Corporations, Universities, and Instrumental Communities: Commercializing Probe Microscopy, 1981-1996

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Introduction

December 1984. Cancun, Mexico. A beach resort thronging with refugees from winter – except in one hotel room, where a dozen people sit listening, explaining, venting frustrations. They are Heini Rohrer, co-inventor of the scanning tunneling microscope (STM), and erstwhile STMers from various universities and corporate labs. Almost four years earlier, Rohrer, Gerd Binnig, and Christoph Gerber got their STM working at the IBM lab in Zurich, Switzerland. It took a long time to recruit new STM builders, and even longer to get the recruits' instruments running. Now, things are at a breaking point. Some new STMers have been working for two years, yet none can replicate the Zurich group's great achievement – images of single atoms on a silicon surface. One by one, they get up, describe their machine, show some blurry images, and ask Rohrer and the others what they're doing wrong.

This story ends happily. By March of 1985, several groups had atomic resolution of silicon and presented spectacular images to packed crowds at the American Physical

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Society meeting. Within five years, these physicists, chemists, electrical engineers, and surface scientists oversaw the rapid expansion of tunneling microscopy and its proliferation into related techniques such as atomic force microscopy (AFM), magnetic force microscopy (MFM), and near-field scanning optical microscopy (NSOM) – known collectively as probe microscopes. Today 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.

Stories about scientific instruments have long been a staple for historians of science and technology. This literature was crucial in elucidating the artifactual basis of scientific knowledge and the technological considerations underlying even the "purest" research.[1] These narratives, however, have largely focused on the "pre-Cancun" phase, from invention to replication. This can be a remarkably elastic phase, since some techniques (e.g. mass spectrometry or the laser) draw adherents simply through the opportunities afforded for newcomers to continually invent new variants.[2] A few exemplary works, such as Nicolas Rasmussen's history of biological electron microscopy or Christophe Lécuyer and Timothy Lenoir's study of Varian's popularization of nuclear magnetic resonance, have examined the "post-Cancun" phase of dispersion and

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[1] Some studies in this vein include Robert Kohler, *Lords of the Fly* (Chicago: University of Chicago Press, 1994), Boelie Elzen, "Two Ultracentrifuges: A Comparative Study of the Social Construction of Artefacts," *Social Studies of Science* 16 (1986): 621-662; and Thomas P. Hughes, "Model builders and instrument makers," *Science in Context*, 2 (1988): 59-75.

[2] See Joan Lisa Bromberg, *The Laser in America, 1950-1970* (Cambridge, MA: Massachusetts Institute of Technology Press, 1991) and Michael A. Grayson, ed., *Measuring Mass: From Positive Rays to Proteins* (Philadelphia: Chemical Heritage Press, 2002). Indeed, since there are now more than forty *named* types of probe microscope, one could easily present the history of the field as an unending series of invented variations. My focus here is less on these variants, most of which have few users, and more on the bulk of more or less standardized, usually commercially available, instruments. For an analysis of the dichotomies between builders and buyers in probe microscopy, see Cyrus C. M. Mody, "How Probe Microscopists Became Nanotechnologists," in *Discovering the Nanoscale*, ed. Davis Baird, Alfred Nordmann, and Joachim Schummer (Amsterdam: IOS Press, 2004), 119-133.

commercialization.[3] Even here, though, the emphasis has been on singular institutions, whether corporate (e.g. RCA or Varian) or non-profit (e.g. Stanford or the Rockefeller Foundation), rather than the shaping of instruments by actors distributed across multiple corporate, academic, and national organizations, arrayed in shifting relationships of patron-client, consumer-producer, inventor-replicator, builder-user.

This is unfortunate because relationships between corporations and universities built around such research technologies are an important node in public debates about academic capitalism and the future of higher education.[4] This old and wide-ranging debate gained momentum in the 1970s as changes in law, academic culture, corporate research, and national science funding have pushed universities to patent professors' research, incubate start-up companies, and form substantial (sometimes controversial) corporate partnerships. Proponents of change want universities to integrate fully with the market; nay-sayers decry the academy's loss of independence and critical voice.

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[3] Nicolas Rasmussen, *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940-1960* (Stanford: Stanford University Press, 1997); Timothy Lenoir and Christoph Lécuyer, "Instrument Makers and Discipline Builders: The Case of Nuclear Magnetic Resonance," *Perspectives on Science,* no. 3 (1995): 276-345. An excellent study of the transition from pre- to post-Cancun for several instrumental communities is Stuart Blume, *Insight and Industry: On the Dynamics of Technological Change in Medicine* (Cambridge, MA: MIT Press, 1992). For analyses of the dispersion/dissemination of experimental tools, see Kathleen Jordan and Michael Lynch, "The dissemination, standardization, and routinization of a molecular biological technique," *Social Studies of Science* 28, no. 5-6 (1998): 773-800 and David Kaiser, *Drawing Things Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[4] I draw the phrase "research technologies" from Terry Shinn, "Crossing Boundaries: The Emergence of Research-Technology Communities," in *Universities and the Global Knowledge Economy: A Triple Helix of University-Industry-Government Relations*, ed. Henry Etzkowitz and Loet Leydesdorff (London: Pinter, 1997), 85-96. For a sampling of the academic capitalism debate, see Norman E. Bowie, ed., *University-Business Partnerships: An Assessment, Issues in Academic Ethics* (Lanham, MD: Rowman & Littlefield, 1994); Derek Bok, *Universities in the Marketplace: The Commercialization of Higher Education* (Princeton: Princeton University Press, 2003); Roger L. Geiger, *Knowledge and Money: Research Universities and the Paradox of the Marketplace* (Stanford: Stanford University Press, 2004); and the essays in Donald G. Stein, ed., *Buying In Or Selling Out: The Commercialization of the American Research University* (New Brunswick: Rutgers University Press, 2004).

As Steven Shapin notes, both sides argue the issue abstractly, leading to ludicrous over-praisings of new patent laws and overly dire warnings about corporate "influence".[5] What empirical work there is focuses exclusively on particularly entrepreneurial disciplines/industries (biotechnology, microelectronics), universities (Stanford, MIT), or regions (Silicon Valley, Route 128).[6] Yet many commercialized research tools emerged from work done across disciplines, at multiple universities *and* corporations around the world. To understand how technologies move among universities and firms and become commercial products, we need a multi-institutional, multi-regional, multi-discplinary unit of analysis – what I will call an "instrumental community".

By instrumental community I mean the porous group of people commonly oriented to building, developing, using, selling, and popularizing a particular technology of measurement. Such communities are "instrumental" primarily in focusing on new research tools – scientific instruments. Because such communities include academic and commercial participants, though, they will seek ways to morph those tools into

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[5] Steven Shapin, "Ivory Trade," *London Review of Books* 25, no. 17 (2003): 15-19.

[6] For disciplines/industries, see Christophe Lécuyer, "Making silicon valley: engineering culture, innovation, and industrial growth, 1930-1970" (Stanford University dissertation, 1999) and Martin Kenney, *Biotechnology: The University-Industrial Complex* (New Haven: Yale University Press, 1986). For studies of MIT and Stanford, see Bernard Carlson, "Academic entrepreneurship and engineering education: Dugald C. Jackson and the cooperative engineering course, 1907-1932," *Technology and Culture* 29 (1988): 536-69; David Noble, *America By Design: Science, Technology, and the Rise of Corporate Capitalism* (Oxford: Oxford University Press, 1977); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*, 1 ed. (New York, NY: Columbia University Press, 1993); John Servos, "The industrial relations of science: Chemical engineering at MIT, 1900-1939," *Isis* 81 (1980): 531-49; R. S. Lowen, "Transforming the University - Administrators, Physicists, and Industrial and Federal Patronage At Stanford, 1935-49," *History of Education Quarterly* 31, no. 3 (1991): 365-388; C. Lecuyer, "Academic science and technology in the service of industry: MIT creates a "permeable" engineering school," *American Economic Review* 88, no. 2 (1998): 28-33; Henry Etzkowitz, *MIT and the Rise of Entrepreneurial Science* (London: Routledge, 2002). For Silicon Valley, Route 128, and emulations thereof, Anna-Lee Saxenian, *Regional Networks: Industrial Adaptation in Silicon Valley and Route 128* (Cambridge, MA: Harvard University Press, 1993); Peter Hall and Ann Markusen, eds., *Silicon Landscapes* (Boston: Allen & Unwin, 1985). Manuel Castells and Peter Hall, *Technopoles of the World: The making of twenty-first-century industrial complexes* (London: Routledge, 1994); S. W. Leslie, "Regional disadvantage - Replicating silicon valley in New York's capital region," *Technology and Culture* 42, no. 2 (2001): 236-264; Martin Kenney, ed., *Understanding Silicon Valley: The Anatomy of an Entrepreneurial Region* (Stanford: Stanford University Press, 2000).

industrially-relevant devices. Thus, such communities are also "instrumental" in focusing on new ways of doing or making things. Instrumental communities are ubiquitous; many tools have had moments like Cancun in 1984, when a community suddenly coalesces around the technology. Instrumental communities are also enduring; indeed, the best recent work in history of instrumentation examines pre-twentieth century artisanal instrument makers and their cultivation of communities of patrons and customers.[7] In the past century (and increasingly since the 1970s), though, examining the formation and organization of instrumental communities means better understanding the interplay of corporate and academic organizations – spanning multiple disciplines – that shapes the development and proliferation of high technologies.

Probe Microscopy

The cultivation of an instrumental community can begin with the invention of the instrument itself; indeed, such promotional activities are integral to invention.[8] Community-building meets organizational or disciplinary goals, and provides a safety net for institutionally-precarious inventors.[9] The STM, for example, originated in 1979 at IBM Zurich as a characterization tool in making thin films for a commercially important

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[7] Klaus Hentschel, *Mapping the Spectrum: Techniques of Visual Representation in Research and Teaching* (Oxford: Oxford University Press, 2002); Myles W. Jackson, "Buying the dark lines of the spectrum: Joseph von Fraunhofer's standard for the manufacture of optical glass," in *Scientific Credibility and Technical Standards in 19th and Early 20th Century Germany and Britain*, Jed Z. Buchwald, ed. (Dordrecht: Kluwer Academic, 1996), 1-22; and David Pantalony, "Seeing a voice: Rudolph Koenig's instruments for studying vowel sounds," *American Journal of Psychology* 117, no. 3 (2004): 425-442.

[8] The STM, for instance, was preceded by a very similar instrument called the Topografiner, built by Russell Young at the U.S. Bureau of Standards in 1969-70. Yet because Young never convinced any wider group of people to attach themselves to the Topografiner, it is fair to say that probe microscopy was not "invented" until Binnig and Rohrer came along. See John Villarrubia, "The Topografiner: An instrument for measuring surface microtopography," in *A Century of Excellence in Measurements, Standards, and Technology – Selected Publications of NBS/NIST, 1901-2000*, ed. David R. Lide and Dean R. Stahl (Boca Raton: CRC Press, 2001), 214-218.

[9] Indeed, inventors of instruments (or those who take credit for having invented them) often seem to have vexed positions within the firms that employ them. See, for instance, the description of Kary Mullis' antagonistic relationship with Cetus in Paul Rabinow, *Making PCR: A Story of Biotechnology* (Chicago: University of Chicago Press, 1996).

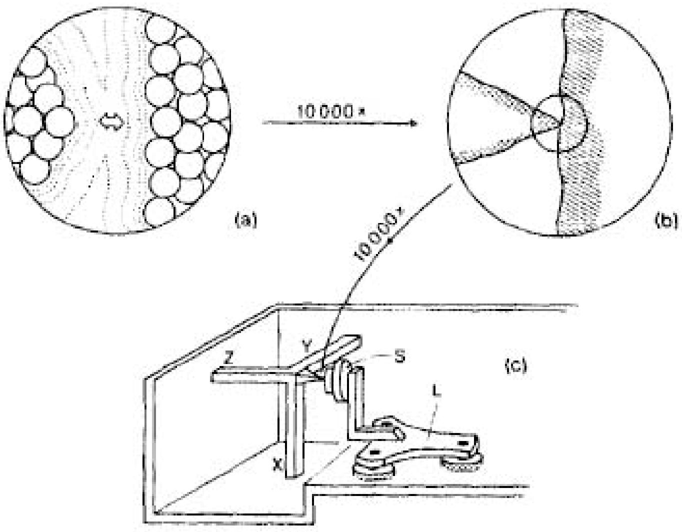

Figure 1: How a probe microscope works. All scanning probe microscopes bring a very small solid probe very close (usually to within a nanometer – one billionth of a meter) to a sample and measure the strength of different kinds of interactions between probe and sample to determine the height (and other 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 first, 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. Image from Binnig, G., Rohrer, H. Scanning Tunneling Microscopy. *Physica B and C* 127:37-45.

supercomputer.[10] When the supercomputer project died, Binnig and Rohrer lost the organizational justification for their work – so they prolonged its life, first by hiding it in IBM's bureaucratic folds, later by forming alliances with academic researchers. They assiduously cultivated academic STM replicators in Spain, Germany, Switzerland, California, and elsewhere, while also using the instrument to address basic research questions (particularly in the subfield of surface science) to attract attention from both corporate and academic scientists. When these efforts succeeded, IBM promoted STM at its labs in Zurich, Yorktown Heights, New York and Almaden, California; while IBM's research rival, Bell Labs, recruited STM groups in response. Binnig and Rohrer skillfully exploited the porosity of the corporate-academic boundary; by enrolling academic interlocutors, they created a corporate space for STM despite losing an immediate commercial objective.

This porosity existed because corporate labs long depended on academic groups for recent Ph.D.s as postdocs or junior staff scientists; in subdisciplines such as surface science where they needed workers, corporate labs fostered professional societies, journals, and other extramural institutions to maintain academic participation.[11] Some researchers moved between corporate and academic research, growing networks between institutions – veterans of corporate labs became reliable feeders to, and consultants for, those labs once they established their own academic groups. Most of the first academics

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[10] G. Binnig and H. Rohrer, "The Scanning Tunneling Microscope," *Scientific American* 253, no. 2 (1985): 50-6 and G. Binnig and H. Rohrer, "Scanning Tunneling Microscopy - From Birth to Adolescence," *Reviews of Modern Physics* 59, no. 3 (1987): 615-625.

[11] For descriptions of the big corporate labs and their relations with universities, see George Wise, *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research* (New York: Columbia University Press, 1985); Michael Riordan and Lillian Hoddeson, *Crystal Fire: The Birth of the Information Age* (New York: Norton, 1997); Lillian Hartmann Hoddeson, "The roots of solid-state research at Bell Labs," *Physics Today* (1977); Leonard Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (Cambridge, UK: Cambridge University Press, 1985).

to replicate the STM, therefore, had direct ties of collaboration or employment to IBM, the Zurich lab, or Binnig and Rohrer personally; and a few schools (Cornell, Penn State, Caltech, Berkeley, and Stanford) supplied the postdocs and managers who developed STM at Bell Labs and IBM Yorktown and Almaden [JD2; JS1; JV1; RF1; JF1; JG2].

Within such a small instrumental community, big corporate labs took up much of the air. Early work centered on getting microscopes to minimal operational benchmarks – usually atomic resolution of materials the Zurich team had imaged. These materials – particularly silicon – were metals and semiconductors with long histories in research and manufacturing at IBM and Bell Labs. Corporate STMers could draw on in-house expertise in quickly making and understanding samples. Post-Cancun, therefore, they turned these minimal benchmarks into fields of active research, racing to achieve atomic resolution of more and more metals and semiconductors.[12] Early academic STMers – such as Paul Hansma at the University of California at Santa Barbara, Calvin Quate at Stanford, and John Baldeschwieler at Caltech – initially competed in these races, but soon realized they did not possess the necessary expertise or resources to keep up.

Thus, Hansma (followed by Quate and Baldeschwieler) began carving niches where they were not in direct competition with their corporate colleagues, but could still benefit from corporate-academic interaction [PH1]. This meant instrument designs that drew on corporate STM work, but were targeted for applications other than metals and semiconductors. For instance, almost all early corporate STMers built ultrahigh vacuum (UHV) microscopes, since metals and semiconductors grow oxide films or collect impurities in air; in response, Hansma, followed by most academic STMers, shifted

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[12] Such races appear to be a recurring feature of young instrumental communities. They are especially visible in Bromberg (n. 2 above).

toward air operation. Air and UHV STMs have similar electronics and mechanics, but academics preferred air STM for simplicity and price, and to appeal to a disciplinarily-diverse audience of other academics, rather than the disciplinarily-restricted corporate surface scientists. That is, because they feared being drowned out by corporate colleagues, academic STMers shifted to new research milestones and developed flexible designs to yield new applications and attract new audiences. Clearly, this was commerce "influencing" academic research; yet it contradicts the dire picture of influence painted by some analysts. Corporations shaped academic work without "buying" professors or students, and their influence made the STM community more diverse and opened spaces for unexpected innovation.[13]

Many other university-industry linkages were woven into early STM. From 1981 through 1986, all STMs (and AFMs, after 1985) were home-built rather than purchased off-the-shelf. "Building" an instrument, though, encompasses a diverse spectrum of practices, ranging from building-from-scratch to buying-and-assembling. Commercialization of research is a creep along this spectrum rather than a sudden decision to sell one's knowledge. STM "builders" spent much time locating commercial sources for microscope components, fixing faulty instruments by thumbing through manufacturers' catalogs for new op amps or probe materials [CT1; JG2]. STM designs were shaped by commercial availability of components; but STM builders were also active consumers. On the one hand, they took commercial products and adapted them for

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[13] This is a more common phenomenon than has been recognized. For instance, some of today's academic nanotechnology centers can be traced back to efforts in the 1970s and 1980s to build facilities where electrical engineering and applied physics faculty could train students in the materials and methods used in the semiconductor industry. Since such facilities were never as well-resourced as companies like Intel and AMD, these faculty shifted milestones by broadening their research for a multidisciplinary audience – giving current nanotechnology its interdisciplinary character [HC1].

unforeseen uses; on the other, they negotiated with suppliers for equipment (vacuum chambers, piezoelectric crystals, video output devices, etc.) geared to their specific applications.[14] Some suppliers, such as Burleigh (a piezoceramic maker in upstate New York) modified components and forged enduring associations with STM builders to design products specifically for the STM market [DF1; JG2].[15] Creating the tools of university research – from buildings to microscopes to reagents – encompasses a wide variety of corporate-academic linkages. Some activities (ordering materials from a catalog; contracting with a builder to remodel a laboratory) are relatively unremarkable; others (professors consulting with manufacturers; home-built academic designs being transferred to corporate interests) attract praise and protest.[16]

Once annual STM Conferences began after Cancun, the labor of microscope-building decreased as old-timers directed newcomers to reliable components. Through this, tacit knowledge of STM-building became standardized through STMers' nearly ritualistic allegiance to certain brands.[17] Old-timers recommended some brands to push balky microscopes toward reliable operation; newcomers, eager to make up lost time,

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[14] 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 Ronald Kline and Trevor Pinch, "Users as Agents of Technological Change: The Social Construction of the Automobile in the Rural United States," *Technology and Culture* 37, no. October (1996): 763-95. For users' pressure on companies (often – as in instrumental communities – through threats to form their own cooperatives or firms), see Claude S. Fischer, *America Calling: A Social History of the Telephone to 1940* (Berkeley: University of California Press, 1992). For an overview of different kinds of user activity, see the essays in Nelly Oudshoorn and Trevor Pinch, eds., *How Users Matter: The Co-Construction of Users and Technologies* (Cambridge, MA: MIT Press, 2003).

[15] For an analysis of a similar phenomenon in particle physics in the 1930s, see Chapter 3 of Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

[16] For similar instances of negotiations between academic users of laboratory products and the products' manufacturers, see Michael Lynch, "Protocols, practices, and the reproduction of technique in molecular biology," *British Journal of Sociology* 53, no. 2 (2002): 203-220.

[17] For treatments of the concept of tacit knowledge, especially as applied to instrument-building, see H. M. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics," *Sociology* 9, no. 2 (May) (1975): 205-224; Harry Collins, "Tacit Knowledge, Trust, and the Q of Sapphire," *Social Studies of Science* 31, no. 1 (2001): 71-86; and Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (New York: Harper Torchbooks, 1962).

rarely questioned such recommendations. For instance, IBMers used a trademarked rubber called Viton (from Dupont) to dampen vibration, because Viton could survive ultrahigh vacuum [CG1; RT1; VE1]. Later, academics adapted IBMers' designs, and Viton became a hallmark of tunneling microscopy, even in academic STMs used in air or fluid, not vacuum.[18]

Corporate-academic linkages also guided what *materials* campus lab groups examined with their microscopes and how microscope designs were geared to those materials. Sometimes this was "direct" influence – though it is unclear who influenced whom. 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 '70s as a non-destructive characterization tool for manufacturing [MK1; JF1; DR1].[19] Non-destructive testing held tremendous promise for microelectronics, where chips are inspected throughout manufacturing yet where traditional tools (especially electron microscopy) require breaking and discarding expensive silicon wafers. Quate moved into STM believing it could be the next generation non-destructive evaluation tool; and he was quickly followed by his former students and postdocs at IBM.[20]

STM, though, requires a conducting (metal or semiconductor) sample, whereas most microelectronic materials have an insulating oxide layer. Indeed, controlled growth of oxides is crucial to turning silicon wafers into integrated circuits. This was unproblematic for corporate surface scientists tasked with generating basic knowledge

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[18] For similar instances of practices spreading through an experimental community through transmission of knowledge about particular *brands*, see Jordan and Lynch, "Dissemination, standardization, and routinization" (n. 3 above).

[19] See C. F. Quate, "Acoustic Microscopy - Recollections," *IEEE Transactions On Sonics and Ultrasonics* 32, no. 2 (1985): 132-135 for a brief description of scanning acoustic microscopy at Stanford.

[20] 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.

about materials like silicon and gallium arsenide. Yet STM's restriction to conducting materials blocked its use in non-destructive testing and hindered movement into fields other than surface science. Those who wanted to carve interdisciplinary niches for STM saw its sample constraints as suffocating; chief among these were Gerd Binnig (an IBM employee but not a surface scientist) and Cal Quate (and his former students and postdocs at IBM). So when IBM allowed Binnig to take a sabbatical at Stanford in 1985-6, he and Quate pushed past the STM to invent the AFM – which, because it uses interatomic forces rather than tunneling to sense height, can map insulating materials. Thus, Quate positioned his research much further downstream in IBM's manufacturing cycle than most of IBM's own STMers and, together, IBM and Stanford dramatically shifted the world of academic and corporate probe microscopy.

As Daniel Lee Kleinman has shown, though, corporate influence over academic research usually flows through indirect control over experimental materials rather than overt guiding of objectives.[21] For instance, when academic STMers designed microscopes for use in water or air they needed new "yardstick" materials (given the unsuitability of metals and semiconductors). Gold, paraffin, and graphite vied for the job; but graphite won out partly because ultrapure samples could be obtained cheaply [AG1]. 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, standardized samples. Thus, these dispersed academic

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[21] Daniel Lee Kleinman, *Impure Cultures: University Biology and the World of Commerce* (Madison: University of Wisconsin Press, 2003).

researchers built networks in their instrumental community by relying on corporate largesse; and as dependence on this largesse spread, it formed the basis for a standardized research infrastructure.

The Road to Commercialization

Crucial to commercialization of academic work on STM and AFM was the early segregation of the community into two distinct, dynamically linked, styles: on one hand, surface science STMers located predominantly in corporate labs; and, on the other, early academics (Quate, Hansma, Baldeschwieler, etc.), along with their collaborators and Quate's former students at IBM.[22] Corporate surface science STMers, particularly at Bell Labs and IBM, worked in large, resource-rich institutions alongside many people qualified to judge their work, whether competitors assigned to very similar projects, or managers with the power to review STMers' work and promote or fire them [JF2; DB1]. Postdocs and junior staff scientists who built STMs were under tremendous time pressures and so stuck to institutionally-approved projects. STM-building in corporate labs was a delicate balance; researchers had to design and use their microscopes to demonstrate individual initiative, but also had to integrate with a disciplined and inward-looking corporate style.

Academic STMers were more dispersed, and looked to a more diffuse audience. Where the corporate labs built what Terry Shinn calls "narrow niche" instruments geared to a thin slice of applications, academic groups moved toward a "research technology"

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[22] The pedagogical roots of this split are analyzed in Cyrus C. M. Mody, "Instruments in Training: The Growth of American Probe Microscopy in the 1980s," in *Pedagogy and the Practice of Science: Producing Physical Scientists, 1800-2000*, ed. David Kaiser (Cambridge, MA: Massachusetts Institute of Technology Press, 2005), 185-216.

paradigm of generic tools relevant to a variety of disciplines.[23] Because they had yet to find suitable applications for these tools, Quate, Hansma, and other early academic STMers trained students primarily to *build* highly flexible microscopes, and only secondarily to *use* them. This led to a proliferation of microscope designs such as STM in water, air, oil, and gas, and the critical shift to AFM. It also led students to try out microscopes on readily available cultural materiel, rather than scientifically disciplined specimens – leaves of houseplants, polaroids, bone from ribeye steaks, ice, the electrochemistry of Coke versus Pepsi, etc. [CP1; JN1]. This playfulness was accompanied by bricolage in instrument-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.

It was difficult, though, to make such images interesting or credible. Even where some disciplinary community had expertise about these materials – biochemists, for instance, know about bone and leaves – the specimens were prepared so haphazardly that images of them were unintelligible. These academic STM and AFM groups had little in-house expertise about what questions to ask concerning these samples, or how to interpret images of them. Thus, group leaders imported collaborators (postdocs or junior professors) for a few weeks or months to learn a little probe microscopy and leave to found new STM and AFM groups and report on the microscopes to their home

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[23] Shinn (n. 4 above). By manufacturing the *relevance* of STM and AFM to these disciplines, the academic groups turned those fields into "relevant social groups" party to the eventual shape of the technology; see Wiebe E. Bijker and Trevor Pinch, "The Social Construction of Facts and Artifacts: Or How the Sociology of Science and the Sociology of Technology Might Benefit Each Other," in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, ed. Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch (Cambridge, MA: Massachusetts Institute of Technology Press, 1987), 17-50.

disciplines.[24] These visitors fit into an ongoing tradition of teamwork at the California schools; where corporate surface science STMers worked in insulated, highly competitive, hierarchical groups (a manager overseeing postdocs and technicians), the academic groups thrived on a free-floating atmosphere in which students and postdocs simultaneously worked on their "own" as well other microscopes, and people and materials moved from project to project as needed. When they left, some visitors founded similar microscope-building programs; others simply took a second-hand instrument with them to aid in the struggle to carve a distinctive place in their home disciplines and secure tenure [JN1; JH1; BD2; SL1; AG1; CB1].[25]

Eventually, this dynamic fostered commercial production of STMs and AFMs. Outright commercialization, though, was preceded by a gray market in which researchers produced surplus microscope parts that they traded with acquaintances in the expanding network of STMers and AFMers – sometimes for money, more often for other tokens such as prestige or experimental materials.[26] Once IBM Research management saw the STM's discovery-making potential, for instance, they pushed expansion of STM work at their facilities. New STMers were hired to build microscopes – a laborious task with few guarantees of success. Indeed, the first replication at Yorktown failed, and the next took almost two years to catch up with Zurich [RF1; JG3; JD2]. Expansion proceeded more

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[24] This importation of credibility/knowledge through collaboration is apparent in many instrumental communities. It is best described in Rasmussen (n. 3 above). The coordination of different kinds of personnel and expertise around a common instrumental focus is nicely captured by the "trading zone" concept in Galison (n. 15 above), and the idea of circulating "boundary objects" described in Susan Leigh Star and James R. Griesemer, "Institutional ecology, 'translations' and boundary objects: amateurs and professionals in Berkeley's museum of vertebrate zoology, 1907-1939," *Social Studies of Science* 19, no. 1989 (1989): 387-420.

[25] 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 (n. 3 above).

[26] Pierre Bourdieu, "The Forms of Capital," in John Richardson, ed., *Handbook of Theory and Research for the Sociology of Education* (New York: Greenwood Press, 1986), 241-258.

quickly in Switzerland, where newcomers could interact face-to-face with the inventors. Still, the labor investment was daunting, especially to postdocs operating on limited time horizons.

Thus, IBM began making semi-standardized, batch-produced STM packages available to its researchers.[27] The first was the "Blue Box" designed by Othmar Marti, a Swiss graduate student doing doctoral work at IBM Zurich [OM1; JG1]. The Blue Box was primarily an electronics package – researchers constructed the hardware themselves, often using the Zurich team's designs. 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 shop to develop and batch-produce complete STMs to "sell" to other Yorktowners [JD2; RT1; BH1].

By 1990, 10 to 20 of these CSS STM's were in use at Yorktown and the nearby Hawthorne facility; some also traveled to academic groups when postdocs left to become professors [DB1]. Yorktown management encouraged use of the CSS STM by making its purchase a zero-cost budget item. 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; and advancement also required navigating competitive institutional politics, where groups worked in parallel on similar projects and were

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[27] See Philip Scranton, *Endless Novelty: Specialty Production and American Industrialization, 1865-1925* (Princeton: Princeton University Press, 1997) for an analysis of batch-production.

rewarded relative to each other. Postdocs using the CSS STM found they were viewed as partisans of Demuth's style of microscopy. To avoid alienating other factions at Yorktown, and display their own experimental prowess, they redesigned and rebuilt large parts of the CSS instrument. That is, the organization of Yorktown research kept the CSS microscope from turning into a widely-commercialized black box.[28]

The CSS STM was a *kind* of commercialization of tunneling microscopy, for the internal IBM market. Had Yorktown culture promoted formation of start-ups or collaborations with instrument manufacturers, the CSS microscope could have become the first mass-marketed STM.[29] After the early '90s recession made IBM leaner and more outward-looking, Big Blue did market an AFM – Yorktown's "SXM" – to the semiconductor industry. This exception, though, proves the rule. The SXM was invented by a former Quate postdoc, and owed much to Quate's style of work. Yet commercialization was hindered by its IBM origins – though capable of astonishing resolution of the sidewalls of integrated circuit features, it was too finicky and unreliable

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[28] For the classic analysis of the instrument as "black box" (i.e., a technology that takes over epistemic responsibility from the experimenter by virtue of the inaccessibility of its workings), see Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, 2 ed. (Princeton, NJ: Princeton University Press, 1986). My point here is that the "blackness" of the black box is continually reshaped in order to draw boundaries or form networks within an instrumental community. Commercialization can be a *gradual* process of making the black box more "translucent" and open to intervention; see Kathleen Jordan and Michael Lynch, "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. Fujimura (Princeton, NJ: Princeton University Press, 1992), 77-114. At the same time, the presentation of a commercial microscope architecture as "closed" (probe microscopists' term for a black box) or "open" (their term for translucent) is a political move designed to orient that microscope toward a particular disciplinary audience. See Mody "How probe microscopists became nanotechnologists" (n. 2 above).

[29] For a similar analysis of how corporate culture at IBM sometimes kept innovations from percolating, see Ross Knox Bassett, *To the Digital Age: Research Labs, Start-Up Companies, and the Rise of MOS Technology* (Baltimore: Johns Hopkins, 2002).

(it needed a Ph.D. to operate) to attract an industry devoted to tools kept in continuous operation by relatively unskilled workers [JG3].[30]

Yorktowners were clearly not buying an unproblematic black box that commodified microscope-building knowledge; groups that treated it this way often couldn't get it to work [DB1]. Most Yorktowners saw an institutionally-driven need to prove they could acquire this knowledge on their own. Most buyers, therefore, were purchasing *time*, a chance to enter a dynamic instrumental community quickly, but with the instrument-building credentials of veterans. In return, designers of the CSS STM received prestige and influence, rather than money. Similar motivations governed the first academic STM start-ups. Established STM and AFM groups – especially Quate's, Hansma's, and Baldeschwieler's – had long given advice and blueprints to new builders. Often, this consisted of guidance in *buying* reliable components for a homemade instrument and *assembling* purchased components in accordance with circulated blueprints. Whatever couldn't be bought was made by hand or batch-produced. These custom components circulated widely as gifts.[31] Software, in particular, passed from group to group, and student cohort to cohort within research groups. Both academic and corporate groups wrote code that they gave to collaborators, strengthening their group's position in the instrumental community, and ensuring access to collaborators' modifications to the code [SL1; MS1]. Sometimes code was given for free, sometimes at nominal cost; profit was not the motive for dissemination.

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[30] Likewise, attempts at spinning off small companies from Bell Labs' probe microscope research in the '90s fell apart partly because of managers' inexperience dealing with commercialization efforts by their subordinates. Moreover, the size and inward focus of both IBM and Bell Labs hindered these companies from cleanly patenting (and hence reaping profits from) their STM and AFM work [JG3].

[31] Davis Baird, "Scientific Instrument Making, Epistemology, and the Conflict between Gift and Commodity Economies," *Ludus Vitalis* Supplement 2 (1997): 1-16. The "moral economy" of making gifts and exchanges in an instrumental community is described in Kohler (n. 1 above).

The most well-traveled *hardware* innovation was the microfabricated AFM cantilever. One perceived defect of early AFMs was that probes were laboriously hand-made 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.[32] Although these cantilevers could yield exquisite AFM images, each required considerable time and training, and results were so particular to one cantilever and its maker that images taken with different cantilevers were difficult to compare. Hand-made 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 shared students with other electrical engineering professors at Stanford, allowing them to learn AFM before going to the clean rooms to learn to pattern and etch silicon into small, standardized batches of cantilevers. By 1990, Quate began sending surplus probes to friends and collaborators, sometimes so he and his students could share authorship of collaborators' papers. Quickly, Quate-type probes became essential to the AFM infrastructure [BD2; MK1; TA1].

Generally, only *parts* of microscopes circulated in this way. Quate, Hansma, and Baldeschwieler sometimes gave whole microscopes to long-term collaborators, but not to casual acquaintances. Most newcomers wanted the credentials and experimental control afforded by building their own microscope, anyway; but few wanted to take the time to build an STM from scratch. Thus, by 1986, demand grew for a commercial microscope to get these researchers up to speed. In this environment Doug Smith, a Quate student,

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[32] 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).

founded the Tunneling Microscope Company. Smith's company was an extension of, rather than a break from, the earlier probe microscope gray market. Like the Blue Box and the CSS STM, Smith's instruments were more starter kits than black boxed devices, sold to people with the skill (but not the time) to build one themselves. To use the instrument, customers needed to construct much of it on their own [JF1; NB1; RC1]. All 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.

Smith had only one "employee", a fellow student who helped put together scanners, and he recruited customers by word of mouth [MK1]. 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 postdoc [MK1; JN1]. Quate himself was ambivalent about creeping commercialism in his lab. He 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" [MK1].

Meanwhile, in Santa Barbara, Virgil Elings, Hansma's colleague in the UCSB physics department, heard about STM from Niko Garcia, a visiting Spanish academic with close ties to IBM Zurich. 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 co-found a company with Hansma. Hansma was even more wary than Quate of commerce encroaching on his lab's activities, so he declined; but he gave Elings the same advice and schematics he made available to other STMers [PH1; VE1]. 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") [VE1; MT1].

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 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 specifically for non-builders 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 by the annual STM Conference in 1987. Though his plan was always to sell a computer-controlled microscope (hence *Digital* Instruments), his former student (and DI co-founder), Gus Gurley, was brought in too late to finish all the code 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 didn't 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. Digital retained much of the flavor of a lab like Quate's or Hansma's; and Elings only slowly came to sell a more and more complete microscope, to rely less and less on customers' design expertise, and to view money as the primary token of exchange. 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 1's immature design, and for its lack of "serious" applications [VE1; DC1]. In following Hansma's lead, Elings designed an air STM, rather than the expensive, narrow-niche 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 ultrahigh vacuum (which, in any case, ruined samples relevant to everyone except surface scientists). Yet it was unclear in the '80s what air STM could be used for, or what the images it produced meant. Only in 1991-2 did a consensus develop that air STM was not, in fact, 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 STM and AFM).

Thus, the Nanoscope I and other air STMs had little rigorous application, though until 1992 it seemed possible such applications would appear if researchers could buy cheap commercial STMs and adapt them for unforeseen purposes. The Nanoscope I was a "toy" because it was meant to be superseded and because no one knew what it was good for. Buyers were envisioned as instrument-savvy and willing to "play" with their new toy until they found promising applications – i.e., customers were seen as similar to DI's engineers.[33] By assuming some parity between its designers and users, DI used the market to elicit design changes before producing the more black-boxed, all-digital Nanoscope II. Innovation came quickly because participants in the probe microscopy formed a savvy and critical body of consumers; in fact, people who had built their own STMs formed a small but elite group of DI's customers. As this shows, analyses of academic commercialization (both pro and con) should focus less exclusively on production, and more on professors' strategic mixing of consumption and production. Academic researchers are well-positioned to be active consumers whose reinterpretations of products flow back to the manufacturers and reshape design and marketing. As recent work in the history and sociology of technology has shown, *users* of many technologies actively shape the design and use of artifacts. This phenomenon is especially pronounced in instrumental communities, where the boundary between producers and consumers is especially porous.[34]

The assumed resemblance of engineers and early customers also reinforced DI's self-image as a free-wheeling start-up, with little marketing and no customer service. Elings' simply advertised in *Physics 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

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[33] This analysis resonates with Barry Dornfeld, *Producing Public Television, Producing Public Life* (Princeton: Princeton University Press, 1998) and Trevor Pinch and Frank Trocco, *Analog Days: The Invention and Impact of the Moog Synthesizer* (Cambridge, MA: Harvard University Press, 2002), which also describe producers' use of themselves as a template in imagining consumers. My thanks to Christina Dunbar-Hester and Trevor Pinch for discussion on this point.

[34] Similar markets are apparent among hobbyists and artists. See Kristen Haring, "Technical Identity in the Age of Electronics" (Harvard University dissertation, 2002) for the former and Pinch and Trocco for the latter.

1990 estimates that in the first three years, DI sold more than 300 Nanoscopes at $25,000 to $35,000 each.[35] 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 UHV STM, and the corporate labs became less dominant. High demand created a waiting list, prompting a policy that researchers who wanted a microscope quickly could promise to name DI's founders or employees as co-authors on papers generated with Digital's products.

DI recognized that to create a market among scientists and engineers, it had to demonstrate its trustworthiness as a producer both of microscopes and of knowledge.[36] Through its customers, DI associated credible facts with its instrument and its employees, enticing consumers of those facts to join the probe microscopy community by becoming consumers of its products. Notably, because the community still largely operated on a gray market, Digital had to rely heavily on barter – such as trading waiting list position for authorship. Such trades gradually diminished as the probe microscopy community became more commercial; indeed, "commercialization" often signifies the narrowing of the varieties of exchange as a technology stabilizes, rather than the encroachment of a peculiarly corporate ethic into the academy.[37]

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

[36] This analysis draws on Latour and Woolgar's concept of the "cycle of credit", but from the perspective of the instrument seller, rather than the buyer. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, 2 ed. (Princeton, NJ: Princeton University Press, 1986).

[37] There is a complicated relationship between "commercialization" and "stabilization" of a technology; for analyses of stabilization, see Wiebe E. Bijker, *Of Bicycles, Bakelite, and Bulbs: Toward a Theory of Sociotechnical Change*, ed. Wiebe E. Bijker, Trevor Pinch, and Geof Bowker, *Inside Technology* (Cambridge, MA: Massachusetts Institute of Technology Press, 1995) and Paul Rosen, "The Social Construction of Mountain Bikes: Technology and Postmodernity in the Cycle Industry," *Social Studies of Science* 23 (1993): 479-513.

Roots of DI's Success

Because DI was the first serious STM manufacturer, its successors played on its terms. Some competed head-on for the general, multidisciplinary market; in the end, these firms faltered. Those who concentrated on smaller subcommunities have fared better, coping with the prospect that DI might someday target their market niche. Unlike the majority of academic start-ups, DI has been immensely profitable and innovative; yet this unusual success highlights broader truths about the commercialization of academic knowledge. In particular, the contingent character of the instrumental community surrounding STM and AFM – and DI's role in it – elucidates debates about how universities can promote entrepreneurial activities among their faculty, and whether academic entrepreneurialism can be a driver in regional economies.

From a regional perspective, DI clearly benefited from the geography of probe microscope research, but in unexpected ways. Hansma and Elings both molded their free-form experimental styles to their group members' notions of "California culture" [SG1; JN1].[38] At UCSB, for instance, (in contrast to STM groups at IBM and Bell Labs) Hansma's group integrated self-cultivating hobbies (photography, woodworking, meditation, travel) with gray market activities in the laboratory – undisciplined exploration of the instrument's capabilities, trolling for new applications, collaborations between instrument builders and representatives of various disciplines. Eventually, this mélange of exploratory individualism and easy-going collaboration fed into DI and similarly off-beat start-ups at universities along the West Coast. None of the successful STM and AFM manufacturers were big, established companies like Hitachi, Philips, or

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[38] This was also true at other centers of commercialization (Stanford, Berkeley, and Caltech), though what counted as peculiarly "California" took on decidedly local flavors.

IBM (though big companies did make forays), and except for small European firms almost all manufacturers got their initial designs through collaborations with academic (or quasi-academic national lab) groups in the American West or Southwest.

Digital's greatest regional stimulus, though, was largely negative. DI emerged within, but was separate from, the enormous military-industrial manufacturing complex of Santa Barbara and the Los Angeles basin. Many early DI employees wanted to stay close to the area's picturesque and casual environment, and saw DI as the only alternative to local defense firms.[39] This offers a curious lesson for those hoping to stimulate regional innovation. Universities do participate in local culture, but they have little control over which aspects of local culture professors adopt as emblematic of their experimental styles; nor can universities control how those professors integrate local culture with the values and social contours of widely distributed instrumental communities. Hansma and Elings were able to construct their own stylized "Californias" in guiding lab work largely because the STM community's division of labor encouraged academic groups to be more exploratory than corporate ones.

Likewise, DI's success had roots in institutional arrangements at UC Santa Barbara, though not the patent and tech transfer offices favored by proponents of academic entrepreneurialism. Rather, DI and Elings thrived at UCSB's disreputable margins. When he arrived in the late '60s 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 their less prestigious Master's of Scientific Instrumentation program – a lucrative but unloved

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[39] The same can be said for employees at the smaller start-up manufacturers in LA – Quanscan, Topometrix, Pacific Scanning, Quesant.

backwater [JW1]. Uncowed, Elings transformed the master's program into his personal empire and a fountainhead for patents and start-up companies.

Crucially, 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 industrially-relevant meters and tools. Initially, Elings relied on orthodox classroom instruction; but soon, he drifted toward an alternative method that prized tacit over formal knowledge, participation over instruction. Instead of 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 how to understand customers' needs and design technologies to answer them. This made former master's students the most important source of early employees for all Elings' ventures, especially Digital Instruments.

So UC Santa Barbara 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 Elings' commercial ventures hindered his academic career, the UCSB physicists made it more likely his next enterprise – 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.

All the same, the similarity of work in the Hansma group to the pedagogical style Elings derived for his master's program is striking. Both Elings and Hansma saw tacit, rather than formal, knowledge as primary in instrument-building. This 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 [MT1; JM3; BD2; HH1; PH1; DB2]. This diversity was unthinkable at other centers of probe microscopy. The shared emphasis on tacit knowledge meant both DI and the Hansma group thrived on self-cultivating activities seemingly unrelated to technical matters; DI, for instance, held a weekly "inventing session" where employees brainstormed solutions to esoteric (i.e. non-AFM-relevant) technical questions (e.g. "how do you make a self-balancing laundry machine") to become better inventors and hone their skills at weathering Elings' intense skepticism [DB2; PM1].

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), Hansma graduates and postdocs poured into the company [SG1; CP1; JC2]. 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 [BD2; JM3]. 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 [JH1].

In turn, once the "toy business" ended in the early '90s, Elings began to imitate Hansma's tactic of bringing in postdocs to guide instrument-builders' efforts. DI 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 [SM2; MA1].[40] Though DI was a profit-making venture, its success arose partly from the Hansma group practices that it mirrored – doing research, publishing articles, training and "graduating" employees. These practices were then widely emulated; other start-ups, such as Park Scientific Instruments (founded by Sung-Il Park and Sang-Il Park, two unrelated Stanford postdocs), Molecular Imaging (founded by Stuart Lindsay and Tianwei Jing, one of his former postdocs), and Quanscan/Topometrix (founded with John Baldeschwieler's help by Paul West, one of his former postdocs), all brought aspects of academic research to their companies.[41]

This kind of cultural/organizational parallelism from university to start-up is well-documented in other fields of commercialization, especially biotechnology; there, it was

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[40] Instrument manufacturers' use of researchers to produce articles and applications notes is a prominent feature of both Rasmussen (n. 3 above) and Lenoir and Lécuyer (n. 3 above).

[41] The copying of experimental/organizational style first from university to start-up, and then among a field of start-ups, is a classic example of "institutional isomorphism" – the spread of innovations across organizations due to competition for personnel whose professional affiliations demand particular organizational forms or practices. Paul J. DiMaggio and Walter W. Powell, "The iron cage revisited: Institutional isomorphism and collective rationality in organizational fields," *American Sociological Review* 48 (1983): 148-160 and Paul J. DiMaggio, "Constructing an Organizational Field as a Professional Project: U.S. Art Museums, 1920-1940," in *The New Institutionalism in Organizational Analysis*, Walter W. Powell and Paul J. DiMaggio, ed. (Chicago: University of Chicago Press, 1991), 267-292.

used to entice professors to exit the ivory tower, leading to trouble when professor-entrepreneurs were reluctant to turn their companies from research to profit-making. With STM and AFM, in contrast, corporate-academic isomorphism was successful less as a deliberate strategy than as a contingent and emergent harmonization of practices. It was a largely accidental outcome of the organization of this instrumental community that Quate, Hansma, and other academics promoted a gray market of circulating people, practices, and technologies that fostered successful commercialization. The pedagogy associated with this gray market was hardly "sullied" by commercialism; yet when people tied to these groups commercialized the instruments, they used the pedagogy of the gray market to organize firms and markets.

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. Commerce and academic research are distributed activities that only weakly map onto institutions such as universities and companies. Commercialization has many guises; there are many different ways to align corporate and academic interests, and many tokens for which to barter academic knowledge. It was the looser, *indirect* ties between corporate and academic groups that fostered growth of STM and AFM and encouraged start-ups to emerge from academic groups, 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've 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, etc. 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 reach this point, though. 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. The presence of gray markets in instrumentation *can* enhance national economic growth over time; yet university administrators who hope that this or that gray market can be converted into a profit-making start-up to enhance local, short-term economic growth 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 in which the cultural map is unconducive to profit-making nevertheless provide the infrastructure and knowledge/labor pool for communities in which profit may be enormous. Policy-makers 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. Policy makers 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. By focusing on the wider instrumental community surrounding a technology, we can see that the university may actually be more influential in maintaining a pool of skeptical, independent *consumers* who can threaten start-ups with the prospect of making their own tools or even founding their own firms.

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

AG1: Andy Gewirth: Hansma collaborator; University of Illinois; 6/26/01
AY1: Ali Yazdani: IBM Almaden postdoc; University of Illinois; 6/26/01
BD2: Barney Drake: Hansma group technician; UCSB; 10/18/01
BH1: Bob Hamers: IBM Yorktown researcher; University of Wisconsin; 5/9/01
BS1: Brian Swartzentruber: Bell Labs technician; University of Wisconsin; Sandia National Laboratory; 1/10/03
CB1: Carlos Bustamante: Hansma collaborator; University of Oregon; UC Berkeley; 10/17/01
CG1: Christoph Gerber: IBM Zurich technician; 11/12/01
CP1: Craig Prater: Hansma graduate student; Digital Instruments engineer; 3/19/01
CT1: Clayton Teague: National Institute of Standards and Technology; 6/28/02
DB1: Dawn Bonnell: IBM Yorktown postdoc; University of Pennsylvania; 2/26/01
DB2: Dan Bocek: UCSB undergraduate; DI engineer; Asylum Research; 3/23/01
DC1: Dan Chernoff: Sohio Research; Advanced Surface Microscopy; 9/5/01
DF1: Dave Farrell: Burleigh Instruments; 5/29/01
DR1: Dan Rugar: Quate student; IBM Almaden researcher; 3/14/01
HC1: Harold Craighead; Bell Labs researcher, Cornell University professor, 5/26/05
HH1: Helen Hansma: UCSB professor; 3/19/01
JD2: Joe Demuth: IBM Yorktown manager; 2/22/01
JF1: John Foster: Quate student; IBM Almaden researcher; 10/19/01
JF2: Jane Frommer: IBM Almaden researcher; 3/14/01
JG1: Jim Gimzewski: IBM Zurich researcher; UCLA; 10/22/01
JG2: Jene Golovchenko: Bell Labs; Harvard; 2/20/01
JG3: Joe Griffith: Bell Labs; 2/28/01
JH1: Jan Hoh: Hansma postdoc; Johns Hopkins; 6/10/02
JM3: James Massie: Elings master's student; DI engineer; 10/18/01
JN1: Jun Nogami: Quate postdoc; Michigan State; 6/28/01
JS1: Joe Stroscio: IBM Yorktown postdoc; National Institute of Standards and Technology; 6/28/00
JV1: John Villarrubia: IBM Yorktown postdoc; National Institute of Standards and Technology; 6/28/00
JW1: Jerome Wiedmann: Elings master's student; DI employee; 10/18/01

KB1: Ken Babcock: DI employee; 3/23/01
MK1: Mike Kirk: Quate student; Park Scientific Instruments; KLA-Tencor; 10/12/01
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 Santa Barbara; 3/19/01
PM1: Pete Maivald: DI employee; 10/18/01
PW1: Paul Weiss: IBM Almaden postdoc; Penn State; 5/3/01
RC1: Rich Colton: Naval Research Lab; Baldeschwieler collaborator; 6/27/02
RF1: Randy Feenstra: IBM Yorktown researcher; Carnegie-Mellon; 5/2/01
RT1: Ruud Tromp: IBM 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
TA1: Tom Albrecht: Quate student; IBM Almaden researcher; 3/14/01
VE1: Virgil Elings: UC Santa Barbara; Digital Instruments; 3/20/01