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Explaining the move toward the market in US academic science: how institutional logics can change without institutional entrepreneurs

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Abstract Organizational institutionalism has shown how institutional entrepreneurs can introduce new logics into fields and push for their broader acceptance. In academic science in the United States, however, market logic gained strength without such an entrepreneurial project. This article proposes an alternative "practice selection" model to explain how a new institutional logic can gain strength when local innovations interact with changes outside the field. Actors within a field are always experimenting with practices grounded in a variety of logics. When one logic is dominant, innovations based on alternative logics may have trouble gaining the resources they need to become more broadly institutionalized. But if a changing environment starts systematically to favor practices based on an alternative logic, that logic can become stronger even in the absence of a coherent project to promote it. This is what happened in US academic science, as growing political concern with the economic impact of innovation changed the field's environment in ways that encouraged the spread of local market-logic practices.

Keywords Universities · Practice selection · Ecology · Biotechnology · Patenting · University-industry research centers

Since the 1990s, the central focus of organizational institutionalism has shifted from explaining stability to explaining change (Greenwood et al. 2002; Schneiberg and Clemens 2006; Beckert 2010).[1] As part of that shift, a number of scholars began to examine how new institutional logics take hold within a field (Friedland and Alford 1991; Rao 1998; Lounsbury et al. 2003).

[1]There are of course several new institutionalisms; my discussion is oriented toward the organizational variety. The shift toward explaining change, however, has taken place across institutionalisms (e.g., Pierson 2004; Thelen 2004).

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One prominent answer has focused on the role of the institutional entrepreneur. This explanation, which emerged partly in response to critiques of the lack of actors in institutional theory (DiMaggio 1988), suggests that new logics take hold as the result of cultural work done by entrepreneurial individuals or groups. Such entrepreneurs are particularly likely to emerge when fields are unstable or in crisis (Clemens and Cook 1999). These entrepreneurs, who are often from the margins of a field or outside it (Hirsch 1986; Leblebici et al. 1991), work to promote a frame that can make sense of the field in a new way. If they have sufficient social skill (Fligstein 2001) and political circumstances are favorable, their logic may be widely adopted, helping resolve instability as well as becoming the perspective through which new decisions are made (Rao 1998). Variations on this story have been found in diverse fields, including higher-education publishing (Thornton 2004), the radio broadcasting industry (Leblebici et al. 1991), the software industry (Garud et al. 2002), and French cuisine (Rao et al. 2003).

Institutional entrepreneurship has thus proven to be a very useful concept for explaining field-level change. More recently, however, it has also come under criticism—often by those who have themselves used the concept—for being invoked "as a deus ex machina" (Thornton and Ocasio 2008, p. 1006) or its tendency toward "'hero' imagery" (Lounsbury and Crumley 2007, p. 993). Fields and their logics can also change significantly even in the absence of any strong version of institutional entrepreneurship, which has led Lounsbury and Crumley (2007, p. 1006) to call for a "more distributed notion of institutional entrepreneurship."

At the same time, institutionalists have shown a growing interest in looking more closely at how institutions work at the local level. Scholars have called for "inhabited institutionalism" (Hallett and Ventresca 2006; Binder 2007; Hallett 2010), for a return to "microfoundations" (Powell and Colyvas 2008), for understanding institutions as the result of "creative syncretism" (Berk and Galvan 2009), and for a focus on "institutional work" (Lawrence and Suddaby 2006; Lawrence et al. 2009). These efforts draw on microsociological traditions of pragmatism, symbolic interactionism, and sensemaking, among others, to identify mechanisms and processes through which institutions and their logics are locally instantiated and sometimes changed.

I build on these recent developments to explain why institutional logics have shifted in one significant field, that of academic science in the United States. In US academic science, the influence of market logic has increased dramatically since the 1970s, as reflected in the spread of activities like patenting, industry collaboration, and faculty entrepreneurship. Yet in this field, the rapid spread of such activities did not result from an entrepreneurial effort to promote market logic as a solution to economic problems, even though such problems existed.[2]

I use this case to propose one particular path through which a relatively weak institutional logic can gain strength even in the absence of an intentional effort to promote it. Specifically, I argue that actors within a field are always experimenting locally with practices grounded in a variety of institutional logics. When one logic is

[2] I should note here that the empirical claims I make apply solely to changes that occurred within the United States, and I do not attempt to address the increasing role of market logic in academic science more globally. The United States, however, is an important case, since it was an early mover in marketization and its policies have served as a model for other countries (Forero-Pineda 2006).

dominant, innovations based on alternative logics may have trouble gaining the resources they need to become more broadly institutionalized. At times, however, the environment outside the field may change in ways that start to favor practices systematically that are based on an alternative logic. If that happens, the logic can gain strength even in the absence of a coherent effort to promote it. This, I argue, is what happened in the field of academic science, where in the late 1970s and early 1980s an exogenous shift in public policy changed the environment in ways that removed barriers to and provided resources for preexisting experiments with market-logic practices.

The rest of the article develops this basic argument in five sections. The first presents the theoretical model, placing it in the context of recent literature on institutional logics and how they change. The second summarizes the historical case, highlighting its fit with the model I propose, and the third describes the research strategy. The fourth and longest section presents empirical evidence, using it to demonstrate how a change in a field's environment can systematically encourage the growth of local innovations grounded in a particular institutional logic. Finally, I discuss other fields where this model might apply, the way it intersects with existing research on institutional change, and possible directions for further research.

Explaining change in institutional logics

While the institutional entrepreneur remains prominent in explanations of why particular institutional logics gain strength within fields, recent work has complicated the story of the strategic outsider entrepreneur mobilizing resources and using social skill to promote a new logic successfully (see Hardy and Maguire 2008; Thornton and Ocasio 2008 for reviews). Sometimes change is driven by powerful insiders, not by players marginal to the field (Rao et al. 2003; Greenwood and Suddaby 2006). Often, there is no decisive shift from one logic to another, but an ongoing interaction between two or more logics (Marquis and Lounsbury 2007; Reay and Hinings 2009; Dunn and Jones 2010; Greenwood et al. 2010), or a hybridization of multiple logics (Glynn and Lounsbury 2005; Mars and Lounsbury 2009; Battilana and Dorado 2010). And social movement dynamics can play a role in precipitating change, acting to destabilize fields, as sources of new logics, and as resources to be mobilized by entrepreneurs (Lounsbury et al. 2003; Rao et al. 2003; Weber et al. 2009; see Schneiberg and Lounsbury 2008 for a review).

In most of these cases, however, there is some kind of active intent to encourage a new logic, even if there is no clear-cut institutional entrepreneur (but see Dorado 2005). But sometimes, as in the case of US academic science, a particular logic clearly gains strength at the field level without a field-level effort to promote it. This situation, while not an uncommon one, is relatively understudied.

To help think about such cases, I draw on two familiar concepts. The first is that of practice. The problem of explaining how institutions are reproduced and sometimes changed at the local level is one of long standing (Barley and Tolbert 1997). A number of scholars have used the concept of practice (or similar ideas; see Binder 2007; Schneiberg 2007; Berk and Galvan 2009) as a way of thinking about how institutions are enacted on the ground as well as how individual action has the potential to reshape them or create new ones (Seo and Creed 2002; Colyvas and Powell 2006; Lounsbury 2007; Lounsbury and Crumley 2007; Friedland 2009a, 2009b; Purdy and Gray 2009; Owen-Smith 2011).

Drawing on this work, I begin with three assumptions about the nature of institutional fields. First, I suggest that institutionalized practices are an important connector between the local world of individual action and institutional logics that are strong at the level of the field. While a field's institutional logic(s) may constrain or shape individual action directly, they are also enacted through institutionalized practices that are reproduced locally within the field.

Second, individuals spend much of their time reproducing already-institutionalized practices, thus instantiating their accompanying logics. But as actors who have both interests to advance and the capacity for independent action, they also have the potential to act creatively in initiating new practices. Such innovations may, and most commonly will, draw on a logic that is already strong within the field. But they may also borrow other socially available logics that are not at the moment very visible in their field (Friedland and Alford 1991; Schneiberg 2007). One should therefore expect that even in a field strongly dominated by one logic, one will nevertheless also find individuals experimenting locally with practices grounded in other logics.

Third, while most local innovations will be ephemeral, some will eventually develop into newly institutionalized practices at the level of the field. For a local innovation to become an institutionalized practice, though, enough actors within the field must adopt it for it to become routine and taken-for-granted. There are two main routes through which that can happen. One is that the innovation provides such obvious advantages that, once they find out about it, other actors choose to adopt it on their own. In this case, we would expect to see it spread through standard diffusion processes (Strang and Meyer 1993; Strang and Soule 1998; on the distinction between diffusion and institutionalization, see Colyvas and Jonsson 2011). Another is that an institutional entrepreneur begins to promote the practice, either persuading others to adopt it or working to change field-level rules in ways that will encourage or require its adoption. A possible barrier to dissemination, on the other hand, is incompatibility with the field's dominant logic(s), which might lead others in the field to perceive the practice as illegitimate.

A final, critical, key for a local innovation to become an institutionalized practice is that potential adopters must be able to secure the resources needed to reproduce it on an ongoing basis. While some practices (using a new piece of jargon, perhaps) can be put into action with very few resources, many (restructuring an organization in response to a new management fad, for example) will require significant material resources in order to be implemented.

This leads to the second concept I draw on, that of an ecology. The predominant way of thinking ecologically about fields is as populations of organizations (Hannan and Freeman 1976). But there have been efforts to think ecologically about institutional processes as well. Haveman and Rao's (1997) attempt to show how changes in the technical and institutional environment of the early thrift industry selected for organizational forms that embodied particular institutional logics provides one excellent example (also see Scott et al. 2000; Marquis and Lounsbury 2007; see Haveman and David 2008 for a review of the relationship between institutional theory and population ecology). Building on this general approach, I suggest that thinking of an institutional field as an ecology of institutionalized practices embedded in a larger resource environment can be useful for understanding how institutional logics change. In particular, I suggest that some environments may tend to select for practices based on one logic or another. If that is the case, then changes in the larger environment may encourage the growth and spread of practices based on a nondominant logic, helping to strengthen that logic within a specific field.

A practice selection model of change

Putting these pieces together, then, I suggest one possible path to change. Individuals in a field will always experiment with new practices that solve local problems. Most of the time, they will base these practices on a logic that is strong within their field, since strong logics are both cognitively available and widely seen as legitimate. Occasionally, however, actors will experiment with other socially available logics even though those alternative logics are weak within their field.

Most of these local practices, regardless of their associated logic, will be transitory, but a few will be widely adopted and eventually become institutionalized. In a relatively stable field, local innovations grounded in weak logics will find it very difficult to spread and become institutionalized practices. This may be because they are seen as illegitimate, but it may also be because it is hard to secure the resources needed to reproduce them on an ongoing basis.

A change external to the field, however, such as a shift in government policy or a major technological development, may begin to select systematically for practices that are grounded in an alternative logic by providing resources for them and removing barriers to their spread. If this happens, local innovations based on the alternative logic—which were there all along, but had never spread very far—may start to disseminate and become institutionalized practices. This in turn makes the alternative logic (and its success) more visible within the field, further encouraging its use. If enough such innovations become institutionalized practices, the alternative logic can eventually gain strength at the level of the field, even though no entrepreneurial individual or group ever actively promoted its adoption. (See Fig. 1 for a visual representation.) I next briefly sketch what this process of practice selection looked like in the field of US academic science.

Selection for market-logic practices in academic science in the United States

Academic science in the United States has always drawn on multiple institutional logics. One that has historically been strong, and that was arguably dominant in the decades following World War II, is the logic of science (Friedland and Alford 1991). This logic sees the search for truth as having intrinsic value, and the central purpose of science as the pursuit of knowledge. Practical results, if any follow, are an agreeable but secondary purpose. Another logic, which has come to play a larger role in recent decades (Dickson 1984; Slaughter and Leslie 1997), is the logic of the market.[3] Market logic evaluates science on whether it produces results that have

[3] Friedland and Alford (1991), using slightly different language, refer to the logic of capitalism.

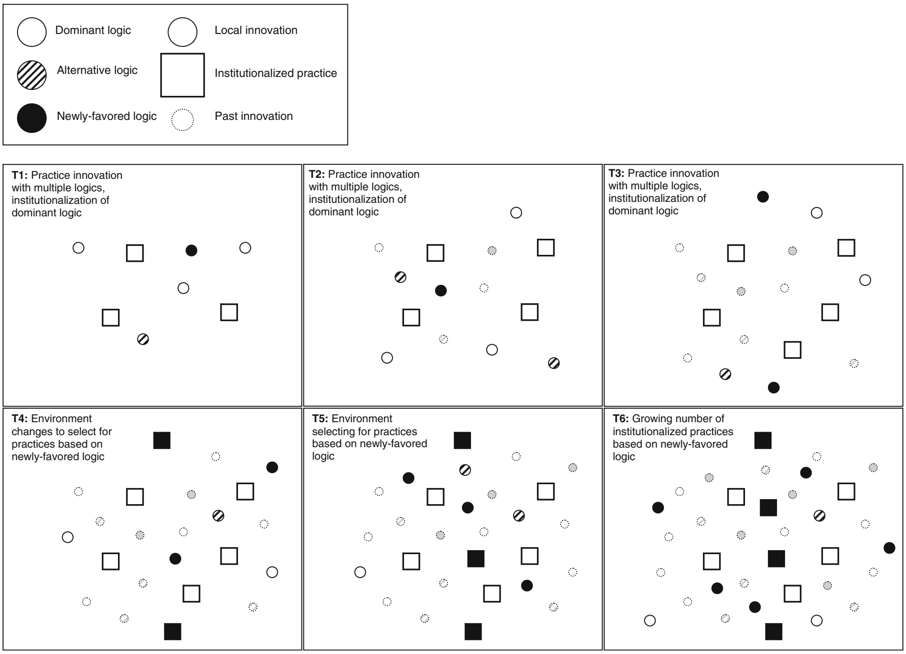

Fig. 1 Differential selection of practices based on newly-favored institutional logic

economic value. Science and market logic are not the only logics that have historically been significant in this field; certainly the logic of the state—that a key purpose of academic science is, for example, to support national defense—has also played an important role. But science logic and market logic have both been influential, and their relative influence has clearly shifted over time.

Prior to World War II, multiple competing logics were visible in academic science, with none clearly dominant (Geiger 1986). But during the 1950s and 1960s, academic scientists were able to convert the success of the Manhattan Project and other wartime science efforts into a system of federal science funding that was rapidly expanding, heavily oriented toward basic science, and whose distribution was strongly influenced by scientists themselves (Kleinman 1995). The National Science Foundation (NSF), which focused heavily on basic science, was growing exponentially during this period (NSF 2003, table B), and institutions like peer review, which encouraged scientists to evaluate research in terms of its contribution to a body of knowledge, rather than its practical impact, were becoming the norm in federal funding agencies (Chubin and Hackett 1990). Even mission-driven agencies, like the Office of Naval Research and the National Institutes of Health (NIH), were surprisingly oriented toward basic, undirected research during the postwar decades (Endicott and Allen 1953, p. 341; Geiger 1993, p. 19).

Even in this period, however, local experiments with market logic could be found in academic science. Three of the most successful were industrial affiliates programs, in which companies provided annual financial support to universities in exchange for privileged access to research; research parks, which began to be established during the 1950s; and industrial extension programs, which were modeled after the land grant universities' agricultural extension services. Each of these practices was grounded in the assumption that academic science was worthwhile because it led to inventions or discoveries with value in the marketplace, and each of them was adopted by a number of universities during the 1950s and 1960s.

But while these market-logic innovations experienced varying degrees of diffusion, none became widely institutionalized at the time. That was less because they were seen as illegitimate than because their adopters had trouble securing the financial resources needed to sustain the practices. These practices did not provide enough value to industry for industry to support them sufficiently in the long run, government funding for them was spotty at best, and universities had no motivation to subsidize them internally. As a result, they puttered along, remaining visible here and there but not expanding dramatically.

The 1970s, too, saw innovations with market logic. Experiments with university patenting (which had existed in the past, but began to expand gradually after 1968), faculty entrepreneurship in biotechnology, and university-industry research centers could all be found during this decade. But as had been the case with the market-logic experiments of the 1950s and 1960s, these experiments of the 1970s initially experienced only moderate success. Both regulatory barriers and resource limitations limited the ability of those in the field of academic science to adopt and sustain them. As late as 1977, none of these practices appeared poised for takeoff.

At just about this time, however, the national policy environment was experiencing a significant shift. Policymakers, increasingly concerned with the ongoing stagnation of the US economy, became very interested in a new (to them) argument: that economic growth was driven by technological innovation, and thus that improving innovation could help to fix the economy. During the late 1970s, this became a very prominent theme among US policymakers. Many new policies were proposed with the intent of solving the "innovation problem," and many old proposals were reframed in terms of the contribution they could make to improving innovation. Over the next few years, this argument played a major role in shaping policy decisions in areas ranging from tax policy to pension policy to patent policy and beyond.

The innovation argument focused primarily on US industry, not on universities, and many of the policies that were connected to it—for example, the creation of the Court of Appeals for the Federal Circuit, a federal patent court—had only indirect effects on universities. Collectively, however, they reshaped the resource and regulatory environment of academic science in ways that unleashed the growth of market-logic practices, which had up to that point been either restricted or resource-starved. In particular, these policy decisions allowed the practices of biotech entrepreneurship, university patenting, and university-industry research centers to take off in the last 1970s and early 1980s. By the latter part of the decade each of these once-local practices was well on its way to becoming institutionalized within the field.

The expansion of these practices did not result from a larger entrepreneurial project intending to encourage market logic in universities. Some specific market-logic practices, like university patenting, did have entrepreneur-like promoters. These entrepreneurs, however, were focused on encouraging the adoption of a specific practice, not on advocating a whole new way of thinking about academic science or changing the justification for its existence. Other practices, like biotech entrepreneurship, lacked such promoters entirely, and instead were widely copied after early innovators became enormously successful. But despite the absence of efforts to encourage the adoption of market logic at the level of the field, the collective effect of the growth and spread of these local practices was to strengthen market logic significantly in academic science. In the next section, I will review the research strategy used to reach these conclusions and identify this pathway to change.

Research strategy

Comparing the trajectories of three successful market-logic practices

The initial purpose of the project was to identify reasons that market logic gained strength in academic science in the United States over the past 30 years. Institutional theory would suggest one should look for individuals or groups who were actively working to promote such a change in logic. But such entrepreneurs were not in evidence at the field level. So I reframed the question by asking about the origins of three *practices* frequently associated with "academic capitalism" (Slaughter and Leslie 1997) or the "entrepreneurial university" (Etzkowitz et al. 2000). These practices—faculty entrepreneurship in the biosciences, university patenting, and university-industry research centers—were selected based on a review of the secondary literature. Others exist, but these are arguably the most widely discussed, and each embodies market logic in that it sees science as having worth because it creates economic value.

The first phase of the project, then, was to conduct case studies of the development of each practice in order to identify reasons it was adopted. The logic here was that of Mill's (1851) method of agreement, looking for commonalities among very different cases that might help explain their one important shared characteristic. Existing research suggested that potential factors might include universities opportunistically seeking new resources, government cutting back on funding for academic science, and industry becoming interested in outsourcing more of its research to academia (Slaughter and Leslie 1997; Geiger 2004; Slaughter and Rhoades 2004). Thus I was oriented toward the question, "How much of this change was driven by universities, government, or industry?" while remaining alert for other possible causes. I searched broadly, but also sought out specific types of sources that could potentially support or disconfirm each of these possibilities.

As my knowledge of each case grew, I began to identify inductively a set of factors that were important in the expansion of each practice and tentatively grouped them into necessary and facilitating conditions. (No single factor appeared to be sufficient in any of the three cases.) In repeated iterations, I continued to seek evidence that would support or disconfirm these developing explanations, and used counterfactual comparisons to decide which conditions were necessary. Of course making such a determination is a judgment call, not a science, but the argument I present depends on the preponderance of evidence rather than on making this distinction with 100% accuracy. The project as a whole draws on multiple kinds of historical evidence, including approximately fifty oral histories, twenty interviews, a hundred volumes of congressional hearings and reports, 350 magazine and newspaper articles, primary sources from seven archives, and well over a thousand additional primary and secondary documents. To avoid extending a lengthy bibliography even further, I cite good secondary sources in this article when possible, but the claims made are based on extensive primary research (see Berman 2012).

Comparison of the three cases suggested that neither response to government cutbacks nor industry interest was a major driver of any of the three practices. The one clearly shared factor in their institutionalization was the importance of government policy decisions. These policy decisions, however, were quite diverse. They were made at the state and federal level, in the legislative, executive and judicial branches of government, and by a variety of political actors who hoped to achieve very different political goals. Despite this diversity, however, the shared timeframe of the key decisions—all but one were made between 1978 and about 1985—suggested a closer look at the role played by policy.

Comparing ten policy decisions

A second phase of the project, then, examined the ten policy decisions that were important for the institutionalization of these practices. A similar strategy of comparing the histories of each policy decision, looking for commonalities among this diverse group, identified a particular argument that played a role in almost every case. That argument, that the United States needed to improve its capacity for technological innovation because innovation drives economic growth, was not very visible prior to 1977, but rapidly became prominent in policy discussions at about that time. Despite a great deal of research, no other common factor emerged. Again, using historical evidence and counterfactuals, I attempted to evaluate the role played by innovation arguments in shaping the significant policy decisions. In a substantial majority of these decisions, these arguments appeared to be a necessary condition for the decision to be made. Of the ten instances, innovation arguments played a decisive role in six, were a contributing factor in one, were clearly present in two more (but I lacked enough evidence to evaluate their importance), and were absent in only one decision—the one that predated the rise of such arguments to prominence.

The importance of innovation arguments to so many of the policy decisions that were critical to the expansion of key market-logic practices in academic science strongly suggests that this external shift affected the direction the field took. Some small subset of these decisions might have been made even in the absence of concern with innovation. And certainly many policy proposals framed in terms of their contribution to innovation were never realized. But the rise of concern with innovation appears to be the only condition that was necessary for the institutionalization of each of these three practices. Examining the historical development of each practice also supports the idea that government decisions were a necessary condition for institutionalization: in each case, people could be found experimenting with the practice by the late 1970s, but there were specific barriers to its further adoption that were subsequently removed, directly or indirectly, by government intervention.

Testing a model of practice selection

These observations—that the three institutionalized practices had diverse origins, that each at one point looked unlikely to spread widely, and that not a specific policy change but a change in the larger policy environment appeared to be the necessary condition for their takeoff—formed the basis for a tentative explanation of how market logic could gain strength without field-level institutional entrepreneurship. I hypothesized that local innovations based on market logic might be reasonably common even during periods when market logic was weak, but that before the late 1970s, the rules and resources that would allow such practices to spread more widely were not available. The rise of innovation concerns, however, changed the policy environment in ways that systematically favored market-logic practices, allowing some of them to spread, become institutionalized, and thereby strengthen market logic within the field.

The final phase of the study tested this theory. I identified three of the most successful local experiments with market-logic practices during the 1950s and 1960s: industrial affiliates programs, research parks, and industrial extension programs. Examination of these additional cases showed that while each practice gained some traction during the postwar decades, it found itself in a challenging resource environment that limited its adoption. The barriers encountered by these practices looked quite similar to those encountered by biotech entrepreneurship, university patenting, and university-industry research centers in the years before policy decisions facilitated their expansion, reinforcing the idea that those policy decisions were critical to the expansion of market-logic practices. It also seemed plausible that had innovation become an important policy issue in the 1950s and 1960s, rather than the 1970s and 1980s, government interventions might have allowed at least some of these practices to expand and become institutionalized.

Two final pieces of evidence provided additional support. First, while the growth of industrial affiliates programs, research parks, and industrial extension programs had stagnated by the end of the 1960s, research parks *did* eventually experience a second, larger wave of growth in the 1980s, after government subsidies intended to strengthen innovation became available for them. Second, while university patenting was a practice that expanded rapidly after the 1970s, it also existed locally in prior decades as well, and can be seen as another early experiment with market logic that only took off once the policy environment changed. The next section presents the empirical evidence for this story of the rise of market logic in US academic science chronologically.

The rise of market logic in US academic science

Experiments with market logic in the 1950s and 1960s

During the 1950s and 1960s, the logic of science was strong and visible in academic science in the United States. As already mentioned, however, local innovations with market logic could nevertheless be found, and industrial affiliates programs, research parks, and industrial extension programs were arguably the most successful of these. Each of these market-logic practices was successful in some locations and spread to a certain extent, but encountered barriers—most notably, resource limitations—that kept it from diffusing more broadly and becoming a fully institutionalized practice.

In industrial affiliates programs (also called industrial liaison or associates programs), firms paid an annual membership fee—typically $5,000 to $10,000 a year—

to a university department or research group in exchange for access to faculty and their work through symposia, visits, and circulation of preprints, as well as less formal channels. The Massachusetts Institute of Technology (MIT) appears to have established the first industrial affiliates program, tied to its Laboratory for Nuclear Science and Engineering, in 1948 as a way of bringing in revenue and building long-term relationships with industry.[4] The effort was successful, and by 1955 MIT had more than a dozen affiliates programs as well as a central Industrial Liaison Program office to manage them. The university was raising more than $800,000 a year with the participation of about seventy companies (MIT 1955, pp. 214, 342–343).

During the 1950s and 1960s, a number of other schools copied this model. Stanford was among the first, when in the mid-1950s an MIT PhD who had been recruited there launched his own successful affiliates program in microelectronics. Within a few years Stanford's aeronautical engineering department was circulating similar plans, and other disciplines quickly followed (Leslie 1993, pp. 72, 127–128). Caltech's affiliates program also appears to have been successful, with forty-one firms paying $10,000 a year to participate in 1957 (Scientists in the news 1957). By the mid-1960s, a number of other universities—including Lehigh, Mount Holyoke, the University of Buffalo, the University of Pennsylvania, and the University of Texas—had established them as well (News and notes 1952, 1953; Scientific meetings 1956; Scientists in the news 1957; Announcements 1963; Calendar of events 1967). In 1969, MIT claimed that its program "ha[d] served as a model for over fifty programs of this type" (MIT 1969, p. 717). Many of these, however, appear to have been ephemeral and left behind little trace.

University research parks had even more success during the 1950s and 1960s. Stanford established the first one in 1951, when its large expanses of open land left it with an increasing tax burden. The Stanford Industrial Park was seen as a way of using some of the land to develop an income stream that would be in keeping with the university's larger mission (O'Mara 2005). Stanford's park was quickly perceived as successful, and Cornell, the University of Oklahoma, and Research Triangle all established parks during the 1950s as well (Link and Link 2003, p. 82). More followed, and by the early 1960s, research parks were seen as a "mushrooming" trend, with ten established and another fourteen in the planning stages (First annual survey 1962). Many of these explicitly copied the Stanford model (O'Mara 2005).

Parks other than Stanford's, however, were not typically as successful during these decades. While a 1965 article in *Industrial Research* described recent years as a period of "accelerated development" (Schwitter 1965) for research parks, only Stanford's clearly had more than a 30% occupancy rate as of 1962 (First annual survey 1962). Even Research Triangle Park, which would later become an exemplar, struggled until the mid-1960s (Luger and Goldstein 1991, p. 78). While the organizational form spread considerably during this decade, many of these early parks were not very successful.

Industrial extension programs, while they also focused on the economic value of science, had a different sort of origin. Intended to help regional businesses solve scientific and technical problems, they were started largely as a result of the State

[4] History of the MIT Industrial Liaison Program, Finding Aid, [Massachusetts] Institute [of Technology] Archives and Special Collections AC 265.

Technical Services Act of 1965 (Carter 1965), not through local entrepreneurial innovation. The federal State Technical Services Act allocated a modest $20 million in grants to individual states for the purpose of industrial extension efforts, which could then be administered in a number of ways. Many states chose to make a university responsible, and by 1968, twenty-eight had a university or university system managing their technical services program and nearly 200 universities were actively involved (US House 1968, p. 5). The industrial extension programs undertook a variety of activities, but among the most popular and successful were field services, in which university representatives went to visit companies to help them solve technical problems, identify areas where technology could increase efficiencies, and connect firms with the resources needed to make such changes (see US House 1968, pp. 5–7 for examples). While both the organization and effectiveness of these industrial extension programs varied from state to state, a 1969 evaluation by the Arthur D. Little consulting firm found that overall the programs were both useful and economical (US House 1971, pp. 201–349).

Thus industrial affiliates programs, research parks, and industrial extension programs were each small-scale market-logic practices that had spread significantly by the end of the 1960s. The first two were local innovations that began to diffuse without any intentional effort to promote them, as other organizations in the field copied the successful actions of a high-status institution. In the third case, a top-down policy decision driven by non-academic concerns (Katz 1978, pp. 138–142, 168–171) provided motivation for the practice. Neither route, however, led these market-logic practices to become well-established during this era.

The main reason they did not was resource limitations. For these practices to become institutionalized, those who adopted them needed to be able to secure enough resources to reproduce them on an ongoing basis. But while none of these local practices appear to have encountered significant challenges on the grounds of illegitimacy, they all quickly ran into resource problems. Industrial affiliates programs were meant to finance themselves with membership fees; as Stanford's Frederick Terman described it, support would "be requested on the basis of enlightened self-interest rather than as an educational donation to Stanford."[5] But few firms were willing to maintain such time-intensive and costly relationships with more than one or two schools, so while elite early-moving institutions were able to create successful programs, such efforts often foundered elsewhere. Research parks encountered similar problems. Many, perhaps most, never found enough tenants, and by 1971 observers were describing a "research park shake-out." A review cited Stanford, Research Triangle, and MIT's Technology Square as successes, but estimated that three-quarters of parks were either "hurting badly or experiencing slow growth…. It is doubtful if the research park movement ever will duplicate the rapid growth experienced in the 1960s" (Danilov 1971, pp. 45, 47).

Both of these practices were limited because the market did not provide enough resources for them to become widespread. Another option, of course, was that government could have supported such efforts. There was no strong interest among policymakers at the time, however, in doing so. Even industrial extension programs,

[5] F. E. Terman to David S. Jacobson, 20 June 1958, Folder 1, Box 18, Series VII, Stanford University Archives SC 160.

which did get their financial support directly from the government, suffered from underfunding during their short lifespan and fell by the wayside entirely when Congress decided to kill the State Technical Services Act in 1969 (Hamilton 1969). Without sufficient funding from state or market, these market-logic innovations remained small in scale.

Experiments with market logic in the 1970s

During the 1970s, people in academic science continued to experiment with market-logic practices. But several of these experiments, unlike their counterparts during earlier decades, would eventually become widely institutionalized. In their early stages of development, however, the market-logic practices of the 1970s encountered barriers to their spreading—tied both to both resource limitations and regulatory hurdles—that did not look so different from the barriers seen by the practices of the 1950s and 1960s.

Faculty entrepreneurship in the biosciences

Unlike such fields as chemistry and engineering, the biosciences had little tradition of university-industry relations (Vettel 2006). Although great breakthroughs were being made in the 1950s and 1960s in molecular biology, those breakthroughs did not lend themselves to application. As one early biotech entrepreneur said of the Nobel Prizes awarded in this era, they "might as well have been awarded in astronomy. You can look at the stars all you want; you can't move them around" (Cape 2003). This reality, coupled with a tradition of disdain for industry among academic biologists (Kornberg 1997), led to a decided distance between the two.

But the potential for such relations changed with the 1973 publication of an easily replicable method of recombining DNA (Cohen et al. 1973), a development with very clear practical applications. In 1976 venture capitalist Robert Swanson approached a co-inventor of recombinant DNA (rDNA) technology, University of California, San Francisco (UCSF) biochemistry professor Herbert Boyer, about the possibility of starting a company to commercialize the technology. At the time, the suggestion was quite radical (Boyer 1994; Swanson 1997), and the pair's founding of Genentech caused significant turmoil at UCSF (Rutter 1998). But Swanson and Boyer's early success with Genentech would soon make the faculty entrepreneur/venture capitalist pairing a standard model for a new type of firm (Pisano 2006). In the next 2 years Swanson and Boyer were emulated by a few others, and by the summer of 1978 three more academic-driven startups (Cetus, Biogen, and Genex) were focusing on rDNA and one (Hybritech) was being formed to exploit another scientific advance, hybridoma technology (Cooke 1978; Byers 2006). Shortly thereafter, the number of such startups exploded. By 1980 about forty had been formed, Genentech had gone public for half a billion dollars, and the academic biosciences had permanently changed (Scherer 1980; US OTA 1984a, p. 93).

In retrospect the massive success of the start-up driven biotech industry seems to be the inevitable result of a technical breakthrough with practical implications. But other paths were possible—even likely. In the early years, biotech entrepreneurs faced two major hurdles. First, fears about the safety of recombinant DNA led to a strong push for Congress to regulate it as a biohazard and restrict research on it. Between January and April 1977 more than a dozen bills and resolutions on rDNA were introduced (Wright 1994, p. 224), and in the first half of 1977 most observers seemed to agree with the assessment that legislation was "inevitable" (Academy opposes 1977) or "probably unstoppable" (Wright 1994, p. 224). Such regulation, which could have required research to be done in costly containment labs and limited the quantity of recombinant DNA labs could work with, would have made it much harder for entrepreneurs to commercialize this science.

Second, and more critically, until late 1978 very little venture capital was available to finance such research. After a venture capital boom peaked in 1969 (Reiner 1989), the 1970s saw a deep slump in venture capital availability, with a mere $89 million of new funds raised nationally during 1976 and 1977 (Perez 1986, p. 30). A series of increases in the capital gains tax rate and a new restriction on pension funds' ability to invest in venture capital had helped nearly to shut down an already small venture capital industry (Gompers 1994). Biotech startups needed years of funding in order to develop products—for Genentech, which brought the first recombinant DNA product to market in 1982, 6 years and hundreds of millions of dollars. Genentech was not unusual in this regard. Cetus, its early competitor, employed 200 in 1979, yet had revenues of $7 million (Wade 1979, 1980a). As late as 1987, *no* biotechnology firm, of the hundreds in existence at that point, had "been able to report a profit solely from the sale of biotechnology products" (US OTA 1988, p. 81).

Without the availability of large quantities of venture capital, Genentech and the dozens of firms that emulated it simply couldn't have survived to reach successful IPOs as independent companies. This is not to say that rDNA technology would not have been commercialized; in all likelihood it still would have. But if the venture capital situation that existed as late as mid-1978 had persisted, biotech's path to commercialization seems unlikely to have been driven by academic scientists acting as entrepreneurs. Instead, a more plausible scenario is that it would have been developed at a slower pace in large multinational corporations—with the involvement, but not under the ownership, of academics.

University patenting

While bioscience entrepreneurship was almost unheard of before the 1970s, US universities had occasionally engaged in patenting since the early twentieth century (Mowery and Sampat 2001b). But while the practice could be found locally, it was not common. There were several reasons for this, including the relatively small scale of research, the fact that patenting's rarity in academia meant that it might never occur to many academic inventors to pursue a patent, and general skepticism about the appropriateness of patenting university research. But the most significant factor limiting university patenting was federal patent policy.

By the late 1960s, as much as 74% of academic R&D funding was being provided by the federal government (NSF 2005, Table 1). All this funding—and other research money that was commingled with it—was governed by federal patent policy. But the decentralized system of federal research funding that developed after World War II had led to a proliferation of patent policies among funding agencies, with twenty-two different federal statutes governing patent policy by 1980 (Eisenberg 1996). Much

Table 1 Factors affecting institutionalization of three market-logic practices in US academic science (policy decisions in bold)

	Biotech entrepreneurship	Patenting	University-industry research centers
Necessary conditions	Scientific breakthroughs	**Creation of institutional patent agreements by network of government administrators**	**NSF's I/UCRC program**
	Academic location of expertise		**State funding for UIRCs**
	Organizational model of Genentech	**Passage of the Bayh-Dole Act**	
	Generous venture capital environment	Emergence of biotechnology industry	
	(driven by **capital gains tax cut**, **ERISA decision**, and promise of new technology)		
Facilitating conditions	**Congressional decision not to regulate rDNA research**	Efforts of Research Corporation to train universities to patent	Growing interest of universities and industry in collaborating by late 1970s
	Diamond v. Chakrabarty	Increasing organization of professional community of university patent administrators	**Expansion of NSF support for UIRCs in the 1980s**
		Strengthening of US patent regime	
		(***Diamond v. Chakrabarty***, **Court of Appeals for the Federal Circuit**)	

federally-funded research could not be patented at all, and universities were discouraged from patenting even what was permissible by the overall complexity of the system.

By the 1970s, however, patenting was beginning to increase. While about 100 patents were issued to universities each year in the mid-1960s, that number had risen to about 250 by the mid-1970s (Mowery et al. 2004, p. 46). There were two main reasons for this. One was that the massive increase in federal research spending that took place during the postwar decades was resulting in more outputs of every kind, including patents (Henderson et al. 1998). But another was the entrepreneurial efforts of several groups to encourage the practice. These included the Research Corporation (a nonprofit patent management group that was training universities in patent administration; see Mowery and Sampat 2001a), a Washington network of supporters of university patenting who were working for policy change, and an emerging professional community of university patent administrators (Berman 2008).

By the late 1970s, the efforts of supporters in Washington had led to some concrete changes in federal patent policy that did make university patenting easier. Most significantly, after 1973 both NIH and NSF, accounting for two-thirds of federal R&D spending at universities (NSF 1997), had eased patenting restrictions considerably (Berman 2008). And by the end of the 1970s, patenting had more than tripled from 1960s levels (Mowery et al. 2004, p. 47). But a 1977 policy reversal at NIH laid bare the precariousness of the whole project of university patenting. Since the new policies had been established though regulatory change and agency discretion rather than through statute, they were easily reversed or changed. When patenting opponent Joseph Califano became secretary of the Department of Health, Education and Welfare (which housed NIH), he effectively shut down the system that had allowed universities to routinely patent NIH-funded inventions (Berman 2008). It was evident to supporters that without legislation giving universities the clear right to patent, institutionalization of the practice could not proceed. Yet people had been trying to pass such legislation for 30 years with little success (Eisenberg 1996). Thus as late as 1978 it appeared very unlikely that university patenting was about to become a fully institutionalized practice.

University-industry research centers

While a handful of industrial liaison programs were scattered throughout the nation's universities by the beginning of the 1970s, university-industry research centers (UIRCs) were still almost unheard of (Ikenberry and Friedman 1972). A UIRC is an organization that brings university and industry representatives together, with some degree of industry funding, to support research on questions of interest to both. A cross between an industrial liaison program and the "organized research units" that became a common means of organizing interdisciplinary research during the 1960s, a handful of local experiments with UIRCs were made during the 1970s.

A small NSF program created in 1972, the Experimental R&D Incentives Program (ERDIP), was one important early instigator. Meant to test new ways to "improve the climate for technological innovation through experimentation" (Shapley 1973, p. 1105), ERDIP initiated a number of projects, including one called the University-Industry Cooperative Research Centers Experiment (Block 1977, pp. 22–23). This experiment provided 5 years of funding to three centers intended to perform joint university-industry research. Two of them, a Furniture R&D Applications Institute based at North Carolina State University, and the New England Energy Development Systems Center, which brought together thirteen universities at an independent site, were unsuccessful. But the third, the MIT–Industry Polymer Processing Program, in which a group of firms interested in polymers jointly developed an ongoing research agenda with MIT faculty and graduate students, was a success and had become fully industry–supported by the end of the funding period (Colton 1982, pp. 85–101).

Outside of NSF-initiated efforts, a handful of UIRCs were created independently during the 1970s at other universities by individual scientists trying to solve local problems. At Caltech, for example, engineering professor Carver Mead started to build closer ties with industry around 1977 after becoming interested in the automated design of integrated circuits, which required access to expensive fabrication facilities. With Ivan Sutherland he created the Silicon Structures Project, which brought together companies like Xerox and IBM with Caltech scientists to work collaboratively on chip design (Mead 1996, pp. 10–14). By 1980, the Silicon Structures Project had five additional sponsors, including Hewlett-Packard, Digital Equipment Corporation, and Burroughs, each of which was contributing $100,000 a year as well as a company scientist to the project (Joining hands 1980; Mead 1996, p. 12). Analogous experiments could be found at engineering-oriented schools like Carnegie Mellon (the Processing Research Institute in 1971 and the Robotics Institute in 1978), Rensselaer Polytechnic Institute (the Center for Interactive Computer Graphics in 1977 and the Center for Manufacturing Productivity in 1979), the University of Delaware (the Center for Composite Materials and the Center for Catalytic Science and Technology, both in 1978), and the University of Minnesota (the Microelectronics Information Sciences Center in 1979).[6]

But while these centers achieved some local success, they also encountered some of the same problems as industrial liaison programs had. Once again, it proved challenging for universities to provide something of enough value to industry that industry would be willing to support it at a level of ongoing sustainability. Particularly during times of belt-tightening, firms saw UIRC funding as expendable. As a leader of Minnesota's Microelectronics Information Sciences Center later noted, "the successes of [its] type of research funding were not strong enough to prevent the founding companies from stopping their funding during the electronics crisis of the mid-80s" (Cibuzar 1993, p. 170). Furthermore, such centers often highlighted the culture gap between universities and industry. Industry frequently saw UIRCs' work as irrelevant to industry needs, while universities tended to think the centers were too project- or development-focused (NSB 1983b, p. 246; Mead 1996, p. 12). This was a problem, because without high levels of ongoing industry support, UIRCs could not survive. So by the late 1970s, while UIRCs were seen as a promising idea, they were also seemed suitable to a very narrow set of circumstances (Gray and Walters 1998, p. 10). They did not appear to be an organizational form for which institutionalization was imminent.

A new policy frame changes the environment of US academic science

Today, it seems self-evident that technological innovation is important to the economy. But it was only in the 1950s and 1960s that innovation became a topic of significant interest to economists (see Warsh 2006 for one review), and innovation did not become a politically important issue until the late 1970s. During the Sputnik years, while policymakers were interested in science and technology, it was largely because of the role it could play in winning the Cold War, not because of its impact on economic growth; in the first half of the 1960s, the defense-related agencies accounted for roughly 90% of all federal R&D spending (NSF 2003, table B). As one senator noted in 1963, "The direct relationship between science and technology … and our military capability and effort in space exploration … is spectacular and obvious.

[6] See Joining hands (1980), Smith and Karlesky (1977:67), Prager and Omenn (1980:381), US GAO (1983:8), NSB (1983b:45-46), US OTA (1984b:30), Cibuzar (1993), and Leslie (2001), among others.

Not nearly so obvious is the dependence of our general economic and social well-being on science and technology" (US Senate 1963, p. 2).

By the late 1970s, however, the state of technological innovation had come to be seen as a very important economic issue, and one where government action was sorely needed. Magazines and newspapers were writing articles with titles like "Vanishing Innovation" (1978), "The 'Innovation Recession'" (1978), and "Something's Happened to Yankee Ingenuity" (Graham 1978), and President Jimmy Carter's administration was conducting a twenty-eight-agency Domestic Policy Review on Industrial Innovation (The right way 1978). Members of Congress were considering several dozen bills affecting innovation and had formed an industrial innovation task force (Innovation 1979). In the *Congressional Record*, the phrase "technological innovation" was used eighteen times as often in the 1979 to 1981 period as it had been in the early 1960s, and the word "innovation" was used near "economy" or "economic" nine times as often.[7] By mid-1980, one lobbyist had identified no fewer than seventy-five active bills that purported to solve the "innovation problem" (A policy for industry 1980).

While the reasons for the political emergence of the innovation frame are complex, at least four stand out. First, the 1960s and 1970s saw economists build an increasing body of knowledge about the contribution of technological innovation to economic growth and productivity, knowledge that did find its way into policymaking (Walsh 1976; Nelson 1997). Second, in the late 1960s indicators began to emerge that the United States might be losing its global lead in terms of technological innovation and that this was contributing to an emerging trade deficit (NAE 1971). Third, and partly in response to such indicators, by the early 1970s representatives of R&D-intensive firms were becoming concerned about the state of US innovation and began pushing government to do something to strengthen it. And finally, overshadowing all these developments, the deteriorating state of the US economy during the 1970s heightened policymakers' attention to possible explanations for (and solutions to) its stagnation. Collectively, these factors helped push a new level of policy concern with technological innovation. This concern in turn shaped political decisions in ways that in the long run had the collective effect of pushing academic science toward the market.

Policy effects of the innovation argument

As described earlier, the case studies of three successful market-logic practices in academic science led me to identify ten important policy decisions that were necessary or facilitating conditions for the institutionalization of those practices (see Tables 1 and 2). Closer study of the histories of each policy decision suggested that the innovation frame was an important factor that they shared (see Table 3). Innovation arguments were very visible in nine of the ten relevant policy decisions,

[7] Based on a search of the *Congressional Record* on HeinOnline conducted 28 July 2010 using the terms "technological innovation" and "'innovation economy'~20 OR 'innovation economic'~20." The trend holds across a variety of searches on technology, innovation, and the economy. Numbers are corrected for the increasing length of the *Congressional Record* during this period and reflect the number of pages that contain the search term, not the total number of times the term is found.

Table 2 Policy decisions important to the expansion of three market-logic practices

Year	Policy decision
1968, 1973	National Institutes of Health and National Science Foundation adopt institutional patent agreements
1978	Congress decides not to legislate restrictions on recombinant DNA research
1978	Congress slashes capital gains tax rates
1978	NSF starts its Industry/University Cooperative Research Centers program
1978	Department of Labor proposes clarification to the Employee Retirement Income Security Act (ERISA) that allows pension funds to invest in venture capital
1978–1985	States establish and expand financial support for university-industry research centers
1980	Supreme Court's *Diamond v. Chakrabarty* decision makes life forms patentable
1980	Bayh-Dole Act gives universities the right to patent inventions resulting from federally funded research
1982	Congress creates the Court of Appeals for the Federal Circuit, a specialized patent court
1984	NSF expands support for university-industry research centers with its Engineering Research Center program

including all of those made after 1975.[8] These arguments appeared to be necessary conditions in six of the decisions, and present but probably not necessary in one more. In the remaining two cases, innovation arguments were visible but I lacked enough information about how the decision was made to evaluate the importance of their role. While space prohibits a detailed consideration here of the role played by the innovation frame in every case, I discuss three types of political effects these arguments had and give representative examples of each, highlighting the mechanisms through which such arguments had political effects.[9]

One way that growing concern with technological innovation shaped policy decisions was that many existing policy debates were reframed in terms of the need to improve the state of innovation in the United States and thereby help the economy. Positions that could be reframed in this way were given a political boost, even if their original proponents had no particular interest in economic issues or the question of innovation. While of course such a reframing was not sufficient for a proposal's success, in some cases it appears to have marked a turning point in the proposal's political fortunes. This was the case for the decision not to restrict recombinant DNA

[8] Innovation arguments were absent in the discussions that led to the creation of institutional patent agreements (which made it easier for universities to patent agency-funded research) at NIH in 1968 and NSF in 1973. In general, such arguments were not very visible politically before 1977. The IPA decisions, however, were not completely distinct from the passage of Bayh-Dole, since the same group of people who worked to implement IPAs later turned their efforts to pursuing such legislation and explicitly decided to reframe their project in terms of innovation rhetoric once it became popular around 1978 (Berman 2008). So the absence of innovation arguments in the IPA decisions does not appear to be a strong piece of evidence against the critical role of innovation rhetoric in changing the policy environment in ways that eventually expanded market logic in academic science.

[9] A fuller account of how innovation concerns shaped the policy decisions that are discussed only briefly here can be found in Berman (2012).

Table 3 Role played by innovation frame in ten key policy decisions.

Role played by innovation arguments	Policy decision
Necessary	Decision not to restrict rDNA research
	NSF Industry/University Cooperative Research Centers
	State support for university-industry research centers
	Bayh-Dole Act
	Court of Appeals for the Federal Circuit
	NSF Engineering Research Centers
Contributing	Capital gains tax cut
Present (but cannot fully evaluate)	ERISA clarification
	Diamond v. Chakrabarty
None	Institutional patent agreements

research in 1978, for the passage of Bayh-Dole in 1980, and for the creation of the Court of Appeals for the Federal Circuit in 1982.

The debate over whether to restrict recombinant DNA research through legislation was most active from early 1977 to early 1978. Fears about the public health risks of such research and the moves of a number of municipalities (including Cambridge, Berkeley, Princeton, and Ann Arbor) to place a moratorium on rDNA research pushed Congress to take up the issue (Wright 1994). The level of concern was significant enough that by mid-1977 most observers, even those opposed to legislation, viewed it as "inevitable" (Administration to seek 1977; Academy opposes 1977; Norman 1977, p. 2). During this period of debate passing references were frequently made to the potential commercial applications of rDNA. At this time, however, opponents of legislation did not argue that restrictions might threaten those applications (US Senate 1977c).

In November 1977, however, National Academy of Sciences president Philip Handler took the unusual step of announcing to Congress—prior to publication of the research—that Genentech's Herbert Boyer had successfully produced the hormone somatostatin using recombinant methods (US Senate 1977a, pp. 13–14). This announcement was made just as concern with the state of US innovation was gaining political momentum. While as late as June *Science* had been complaining that President Carter was not paying enough attention to the role of R&D in economic growth (Carey 1977), by November the Carter administration had, under pressure from industry (Vanishing innovation 1978), decided to undertake a massive policy review of the health of industrial innovation (Federal aid 1977). Handler's opposition to rDNA regulation seems much more likely to have originated with a desire to protect the autonomy of scientists than concern with the state of US competitiveness. But it was a good moment for refocusing the conversation on the potential threats of rDNA regulation to a nascent industry.

And the November 1977 hearings did indeed turn out to mark a turning point in the political discussion. Similar arguments were repeated by a variety of opponents of legislation: it could result in "an exodus of industry," have "economic consequences," cause the technology to be "exploited elsewhere," and cause industry to "move elsewhere" (US Senate 1977a, pp. 260, 290, 341, 368). Historians have claimed that the "argument, and implicit threat, from leaders of industry and science that a new field would be held back and its 'benefits' squandered if legislation was enacted was influential. No legislator wanted to be accused later on of blocking a potentially valuable source of international trade" (Wright 1994, p. 278; see also Hughes 2001, p. 566). While debate over legislation restricting rDNA research continued into early 1978 (US House 1978a), proponents of legislation found themselves making little headway, and serious discussion of the possibility ended within a few months.

The trajectories of the Bayh-Dole Act and the creation of the Court of Appeals for the Federal Circuit both followed similar patterns, in which existing policy proposals that had been making little headway were strengthened after being reframed around the innovation issue. In the case of Bayh-Dole, supporters of university patenting had primarily been interested in improving the utilization of publicly funded research, but had hit a wall in advancing their policy agenda. In 1978, however, supporters made a strategic decision to reframe their proposal in terms of its contribution to US economic competitiveness (Lasken 2005). This energized their campaign significantly, but as key proponent Norman Latker noted, "That was an issue that came up after the fact that we could use to support the bill," rather than a driving motivator (Latker 2005).

Similarly, discussion of the possibility of creating a national patent court had been going on for some time and was largely driven by the desire to solve the problem of forum-shopping, in which parties to a lawsuit tried to make sure it was decided in a friendly venue (US House 1977a, pp. 226–229). But when the Attorney General's Office for Improvements in the Administration of Justice took up the issue in 1978 and began looking for Congressional support, it found that the House Judiciary Committee "seemed more interested in industrial innovations than it did in the idea of the Federal Circuit" (Meador 1992, pp. 615–616). Fortunately for proponents, "It had always been one of our arguments in support of the centralized patent jurisdiction that predictability as to the validity of patents was important in promoting investment in research and production. Thus, the Federal Circuit and proposals for industrial innovations made a natural match" (Meador 1992, p. 615). Reframing their proposal around the newly significant issue of industrial innovation helped reformers gain support in the House as well as among industry leaders. In each of these policy decisions, then, the rise of concern with technological innovation provided an opportunity for preexisting policy projects to be reframed in ways that were newly politically appealing.

A second path to political effects was that concern with the economic impact of innovation drove the creation of entirely new policy proposals intended to strengthen technological innovation and thus the economy. The three policy decisions that provided support for UIRCs—including the creation of NSF's Industry/University Cooperative Research Centers (I/UCRC) program in 1978 and its Engineering Research Centers program in 1984, as well as the proliferation of state-level UIRC programs that took place between about 1978 and 1985—all fell into this category. The establishment of the I/UCRC program is emblematic of how this process worked. NSF had traditionally focused its support on academic science and been reluctant to fund industry science. While the National Science Board (NSB, the advisory group governing NSF) had established an NSF–Industry Relations Committee in 1971, as late as 1975 it was reaffirming NSF's policy of providing support to for-profit organizations only in exceptional cases (NSF 1976, p. 1; US House 1983, pp. 130–131).

As concern with industrial innovation began to rise, Congress requested that the agency report on the potential benefits and drawbacks of allowing industry scientists to compete directly with academic scientists for NSF funding (NSF 1976). The NSB initially reacted unfavorably, but had second thoughts after Senator Edward Kennedy, a strong supporter of NSF, introduced into NSF's appropriations bill a measure that would have required that the agency allow industry to apply for funding on equal terms (House, Senate split 1977). The measure did not end up in the final bill, but soon after, the board acknowledged that it was "politically expedient for the Foundation to support basic research in industry and that, if the NSF did not take the initiative in such plans, legislation would be passed to require it" (US House 1983, p. 135).

The NSB responded with a proposal that would preempt some of these concerns, but without requiring the undesirable (from its perspective) step of allowing industry to compete directly with universities for grants. Members of the board began arguing to Congress that the appropriate role for NSF was not funding industry research itself, but supporting university-industry collaboration (US House 1977b, pp. 968–1006; US House 1983, p. 447), and in January 1978 NSF director Richard Atkinson officially requested support for "an expanded effort in the area of university-industry cooperative research programs," emphasizing the "strong base of economic data indicating relationships between research and development activities and the gross national product" (US House 1978b, p. 4).

Congress reacted favorably to Atkinson's proposal, and the following year NSF established the I/UCRC program as well as starting to support university-industry collaboration through targeted grants. The actual amount of money requested was in fact quite small—$1 million in 1979—and centers were established at a very modest rate, with only four funded between 1979 and 1981 (US GAO 1983, p. 33). In congressional hearings, however, NSF representatives drew a disproportionate amount of attention to these efforts (US House 1978b), presumably in an effort to show that they were indeed doing something about the problem of industrial innovation. *Business Week* quoted one "knowledgeable source" on NSF's success at securing funding for these collaborative efforts: "One of the [persuasive] arguments was the importance of industrial innovation" (Digging a spur 1979; bracketed text in original).

Fears about the status of US innovation led to the creation of other UIRC programs as well, including a wave of state-level programs to support UIRCs. In the late 1970s the Council of State Planning Agencies, a tiny Washington-based nonprofit, began heavily promoting the idea that states needed to focus on bottom-up, technology-oriented economic development, pushing their ideas "relentlessly" to state officials through the National Governors Association and elsewhere (Osborne 1988, pp. 33–34). Programs to support such development caught on rapidly, with one survey identifying four states with programs "for the development of science and technology industries" in 1979, but thirty-three states with such programs in 1984 (de Laski 1985, pp. 24–25). While not all of these programs supported UIRCs, Coburn and Berglund (1994, p. 17) note that "government-sponsored university-industry technology centers were by far the most prevalent means of providing technology development services for industry during this period."

Back at the federal level, the success of the modest I/UCRC program helped to drive other, larger center programs—notably NSF's Engineering Research Centers (ERC) program. The ERC program was conceived in 1983 to harness scientific and technological innovation in service of the economy, and particularly to improve US "competitiveness," the latest buzzword, by bringing engineering education more in line with industry needs (Belanger 1998). While there were some continuities between I/UCRCs and ERCs, ERCs were focused explicitly on education and were broader in scope. They were also much larger. While NSF typically spent $50,000 to $100,000 a year on an I/UCRC, ERCs—which were typically expected to raise about half their support from industry—received about $2 million a year from NSF (Walsh 1986; Gray et al. 1987; Belanger 1998, p. 219). Thus in a number of cases, the increasing salience of innovation arguments led federal or state officials to initiate on their own new policies that seem unlikely to have been put into place in the absence of such concerns.

Finally, there are also cases in which innovation arguments were clearly present and apparently contributed to the positive reception of a particular policy proposal, but in which those arguments seem much less likely to have been decisive. The dramatic 1978 cut in the capital gains tax, which contributed to a massive increase in venture capital availability (Gompers and Lerner 1998) and thus helped create the conditions for biotech entrepreneurship to take off in 1979 and 1980, is one example. Between 1969 and 1976, the maximum capital gains tax rate in the United States increased from 25% to 49% (US CBO 1988, pp. 30–40), and President Carter, who entered office in January 1977, hoped to raise it further still (The business stakes 1977). This mobilized groups who thought the rates were already too high, including the small National Venture Capital Association (NVCA), the much larger American Electronics Association (AEA), and the American Council for Capital Formation, which was formed by capital-gains-sensitive timber interests (Johnson 1980; Martin 1991, p. 17). During 1977, they launched an all-out effort to counter the Carter administration's proposal, testifying to Congress, conducting industry surveys, and commissioning studies in support of their arguments (Johnson 1980, pp. 45–126). While a tax *increase* seemed unlikely by the end of the year, an actual tax *cut* seemed equally remote (American venture capital 1978).

In March 1978, however, tax cut proponents gained an important ally when Edwin Zschau, chairman of AEA's Task Force on Capital Formation, convinced Wisconsin Republican representative William Steiger to sponsor a bill (Lardner 1978; Johnson 1980, pp. 82–83). Zschau focused on innovation-oriented arguments for a tax cut, emphasizing the ways it would encourage the development of high-technology industries like electronics, in the process creating jobs, exports ("to lessen the trade deficit which reached a new record $26.7 billion in 1977"), tax revenues ("which result from the rapid growth for which small high-technology companies have become famous"), and the development of new technology (which would "improve productivity"; US House 1978c, pp. 1310–1311). Steiger, apparently inspired by these arguments, introduced an amendment to the Carter administration's tax reform package that would have reduced capital gains taxes to 1969 levels (Cowan 1978). Though the idea was not at first taken seriously, an April *Wall Street Journal* editorial pushed it into the public eye and it became an "overnight sensation" (Pine 1978; Wanniski 1978). The passage of California's Proposition 13, which slashed property taxes, in June, marked a sharp turn in an anti-tax direction for the nation (Martin 2008), and by November the seemingly quixotic effort to reduce the capital gains tax drastically had actually become a reality (Johnson 1980, p. 298).

The capital gains tax cut resulted from a larger and more complex set of political changes than just an increasing concern with innovation, and it is important not to overstate here the role played by innovation arguments. But such arguments did have a resonance that they had not had five years earlier. As one venture capitalist later recalled about his political efforts on behalf of the NVCA,

> we had real serious material and evidence from a disinterested third party—MIT and IEEE [that is, the report "The Role of New Technical Enterprises in the US Economy" (Flender and Morse 1975)]—that it was small enterprise that really provided the thrust of new employment opportunities in the country…We started out, one at a time, going in [to legislators' offices] and sitting down and showing what we had and what we're up to. Not all the times were we successful, but many times it was like turning a light on in a barn. These guys would say, What, I don't believe this. This is fantastic. Where have you guys been? (Lea 2008, p. 89).

In the right political environment, this kinds of argument could be very convincing.

In the case of the capital gains tax cut, enough information is available to make a reasonable case that innovation arguments were important, but not decisive. I have put two other cases in this category as well, however, because in those cases innovation arguments were clearly visible, but I lacked enough information to evaluate their importance fully. One of these was a 1978 proposal by the Department of Labor to clarify a rule that went into effect with the Employee Retirement Income Security Act (ERISA) in 1974. Pension funds read it as preventing them from putting any portion of their holdings in venture capital or other high-risk investments (Longstreth 1986, p. 33), but the Department of Labor proposal made it clear that pension managers were permitted to invest in smaller, riskier companies as part of a larger prudent portfolio (Rules and regulations 1978). The fraction of new venture capital investment coming from pension funds increased dramatically after this decision was made, from 15% in 1978 to 31% of a much larger base in 1979, and economists have found the ERISA clarification to have been quite significant in increasing venture capital availability (Gompers and Lerner 1998). The NVCA and AEA promoted ERISA changes as well as the capital gains tax cut using similar language about technological innovation and the economy (US House 1977c; US Senate 1977b). But the politics of the decision were less visible because it was made within the Department of Labor rather than Congress, so it is difficult to evaluate the role played by innovation concerns.

Similarly, innovation arguments were very visible in arguments around *Diamond v. Chakrabarty*, the 1980 Supreme Court decision that upheld the patentability of life forms. Genentech, the Pharmaceutical Manufacturers Association, the University of California, and the American Society for Microbiology all filed *amicus* briefs in support of Chakrabarty, claiming that "patents encouraged technological innovation, and they should be allowed to encourage it in genetic engineering, since the field was recognized as a richly promising contributor to the nation's high-technology competitiveness" (Kevles 1994, p. 130). But Supreme Court decisions obviously are made behind closed doors, and thus it is difficult to know what role such arguments played in shaping the 5–4 decision. Thus in several policy decisions, technological innovation arguments were clearly visible, but either had only a contributing role in the decision's being made or were such that it is hard to know exactly what sort of role they actually played.

How the new political environment shaped academic science

The political decisions shaped by innovation concerns were diverse. They were supported by a variety of interest groups and represented a wide spectrum of beliefs about the proper role of government in the economy, from the interventionist approach taken by UIRC programs to the free-market views reflected in tax cuts and the strengthening of intellectual property rights. A number of them—particularly the capital gains tax cut, the ERISA clarification, the *Chakrabarty* decision, and the creation of the Court of Appeals for the Federal Circuit—had little to do with universities. Nor were they intended to change universities; the problems that it was argued they would solve were mainly problems of industrial innovation. Even in the case of policies that affected universities directly, relatively little attention was paid to how they would affect the institution of academic science or universities as a whole. Critics of the Bayh-Dole Act, for example, focused on the question of whether allowing private parties to patent publicly funded research was a giveaway, not on whether it would have negative effects on the university (Berman 2008).

Yet while there was no explicit intent to encourage academic science to adopt market logic, the policies associated with the innovation issue nevertheless systematically encouraged the expansion of such practices. The essence of market logic, after all, is thinking about science as having economic value. Policies aimed at using science or technology to strengthen the economy necessarily saw science in terms of its market role. Regardless of where these policies were coming from ideologically, then, they tended to promote market-logic activity. The collective result was a larger change in the external environment that encouraged and selected for market-logic practices that previously would not have spread widely enough to become institutionalized. Ultimately, this led to a rise in the significance of market logic in the field as a whole.

The institutionalization of market-logic practices

In this new environment, the practices of faculty entrepreneurship in the biosciences, university patenting, and university-industry research centers each took off. By the mid-1980s, all of them were well on their way to becoming institutionalized, in the sense of being widespread and taken-for-granted ways of doing things in academic science.

Faculty entrepreneurship in the biosciences

At the beginning of 1978, a small handful of biotech companies, all founded with the involvement of academics, were in existence. But they faced some formidable barriers, and no one seems to have anticipated that within 3 years entrepreneurship would become widespread within the academic biosciences. The threat that Congress was going to restrict rDNA research was fading away, but companies like Cetus and

Genentech were burning through cash at an alarming rate, with no products near the market and few revenues. As the *Economist* wrote that year, “Genentech has run smack up against one feature of biotechnology that may distinguish it from integrated circuits: entry costs in bulk manufacture too high to be hurdled by venture capital” (Industry starts 1978). And it was a particularly bad time to be looking for even conventional levels of venture capital, with availability at a historic low (Perez 1986, p. 30).

After the capital gains tax cut and the ERISA decision, however, the venture capital environment was transformed. While venture capital firms raised only $20 million in new funds in all of 1977, by June 1979 *Newsweek* was claiming that “in the last year, entrepreneurs have raised an estimated $750 million for investments in fledgling companies—an amount roughly equal to all the venture capital raised between 1969 and 1977” (Pauly 1979). By the end of 1979, venture capitalists were actually complaining that “there’s too much money chasing too few good investments” (Wallets open 1979).

This new venture capital environment accelerated the spread of faculty entrepreneurship in biotechnology. By 1980 the four best-known firms were worth an estimated $500 million (Wade 1980a, p. 688), and dozens more, typically involving from five to several dozen academics each (Kenney 1986, pp. 101–103, 149–154), had been established (US OTA 1984a, p. 93). The peak year for firm formation was 1981, when forty-three new companies were created (US OTA 1984a, p. 93). By this point even some academics who had been vocally skeptical of entrepreneurship, like Stanford’s Paul Berg, were starting companies (Berg 1997); others who had never considered entrepreneurship became intrigued by the involvement of friends and colleagues and wound up starting companies themselves (D’Andrade et al. 2001, pp. 99–109).

While by 1982 some observers claimed that, for example, “there is no notable [molecular] biologist … anywhere in America who is not working in some way for a business” (Hilts 1982, p. 185), in reality active entrepreneurship was still far from universal (Etzkowitz 1983, p. 201). But the practice of entrepreneurship was itself becoming normative, if not required, and it was faculty critics of the “rising entrepreneurial tide” who now were receiving a “less than enthusiastic reaction” from their colleagues (Boly 1982). As *Science* noted, “Scientists who 10 years ago would have snubbed their academic noses at industrial money now eagerly seek it out” (Culliton 1982, p. 960). In a remarkably short period of time, the practice of faculty entrepreneurship had moved quickly along the path toward institutionalization in the biosciences.

University patenting

By the end of the 1970s, patenting was already on the rise at universities. Between 1969 and 1980, the number of patents issued to universities increased by an average of 7.3% per year, and the number of universities receiving any patents at all rose from thirty to seventy (NSB 1983a p. 134–135). But the practice was fragile, as was demonstrated by the reversal of NIH’s liberal policy governing patents on the research it funded. And it was modest in scale, with the total number of patents issued to universities in 1980 still below 400 (NSB 1996, appendix table 5-42). Only twenty universities actually had technology transfer offices (Geiger 2004, p. 217), and according to one central member of the university patenting community, “only maybe half a dozen were really active in the business” (Bremer 2001, p. 167).

In 1980, though, the passage of the Bayh-Dole Act gave universities the clear right to patent government-funded research and encouraged them to do so. This helped tip the balance toward the full institutionalization of university patenting. The rate of increase in patents issued to universities rose, and between 1980 and 1990 the number being assigned to universities each year tripled (NSB 1996, appendix table 5-42; 2006, appendix table 5-67). Research suggests that at universities like Stanford and the University of California, which were already actively patenting by 1980, “Bayh-Dole was an important, but not a determinative, factor,” while its effect was more critical at universities that had not previously had systematic patenting efforts (Mowery et al. 2001, p. 105; see also Matkin 1990, p. 74). The increasing normativity of the activity was also reflected in the rise in the number of technology transfer offices—to seventy-nine by the end of the decade (Geiger 2004, p. 217). Membership in the Society of University Patent Administrators doubled between 1982 and 1984 as well (NSB 1985, p. 110), as new entrants sought to learn from those with longer track records.

The general strengthening of intellectual property rights that took place during the 1980s, including the *Chakrabarty* decision and the creation of the Court of Appeals for the Federal Circuit, also played a role in encouraging university patenting (Dunner et al. 1995, p. 154; Abramson 2007, pp. 78–80). While it is difficult to isolate the effects of this larger shift on university patenting specifically (and to the best of my knowledge no one has attempted to do so), it did make intellectual property more valuable and more things—including those being produced in universities as the result of the biotech revolution—patentable. The total number of patents being issued annually in the United States increased by 50% in the 1980s after remaining relatively flat for decades (US PTO 2011). It seems reasonable to assume that the same policy decisions that encouraged patenting outside the university also provided new incentives for universities to patent.

By the mid-1980s, then, patenting was becoming a well-established and accepted practice in academic science. As the president of the Association of American Universities noted in 1985, “fifteen years ago, there were few parts of the academic world where [the] term [intellectual property], if indeed it was known at all, would have been viewed as anything other than alien and unwelcome. Today, the idea that the products of the mind constitute a kind of property—and valuable property at that—is part of common campus discourse” (Rosenzweig 1985, p. 41).

University-industry research centers

In the late 1970s, UIRCs were an innovative practice that a number of people were experimenting with locally—a few with government support, and a few independently—but that was proving difficult to sustain, both for financial reasons (universities could not provide enough value to industry to justify a substantial long-term commitment) and because of the difficulty of managing the university-industry culture gap. Starting around 1978, however, increasing levels of federal and state money were targeted toward these sorts of endeavors, again with the idea that UIRCs would improve the nation’s capacity for technological innovation.

NSF's I/UCRC program played a small but important role. In addition simply to providing financial support for such collaborations, it drew on the example of the one successful UIRC that had come out of the earlier ERDIP program to provide an organizational model for new centers. NSF played an active role in helping new centers learn from past experiences, compiling lessons learned, for example, into a Practice Manual, which was already in its second edition by 1984 (Eveland et al. 1984). Perhaps as a result, most I/UCRCs did manage to become self-supporting after their five-year grant ended (McGowen 2008).

NSF's move toward larger centers with the establishment of the Engineering Research Center program in 1984 meant that much more federal money would be channeled toward support of UIRCs. The agency supported twenty-nine ERCs between 1985 and 1990 at a cost of more than $100 million (Feller et al. 2002, p. 459; Bozeman and Boardman 2004). Other NSF center programs followed, with large Science and Technology Centers being established in the late 1980s and smaller State/Industry University Cooperative Research Centers in the early 1990s. Across the three center programs it sponsored by 1990, NSF had directly funded at least eighty-three UIRCs with a total of perhaps $200 million (Gray and Baneth 1992; Fitzsimmons et al. 1996, p. 10; Feller et al. 2002, p. 459).

But while NSF support was important, the spread of state UIRC programs was probably even more significant in making UIRCs common, largely because of the sheer amount of money involved. State programs varied considerably, and while some supported centers of modest size, others were funded at levels as high as $12 million a year (Walker 1982). National data on state programs are spotty, but a study conducted by the state of Minnesota (1988, p. 2) found that states had allocated more than $225 million to "technology or research centers" in the last year alone. While state and local governments provided only 8% of all university R&D funds in the late 1980s, 69% of UIRCs received some state funding between 1985 and 1990 (Cohen et al. 1994, p. 8; NSF 2005, Table 1).

This large influx of targeted government support for UIRCs meant that an organizational form that had previously struggled to reproduce itself now found doing so much easier. It is suggestive that a number of the centers founded independently in the 1970s had been incorporated into an NSF or state-level center program by the mid-1980s (Berman 2012). This turn to the state was doubtless influenced by the availability of new resources, but also reflected the challenge of maintaining adequate levels of support from industry alone.

Overall, then, the number of UIRCs increased rapidly during the 1980s and the organizational form became commonplace. While a relative handful existed during the 1970s, by 1983 the National Science Board found a national population of about 250 centers (NSB 1983b, pp. 11, 16). By 1990, based on their own national survey, Cohen, Florida and Goe (1994, p. 6) estimated the existence of just over a thousand UIRCs. Such centers were becoming a common and taken-for-granted way of organizing research activity on university campuses.

Market logic in the field of academic science

The new policy environment created by the increased concern with the economic impact of innovation systematically favored market-logic practices. The innovation-driven policy decisions provided new resources for such practices both directly (through support for UIRCs) and indirectly (by increasing the availability of venture capital), as well as by strengthening the university's ability to treat science as a product (by allowing more patenting and expanding intellectual property rights). This permitted a variety of local experiments with market-logic practices—experiments that in an earlier era would have been unlikely to become institutionalized—to spread widely and reproduce themselves. This new environment facilitated the institutionalization of market-logic practices beyond biotech entrepreneurship, university patenting, and UIRCs as well. Research parks, for example, had experienced a first burst of growth in the 1960s; after stagnating through the 1970s, government subsidies helped to support a new and larger wave of research park development in the 1980s (Luger and Goldstein 1991), leading them to become a more fully institutionalized practice in academic science.

This did not mean that every older experiment with market logic would take off. Industrial extension, which mostly disappeared after the State Technical Services Act was killed in 1969, never really came back. While in the 1960s universities had seen themselves as a resource for industry, providing the knowledge needed to solve its problems, by the 1980s they had switched to seeing themselves as economic engines, driving the creation of new products, firms, and industries with their technological innovations (Berman 2012). The UIRC model of collaboration fit this new way of understanding university-industry relations better than the industrial extension model.

Nor would every new market-logic experiment thrive. An early-1980s wave of large-scale university-industry research partnerships in biotechnology (Kenney 1986, p. 56) proved to be a one-time thing. Made at a moment when a lot of money was sloshing around this emerging industry, these partnerships—which were not directly subsidized by government—turned out, like many predecessor practices, not to provide enough value to industry to justify their long-term support.

But the shift in the policy environment did make new resources available for enough market-logic practices that the logic was able to gain a foothold in the field as a whole. The growth of these individual practices facilitated the strengthening of market logic in the broader field in three ways. First, as activities like faculty entrepreneurship, patenting, and UIRCs became larger in scale, they themselves organized a larger fraction of all activity in the field. More people were directly involved in the practice of market logic, and being exposed to such activity encouraged others to emulate it as well (Stuart and Ding 2006).

Second, the visible success of such activities encouraged further experimentation with market logic. The massive rewards that went to individual scientist-entrepreneurs during the early years of the biotech boom, for example, encouraged a wave of experimentation by university administrators with various kinds of research partnerships, venture capital investments, and support for faculty startups (Wade 1980b) in an effort to secure some of those rewards for their organizations.

Finally, the success of some market-logic practices caused actors within academic science—particularly administrators—to notice its new political appeal, and more specifically the appeal of innovation arguments. This not only provided additional reasons to experiment with market-logic activities, but increasingly led to the public justification of existing activities specifically in terms of their economic impact. While in the 1960s universities saw the building of research parks primarily as part of their service mission (Schwitter 1965), in the 1980s research parks were much more likely to be justified as sources of economic growth (Savoye 1983).

Change in the policy environment initiated the strengthening of market logic by tending to select for practices grounded in it. But as those practices grew and awareness spread that the environment *had* changed, additional dynamics were set off within the field. So while the changed environment did select for certain kinds of practices, once it had started to do so dynamics internal to the field tended to reinforce and accelerate the shift.

Practice selection and the strengthening of institutional logics

The case of US academic science thus demonstrates one path through which a new logic can gain significant influence in a field even in the absence of a coherent entrepreneurial project to promote it. In academic science, actors were innovating with market-logic practices before the 1970s as well as after. But until the late 1970s, resource limitations and regulatory barriers made it difficult for those practices to be widely reproduced. Starting in the late 1970s, though, a shift in the field's broader environment—driven by a new set of political concerns that were largely exogenous to academic science—began to favor such practices. As several market-logic practices spread and were institutionalized, dynamics internal to the field further strengthened the influence of market logic. More generally, this case suggests that thinking of the field as an ecology of practices can help in understanding institutional change when institutional entrepreneurship is limited or missing. Different environmental conditions can select for practices based on one logic or another; a shift in those conditions can make it easier for local innovations grounded in an alternative logic to gain resources, spread, and become institutionalized. If enough such practices are institutionalized, the alternative logic will gain strength in the field as a whole.

Some may note that my focus on the necessity of resources in allowing practice innovations to spread runs counter to one of the core tenets of institutionalism, which is that institutions persist because they are legitimate, and alternative logics are rejected because they are illegitimate, not because they lack instrumental value. I deemphasize legitimacy here largely because it played a limited role in the particular case I studied. In academic science, while market logic was less legitimate than other logics in the postwar decades, it was not strongly illegitimate (Kleinman et al. 2011). The successful market-logic practices of the 1970s did encounter some resistance because they were seen as illegitimate, but earlier, less successful practices did not seem to be challenged on those grounds, suggesting that lack of legitimacy was not the main reason those earlier practices did not spread. While I believe legitimacy can be decisive in explaining some cases of institutional stability or change, it appears that at least when the illegitimacy of an alternative logic is only moderate, a big influx of resources can overcome resistance to a new logic.

This larger argument for practice selection is of course theory-building based on a single case, and further work is required to evaluate whether the model applies to other fields. But there are a number of similar cases in which it seems potentially applicable. For example, there are a variety of fields that depend heavily on the government for resources and in which market logic has come to play a larger role over the past several decades, including the military, transportation infrastructure (like ports and highways), utilities, and K–12 education. While there may have been entrepreneurial projects to promote market logic in some of these cases, in at least one (the military; see Avant 2005) the shift seems to have taken place in a more distributed fashion. Similarly, the field of news media is one in which a reshaping of the larger environment—in this case, as the result of technological change—seems to be eroding the power of professional logic while strengthening practices based on democratic logic and market logic, again without field-level entrepreneurship.

This focus on practice selection, however, is proposed not primarily as an alternative to institutional entrepreneurship explanations but as a complement to them. Entrepreneurship can be important in the institutionalization of new practices even when there is no effort to change the field as a whole. For example, the spread of university patenting involved both an institutional entrepreneur and the support of a movement-like new professional association, but they were trying to establish a specific practice, not a new logic in the field (Berman 2008). There are other cases in which the relationship between field-level entrepreneurship and practice selection may be even more complex. In healthcare, market logic appears to have gained strength partly because of the institutionalization of many local experiments that proved successful in a changing resource and regulatory environment (Scott et al. 2000). Yet there are also clear elements of field-level entrepreneurship as well, as actors from the fringes of the field promoted market logic as a means through which medicine could be made more cost-effective and less wasteful (Schmidt 1999).

Future research, then, might look not only at the generalizability of this practice selection model, but also at the interaction among practice selection, movement dynamics, and institutional entrepreneurship. Might field-level entrepreneurship projects sometimes grow out of smaller-scale efforts to institutionalize specific innovative practices? Could field-level entrepreneurship have effects, not only by changing field-level rules, but also by raising the visibility of an alternative logic in ways that encourage more local innovators to draw on it? Or might institutional entrepreneurship aimed at one field (like the state) lead unintentionally to the kind of environmental change that selects for new logics in other fields? A related set of questions would specify the conditions that precipitate particular paths to field-level institutional change. For example, scholars have frequently noted that field-level change is often a response to crisis, though crisis is clearly not a prerequisite for change. Perhaps crises or exogenous shocks are more likely to result in field-level entrepreneurship, while more subtle or gradual changes (like institutional drift; see Hacker 2004) tend to lead to practice innovation.

Incorporating the idea of practice selection into our understanding of institutional change extends recent scholarship in two areas. First, an exploration of practice as a site of both institutional reproduction and institutional change builds on other efforts to use practice as a way to resolve the embedded agency problem (Seo and Creed 2002; Lounsbury 2007; Friedland 2009a; Owen-Smith 2011). Lounsbury and Crumley (2007), for example, suggest that normal variations in practice can lead to the establishment of distinct new practices when field-level actors define those variations as anomalous. Considering the resource and regulatory environment as exerting selection pressures on such variations is another angle on the same basic approach.

While those who are most concerned with the local level may find the model I present dissatisfying, as it neglects the details of how local innovations develop—and I would certainly agree that much more work on local practices is needed—my goal here is to connect macrolevel shifts in the environment with the likelihood of success of microlevel innovations, rather than to understand those innovations themselves.

Second, thinking about practice selection as a means through which institutional logics gain or lose influence is compatible with recent work that emphasizes the coexistence of institutional logics in fields (Glynn and Lounsbury 2005; Marquis and Lounsbury 2007; Schneiberg 2007; Greenwood et al. 2010). Practice selection can potentially explain how a particular institutional logic gains strength incrementally, rather than replacing another logic wholesale, as in a crisis-and-field-transformation story. And attention to environmental conditions can help in explaining how multiple logics can compete and coexist for extended periods of time, as has been the case for academic science over the past 20 years, without the balance tipping fully one way or the other.

It is clear that there are many paths to institutional change, and that at different times and places the path taken depends on individual creativity, the mobilization of human, cultural, and material resources, the structure of the existing field, and the environment outside the field to varying degrees. Practice selection is one way, and hopefully a useful one, of thinking about how the external environment can affect the potential of individual creativity to lead to more durable institutional change.

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