community, was the brainchild of Virgil Elings, one of Paul Hansma’s colleagues in the physics department at UCSB. Elings’ first contact with the STM community, though, was Niko Garcia, a Spanish academic with close ties to IBM, who came to give a lecture at UCSB. After talking with Garcia and Hansma and attending the 1986 STM Conference in Spain, Elings saw a market for an off-the-shelf STM and offered to cofound a company with Hansma. Hansma was even more wary than Quate of commerce encroaching on his lab’s activities, so he declined. However, he gave Elings the same advice and schematics he made available to other STMers.[28] With this, Elings and his son built a prototype in their garage and entered it in a junior high science fair (where it took last place, since, as the judges pointed out, “everybody knows you can’t see atoms”).[29]

[28] <PH1>, <VE1>.

[29] <MT1>, <VE1>.

For Elings, building the prototype was a chance to make sure the Hansma design was commercializable, but also to test—and discard—many axioms of STM-building that had accrued since 1981. Elings saw STM-builders' trade secrets as geared to instruments that were finicky and difficult to operate; and he saw possession of these trade secrets as limiting the STM community to those deemed serious enough to build their own microscopes. Elings wanted, eventually, to make STMs for nonbuilders who demanded a simple to operate black box. Thus, he delighted in debunking the STM-builders' recipes by creating a more streamlined, easy to use, more durable tool.

Elings wanted DI to be the first to market a commercial microscope in time for the annual STM Conference in 1987. His initial plan was to sell a computer-controlled microscope (hence *Digital* Instruments). However, by the time he brought in a former student, Gus Gurley, as cofounder, it was too late for Gurley to write the necessary software in time. Instead, Elings marketed the analog Nanoscope I as DI's first product. Probe microscopists from this era—both builders and buyers—remember their first acquaintance with the Nanoscope as a turning point. Now, for the first time, researchers could join the STM community without having to build any part of their microscope. Moreover, unlike Smith's clients, DI's customers did not need to have personal ties to the community. People could (and did) simply call up Digital Instruments and order a microscope.

Yet though it marked an important shift, the Nanoscope I still illustrates the gradual, emergent character of commercialization. Like the CSS STM and Doug Smith's instrument, the Nanoscope I was more a kit than a full-fledged, black-boxed research tool. Indeed, Elings now calls this era at DI the "toy business"—both for the Nanoscope I's immature design, and for its lack of serious applications.[30] In following Hansma's lead, Elings designed an air STM, rather than the expensive, narrowly-focused ultrahigh vacuum instruments used at IBM and Bell Labs. This made sense in opening up a broad market, since few disciplines were willing to deal with or pay for an ultrahigh vacuum chamber (which, in any case, ruined samples relevant to almost everyone except surface scientists). Yet it was unclear in the 1980s what air STM could be used for, or what the images it produced meant. Only in 1991 to 1992 did a consensus develop that air STM was often not relying on tunneling for its contrast mechanism, and that many well-publicized air STM images (particularly of DNA) were erroneous. As a result, most air STMers abandoned the technique and followed Quate and Hansma to AFM, usually by buying one of DI's newly-available Multimodes (capable of running an STM *or* AFM).

At this point, Elings had no sales force. He simply advertised in *Physics*

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[30] <DC1>, <VE1>.

Today ("$25,000 for atomic resolution") and orders came in. Instruments were FedExed to buyers, who put them together and got the microscope running on their own. Despite this minimal marketing and customer service (and limited product utility), the toy business was successful. An advertisement from 1990 estimates that in the first three years, DI sold more than 300 Nanoscopes at $25,000 to $35,000 each.[31] The probe microscopy community expanded quickly, and the center of gravity shifted as well. As more people bought instruments, AFM and air STM began to outweigh ultrahigh vacuum STM, and the corporate labs became less dominant. High demand created a waiting list for DI's instruments, prompting a policy that researchers who wanted a microscope quickly could promise to name DI's founders or employees as coauthors on papers generated with Digital's products.

9.7 The Start-Up Era

The end of the toy business roughly corresponded to the end of DI's monopoly on commercial probe microscopy. By 1990, there were several new STM and AFM manufacturers whose products and strategies differed considerably. Some competed with Digital Instruments for the general-purpose microscope market, some targeted specific disciplinary niches. Some made easy to operate black boxes, some built "open architectures" for researchers who wanted to tinker with and modify the device. Some survived, others floundered. All were small companies, mostly founded out of universities specifically to make probe microscopes, though a few moved into microscopy from other product lines. No big firms made more than desultory attempts to sell STMs or AFMs—though a few (Hitachi, IBM, Perkin Elmer) started down that road.

These start-ups were founded for a very diverse set of reasons. Most debates about academic capitalism simply assume that, under the right set of incentives, professorial start-ups are inevitable; for good or ill, the inducements for professors to commercialize their work will be irresistible. Maybe so, but this assumption looks less reliable when the contingent and often counterintuitive reasons why people found start-ups are examined. Digital Instruments provides a striking example. Digital Instruments and Elings thrived at UCSB's disreputable margins. When he arrived in the late 1960s as a brash, confrontational professor, it was hoped Elings would build UCSB's reputation in high energy physics. His swagger, though, led to conflict with his department, which sidelined him into running its lucrative but unloved Master's of Scientific Instrumentation program.[32] Un-

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[31] From FASEB Journal, v. 4, n. 13 (1990), p. 1.

[32] <JW1>.

cowed, Elings transformed the master's program into his personal empire and a source for patents and start-up companies.

In the master's program, students from many educational backgrounds (biologists, engineers, even psychology majors) learned to build all kinds of measurement technologies—not just research instruments but also meters and tools for industry. Elings developed a pedagogical method that prized tacit over formal knowledge, participation over instruction. Instead of using textbooks and lectures, he simply connected students with professors on campus who needed instruments built and let them learn by doing. Because student projects were based on finding solutions to real problems faced by local researchers, they often yielded technologies Elings could market to those researchers' subdisciplines. Students learned to understand customers' needs and design technologies to answer them. This made former master's students the most important source of early employees for Digital Instruments.

So UCSB did, in a way, encourage creation of DI, though no school would replicate their path. By sidelining a brilliant but difficult professor to the poorly-regarded master's program, they encouraged him to reject campus culture, denigrate academically-instilled formal knowledge, and be receptive to the commercial possibilities of the tacit knowledge his students accrued. Moreover, in making clear that Elings' commercial ventures hindered his academic career, the UCSB physicists made it more likely that Digital Instruments would be his bridge to leaving academia. Tension between Elings and UCSB even smoothed technology transfer from Hansma to DI, since Elings' hostility toward academic researchers meant he rejected Hansma's designs until they had been engineered to look more like commercial products than most home-built instruments.

Disgruntlement of a different kind motivated the engineers who worked for DI and its competitors. Several of the early STM and AFM manufacturers were founded in the heart of the West Coast military-industrial complex. Graduate students who had grown accustomed to the picturesque surroundings and lifestyle of southern California often sought employment nearby, usually with defense firms like Lockheed and Hughes. Yet defense work galled many of these engineers, driving them to probe microscopy. Much of Elings' early workforce came to DI for this reason; and in Los Angeles Paul West, one of John Baldeschwieler's former postdocs at Caltech, grew so frustrated with defense work that he started his own probe microscopy company, Quanscan. As the West Coast start-ups matured, they took on large numbers of students from their affiliated academic groups as collaborators or summer employees. As Sumell, Stephan, and Adams (chapter 8, this volume) point out, that kind of corporate experience during graduate school can be a strong inducement to stay in-region. And, in fact, by the early 1990s Quate, Hansma, and Baldeschwieler graduates were routinely staying on the West Coast either to work at (respectively) PSI, DI, or Topometrix, or to join new STM and AFM start-ups founded by engineers who had left those companies.[33]

The STM and AFM start-ups positioned themselves relative to each other in ways that mirrored the relationships between the academic groups with which they were associated. Digital Instruments may not have been officially affiliated with the Hansma group at first (indeed, even at the best of times, there was always some suspicion between the two groups)—and Elings was certainly proud of the ways the Nanoscope differed from Hansma's microscopes—but DI's products bore an obvious genealogical kinship with Hansma's STMs and AFMs, and DI drew on Hansma's reputation in the community. In return, Hansma's design innovations spread much farther and faster than those of professors not affiliated with a start-up. Hansma's peers in the probe microscopy elite noticed how DI helped spread Hansma's ideas and resolved to affiliate with their own microscope manufacturers. Usually this meant helping former students and postdocs found start-ups. For instance, two Stanford postdocs, Sung-Il Park and Sang-Il Park (no relations) started Park Scientific Instruments (PSI) in 1989 with Quate's assistance and quickly became the major employer of Quate group veterans. Park Scientific Instruments' designs traveled much more directly from Stanford than DI's designs did from UCSB. Moreover, the research on new AFM applications conducted at Park Scientific often picked up just where Quate's own research left off.[34] Commercialization was the continuation of academic science by other means.

Similarly, just as John Baldeschwieler's group at Caltech always lagged behind Quate's and Hansma's in popularizing its discoveries and innovations, the company he helped Paul West found, Quanscan, lagged behind DI and Park Scientific in marketing commercialized versions of the Caltech designs. But not all probe microscope companies were founded as proxies for the competition between academic groups in an instrumental community. Some were started to extend academic *collaborations.* For instance, Stuart Lindsay, a physics professor at Arizona State University, had been an early collaborator of Hansma's, helping to adapt the STM for electrochemistry and biophysics. Once Hansma's designs were commercialized by DI, Lindsay pressed Elings to adapt the Nanoscope for Lindsay's colleagues in electrochemistry and biophysics—to no avail, since Elings was usually hostile to adapting the Nanoscope for anyone (DI had a strict no-custom-instruments policy), especially when the suggestion came from outside the

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[33] That is, by the late 1990s one could see a probe microscopy cluster forming, primarily around Santa Barbara and Los Angeles, with start-ups like Pacific Nanotechnology, Quesant, Asylum Research, and Nanodevices founded by veterans from DI and Topometrix. Engineers who left Park Scientific tended to drift into established Silicon Valley firms such as KLA-Tencor.

[34] <DB3>, <FG1>, <JN1>.

company. So Lindsay founded his own company, Molecular Imaging, to make attachments to the Nanoscope that would make it more compatible with electrochemistry and biophysics—attachments that DI grudgingly distributed for a few years until it developed its own competing line.[35]

Lindsay's other motivations for founding a start-up say a great deal about how commercialization and pedagogy fit together in instrumental communities. Long before Molecular Imaging, his group—like Hansma's at UCSB—had become a center for distributing blueprints and (especially) software to new STM builders. One of Lindsay's technicians, Uwe Knipping, developed one of the first and most sophisticated computer-controlled microscopes. Knipping's software formed the basis for Lindsay's academic network-building, but it also caught the eye of two local entrepreneurs, Larry and Darryl McCormick, who founded a company, Angstrom Technology, to commercialize it.[36]

As it turned out, Knipping's architecture was far too sophisticated for a commercial instrument, and the enterprise failed. But Lindsay had gotten a taste for how network-building in the academic domain might be enhanced by commercialization. So a few years later when he encountered a former postdoc of his who was having trouble finding work, Lindsay decided to put the former postdoc in charge of starting a new company—Molecular Imaging—as an extension of Lindsay's research at Arizona State University (ASU).[37] This is probably the classic—if woefully understudied—story of commercialization: a professor's technicians and graduate students make a widget, then the professor's colleagues call up asking for their own widget (or blueprints thereof), a student starts making batches of widgets in their garage, and eventually, whether to help position the professor within that instrumental community or to give lab personnel needed work, the widget-making is spun off into its own organization.

In only a few cases were probe microscope start-ups inspired primarily by profit. Paul West's Quanscan, for instance, always had the most venture capital, the most MBAs, and the slickest advertising. Yet West's own motivation was more personal than commercial: he saw entrepreneurship as an intellectual challenge and a path to personal growth.[38] Even so, Quanscan was actually at a disadvantage relative to competitors *because* it was run more like a for-profit business. The venture capitalists continually interfered in operations, the MBAs had trouble understanding the values of an instrumental community that they had never participated in, and the advertising alienated many potential customers.

In contrast, Park Scientific, like DI, was a more rough-hewn affair. Sung-Il Park's barber was hired as the office manager, for instance, and the

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35. <SL1>.
36. <SL1>, <JA1>.
37. <SL1>, <TJ1>.
38. <PW2>, <JB1>, <GA1>.

senior executives were Quate students with no business training.[39] Indeed, the Parks carved a market niche by letting it be known they were more interested in technically sweet innovations than in mundane moneymaking. As "gentleman scientists," they could speak as peers with other researchers and capture customers' trust. Park Scientific gained a reputation for making builders' instruments—well-crafted, reliable, with enough idiosyncracies and innards showing to be reminiscent of a microscope made by a graduate student. Park was even willing to work with individual customers to build a microscope for a specific application (something DI never did)—if the engineering required a certain finesse. Through most of the 1990s this strategy kept PSI running near DI, but Digital ultimately won out because of its skeptical (sometimes hostile) attitude toward the expertise of its customers. Where Park Scientific was willing to relive its Quate group origins by respecting the knowledge of foreign disciplines, DI made one type of microscope for everyone, and hid the workings of that instrument completely from customers' view. This attitude allowed DI to break into the industrial market, where it could sell many more, and more expensive, microscopes to companies that usually wanted low-level technicians to learn how to use the instrument in a day or two—market conditions for which PSI was wholly unprepared.

As much as DI eventually prospered by distancing itself from Hansma's academic model, though, the company's success hinged equally on continual sharing of culture, people, and inventions between start-up and academic lab. Both Elings and Hansma saw tacit, rather than formal, knowledge as primary in instrument-building—Elings because of his work in the instrumentation master's program, Hansma because the contours of the STM community had pushed him to encourage multidisciplinary collaborations and undisciplined instrument-building. This shared emphasis on the tacit meant both men took in people with diverse and unusual educational backgrounds: junior high students, river guides, undergraduates, yoga instructors, retirees, psychology majors, and historians.[40] This diversity was almost unthinkable at other centers of probe microscopy such as IBM or Bell Labs.

Age and gender diversity followed along with diversity in educational background. There were, to be sure, a few women and young (i.e., college-educated but no Ph.D.) people in the corporate labs, but they tended to exit to academia somewhat more quickly than their male colleagues (women usually to run their own academic groups, college graduates to go get Ph.D.s). In the Hansma group and DI, very young people did much of the daily work, while women and older people were often the source of crucial innovations. A retired teacher named Sam Alexander, for instance, to whom

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[39] <DB1>, <BP1>.

[40] <HH1>, <JM3>, <DB2>, <MT1>, <BD2>, <PH1>.

Hansma had donated part of his laboratory, came up with the optical detection scheme that is now the basis of DI's AFMs. And Helen Hansma, Paul's first wife, used her Ph.D. in biochemistry to transition Paul's group into biophysics at the same time that she transitioned back into lab work after many years of rearing children and teaching in the Santa Barbara school system.[41]

As members of DI and the Hansma group became aware of parallels between their organizational styles, they appropriated these similarities to accelerate the two-way flow of people, materials, designs, and knowledge. After the initial phase (when most DI employees were Elings' former master's students), several Hansma graduates, postdocs, and collaborators took high-ranking jobs at DI. Individuals on both sides collaborated to transform Hansma's research into commercial products; for instance, the Hansma AFM (on which DI's fortunes eventually rested) was turned into a product through negotiations between Barney Drake (Hansma's technician) and James Massie (a former Elings student) over which elements of the Hansma design were indispensable and which were too finicky for anyone but the graduate students who built them.[42]

Hansma also, for the first time in his career and at Elings' behest, began patenting his research.[43] As DI's sales increased, the Hansma group kept its place at the forefront of the AFM community through its steady supply of DI instruments and the ability of Hansma's students and postdocs to go up the road to DI to scavenge parts and advice.[44] That is, whatever his initial reservations about commercialization, Hansma came to see the partnership with DI as a way to position himself—intellectually and socially—within his instrumental community.

In turn, once the toy business ended in the early 1990s, Elings began to imitate Hansma's tactic of bringing in collaborators from various disciplines. Digital Instruments built its own group of researchers from biophysics, magnetics, and polymer chemistry, who (like Hansma's postdocs) worked with instrument-builders, developed and published on new STM and AFM applications, and traveled to give talks and attend conferences to spread word about the technique.[45] Though DI was a profit-making ven-

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41. That is, Paul Hansma's lab, and DI, upended the whole notion of an educational mismatch (Bender and Heywood, chapter 7, this volume). For Hansma and Elings, AFM-building was not a static process for which any educational background could be mismatched. Rather, diverse educational backgrounds offered an engine for pushing AFM-building in new, unexpected directions.

42. <BD2>, <JM3>.

43. Quate, Hansma, Lindsay, Binnig, and the other more outward-looking probe microscopists all patented their work, especially after 1986. Many of those patents were probably overlapping and difficult to defend. The point of the patents, though, was initially to tie the academic group to the company with which it was most closely affiliated. Later on, patents were used more to raise the stakes for other academic groups and start-ups to join this elite club.

44. <JH1>.

45. <SM2>, <MA1>.

ture, its success arose partly from academic activities: doing basic research, publishing articles, training and "graduating" employees, giving talks. These practices were then widely emulated by the other start-ups.

9.8 Conclusion

So what does probe microscopy tell us about commercialization of academic knowledge and the value of corporate-academic linkages? First, the development of probe microscopy shows how thoroughly—yet intricately and indirectly—the corporate and academic worlds are connected. The locus of academic research is much wider than the university campus, just as the locus of commerce is wider than the for-profit business. Instrumental communities are distributed across academic and corporate institutions. Commercialization—the transformation of academic research into commerce—is not a simple pipeline from university to firm. Commercialization can play many roles within an instrumental community, and academic research can be traded for many things other than money. Attempts, therefore, to directly stimulate and accelerate the transformation of academic research into cash may well backfire. As we have seen, it was the looser, indirect ties between corporate and academic groups that fostered the growth of STM and AFM and encouraged start-ups to emerge from universities, rather than direct pressure from corporations or overt incentives from governments and universities.

Thus, proponents of academic entrepreneurialism should be wary of focusing too narrowly on increased profit as the fruit of a commercialized university. As we have seen, trading goes on all the time in instrumental communities. The token of exchange is usually a mix of knowledge, prestige, personnel, time, materials, money, opportunity, and so forth. The popularity of various forms of barter changes as the instrumental community changes. Commercialization can restrict some exchanges and make money-based trades more prevalent. Few instrumental communities, though, reach the point where their products can be sold for money. Even within the probe microscopy community, only the atomic force microscope and the magnetic force microscope have been commercial successes. The STM, which provided the first product for microscope manufacturers, was effective in training engineers to build microscopes, but never found industrial application. University administrators who hope that stimulating professors to turn a gray market into a profit-making start-up will bring real patent revenue to their school will almost always be disappointed.

Moreover, development of an entrepreneurial instrumental community may require that its members be drafted from less profitable fields where commercialization did not occur. The STM and AFM community, for instance, initially drew on its members' expertise in low-energy electron diffraction, sandwich tunnel junction spectroscopy, and field ion microscopy—instrumental communities with poor records of commercialization. Later, STM and AFM pulled in participants from many fields (surface science, biophysics, mineralogy, electrochemistry, polymer science—some more commercialized than others) who aided groups like Quate's and Hansma's in their gray market activities. Instrumental communities and disciplines that are not conducive to profit-making nevertheless provide the infrastructure and knowledge/labor pool for communities in which profit may be enormous. Policymakers should not think they can predict which will be which; nor are they likely to succeed if they encourage only the one at the expense of the other. Policymakers may be best advised to encourage professors to foster gray markets within their instrumental communities—whether as consumers, producers, or both. Gray market activities of trading research materials, people, and components of technologies enlarge the outlook of academic research and allow academic scientists to be influential even when they are not profitable.

Finally, both opponents and supporters of corporate involvement in university life have seized on grains of truth. Supporters have it right that corporate-academic linkages are desirable, even necessary, for research and innovation. There was no golden age when faculty operated independent of firms, pursuing disinterested research. Knowledge production in physics, engineering, and chemistry was always aided by faculty consulting and trading of personnel and ideas. The oft-criticized commercialism of the "biotech revolution" merely extended long-standing entrepreneurial practices into molecular biology. The STM and AFM case does, however, give reason for opposing the notion that universities should be run as businesses, squeezing profit where they can and operating along the rational lines of modern management. The probe microscopy community developed rapidly because participants could point to different institutional poles—corporations, universities, national labs. At times, innovation occurred because these poles were opposed—as when Hansma and Quate shifted from surface science and UHV STM to new designs and applications. At other times, innovation occurred because participants strung out hybrid forms between these poles—the gray market of software trading, the CSS STM, and the toy business. Instrumental communities rely on a variety of actors contained in different kinds of institutions. If all these institutions are run on the same highly-managed, profit-driven model, then the movement of people and ideas—and the production of new technologies—will likely be hindered.

Appendix

Interviewees listed by alphanumeric, name, positions held over the period covered by the interview, and date of the interview. All interviews conducted by the author.

AG1: Andy Gewirth: Hansma collaborator; University of Illinois; 6/26/01
BD2: Barney Drake: Hansma group technician; UCSB; 10/18/01
BH1: Bob Hamers: Yorktown researcher; University of Wisconsin; 5/9/01
BP1: Becky Pinto: Stanford; Park Scientific; KLA-Tencor; 2/3/04
BS1: Brian Swartzentruber: Bell Labs technician; University of Wisconsin; Sandia National Laboratory; 1/10/03
BW2: Bob Wolkow: IBM Yorktown; Bell Labs; NRC Canada; 5/22/01
CG1: Christoph Gerber: IBM Zurich technician; 11/12/01
CP1: Craig Prater: Hansma graduate student; Digital Instruments engineer; 3/19/01
DB1: Dawn Bonnell: Yorktown postdoc; University of Pennsylvania; 2/26/01
DB2: Dan Bocek: UCSB undergraduate; DI engineer; Asylum Research; 3/23/01
DB3: David Braunstein: Stanford; Park Scientific; IBM San Jose; 4/3/01
DC1: Don Chernoff: Sohio Research; Advanced Surface Microscopy; 9/5/01
DF1: Dave Farrell: Burleigh Instruments; 5/29/01
DR1: Dan Rugar: Quate student; Almaden researcher; 3/14/01
FG1: Franz Giessibl: IBM Munich; Park Scientific; Uni Augsburg; 11/16/01
GA1: Gary Aden: Topometrix executive; 3/12/01
HG1: Hermann Gaub: Ludwig-Maximilians Universität; 11/14/01
HH1: Helen Hansma: UCSB professor; 3/19/01
JA1: John Alexander: Angstrom Technology; Park Scientific; KLA-Tencor; 10/15/01
JB1: John Baldeschwieler: Caltech; 3/28/01
JD2: Joe Demuth: Yorktown manager; 2/22/01
JF1: John Foster: Quate student; Almaden researcher; 10/19/01
JG1: Jim Gimzewski: IBM Zurich researcher; UCLA; 10/22/01
JG3: Joe Griffith: Bell Labs; 2/28/01
JH1: Jan Hoh: Hansma postdoc; Johns Hopkins; 6/10/02
JM1: John Mamin: UC Berkeley; IBM Almaden; 3/15/01
JM3: James Massie: Elings master's student; DI engineer; 10/18/01
JN1: Jun Nogami: Quate postdoc; Michigan State; 6/28/01
JV1: John Villarrubia: Yorktown postdoc; National Institute of Standards and Technology; 6/28/00
JW1: Jerome Wiedmann: Elings master's student; DI employee; 10/18/01

MA1: Mike Allen: UC Davis; Digital Instruments; Biometrology; 10/12/01
MK1: Mike Kirk: Quate student; Park Scientific Instruments; KLA-Tencor; 10/12/01
MS1: Miquel Salmeron: Lawrence Berkeley National Laboratory; 3/9/01
MT1: Matt Thompson: Digital Instruments; 2/26/01
NB1: Nancy Burnham: Naval Research Lab postdoc; Worcester Polytechnic; 2/20/01
OM1: Othmar Marti: IBM Zurich student; Hansma postdoc; University of Ulm; 11/16/01
PH1: Paul Hansma: UC San Barbara; 3/19/01
PW2: Paul West: Caltech; Quanscan; Topometrix; Thermomicroscopes; 3/30/01
RC1: Rich Colton: Naval Research Lab; Baldeschwieler collaborator; 6/27/02
RT1: Ruud Tromp: Yorktown researcher; 2/23/01
SG1: Scot Gould: Hansma student; DI employee; Claremont McKenna; 3/27/01
SL1: Stuart Lindsay: Hansma collaborator; Arizona State; Molecular Imaging; 1/6/03
SM2: Sergei Magonov: Digital Instruments; 3/21/01
TA1: Tom Albrecht: Quate student; Almaden researcher; 3/14/01
TB1: Thomas Berghaus: Uni Bochum; Omicron; 11/19/01
TJ1: Tianwei Jing: Arizona State; Molecular Imaging; 1/7/03
VE1: Virgil Elings: UC Santa Barbara; Digital Instruments; 3/20/01

References

Bassett, R. K. 2002. *To the digital age: Research labs, start-up companies, and the rise of MOS technology.* Baltimore, MD: Johns Hopkins University Press.

Binnig, G., and H. Rohrer. 1985. The scanning tunneling microscope. *Scientific American* 253 (2): 50–56.

———. 1987. Scanning tunneling microscopy: From birth to adolescence. *Reviews of Modern Physics* 59 (3): 615–25.

Blume, S. 1992. *Insight and industry: On the dynamics of technological change in medicine.* Cambridge, MA.: MIT Press.

Bok, D. 2003. *Universities in the marketplace: The commercialization of higher education.* Princeton: Princeton University Press.

Bromberg, J. L. 1991. *The laser in America, 1950–1970.* Cambridge, MA: MIT Press.

Collins, H. M. 1975. The seven sexes: A study in the sociology of a phenomenon, or the replication of experiments in physics. *Sociology* 9 (2): 205–24.

Creager, A. N. H. 2002. *The life of a virus: Tobacco mosaic virus as an experimental model, 1930–1965.* Chicago: University of Chicago Press.

Elzen, B. 1986. Two ultracentrifuges: A comparative study of the social construction of artefacts. *Social Studies of Science* 16 (4): 621–62.

Etzkowitz, H. 2002. *MIT and the rise of entrepreneurial science.* London: Routledge.

Fischer, C. S. 1992. *America calling: A social history of the telephone to 1940.* Berkeley: University of California Press.

Geiger, R. L. 2004. *Knowledge and money: Research universities and the paradox of the marketplace.* Stanford: Stanford University Press.

Jackson, M. 2000. *Spectrum of belief: Joseph von Fraunhofer and the craft of precision optics.* Cambridge, MA.: MIT Press.

Jordan, K., and M. Lynch. 1998. The dissemination, standardization, and routinization of a molecular biological technique. *Social Studies of Science* 28 (5/6): 773–800.

Kaiser, D. 2005. *Drawing things apart: The dispersion of feynman diagrams in postwar physics.* Chicago: University of Chicago Press.

Kenney, M., ed. 2000. *Understanding silicon valley: The anatomy of an entrepreneurial region.* Stanford: Stanford University Press.

Kirp, D. L. 2003. *Shakespeare, Einstein, and the bottom line: The marketing of higher education.* Cambridge, MA: Harvard University Press.

Kleinman, D. Lee. 2003. *Impure cultures: University biology and the world of commerce.* Madison, WI: University of Wisconsin Press.

Kline, R., and T. Pinch. 1996. Users as agents of technological change: The social construction of the automobile in the rural United States. *Technology and Culture* 37 (4): 763–95.

Knowles, S., and S. W. Leslie. 2001. "Industrial Versailles": Eero Saarinen's corporate campuses for GM, IBM, and AT&T. *Isis* 92 (1): 1–33.

Kohler, R. 1994. *Lords of the fly.* Chicago: University of Chicago Press.

Latour, B., and S. Woolgar. 1986. *Laboratory life: The construction of scientific facts.* Princeton, NJ: Princeton University Press.

Lécuyer, C. 2006. *Making Silicon Valley: Innovation and the growth of high tech, 1930–1970.* Cambridge, MA.: MIT Press.

Lenoir, T., and C. Lécuyer. 1995. Instrument makers and discipline builders: The case of nuclear magnetic resonance. *Perspectives on Science* 3 (2): 276–345.

Mirowski, P., and E.-M. Sent. 2007. The commercialization of science and the response of STS. In *Handbook of science and technology studies,* ed. E. Hackett, O. Amsterdamska, M. Lynch, and J. Wacjman, 635–89. Cambridge, MA: MIT Press.

Oudshoorn, N., and T. Pinch, eds. 2003. *How users matter: The co-construction of users and technologies.* Cambridge, MA.: MIT Press.

Pantalony, D. 2004. Seeing a voice: Rudolph Koenig's instruments for studying vowel sounds. *American Journal of Psychology* 117 (3): 425–42.

Polanyi, M. 1962. *Personal knowledge: Towards a post-critical philosophy.* New York: Harper Torchbooks.

Quate, C. F. 1985. Acoustic microscopy: Recollections. *IEEE Transactions On Sonics and Ultrasonics* 32 (2): 132–35.

Rader, K. 2004. *Making mice: Standardizing animals for American biomedical research, 1900–1955.* Princeton, NJ: Princeton University Press.

Rasmussen, N. 1997. *Picture control: The electron microscope and the transformation of biology in America, 1940–1960.* Stanford: Stanford University Press.

Riordan, M., and L. Hoddeson. 1997. *Crystal fire: The birth of the information age.* New York: Norton.

Scranton, P. 1997. *Endless novelty: Specialty production and American industrialization, 1865–1925.* Princeton, NJ: Princeton University Press.

Shah, S. 2003. Community-based innovation and product development: Findings from open source software and consumer sporting goods. Ph.D. diss. MIT, Sloan School of Management, Cambridge, MA.

Shapin, S. 2003. Ivory trade. *London Review of Books* 25 (17): 15–19.

Shapin, S., and S. Schaffer. 1985. *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life.* Princeton, NJ: Princeton University Press.

Shinn, T. 1997. Crossing boundaries: The emergence of research-technology communities. In *Universities and the global knowledge economy: A triple helix of university-industry-government relations,* ed. H. Etzkowitz and L. Leydesdorff, 85–96. London: Pinter.

Slaughter, S., and L. L. Leslie. 1997. *Academic capitalism: Politics, policies, and the entrepreneurial university.* Baltimore, MD: Johns Hopkins University Press.

Vettel, E. J. 2006. *Biotech: The countercultural origins of an industry.* Philadelphia: University of Pennsylvania Press.

Wise, G. 1985. *Willis R. Whitney, General Electric, and the origins of U.S. industrial research.* New York: Columbia University Press.