

## CHAPTER 3

---

# MANAGING COLLABORATIONS AT THE UNIVERSITY-INDUSTRY INTERFACE

## An Exploration of the Diffusion of PCR and rDNA

Andrew J. Nelson  
*University of Oregon*

---

### ABSTRACT

Collaborations between universities and firms are of growing interest to scholars and practitioners alike, many of whom see these relationships as key to knowledge diffusion. While the literature on university-firm collaborations has made a number of contributions, however, it has not engaged in a deep exploration of the actual processes by which knowledge may “diffuse” through collaboration. In this study, I explore the diffusion of recombinant DNA and polymerase chain reaction—two key techniques in biotechnology—and I employ the frames of “public” and “private” science

to investigate how different incentives shape perceived collaboration costs and, therefore, behaviors. My analysis demonstrates the important role of collaborations not merely in “diffusing” know-how around techniques but also in mutually extending techniques. In contrast to most studies of university–firm relationships, I also point to firms as important partners in diffusion *to* universities. Finally, I demonstrate how public and private science frame collaboration issues differently for university- versus firm-based researchers, resulting in different patterns and network structures whose overall functioning depends, at least in part, on the conflicts between incentives.

## INTRODUCTION

Recent years have witnessed growing interest in collaborations between universities and commercial firms (e.g., Cockburn & Henderson, 1998; Powell, Koput, & Smith-Doerr, 1996; Powell, White, Koput, & Owen-Smith, 2005; Zucker, Darby, & Armstrong, 2002). These inter-organizational collaborations are motivated by a recognition that cutting-edge technologies and fast-moving markets often require knowledge and capabilities that extend beyond those of lone organizations. In turn, a primary aim of most collaborations is to facilitate “knowledge diffusion” among partners.

A number of studies have explored various aspects of university–firm collaborations, including the different forms that such collaborations may take, the effectiveness of these collaborations towards accomplishing firm objectives, the role of policy initiatives in motivating these collaborations, and the mediating effects of geography and other variables (e.g., Audretsch, Link, & Scout, 2002; Ham & Mowery, 1998; Link, Paton, & Siegel, 2002; Mowery, Nelson, Sampat, & Ziedonis, 2004; Perkmann & Walsh, 2007; Schmoch, 1999). By assessing broad patterns, however, this literature has largely overlooked the processes by which collaborations serve to diffuse knowledge around specific scientific techniques.

More generally, the focus on university–firm collaborations tends to be motivated by the presumption that universities and firms subscribe to different norms and incentives surrounding scientific practice (e.g., Cockburn & Henderson, 1998; Fini, Lacetera, & Shane, 2010; Henderson, Jaffe, & Trajtenberg, 1998; Jaffe, 1989; Mowery, Nelson, Sampat, & Ziedonis, 2001; Siegel, Waldman, Atwater, & Link, 2003). These perspectives are reflected, most often, in the language of “public science,” which emphasizes open sharing and a reward system based upon prestige, and “private science,” which emphasizes secrecy and a reward system based upon economic returns (Merton, 1973; Dasgupta & David, 1994; Sauermann & Stephan, 2013; Stephan, 1996). A number of survey-based studies have attempted

to assess differences between universities and firms that stem from these perspectives and have demonstrated, generally, that commercial pursuits can be at odds with open sharing practices (e.g., Blumenthal, Campbell, Anderson, Causino, & Louis, 1997; Campbell et al., 2002; Walsh, Cohen, & Cho, 2007). Other more grounded investigations, however, paint a more complex picture, highlighting how researchers within both universities and firms may struggle with full and open disclosure (Murray, 2010; Owen-Smith & Powell, 2001; Rhoten & Powell, 2007; Vallas & Kleinman, 2008).

These studies of public science versus private science and of universities versus firms typically focus upon patterns of sharing and commercialization. Such a focus is logical given the emphasis of the public-science/private-science framework on the “dissemination” rather than “creation” of results. At the same time, however, this focus also has left us with little understanding or evidence of the ways in which university- and firm-based researchers approach collaborations differently, and of how public and private science may shape these decisions and activities. (For an important exception, see Bikard, Murray and Gans, 2015.)

In this study, I explore the role of collaborative relationships in the diffusion of two techniques that underlie the biotechnology industry: Polymerase Chain Reaction (PCR), which was invented by the biotechnology firm Cetus, and Recombinant DNA (rDNA), which was invented by a team from Stanford and the University of California at San Francisco. Drawing upon extensive interview data with researchers based in both universities and firms, I detail the ways in which collaborations did not simply “diffuse” knowledge from one partner to another so much as they enabled multiple researchers to mutually develop and extend the techniques, thereby enabling future diffusion efforts. In this process, I highlight how firms serve not merely as “recipients” of university knowledge, but also as “creators” and “diffusers.” I also explore the role of public and private science in shaping decisions around these collaborations, detailing the ways in which university and firm patterns vary, generally speaking, while also highlighting important exceptions to these general patterns. Finally, building on these findings, I propose that technology-focused collaborative networks themselves can be held together by the very contradictions between public science and private science that they integrate.

## UNIVERSITY–FIRM COLLABORATIONS

Much of the work on university–industry collaborations places these relationships within the larger context of “university technology transfer.” For example, in a highly influential study, Cohen, Nelson, and Walsh (2002) surveyed R&D managers about the “channels” through which they accessed

university research that they applied to their projects. The survey included such collaborative arrangements as joint ventures, contract research, consulting, and personnel exchange. Agrawal and Henderson (2002) conducted a similar exercise, but from the perspective of academic researchers. (They focused on a set of engineering departments at MIT.) They found that collaborations were responsible for 12.1% of research “transferred” from academia to industry (compared with a normalized figure of 9.1% from the 2002 Cohen et al. survey). Bekkers and Freitas (2008), D’Este and Patel (2007), Faulkner (1994), Meyer-Krahmer and Schmoch (1998), and Scharfetter, Rammer, Fischer, and Frölich (2002) all conducted similar surveys, focused upon different geographies and fields but largely supporting the same results.

It is worth noting that relationships in the other direction—from firms to universities—are almost entirely absent in this literature. In other words, the literature focuses upon universities as a “source” of knowledge and on the ways in which firms can “access” this knowledge. In this way, most scholarship on university technology transfer is largely disconnected from the work of Stokes (1997), Rosenberg (1992 & 1994), Hounshell and Smith (1988), and others who point to the research contributions of industrial R&D groups and to the ways in which these contributions enable later advances by university-based researchers.

More generally, Perkmann and Walsh (2007) argue that the view of academic-industry ties reflected in surveys on “channels of diffusion” is overly simplistic. They propose, instead, that scholars focus on the depth of “relationship” that accompanies a tie. For example, the simple transfer of intellectual property has low relational involvement. By contrast, “situations where individuals and teams from academic and industrial contexts work together on specific projects and produce common outputs” (Perkmann & Walsh, 2007, p. 263) have high relational involvement. Although the term “collaboration” may be used loosely to refer to any point along this relational continuum, it is most appropriately used to describe the “high” end of the relational spectrum where deep personal contacts and mutual goals are the focus.

Yet here, too, the literature suffers from a disconnect. A number of studies focus on deeper collaborative relationships of the sort that Perkmann and Walsh (2007) highlight—that is, moving beyond licensing to explore more relational aspects of collaboration. The vast majority of these studies, however, focus on evaluating the effectiveness of collaborations and of related policy initiatives (e.g., Audretsch et al., 2002; Ham & Mowery, 1998; Link et al., 2002; Mowery et al., 2004); on the effect of collaborations upon the kind of research conducted at universities (e.g., Behrens & Gray, 2001; Geuna, 2001; Perkmann & Walsh, 2009); and on the role of mediating variables such as geography (e.g., Scharfetter et al., 2002; Schmoch, 1999). By

contrast, very few studies examine what actually takes place in a given university–industry collaboration and how collaborations aid in the diffusion of knowledge around specific scientific findings or techniques.

Part of this lack of attention to such micro-level processes is tied to a methodological issue: as with the “channels of diffusion” research, the typical approach to assessing relational collaborations is via a survey of university and/or firm participants (e.g., Cohen et al., 2002; D’Este & Patel, 2007; Mansfield, 1995). While these instruments provide us with a general sense of patterns and relationships, they do not illuminate micro-level processes. Thus, while this research has enhanced our understanding of university–industry collaborations generally, it has spent far less time investigating the actual knowledge diffusion processes that justify such collaborations.

## PUBLIC AND PRIVATE SCIENCE

Another branch of the literature on universities and firms also has relied heavily upon survey data. Its focus, however, has been on the sharing of research results rather than the collaborative patterns underlying their creation. The impetus for this scholarship can be traced to Merton’s (1942, 1973) foundational work on the “ethos of science.” Merton proposed that a set of distinct norms guide scientists’ actions. Chief among these norms is that of “communalism,” by which “the findings of science are a product of social collaboration and are openly disclosed to the community” (Merton, 1973, p. 269). In Merton’s conceptualization, the rewards for scientists lie in community recognition and prestige. Communalism reinforces these goals since public dissemination of results is the mechanism by which a scientist develops a reputation within a community.

Merton (1973, p. 275) was careful to note that commercial pursuits were incompatible with his ethos of science. Many years later, Dasgupta and David (1987, 1994) developed this point more fully by distinguishing two modes of science—“public science” and “private science”—on the basis of what scientists intend to do with their work. To Dasgupta and David, the fundamental distinguishing factor between public and private science is adherence to Merton’s norm of communalism. “What matters, . . .” they write, “is, most importantly, what researchers do with their findings” (Dasgupta & David, 1994, p. 495). Under public science, therefore, researchers adhere to the Mertonian norm of open dissemination of results; under private science, by contrast, a fundamental concern with commercial and financial implications leads to secrecy.

A wide variety of studies have employed the public-science/private-science contrast, typically assuming that universities conform to public science and that firms conform to private science (e.g., Cockburn & Henderson,

1998; Fini et al., 2010; Henderson et al., 1998; Jaffe, 1989; Mowery et al., 2001; Sauermann & Stephan, 2013). The distinction between public science and private science, however, is not a distinction between organizations or organization types. Instead, it refers to the institutional imperatives surrounding knowledge diffusion. As Dasgupta and David (1994, p. 495) write, "The same individual, we suppose, can be either [public-science or private-science oriented], or both, within the course of a day." In other words, what matters is the intended use of research results, not the type of organization in which research is conducted or with which an individual is affiliated. In fact, detailed examinations of academic scientists, specifically, reveal that they can subscribe to both public science and private science simultaneously, attempting to navigate and adjudicate their contradictory prescriptions (e.g., Nelson, in press; Owen-Smith & Powell, 2001; Vallas & Kleinman, 2008). Thus, while Dasgupta and David claim that "loosely speaking" (1994, p. 495) public science may be associated with the world of academic science and private science may be associated with the world of industrial and military research, they are careful to emphasize that it is the institutional imperatives, not the organization types, that matter.

This distinction, however, raises an issue: if public science and private science are not neatly delineated between the academic and industrial realms, then investigations of their effects on the practice of science must move beyond organizational type-casting to assess how individual researchers in each environment conceptualize and respond to the "sharing versus secrecy" dilemma. Applied to the case of collaboration, the framework of public versus private science raises another issue, too: Dasgupta and David's distinction is based upon the "intended use" of research results and to the sharing of these results once they have been obtained; it is not based upon differences in processes that *lead* to these results. For example, while the framework posits that researchers oriented towards private science will focus on the commercial implications of science, it does not suggest how this commercial focus will shape the ways in which they collaborate in order to "generate" science. Similarly, while the framework posits that researchers oriented towards public science will openly share results, it does not suggest whether they will be collaborative in the creation of these results.

Taken together, this review suggests that the existing literature on university-firm collaboration, while useful, has failed to unpack the knowledge sharing processes at the heart of such collaborations and has employed somewhat crude university-firm/public-private science dichotomies while not fully investigating how these perspectives shape collaboration. My goal in this chapter, therefore, is to provide a more nuanced account of knowledge sharing through university-firm collaborations and of the ways in which public and private science shape the collaborative perspectives and actions of participants in each organizational environment.

## DATA AND METHODS

I structure my investigation of knowledge sharing through collaboration around two key techniques in biotechnology. Biotechnology is a strategic setting for this investigation since numerous studies have identified this field, more so than any other, as reliant upon university-firm collaborations (Cockburn & Henderson, 1998; Powell et al., 1996; Powell, 1998; Zucker et al., 2002). Within biotechnology, I focus upon two techniques: PCR and rDNA. Together with Monoclonal Antibodies, most observers consider these two techniques to lie at the very heart of the field and to be essential to its practice (e.g., Galambos & Sturchio, 1998).

Kary Mullis is credited with inventing PCR in 1985 when he was employed by the biotechnology firm Cetus. (See Rabinow, 1996, & *Smithsonian*, 1993). The technique allows a scientist to exponentially amplify a small amount of DNA, thereby providing enough genetic material to facilitate diagnostics and other lab procedures such as DNA "fingerprinting." Stan Cohen of Stanford University and Herb Boyer of the University of California at San Francisco are credited with developing rDNA in 1973 (Hughes, 2001). The technique, also called gene splicing, allows scientists to isolate genes and then insert them into another cell to create modified organisms. PCR thus represents a technique with origins in a commercial firm, while rDNA has origins in universities. Both PCR and rDNA were published and patented, indicating dual attention to open sharing and commercial implications. Moreover, both techniques led to Nobel Prizes, indicating their importance to the field of biotechnology.

Although publications, seminars, and media attention alerted the world to the existence of these breakthroughs, the actual performance of the techniques was not straightforward—especially in the time period shortly after invention. For example, Zucker, Darby, and Brewer (1998, p. 291) argue that rDNA exhibited "natural excludability" for the first several years, such that only a "small initial group of discoverers, their co-workers, and others who learned the knowledge from working at the bench-science level with those possessing the requisite know-how" were able to perform the technique. A primary interest on the part of many researchers thus focused on learning "the requisite know-how"—a feat best accomplished, as I will demonstrate, through collaborative work. (See also Zucker, Darby, Brewer, & Peng, 1996).

My core focus, therefore, is upon how researchers gained the ability to perform the techniques and to apply them to a broadening range of scenarios. This focus roughly equates, then, to "know-how," rather than "know what" or "know why" (Garud, 1997). I acknowledge, however, that these different dimensions of knowledge are, of course, intertwined.

I base my analysis of knowledge sharing through collaboration, and of the influence of public and private science, primarily upon 51 interviews. Interviewees included the inventors of the respective technologies, scientists in their labs, other scientists and senior executives at firms and universities who built upon the technologies, and administrators charged with licensing the technologies. The interviews offer special attention to personnel affiliated with Cetus, Stanford, and UCSF (the inventing organizations) and with Genentech and Perkin-Elmer, which did significant and early development work with rDNA and PCR, respectively. While I conducted many of these interviews myself, I also draw upon two archival collections: The UC Berkeley Bancroft Oral History project performed extensive oral histories with many of those involved in the early years of rDNA research, while the Smithsonian Institution conducted a detailed history of PCR. Together, these two sources provided 3,231 transcript pages of interview material with a total of 34 interview subjects.

The use of interviews from external sources serves three purposes. First, it allows me to draw upon a wider range of interview subjects than I could reasonably contact and interview myself. Second, since I had no influence on the questions in the Bancroft and Smithsonian oral histories, the use of these sources permits me to sidestep the objection that my phrasing of questions may have reflected a latent bias to include a particular response. (Given the importance of collaboration in science and the incredible depth of the Bancroft and Smithsonian studies, all oral histories that I consult addressed the topics of collaboration and public/private science). Third, and perhaps most important, the use of existing oral histories allows me to draw upon interviews spanning the 18-year time period from 1992 through 2009. Given recent shifts in academic norms in biotechnology (e.g., Murray, 2010) and legitimate methodological questions about retrospective bias, the use of interviews over this long historic span allows me to remain “closer” to the relevant time periods discussed.

I complemented the Bancroft and Smithsonian histories with 17 semi-structured interviews that I conducted myself between the years 2005 and 2010. These interview subjects broadened the array of informants from firms and captured reflections from university scientists at earlier stages in their careers, such as postdoctoral fellows.

I began my analysis by flagging passages in which interviewees described collaborating with another person in order to learn how to perform one of the techniques (or in order to teach someone else how to do so). Thus, I focused on deep(er) relationships as the basis of collaboration, rather than examining formal contracts between organizations or other measures of “collaboration” (Perkmann & Walsh, 2007). In other words, my interest lay in the actual interaction between two (or more) individuals as they worked together on the techniques, which I define as “collaborative activity.” I did

not focus on the contractual relationships or other administrative features underlying these interactions.

In analyzing collaborative accounts, I paid special attention to “who shared what with whom” and to how mutual understandings of the techniques changed over the course of a collaboration. My frame, therefore, was not one of “information exchange.” Rather, I examined which specific aspects of performance an interviewee called attention to and I assessed how they described their abilities and resources tied to these aspects. I then traced changes along these dimensions. For example, Daniel Yansura, who would join Genentech in the late 1970s, described to Bancroft Library historian Sally Smith Hughes how he expanded his knowledge of how to perform rDNA in 1977:

**Yansura:** In Marv Caruther’s lab we had done a number of the basic steps. We had worked with synthetic DNA all the time and had been ligating it together. We had barely gotten to the point where we were putting it in plasmids, although we didn’t do that work ourselves; we had to collaborate with a professor called John Sadler at the University of Colorado Medical Center in Denver.

**Hughes:** And he was constructing plasmid vectors?

**Yansura:** Yes, he knew how to do the transformation and the follow-up. And in Marv’s lab we knew how to ligate everything together, so we collaborated on that. Between the two of our groups we had done the first steps in genetic engineering, putting DNA into plasmids and replicating them, and so forth (Yansura, 2002).

Yansura thus breaks out rDNA into different steps, including “ligating” and “putting it in plasmids.” He knew how to ligate, but did not know how to put it in plasmids. Completing the full procedure, therefore, required collaboration with another group.

I did not code “public science” and “private science” directly. Instead, I flagged passages where interviewees discussed related concepts, including open disclosure, publishing, patenting, commercialization, competition, and incentives. I then compared differences across organizational environments and I examined these relationships for larger patterns and groupings. For example, Dave Goeddel, another early Genentech employee, discussed sharing with Bancroft historian Sally Smith Hughes:

**Hughes:** You said something earlier about [co-founder Bob] Swanson [whose background was in venture capital, not science] being concerned about information leaking out. That led me

to think about publication and scientific talks and all that. Was there a policy?

**Goeddel:** I think [co-founder Herb] Boyer [who came from UCSF] pretty much told Swanson, "If we're going to have a company, we're going to publish. That's how we'll continue to have good scientists." So Boyer took the brunt of the conflicts there and I think had convinced Swanson that if he was going to have a top company, people were going to publish and go to scientific meetings. So Bob had agreed to that. There would be certain times—maybe the first one was tPA. We had cloned tPA, were going to publish. Bob wanted to have a meeting and said, "Okay, I'm not going to tell you you can't publish. But does anyone else have tPA clones?" And we said, "No, no one else." "Then why do you need to publish it now?" "Well, we want to publish it first." "Okay, let's wait a few months and then see." And in a few months—it might have been three months or so—we heard a rumor that someone else might have it. We went back and met with Bob and he said, "Okay, publish it. You still get to be first, but you don't give away the information too early" (Goeddel, 2003).

Goeddel thus describes the tension within Genentech between sharing results, in order to attract and retain top-quality scientists, and not sharing results in order to protect Genentech's competitive position. He associated Boyer (who had a background in academe) with a more open perspective, and Swanson (who had a commercial background) with a more secretive perspective. He also describes how this tension around sharing was present in a single organization and how it was resolved through a strategy of delay and competitive monitoring.

I complemented these qualitative data with quantitative data tied to citations. As noted, PCR and rDNA were both published and patented. I identified the original publications and patents stemming from these techniques and then traced 20 years forward from each of these dates. I did not count review publications or "non-science" publications that referenced these articles but did not build upon the techniques. In the case of PCR, this procedure resulted in 6,126 publication citations and 2,014 patent citations. In the case of rDNA, this procedure resulted in 540 publication citations and 285 patent citations. Admittedly, the use of core publications and patents alone provides a limited view of diffusion. For example, Katz and Martin (1997) note that two researchers may collaborate extensively without writing up the results. Or, organizations may "pre-contract" assignment on a patent, such that joint work is not reflected in joint assignment

of a patent. Thus, the data likely understate the extent of collaboration around the techniques. While the use of keywords and/or "two-step" citations may provide a more comprehensive view of the full diffusion of the techniques, these approaches also raise new questions as to the accuracy of claimed links (Nelson, 2009). (Zucker et al., 1996, describe an alternative approach using GenBank, though their focus is only upon publications involving "star" scientists and GenBank is uniquely applicable to rDNA, not PCR.) My intention in presenting limited descriptive statistics and network images based upon these citations is not to present quantitative "proof"—an objective far beyond the aims of this exploratory study—but rather to suggest how processes and tendencies identified through the qualitative analysis may be reflected in quantitative patterns.

## RESULTS

I found that collaboration played an important role in diffusing both PCR and rDNA. A number of interviewees described how in-person collaborations that crossed organizational boundaries were essential for the diffusion of the techniques. In contrast to the dominant "knowledge transfer" perspective in the literature, however, I found that collaborations served to facilitate the mutual performance and extension of the techniques—not merely transferring knowledge, but rather developing it further. In the following sections, I elaborate upon these points.

Although researchers viewed collaborations as highly beneficial, as the existing literature has established, I found that public and private science uniquely shaped the perceived costs of collaboration. To an extent, these differences are reflected in the organizational arrangements and individual perspectives associated with universities and firms, though I also highlight important exceptions to these patterns. Finally, I detail the overall collaboration network structure that develops, in part, on the basis of these roles.

### Diffusion Through Collaboration

The role of collaboration in diffusing rDNA and PCR is reflected, to an extent, in the data on co-publication and co-patenting. Starting from the list of all organizations with publications or patents related to each technology, it is possible to assess whether or not a given organization first appears in the database as a co-publisher/co-patenter with an existing organization. A first appearance alongside an organization that has already demonstrated experience may indicate that the focal organization "learned" through collaboration. Building on Zucker et al.'s (1998) claim of "natural

excludability,” such an effect may be especially important in the first few years after invention.

Figure 3.1 illustrates the collaborative network for the first four years of rDNA. About one half of the organizations are collaborative at all (in this time period around a publication/patent linked to rDNA) and 30% of the organizations first appear via alongside a more experienced organization. (In the case of PCR, the figure is 68%.) This measure of “learning through collaboration” is, of course, very coarse since it only captures those collaborations that resulted in a publication or patent.

Interview data both reinforce the role of collaborations in diffusion and highlight how many collaborations that diffused knowledge of how to perform rDNA or PCR would not be reflected in the co-publication/co-patenting data. For example, Herb Heyneker, who completed his post-doc under rDNA co-inventor Boyer at UCSE, was a central figure in the diffusion of rDNA in Europe. Heyneker recalled a number of collaborations upon his move to the Netherlands in 1977. In the following passage, he describes how he became a “teacher” to his former teacher:

My former boss and promoter in The Netherlands, Pieter Pouwels, is also a very forward-thinking scientist, and he definitely took lessons also, wanted to learn the new technology, and made sure that he came to my lab in Leiden on a day-to-day basis for up to a month to really understand the ins and outs of the technology. Then he took it back to TNO [a prestigious research organization], and I loved that. It was great that he took the time and effort to learn it from me, and I was very glad that I could give something back, in a way. (Heyneker, 2004)

Heyneker’s account illustrates the “chain” that characterized diffusion of each technique: Boyer taught Heyneker, who taught Pouwels, who taught others at TNO. In another segment of the interview, Heyneker describes a similar process (again referring to the year 1977, four years after the technique’s invention):

I received an invitation from Charles Weissmann to visit his lab in Zurich with the goal to help to teach them some of the recombinant DNA technologies. Of course I was very flattered, and I accepted the invitation, and for almost two weeks I was in his lab. We started a project, and I have forgotten even what it was, but mainly it was to teach them techniques: the way we did reactions, the way we analyzed results . . . The work went well. I think the transfer of technology was successful. (Heyneker, 2004)

In both of Heyneker’s accounts, the “transfer” of know-how required weeks of hands-on instruction. In turn, collaboration around a project proved to

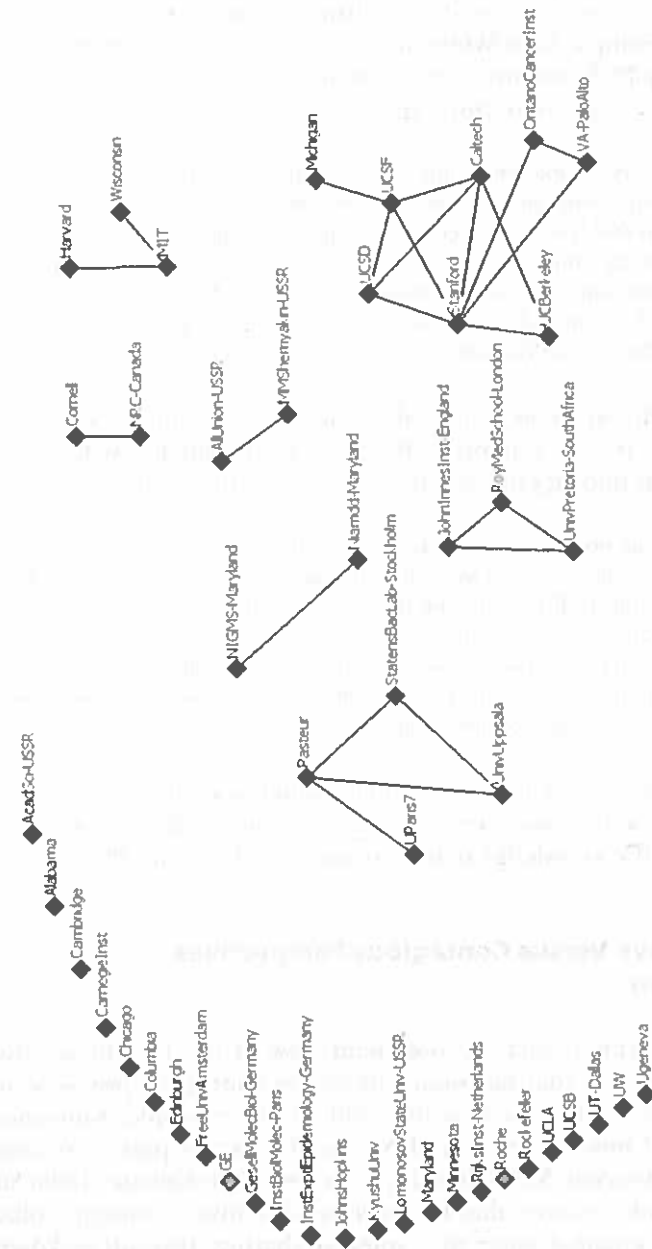


Figure 3.1 Organizations with publications that reference the core rDNA publication and that appeared within three years of this core publication. Two lines between nodes indicate co-authorship on at least one publication. Firms are shown in lighter gray.

be an effective means to structure this training, even if the details of the project itself were secondary.

Similar processes facilitated the diffusion of the firm-invented PCR technique. For example, Tom White, the former Senior Director of Research at Cetus, where PCR was invented, recalled a stream of visitors to Cetus who wanted to learn how to perform the technique:

One of the first people who came . . . to do some of the PCR was Russ Higuchi who had done some characterization of an extinct horse called the Quagga in Wilson's lab and had developed some of the sequencing techniques for PCR products. So he came, and then he was the first in a long line of other people. Svante Pääbo, who's one of the major figures now in the field of ancient DNA, and Ulf Gyllenstein, and those now moving into the lab, like Barbara Bowman and Beth Titus. (Smithsonian, 1993)

White himself left Cetus shortly after the development of PCR and spent time at University of California–Berkeley. He recalled how he helped to diffuse PCR at Berkeley in 1988, three years after the technique's invention:

I resigned my position, and later, by the end of the year, decided to take a sabbatical and go back and work in a laboratory with a collaborator at Berkeley on applying PCR to study the molecular evolution of fungi. That was an extremely satisfying year for me, 1988, in which I was able to not only establish conditions and primers for use with a wide range of organisms, but to transfer the technology to the Botany Department at Berkeley and also the Museum of Vertebrate Zoology. (Smithsonian, 1993)

Both Heyneker and White, among many other interviewees, thus describe how collaborations between researchers from different organizations served to diffuse knowledge of how to perform rDNA and PCR.

### Collaborative Versus Contagious Perspectives on Diffusion

These excerpts reflect the dominant view in the technology transfer literature, which is that diffusion spreads via contagion: just as someone infected with a cold may pass this cold to other people, someone with knowledge of how to perform rDNA or PCR might pass it to someone else (Angst, Agarwal, Sambamurthy, & Kelley, 2010; Valente, 1995; Young, 2009). I found, however, that the process of diffusion through collaboration typically entailed more than one-way sharing. Instead, collaborators tended to contribute different knowledge, capabilities, and/or materials to the relationship. In turn, diffusion was enhanced not because one group

shared something with another group but because the *combination* created new value for both parties. For example, Herb Boyer, the rDNA co-inventor, described how his lab at UCSF collaborated with Don Helsinki's lab at UCSD in order to develop a better plasmid for rDNA experiments. (This collaboration resulted in a 1974 publication, reflected in the link between UCSF and UCSD in Figure 3.1.)

Each scientist brought his own specialty and interest into play. The pSC101 plasmid that we used in the original experiments was a good plasmid, but it wasn't perfect. Each cell would carry maybe a couple of those plasmid molecules for bacterial chromosomes, so the ratio of plasmid to bacterial DNA was fairly small. Purification was that much more difficult because of that . . . Now in contrast to that plasmid, Don Helsinki and his colleagues at UC San Diego and some other people had been working with another bacterial plasmid . . . Now, as it turned out, that plasmid was smaller than pSC101, so it was even better to work with, because the ratio of a plasmid to any cloned DNA would be much more valuable. (Boyer, 2001)

Boyer goes on to describe how the two labs collaborated, allowing Helsinki's lab to learn from Boyer's experience with rDNA but also developing a better plasmid for all subsequent researchers who would work in the field.

Similarly, Irving Johnson, Vice President of Research at Eli Lilly in the 1970s and 1980s, recalled how Lilly needed to partner with Genentech in order to develop synthetic human insulin through rDNA technology:

I think I could say very easily that the idea of striking up relations with small companies and universities was one that I started at Lilly. I felt I had to because I understood and knew what we wanted to do in terms of producing human insulin. I could not hire enough people of appropriate backgrounds, and in some cases even finding the people with the right backgrounds to bring in, to make that kind of an effort entirely internally. I convinced the company that we would have to work with other people. (Johnson, 2006)

Johnson's account reinforces Lilly's need to collaborate in order to apply the DNA technique. The critical point, however, is that Genentech, too, learned from this collaboration: With Lilly's support, collaborating scientists at Genentech and City of Hope Medical Center in Los Angeles (Genentech's collaborator) won the "race" to clone and express the gene for human insulin, an accomplishment that made worldwide media and dramatically bolstered Genentech's reputation (Johnson, 2006).

In fact, Genentech often initiated such collaborations in order to further develop the rDNA technology with which it had expertise. For example, Daniel Yansura, a Genentech scientist, described how Genentech had an early interest in developing vaccines. Another Genentech scientist, Dennis Kleid, had talked with a United States Department of Agriculture (USDA)



group that researched foot-and-mouth disease and thought that foot-and-mouth could be a promising focus for vaccine research:

These researchers [at the USDA] were very excited about working with us to make a vaccine. They had no technology at all to do this. They were strictly using the old biological process; they could grow virus and analyze some of the proteins and so forth, but they had no ability to do recombinant DNA work. So it was a perfect match. They were very interested in it, and for us it was an opportunity to work with a group that knew what they were talking about. We really didn't know much about foot-and-mouth. And this was true of all of the products that we worked on. We always collaborated with a group that was a real expert in that area. (Yansura, 2002)

Although the collaboration did train the USDA researchers in rDNA, thereby diffusing it, the collective project also raised awareness as to new application areas for rDNA and allowed Genentech to further develop its capabilities, both of which also enhanced diffusion.

In PCR, too, inter-organizational collaborations proved essential to further developments that facilitated diffusion of the technique. Most notably, performing the PCR technique manually was incredibly laborious and time consuming, especially using the enzyme that inventor Kary Mullis had originally used to demonstrate the technique. Thus, many observers tie the technique's diffusion to the development of automated equipment. Cetus, however, was not in the business of manufacturing equipment and it established a collaboration with instrumentation firm Perkin-Elmer for the purpose of helping to further diffuse the technique. As Lawrence Haff, a Perkin-Elmer scientist described:

There was a real panic time, because word had leaked out about PCR. People were doing it manually, which was just ridiculous—a degrading human experience we always call it, doing it manually—and people desperately wanted thermocyclers [automated machines to perform the technique] . . . the basic problem was we found out that the prototypes didn't last, so we couldn't possibly sell them, because they would have failed pretty early. We couldn't put thousands out in the field. But there was so much knowledge about PCR and so many people wanted to do it, they were beating down our doors. (Smithsonian, 1993)

Again, Cetus' collaboration with Perkin-Elmer not only resulted in a “transfer” of PCR knowledge from one organization to another, but also facilitated new developments that, in turn, allowed an even broader group of organizations to employ the technique. To employ an epidemiological metaphor, the key process in inter-organizational diffusion was not simple *contagion*, as when an increasing number of participants get infected with

the same thing, but rather *mutation* through collaborative research (see also Bailey and Barley, 2011).

From this collaborative perspective on diffusion, the typical foci of universities and firms on different problems and projects are a variation on the theme: One organization does not “transfer” a technique to another organization so much as two organizations mutually pursue the technique's enhancement or development by bringing unique capabilities. For example, Steven Rosenberg, an early Chiron scientist, argued that universities and firms fill different roles in the diffusion and development of science:

People in academia oftentimes discover proteins, but they don't know how to make them into products. We know how to make them into products. And in order to investigate the biology further, you need to have large amounts of these proteins to be able to make them in a functional form, to be able to purify and characterize them. So then what you find is that someone in academia will make a discovery, we'll collaborate with them to make large amounts of the proteins in functional form, and then they can use those proteins collaboratively with us to try and understand more about the biology, to see if there a possible therapeutic use of the protein? So that works quite well, and we have lots of collaborations. (Rosenberg, 2005)

Rosenberg posits, therefore, that the different foci of universities and firms enable them to come together to more effectively address the questions of predominant interest to each of them (“understand more about the biology” for universities and explore a “possible therapeutic use” for firms).

In contrast to the literature on university–firm collaboration, which has focused primarily on how firms learn *from* universities, these reflections also highlight how firm-based knowledge diffuses *into* universities. Such diffusion is especially evident in the case of PCR, which was invented in a firm. The data offer numerous other examples, however, of firms facilitating diffusion into universities. For instance, in order for a university scientist (or anyone else) to perform rDNA, he or she needed certain restriction enzymes. It was Miles Laboratory and other commercial firms that took up the task of producing and distributing these enzymes to university-based researchers to enable their use of the rDNA techniques. Co-inventor Herb Boyer recalls Miles Laboratory's role in the following interview excerpt:

**Boyer:** Part of the problem in those days was that a lot of the reagents were not available commercially, like restriction enzymes, and that's what we had. We had a number of key restriction enzymes and ligases, and we had been purifying plasmids and developing plasmid vectors. So those are key things. And then once the technology got going, there was a need to provide these things commercially. Nobody purifies

their restriction enzyme today; they buy them. They buy the cloning kits, and the vectors, and all of that. It's a big business now.

**Hughes:** Was there more than one company that came in initially with those products?

**Boyer:** Yes. I can't remember which one was first. I actually went back to Miles Laboratory, and I think they were the first ones to supply the R1 enzyme. I went back and showed them how to make the R1 enzyme, because I was tired of giving it to everybody. [laughter] I wanted to get somebody else to do it for me.

Although rDNA stemmed from two universities, firms such as Miles Laboratory played important roles in facilitating its diffusion to other universities.

### Perspectives on the Costs of Collaboration

Chiron scientist Stephen Rosenberg's characterization of universities and firms, quoted earlier, also raises questions about the ways in which scientists in each environment conceive of collaboration. I found that the reward structures tied to public and private science led to particular perspectives on collaboration that affected collaboration patterns and practices.

#### *Public Science and the Costs of Collaboration*

Numerous interviewees described how the public-science emphasis upon individual credit served as a disincentive to collaboration. Because collaboration involves sharing credit with others, researchers who were motivated by the desire to build an individual reputation perceived collaborations as costly. Herb Heyneker, the molecular geneticist who moved from UCSF to The Netherlands (and later to Genentech), captured the sentiment of many researchers in arguing that the emphasis on recognition in academe is harmful towards collaboration:

Recognition is the only outlet in academe for your work. In industry, if you make a nice invention you get a whole bunch of shares, to say, "Look, I recognize your contribution, and you know what? I'm going to reward you for it." In academe you don't have that outlet. It really is how to inflate your name, how to become a very well-recognized person. And that can have some very negative effects. It is a disincentive to collaborate. (Heyneker, 2004)

Heyneker contrasts financial rewards, which can be spread widely, against academic credit for a discovery, which functions more like a zero-sum game. The Bancroft Library interviewer, Sally Smith Hughes, pushed Heyneker further on this point:

**Hughes:** Would you go so far as saying that because in industry there's the potential for rewards aside from peer recognition that a more collaborative scientific approach is more likely?

**Heyneker:** Yes. For instance, at Genentech there was a paper [in 1982] with something like twelve or fifteen authors on it... we got a lot of comments: "Why so many names on it? Not everybody did it." I said, "Wait a minute. There's a reason we put all these names on it. Everybody contributed and collaborated. Therefore, we were able to be first, and not because somebody is doing it all by himself in a lab and trying to get the recognition." (Heyneker, 2004)

Heyneker thus argues that collaboration is associated with faster progress on a project and that financial rewards are more effective than credit in motivating such collaboration.

Of course, the counterfactual to Heyneker's argument is difficult to test: There are a number of other influences on collaboration—ranging from grant requirements to specific divisions of labor—that still result in significant collaborative activity on the part of universities. Heyneker and other interviewees are unequivocal, however, in the suggestion that this "baseline" might be enhanced even further under a different system of incentives. For example, in discussing collaborations around PCR, Cetus scientist David Gelfand reflected on the challenges posed by environments that are keyed to individual credit:

It's very difficult, as an academic scientist, to do interactionist, collaborative science. The acculturation process is one that is keyed to individual, personal achievement. You first learn that as a graduate student... God forbid you should make the mistake as assistant professor of collaborating with either your former postdoc mentor—a very famous individual, of course, because you wouldn't have studied there had they not been—or graduate student advisor. All of your independent work done as an assistant professor will be ascribed to the brilliance of former major mentors, making it ever so much more difficult to get tenure. Thus one finds himself at age forty being promoted to associate professor with tenure with twenty years of experience of how *not* to collaborate. (Rabinow, 1996, p. 44–45)

Henry Erlich, another scientist from the Cetus PCR group, made a similar point, contrasting his experiences at Stanford and Cetus in 1979:

[At Stanford] there was also a lot of competition between labs, and what struck me as really interesting and unique about the scientific culture at Cetus was that there was a very open interaction, people felt as if they were part of the same team working toward a goal, rather than competing laboratories. So

the nature of the collaboration was somewhat different from collaborations that exist in academic labs, and I think it was a very stimulating environment, and one that really encouraged interaction between different laboratories. (Smithsonian, 1993)

To these scientists, the public science model in which researchers compete on the basis of priority and credit could be harmful for establishing and sustaining collaborations—whether between labs within an organization or outside the organization.

In turn, organizational structures could reflect these associations. For example, Stanford professor Arthur Kornberg argued that universities' emphasis on individual credit went hand-in-hand with individual research autonomy. In turn, Kornberg argued, university laboratories function independently, not *interdependently*:

In a conventional academic department, there are a dozen professors and each one is an entity unto himself; a duchy I would call it. Professors are self-sustaining in getting their money and in acquiring results and publishing them. One can be successful and surrounded by failures. (Kornberg, 1998)

Bill Rutter, who was chair of the Department of Biochemistry and Biophysics at UCSF when Boyer helped develop rDNA, described his struggles to create a collaborative environment upon assuming that role in 1969:

The Department of Biochemistry was filled with people who were either difficult individuals, or they were outliers, that is, with insular laboratories devoted solely to the interests of the professor. They weren't building a cohesive interactive program... People collaborated occasionally, but it was not the usual pattern. Most scientists wanted to set up an independent lab to focus on some subject and became totally competent to deal with that subject. (Rutter, 1998)

These interviewees thus argue that the public-science emphasis on credit is reflected in independent academic labs that have less incentive to collaborate.

The emphasis on labs, however, highlights an important nuance in how public-science-minded researchers approach collaboration. Interviewees described how even though the emphasis on credit and priority acted as a discouragement to collaborations, specific lab cultures could be very collaborative *within* the lab. For example, Dave Goeddel, a Genentech scientist who went on to be CEO of Tularik, recalled his collaborations in graduate school at the University of Colorado in Boulder: "Daniel Yansura [a Masters student in the lab] and I were collaborating on the lac program. It was a big enough thing. Probably 90 percent of what he and I did in graduate school we did collaboratively" (Goeddel, 2003). In fact, Goeddel later hired

Yansura at Genentech. Many other researchers also described the academic lab environment itself as highly collaborative.

Norm Arnheim, a professor of molecular biology at the University of Southern California, argued that such within-lab collaboration, however, has a limited effect:

Most, or many, academic laboratories usually have the major professor interact with one graduate student, each graduate student has his or her own thesis, and there's an interaction between a graduate student and a professor and that's about it. I think that traditionally in science, at least traditionally in the biological sciences, that's usually been the way things are carried out. (Smithsonian, 1993)

Arnheim thus argues that while lab cultures enabled collaboration between a professor and a graduate student, they did little to encourage collaboration beyond that dyad—and certainly not beyond the lab.

Boyer, the co-inventor of rDNA, reinforces this point when recalling his own days as a graduate student: "As a graduate student and postdoc—in those days, anyway—you didn't make a lot of overtures to people who were unknown to you, with whom you didn't work. At least I was somewhat reticent about approaching people for an idea" (Boyer, 2001). Thus, while lab cultures could encourage limited in-group collaborations, they had little effect on encouraging collaborations outside the lab or organization due to the dominant emphasis on individual credit.

#### *Private Science and the Costs of Collaboration*

A number of firm-based observers contrasted the dominant organizational structure in universities against that found in firms. For example, Genentech's Tom Kiley recalled:

There came a time when I heard scientists in universities complain that they could be out-competed by their colleagues that had gone into companies because the companies were in some cases better-equipped, because it was easier to create cross-disciplinary collaborations within a company environment than between academic departments. (Kiley, 2002)

In contrasting his UCSF and Genentech experiences, rDNA co-inventor Boyer also claimed that firms enjoyed superior collaborative environments:

One of the keys to research is the collaboration between different groups. I think research was much more efficient at the company than at a university, much more efficient. One of the things I find particularly gratifying is the number of scientific contributions that have come out of the company since its inception. (Boyer, 1994)

Boyer, thus links collaboration to an increasing number of scientific contributions and he claims that the environment at a firm fostered more collaboration than at a university.

Tom White, a former senior scientist at Cetus, remembered that academics had to be "trained" to be more collaborative when they entered the firm environment. He makes the point via a story of Cetus scientist David Gelfand setting up a new department within the firm:

All of the people he [David Gelfand] hired [for the Cetus Recombinant and Molecular Research Department] basically came from the university. So they came with the concept and training of having worked as individuals in individual labs... So it required a much greater emphasis on collaboration as part of a project team, and the willingness, I think, to accept that to some extent, your work would be dependent on the outcome from colleagues over whom you had no control. That is somewhat different from an academic collaboration where generally an investigator builds their laboratory with relevant expertise, but some sense if you need some cell biologists, you hire postdoctoral cell biologists. If you need some biochemistry, you bring those people into your group. But these sort of larger, multilab, multidisciplinary teams was something that really was required in the biotechnology industry, and so that created the need to work together effectively. (Smithsonian, 1993)

White argued that the lack of public-science inhibitions and lab autonomy in firms facilitated multidisciplinary collaborations. In the emerging field of biotechnology, such collaborations were especially important since the field is, by nature, multidisciplinary. Thus, White also reinforces the view that firms, unlike universities, are better conceived as cohesive organizations and not as collections of autonomous labs.

For organizations focused on the monetary rewards of private science, however, collaboration held a different sort of risk: that they might lose to competitors in the race for a marketable product. The sharing inherent in collaboration opened up the possibility that competitors could be alerted to an organization's activities or, worse, that collaborators might share sensitive knowledge with competitors. Two different accounts of Genentech's policies around collaboration and sharing help to illustrate this point. On one hand, Bob Swanson, the co-founder of Genentech, commented that the potential gains from collaboration outweighed the costs:

[Co-Founder Herb] Boyer's philosophy, which I agreed with, was that you gain more from interaction with your academic peers than you give up by telling the competition where you are. So with interaction you can move quicker; you gain more people willing to collaborate with you. (Swanson, 2001)

But, when questioned about the extent of Genentech's interactions with laboratories at UCSF, former Genentech researcher David Goeddel recalled:

**Goeddel:** I don't think there was a huge amount [of interaction with other groups]... I think in those early days there weren't many. [Co-founder] Bob [Swanson] probably would not have liked it then, although scientists went to meetings and talked about their work. I'm trying to think when the first collaborations came up, but I can't remember that.

**Hughes:** Why wouldn't he have liked it?

**Goeddel:** Bob was always a little more worried than Herb [Boyer] about publications and other people knowing what we were doing.

Comparing Goeddel's and Swanson's understandings and claims, it is clear that collaborations were at least somewhat problematic because they would alert other organizations as to what Genentech was doing.

In the face of these challenges, firms that were engaged in the diffusion of rDNA and PCR employed two key strategies. First, firms maintained collaborations with fewer organizations overall, but worked deeply with these trusted groups. The quantitative data, as noted, provide only a limited view of collaboration partners, due to the process of pulling citations to core publications/patents only and due to inherent limitations in publication- and patent-based measures. Nevertheless, they show an interesting pattern: In every case (except rDNA patents, where there are not enough university patents to be meaningful), universities have significantly more partners around rDNA and PCR than do firms (see Table 3.1). In fact, when we "rank" organizations according to the number of publication partners, there is not a firm to be found among the top 50 in rDNA, even though 16% of all rDNA publishing organizations are firms. In PCR, Cetus is in the top ten and Roche is in the top 25, but beyond that point there is not another firm in the top 100—even though 15% of all PCR publishing organizations are firms! These results, while tentative, offer suggestive evidence that firms do not maintain as many partners tied to a single technology as do universities—regardless of the university or firm origin of the technology.

**TABLE 3.1 Average Number of Partners for Organizations Who Collaborate Around Publications/Patents Tied to the Original Publications/Patents for rDNA/PCR**

	rDNA		PCR	
	Patenting	Publishing	Patenting	Publishing
Universities	1.00 (0.00)	2.55 (2.57)	2.19 (1.85)	5.85 (8.91)
Firms	1.17 (0.41)	1.63 (1.06)	1.37 (0.78)	3.19 (5.45)

*Note:* All differences are significant at the 0.05 Level or higher, except those for rDNA patenting (s.d. in parentheses).

A second firm strategy to address the collaboration danger of alerting competitors was to focus on universities as collaboration partners. Although other studies have focused upon collaborations that enable university-to-firm knowledge flows (e.g., Cockburn & Henderson, 1998; Siegel et al., 2003; Thursby & Thursby, 2004), I found that this strategy also characterized PCR collaborations by Cetus and other firms, whereby knowledge flowed *from* firms *to* universities (recall, for example, the string of academics who visited the Cetus facility following the development of PCR). Firm-based interviewees attributed the preference for university collaborations to competition: Universities were viewed as less of a threat since they do not release products. Conversely, firm-firm collaborations were relatively rare due to concerns about proprietary interests, as indicated by Chiron scientist Steven Rosenberg's story about Chiron moving into a building that connected with Cetus:

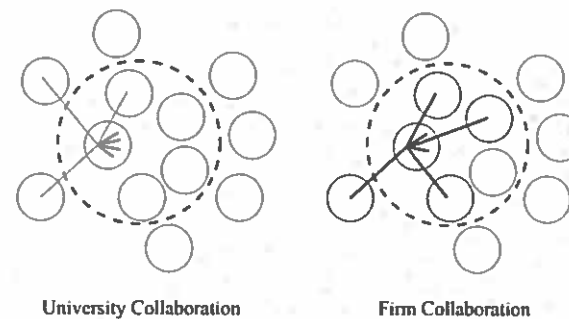
The Cetus people were nice enough to let us push all of our equipment through the walkway to get over to the other building. So that's what we spent the day doing, and it was pouring [rain], the thing [walkway] was leaking, and we were pushing centrifuges and all this stuff over to our new labs. That walkway was walled off for the next ten years, until the Chiron/Cetus merger... because it was a violation of some sort of proprietary interest [to open it up]. There was no communication with us. (Rosenberg, 2005)

Despite (or rather, because of) physical collocation and similar research interests, the two firms did not interact due to proprietary interests.

These tendencies are reflected in the quantitative data on rDNA and PCR publications and patents. Although firms comprise 16% of rDNA publishing organizations, only 8% of firms' collaborative publications are with other firms; although firms make up 66% of rDNA patenting organizations, only 25% of firms' collaborative patents are with other firms. The same patterns hold for PCR: Although firms comprise 15% of PCR publishing organizations, only 5% of firms' collaborative publications are with other firms; although firms make up 66% of PCR patenting organizations, only 33% of firms' collaborative patents are with other firms. The co-publication and co-patenting data, therefore, provide further evidence that firm collaborations are more likely to involve universities.

### Conceptualizing Collaborative Networks

Building on these insights, Figure 3.2 presents a stylized image of collaboration networks in universities versus firms from the perspective of a single researcher in each environment. The thin circles indicate lab boundaries and the thick dashed circles indicate organizational boundaries.

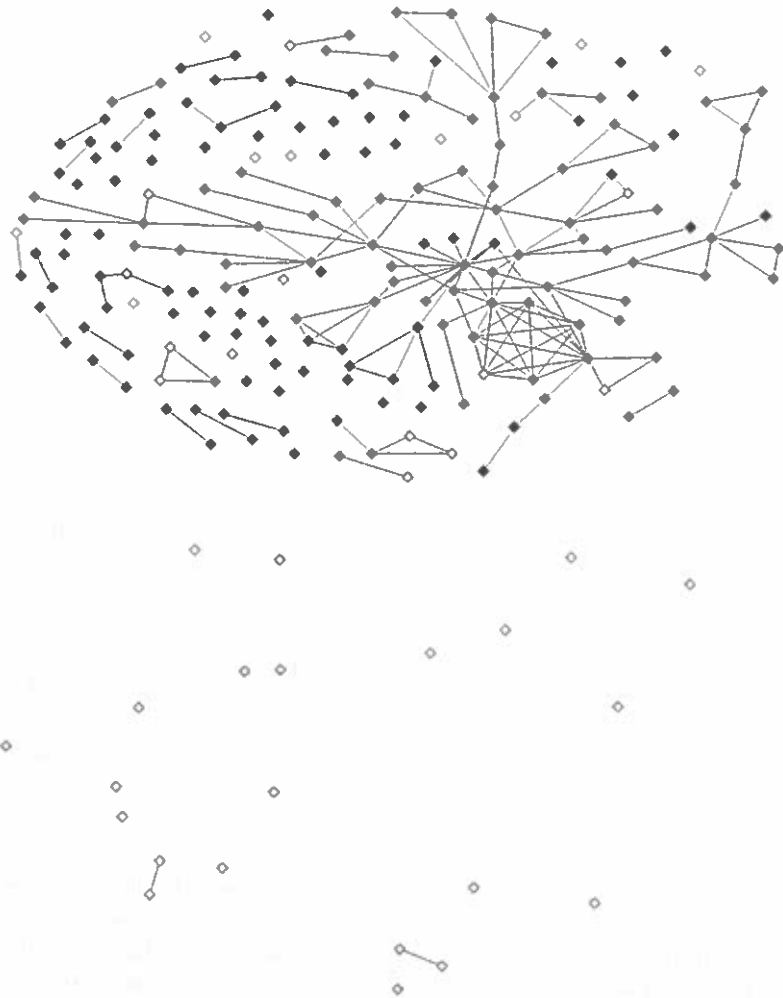


**Figure 3.2** Stylized image of ego-centric collaboration networks for university (left) versus firm (right) scientist. *Note:* Thickness of ties indicates frequency of collaboration. Thick dashed lines indicate organization boundaries and small circles represent lab groups.

Collaborative ties are indicated by straight lines, which connect individual researchers. The thickness of the lines indicates the strength or depth of collaboration. In the university case, an autonomous lab director collaborates directly with students, some of whom collaborate with each other, too. But, collaboration outside of the lab is less common. Due to the autonomous nature of university labs, there is little difference between collaborations with other labs in the same university versus collaborations outside the university. Moreover, the outside organizations that are reached are relatively diverse, composed of universities and firms.

In the firm case, the focal researcher has strong collaborative ties to other lab groups within the firm, often representing different disciplines. Outside ties are more selective, but deeper, than in the university case. Moreover, the outside organizations that are reached are more likely to be universities (or other public research organizations) rather than firms. The stylized diagrams thus give a sense of how public-science and private-science incentives may be reflected in organizations' network structures.

In turn, these differences can aggregate through the network as a whole: Firms often drive collaborations with universities and universities are firms' preferred partners (whereas universities themselves are agnostic as to partner type). As a result, universities play both central and bridging roles in the overall collaborative network, while firms are more periphery players (despite motivating many of the collaborations that take place). The network data based upon citations to core publications support this image. (Again, the data are only suggestive. The relatively small number of organizations overall reinforce the fact that these data do not capture all activity around a technique). The top half of Figure 3.3 illustrates the full co-publishing network around rDNA, while the bottom half of Figure 3.3 illustrates the same



**Figure 3.3** Top image is of all organizations with publications that reference the core rDNA publication and that appeared within twenty years of this core publication. *Note:* Two lines between nodes indicate co-authorship on at least one publication. Firms are shown in lighter gray. Bottom image shows firms only.

network with universities and other public research organizations removed. Although the top image shows a sizable main component and appreciable collaborative activity, the bottom image (with firms only) is almost completely disconnected. Thus, different perspectives on public and private science appear to underlie network cohesiveness on a macro level.

### Embracing Institutional Complexity

The general collaboration tendencies of universities and firms are important to appreciate, as are the influence of public and private science upon these tendencies. At the same time, however, the data show how researchers within both university and firm environments embrace both public science and private science. As a result, a dogmatic association of universities with public science and of firms with private science appears overly simplistic. For example, Tom Kiley, the general counsel for Genentech in 1978, recalled a case in which two recent hires from UCSF, Peter Seeburg and Axel Ullrich, had:

...carried away from the university some biological material, tissues and clones they had used in work that was ongoing. When the laboratories [at UCSF] in which they had worked learned of their intention to join Genentech, a perceived competitor in both the insulin and growth hormone areas, the walls had come down... The university was now calling for the return of those materials... [Seeburg and Ullrich] said patent applications had been filed by the university on their work, in which they had not been credited as inventors... [Ultimately] we settled. We agreed to pay the University of California \$350,000 and change, and modest royalty on growth hormone sales until the royalty payments exceeded five million dollars, with adjustment for inflation.

Genentech and UCSF had been locked in a bitter race to clone and express human insulin, with both groups drawing support from Lilly (who had dominated the non-rDNA insulin market). Their race is interesting for two reasons: First, a university (UCSF) and a firm (Genentech) were competing around the same scientific achievement. (More recently, the firm Celera Genomics raced against a consortium of universities and research groups to sequence the human genome [Philipkoski, 2000].) Second, the competition was framed around both credit in being first (motivated, therefore, by “public science”) and financial rewards (evident in UCSF’s patent applications and the \$350,000 settlement).

As another example, Dave Goeddel, a Genentech researcher, described how *public-science* interests could even interfere with collaborations between firms that were ostensibly focused on *private science*:

**Goeddel:** We had a collaboration with Roche through a laboratory of theirs at the Roche Institute in Nutley, New Jersey... Sid Pestka’s. He had worked on interferon for years, so we were collaborating with him.

**Hughes:** How did that work?

**Goeddel:** Very poorly. It's probably the worst collaboration that I've ever been involved in . . . [Pestka] always did it in a way where he could recognize something as being important, set up the collaboration, and kind of get credit for it himself . . . So we had some difficulties, collaborating with him, not being sure we were getting the right materials.

Goeddel clarifies that the relationship was difficult because another firm-based collaborator was concerned with getting credit in order to bolster his scientific reputation. These examples thus highlight how distinctions between firms and universities and between public and private science were blurred in competitive-collaborative interactions. Collaboration, as a result, must be viewed not merely as a "bumping together" of different perspectives but rather as the intermingling of individuals and organizations that each struggle to balance competing incentives and pressures.

## DISCUSSION

Inter-organizational collaborations between universities and firms are a critical topic for scholars interested in scientific knowledge flows and innovation processes. In this study, I have offered a more micro perspective on these collaborations by tracing the role of collaborations in diffusing know-how around two core techniques in biotechnology. I have demonstrated that diffusion is best conceived as a collaborative, not contagious, endeavor in which multiple organizations—both universities and firms—act to further develop techniques through the "diffusion" process. I have also elaborated upon the ways in which attention to public and private science shape individual and organizational incentives and disincentives around collaboration, ultimately affecting both organizational structures and overall network dynamics. Finally, I have illustrated how individual researchers may respond to both public science and private science simultaneously. Far from being "disinterested," as some scholars read Merton's depiction of science, these researchers hold and attempt to manage multiple, diverse, and sometimes conflicting interests.<sup>1</sup> In the following subsections, I elaborate on each of these points.

### Collaborative Versus Contagious Perspectives on Diffusion

Much of the university-firm collaboration literature employs an "exchange" frame in which "knowledge" is portrayed as a bounded "object"

that is somehow passed between organizations. From this perspective, firms "access" university knowledge through different arrangements (most typically, licensing) and diffusion is a matter of one partner sharing knowledge with another partner. Such exchange arrangements often involve shallow relationships and the underlying processes mimic contagion models in which exposure or access is adequate for diffusion.

In the case of rDNA and PCR, collaborative diffusion rarely took this form. Rather than a "passing of knowledge" from one organization to another, interviewees described collaborative diffusion as a process in which two organizations brought unique expertise or resources to a relationship in order to further develop a technique. In this way, learning and diffusion were by-products of co-development and this development held benefits for both parties, not just a "learner." Moreover, it enabled the further diffusion of the techniques by expanding the application areas, the range of conditions in which they were applied, and the tools that might enable easier performance (as with the PCR thermocycler machines and the Miles Laboratory restriction enzymes for rDNA). This perspective suggests that even when firms are the primary beneficiaries of a collaborative relationship with a university, the notion of university-firm "transfer" is not an accurate reflection of diffusion processes.

These results also highlight a more active role for firms by showing how firms facilitate diffusion *to*, not just *from*, universities. Because this perspective is almost entirely absent in the literature on university-firm relationships, it suggests a number of future research questions. For example, how can universities best position themselves to learn from firms? What factors shape firm decisions to engage with universities? Do university policies on diffusion, such as patenting decisions and licensing terms, shape firm perspectives on collaborative relationships? How can we best understand and measure the influence of firm-based R&D on subsequent university-sponsored research?

Critically, these questions and others may not be independent of the dominant "university-to-firm" view. Rather, the ways in which universities engage with firms may both shape and be shaped by the ways in which firms engage with universities. Thus, a truly collaborative perspective demands a two-sided investigation of diffusion processes that moves beyond "connections" to explore the content, direction, and evolution of these ties (Borgatti & Foster, 2003; Owen-Smith & Powell, 2008).

### Public Science, Private Science, and the Structure of Collaborative Diffusion

The diffusion studies of rDNA and PCR offer multiple examples of both public-science sharing and private-science commercial foci. In the

private-science case, usually associated with firms, internal organizations are structured and incentivized to be highly collaborative. The risks of collaboration, however, suggest more selective external partnerships with organizations that would seem to pose less competitive risk—typically, universities.

At the same time, attention to individual credit in university environments, especially, encourages autonomous and independent labs and, surprisingly, serves as a disincentive to collaboration. In other words, “public science” is not necessarily “collaborative science.” This point is crucial in that it unpacks the effect of a single set of incentives (credit and reputation) upon different aspects of scientific conduct (collaboration in *creation* versus openness in *diffusion*). Moreover, incentives that motivate one behavior may simultaneously discourage the other behavior.

In turn, this finding has implications for the functioning of collaboration networks as a whole. Notably, private science offers unique motivations for collaboration, but it also suggests a preference for collaboration partners who do *not* share these private-science motivations. As a result, the collaboration network is held together, in a sense, by the very contradictions between public and private science that the network integrates—with private science motivating “ties” and public science shaping the preferred “nodes” that these ties connect. Thus, conflicts between public and private science—at least on a field level—are not a problem to be minimized or resolved, but rather a feature to be celebrated as an enabler of collaboration.

At the same time, this finding raises concerns about growing interest on the part of universities in extracting financial returns from research. For example, data from the Association of University Technology Managers show that the number of patents filed by universities, the number of licenses, and revenue realized on the basis of these licenses, have all increased dramatically in recent years. (Recall that rDNA and PCR stem from the 1970s and 1980s). Moreover, almost every university now features a formal administrative unit charged with “technology transfer and licensing” (Association of University Technology Managers, 2014). If universities are preferred collaboration partners and serve as critical “bridging” nodes in networks precisely because of their presumed lack of (or lessened) association with private science, then overt moves by universities to place increased emphasis on private science may undermine the very fabric of collaborative networks. In other words, by becoming more “firm-like” in their use of research results, universities may unwittingly become more firm-like by giving up their valuable roles in collaboration networks. In fact, recent lawsuits suggest that such a shift is already underway. For example, in the case of *Madey v. Duke*, the U.S. Federal Appeals Court found that Duke University’s significant patenting and licensing activities are evidence of a commercial orientation such that Duke is not deserving of a special research exemption for non-profit organizations. Although the rDNA and PCR cases highlight a blend

of public science and private science within each organization type, this blend is not necessarily equal nor, as these recent developments highlight, is the balance necessarily stable.

### Employee Mobility and Individual Relationships as a Locus of Collaboration

On a more micro level, the data on rDNA and PCR encourage deeper investigation of collaboration as an interpersonal, not just inter-organizational, phenomenon. For example, the highly permeable boundaries between universities and firms must be understood partly as a result of interpersonal ties. Almost every researcher in a firm environment had graduate-level training in a university environment. Through the course of this training, these researchers developed relationships with other scientists who joined both universities and firms. Their personal networks, therefore, crossed these organization-type boundaries frequently. Recall, for example, that Herb Boyer, the rDNA co-inventor, co-founded the firm Genentech but also retained his position at UCSF. Tom White, who left Cetus to spend time at UC Berkeley, later left UC Berkeley to join another firm. These individual shifts between university and firm environments helped to reinforce and refresh personal networks, while also minimizing the barriers between organization types.

These findings, therefore, suggest fruitful opportunities at the intersection of the literatures on employee mobility (e.g., Agrawal, Cockburn, & McHale, 2006; Madsen, Mosakowski, & Zaheer, 2003; Møen, 2005) and on inter-organizational collaboration. Although most of the employee mobility literature focuses on macro patterns—as with the collaboration literature—Dokko and Rosenkopf (2010) address more micro mechanisms by showing how mobile individuals carry social capital with them. In turn, this mobile social capital alters patterns of interaction between organizations. My results complement these findings by suggesting that one effect of social capital is to lubricate potential frictions between public science and private science that could accompany inter-organizational collaborations.

Of course, shifting the focus of collaboration studies from “organizational clusters” to “individual relationships” also holds methodological implications. For example, in their highly-cited paper on inter-organizational networks in biotechnology, Powell et al. (1996, p. 120) relay how an executive responded to their list of his company’s collaborations:

[Formal collaboration contracts are only] the tip of the iceberg... [and exclude] dozens of handshake deals and informal collaborations, as well as



probably hundreds of collaborations by our company's scientists with colleagues elsewhere.

Thus, capturing organization-level data for an individual-level phenomenon is likely to lead to woeful underrepresentation. In turn, an important outstanding challenge is to carefully document individual-level links, such as those between mobility and collaboration. Moreover, it is crucial to understand how different incentives tied to public and private science shape the effect of individual-level characteristics, such as social capital, in different environments.

More generally, these findings encourage us to examine how individuals and not just organizations respond to competing incentives. Peppered throughout the collaboration accounts are deep descriptions of personal relationships between researchers. One possibility is that these personal relationships—which have been demonstrated elsewhere to be important for the development of trust and for sharing generally (Bouty, 2000; Janowicz-Panjaitan & Noorderhaven, 2009; Kreiner & Schultz, 1993; Zucker et al., 1996)—also function to adjudicate tensions between sharing and secrecy tied to both public and private science. If so, then more research is needed to understand how these relationships function over time and in conjunction with different diffusion processes. Given the focus of this volume, it is no accident, of course, that such questions are best addressed via qualitative approaches.

Ultimately, the cases of rDNA and PCR highlight a dilemma for social scientists interested in the ways in which different incentive systems shape collaborative behavior around the diffusion of technologies: On one hand, incentives clearly matter and general patterns associated with organizations appear to have notable effects on the ways in which different organizations approach collaboration and, therefore, on the ways in which collaborations serve to diffuse novel scientific techniques. On the other hand, although we can identify, categorize, and attribute “public science” and “private science,” researchers themselves are embedded in deep webs of multiple incentives and relationships that are extremely difficult to disentangle.

For biochemists who employ rDNA and PCR in their work, a continuing challenge lies in moving from small pieces of the puzzle to an understanding of the overall functioning of a large biological system, like a human being. The same challenge, so it seems, might be said to characterize sociologists in our attempts to move from specific incentives and organizational features to a broader understanding of collaborative phenomena. Ironically, part of the way forward in each field will almost certainly rely on insights developed through deeper collaborations.

## ACKNOWLEDGMENTS

For comments on previous drafts, I thank Beth Bechky, Kim Elsbach, Woody Powell, Steve Barley, Michaël Bikard, and conference participants at the UC Davis Qualitative Research Conference, the Roundtable on Engineering Entrepreneurship Research at Georgia Tech, the Technology Transfer Society annual meeting, the Academy of Management annual meeting, and the Max Planck Institute of Economics. I also thank the Kauffman Foundation for financial support.

## NOTE

1. I am grateful to Beth Bechky for pointing out this interpretation.

## REFERENCES

- Agrawal, A., Cockburn, I., & McHale, J. (2006). “Gone but not forgotten: Knowledge flows, labor mobility, and enduring social relationships. *Journal of Economic Geography*, 6(5), 571–591.
- Agrawal, A., & Henderson, R. (2002). Putting patents in context: Exploring knowledge transfer from MIT. *Management Science*, 48, 44–60.
- Angst, C.M., Agarwal, R., Sambamurthy, V., & Kelley, K. (2010). Social contagion and information technology diffusion: The adoption of electronic medical records in US hospitals. *Management Science*, 56(8), 1219–1241.
- Association of University Technology Managers. (2014). *U.S. Licensing Survey Summary*. Northbrook, IL: AUTM.
- Audretsch, D., Link, A., & Scott, J. (2002). Public/private partnerships: Evaluating SBIR-supported research. *Research Policy*, 31, 145–158.
- Bailey, D., & Barley, S. (2011). Teaching-learning ecologies: Mapping the environment to structure through action. *Organization Science*, 22(1), 262–285.
- Behrens, T. R., & Gray, D. O. (2001). Unintended consequences of cooperative research: impact of industry sponsorship on climate for academic freedom and other graduate student outcome. *Research Policy*, 30(2), 179–199.
- Bekkers, R., & Freitas, I. M. B. (2008). Analysing knowledge transfer channels between universities and industry: To what degree do sectors also matter?. *Research Policy*, 37(10), 1837–1853.
- Bikard, M., Murray, F., & Gans, J. S. (2015). Exploring Trade-offs in the organization of scientific work: Collaboration and scientific reward. *Management Science*, 61(7), 1473–1495.
- Blumenthal, D., Campbell, E., Anderson, M. S., Causino, N., & Louis, K. S. (1997). Withholding research results in academic life science: Evidence from a national survey of faculty. *The Journal of the American Medical Association*, 277, 1224–1228.

- Borgatti, S., & Foster, P. (2003). The network paradigm in organizational research: A review and typology. *Journal of Management*, 29(6), 991-1013.
- Bouty, I. (2000). Interpersonal and interaction influences on informal resource exchanges between R&D researchers across organizational boundaries. *Academy of Management Journal*, 43, 50-65.
- Boyer, H. W. (2001). Recombinant DNA research at UCSF and commercial application at Genentech, an oral history conducted in 1994 by S. S. Hughes, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Campbell, E., Clarridge, B., Gokhale, M., Birenbaum, L., Hilgartner, S., & Blumenthal, D. (2002). Data withholding in academic genetics: Evidence from a national survey. *The Journal of the American Medical Association*, 287, 473-480.
- Cockburn, I. M. & Henderson, R. M. (1998). Absorptive Capacity, coauthoring behavior, and the organization of research in drug discovery. *Journal of Industrial Economics*, 46(2), 157-182.
- Cohen, W. M., Nelson, R., & Walsh, J. P. (2002). Links and impacts: The influence of public research on industrial R&D. *Management Science*, 48, 1-23.
- Dasgupta, P., & David, P. (1987). Information disclosure and the economics of science and technology. In G. R. Feiwel (Ed.), *Arrow and the Ascent of Modern Economic Theory* (pp. 519-542). New York, NY: New York University Press.
- Dasgupta, P., & David, P. (1994). Toward a new economics of science. *Research Policy*, 23(5), 487-522.
- D'Este, P., & Patel, P. (2007). University-industry linkages in the UK: What are the factors underlying the variety of interactions with industry? *Research Policy*, 36(9), 1295-1313.
- Dokko, G., & Rosenkopf, L. (2010). Social capital for hire? Mobility of technical professionals and firm influence in wireless standards committees. *Organization Science*, 21(3), 677-695.
- Faulkner, W. (1994). Conceptualizing knowledge used in innovation: A second look at the science-technology distinction and industrial innovation. *Science, Technology, and Human Values*, 19(4), 425-458.
- Fini, R., Laccetera, N., & Shane, S. (2010). Inside or outside the IP system? Business creation in academia. *Research Policy*, 39(8), 1060-1069.
- Galambos, L., & Sturchio, J. (1998). Pharmaceutical firms and the transition to biotechnology: A study in strategic innovation. *The Business History Review*, 72(2), 250-278.
- Garud, R. (1997). On the distinction between know-how, know-why and know-what. *Advances in Strategic Management*, 14, 81-101.
- Geuna, A. (2001). The changing rationale for European university research funding: Are there negative unintended consequences? *Journal of Economic Issues*, 35(3), 607-632.
- Goeddel, D. V. (2003). Scientist at Genentech, CEO at Tularik, an oral history conducted in 2001 and 2002 by S. S. Hughes for the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, CA.
- Ham, R.M., & Mowery, D. C. (1998). Improving the effectiveness of public-private R&D collaboration: Case studies at a US weapons laboratory. *Research Policy*, 26, 661-675.
- Henderson, R., Jaffe, A., Trajtenberg, M. (1998). Universities as a source of commercial technology: A detailed analysis of university patenting, 1965-1988. *The Review of Economics and Statistics*, 80, 119-127.
- Heyncker, H. L. (2004). Molecular geneticist at UCSF and Genentech, entrepreneur in biotechnology, an oral history conducted in 2002 by S. S. Hughes, PhD, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Hounshell, D., & Smith, J.K., Jr. (1988). *Science and corporate strategy: Du Pont R&D, 1902-1980* Cambridge, England: Cambridge University Press.
- Hughes, S. S. (2001). Making dollars out of DNA: The first major patent in biotechnology and the commercialization of molecular biology, 1974-1980. *Isis*, 92(3), 541-575.
- Jaffe, A. (1989). Real effects of academic research. *American Economic Review*, 79, 957-970.
- Janowicz-Panjaitan, M., & Noorderhaven, N.G. (2009). Trust, calculation, and inter-organizational learning of tacit knowledge: An organizational roles perspective. *Organization Studies*, 30, 1021-1044.
- Johnson, I. S. (2006). Eli Lilly and the rise of biotechnology, an oral history conducted by S. S. Hughes, 2004, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Katz, J. S., & Martin, B. (1997). What is research collaboration? *Research Policy*, 26, 1-18.
- Kiley, T. D. (2002). Genentech legal counsel and vice president, 1976-1988, and entrepreneur, an oral history conducted in 2000 and 2001 by S. S. Hughes, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Kornberg, A. (1998). Biochemistry at Stanford, biotechnology at DNAX, an oral history conducted in 1997 by S. S. Hughes, PhD, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Kreiner, K., & Schultz, M. (1993). Informal collaboration in R&D: The formation of networks across organizations. *Organization Studies*, 14, 189-209.
- Link, A.N., Paton, D., & Siegel, D.S. (2002). An analysis of policy initiatives to promote strategic research partnerships. *Research Policy*, 31, 1459-1466.
- Madsen, T. L., Mosakowski, E., & Zaheer, S. (2003). Knowledge retention and personnel mobility: The non-disruptive effects of inflows of experience. *Organization Science*, 14(2), 173-191.
- Mansfield, E. (1995). Academic research underlying industrial innovations: Sources, characteristics, and financing. *The Review of Economics and Statistics*, 77, 55-65.
- Merton, R. (1942). Science and technology in a democratic order. *Journal of Legal and Political Sociology*, 1, 115-126.
- Merton, R. (1973). *The Sociology of Science: Theoretical and empirical investigations*. Chicago, IL: The University of Chicago Press.
- Meyer-Krahmer, F., & Schmoch, U. (1998). Science-based technologies: University-industry interactions in four fields. *Research Policy*, 27, 835-851.
- Møen, J. (2005). Is mobility of technical personnel a source of R&D spillovers? *Journal of Labor Economics*, 23(1), 81-114.

- Mowery, D. C., Nelson, R., Sampat, B. N., & Ziedonis, A. A. (2001). The growth of patenting and licensing by U.S. universities: An assessment of the effects of the Bayh-Dole Act of 1980. *Research Policy*, *30*, 99–119.
- Mowery, D.C., Nelson, R., Sampat, B., & Ziedonis, A. (2004). *Ivory tower and industrial innovation: university–industry technology transfer before and after the Bayh-Dole Act*. Stanford, CA: Stanford, University Press.
- Murray, F. (2010). The oncomouse that roared: Hybrid exchange strategies as a source of productive tension at the boundary of overlapping institutions. *American Journal of Sociology*, *116*, 341–388.
- Nelson, A. J. (2009). Measuring knowledge spillovers: What patents, licenses and publications reveal about innovation diffusion. *Research Policy*, *38*, 994–1005.
- Nelson, A. J. (in press). How to share “A really good secret”: Managing sharing/secretary tensions around scientific knowledge disclosure. *Organization Science*.
- Owen-Smith, J., & Walter W. P. (2001). Careers and contradictions: Faculty responses to the transformation of knowledge and its uses in the life sciences. In Steven P. Vallas (Ed.), *Research in the Sociology of Work: Vol. 10. The transformation of work* (pp. 109–140).
- Owen-Smith, J., & Walter W. P. (2008). Networks and institutions. In R. Greenwood, C. Oliver, R. Suddaby & K. Sahlin-Andersson (Eds.), *The Handbook of Organizational Institutionalism* (pp. 596–623). New York, NY: Sage.
- Perkmann, M., & Walsh, K. (2007). University–industry relationships and open innovation: Towards a research agenda. *International Journal of Management Reviews*, *9*(4), 259–280.
- Perkmann, M., & Walsh, K. (2009). The two faces of collaboration: impacts of university–industry relations on public research. *Industrial and Corporate Change*, *18*(6), 1033–1065.
- Philipkoski, K. (2000). Celera wins genome race. *Wired*. Retrieved from <http://archive.wired.com/science/discoveries/news/2000/04/35479>
- Powell, W. W. (1998). Learning from collaboration: Knowledge and networks in the biotech and pharmaceutical industries. *California Management Review*, *40*(3), 228–240.
- Powell, W. W., Koput, K., & Smith-Doerr, L. (1996). Interorganizational collaboration and the locus of innovation: Networks of learning in biotechnology. *Administrative Science Quarterly*, *41*, 116–145.
- Powell, W. W., White, D. R., Koput, K. W., & Owen-Smith, J. (2005). Network dynamics and field evolution: The growth of inter-organizational collaboration in the life sciences. *American Journal of Sociology*, *110*, 1132–1205.
- Rabinow, P. (1996). *Making PCR: A story of biotechnology*. Chicago, IL: The University of Chicago Press.
- Rhoten, D., & Powell, W. W. (2007). The frontiers of intellectual property: Expanded protection versus new models of open science. *Annual Review of Law and Social Science*, *3*, 345–373.
- Rosenberg, N. (1992). Scientific instrumentation and university research. *Research Policy*, *21*, 381–390.
- Rosenberg, N. (1994). *Exploring the black box: Technology, economics, and history*. Cambridge, England: Cambridge University Press.
- Rosenberg, S. (2005). Early scientist at chiron corporation, an oral history conducted in 1992 by S. Hughes, PhD, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Rutter, W. (1998). The department of biochemistry and the molecular approach to biomedicine at the university of California, San Francisco, an oral history conducted in 1992 by S. S. Hughes, PhD, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Sauermann, H., & Stephan, P. (2013). Conflicting logics? A multidimensional view of industrial and academic science. *Organization Science*, *24*(3), 889–909.
- Scharfetter, D., Rammer, C., Fischer, M. M., & Frölich, J. (2002). Knowledge interactions between universities and industry in Austria: Sectoral patterns and determinants. *Research Policy*, *31*, 303–328.
- Schmoch, U. (1999). Interaction of universities and industrial enterprises in Germany and the United States—a comparison. *Industry and Innovation*, *6*(1), 51–68.
- Siegel, D. S., Waldman, D. A., Atwater, L. E., & Link, A. N. (2003). Commercial knowledge transfers from universities to firms: Improving the effectiveness of university–industry collaboration. *The Journal of High Technology Management Research*, *14*(1), 111–133
- Smithsonian Institution. (1993). History of the polymerase chain reaction, interviews conducted in 1992 and 1993 by R. Kondratas, national museum of American history, The Smithsonian Institution, Washington DC.
- Stephan, P. E. (1996). The economics of science. *Journal of Economic Literature*, *34*, 1199–1235.
- Stokes, D. (1997). *Pasteur's quadrant: Basic science and technological innovation*. Washington, DC: Brookings Institution Press.
- Swanson, R. A. (2001). Co-founder, CEO, and chairman of Genentech, 1976–1996, an oral history conducted in 1996 and 1997 by S. S. Hughes, regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Thursby, J. G. & Thursby, M. C. (2004). Are faculty critical? Their role in university–industry licensing. *Contemporary Economic Policy*, *22*(2), 162–178.
- Valente, T.W. (1995). *Network models of the diffusion of innovations*. New Jersey, NJ: Hampton Press.
- Vallas, S. P., & Kleinman, D. L. (2008). Contradiction, convergence and the knowledge economy: The confluence of academic and commercial biotechnology. *Socio-Economic Review*, *6*(2), 283–311.
- Walsh, J., Cohen, W., & Cho, C. (2007). Where excludability matters: Material versus intellectual property in academic biomedical research. *Research Policy*, *36*(8), 1184–1203.
- Yansura, D. G. (2002). Senior scientist at Genentech, an oral history conducted in 2001 and 2002 by S. S. Hughes for the regional oral history office, the Bancroft Library, University of California, Berkeley, CA.
- Young, H.P. (2009). Innovation diffusion in heterogeneous populations: Contagion, social influence, and social learning. *American Economic Review*, *99*(5), 1899–1924.

- Zucker, L. G., Darby, M. R., Armstrong, J. S. (2002). Commercializing knowledge: University science, knowledge capture, and firm performance in biotechnology. *Management Science*, *48*, 138–153.
- Zucker, L. G., Darby, M. R., & Brewer, M. B. (1998). Intellectual human capital and the birth of U.S. biotechnology enterprises. *American Economic Review*, *88*, 290–306.
- Zucker, L. G., Darby, M. R., Brewer, M. B., & Peng, Y. (1996). Collaboration structure and information dilemmas in biotechnology: Organizational boundaries as trust production, In R. M. Kramer and T. Tyler (Eds.), *Trust in Organizations* (pp. 90–113). Newbury Park, CA: Sage.

## CHAPTER 4

---

# PROGRAMMERS AND PROFESSIONALISM

## Bringing Technology, Organizations, and Work Back In

Steven J. Kahl  
*Tuck School of Business at Dartmouth College*

Greg Liegel

---

### INTRODUCTION

Programming involves the process of designing and writing code that instructs computers to perform tasks, such as scientific computations or managing business processes. Computer programming emerged as an occupation in the 1950s with the commercial introduction of the computer and has grown in significance as the computer has become fundamental to organizational work. The Bureau of Labor Statistics (BLS) estimates that there were over 1.3 million computer software engineers and programmers in 2008 and expects a 21% increase over the next 10 years (see <http://>