FIELD THEORIES AND THE MOVE TOWARD THE MARKET IN US ACADEMIC SCIENCE

Elizabeth Popp Berman

ABSTRACT

Field theory is one of the most visible approaches in the new political sociology of science, and Fligstein & McAdam's (F&M) Theory of Fields *is the most visible recent attempt to further it. This paper evaluates F&M's theory of field transformation by comparing it with Berman's (2012a) field-based explanation of the changes in the field of US academic science. While F&M's general framework is quite useful, their explanation, which focuses on struggles between incumbents and challengers over whose conception of the field should dominate, does not map neatly onto the changes in academic science, which saw no such field-level struggles. This suggests that tools are also needed for explaining new settlements that do not result from intentional efforts to establish them. In particular, the case of US academic science shows that local innovations with practices based on alternative conceptions of the field can lead to field-level change. Attention to the interaction between local*

Fields of Knowledge: Science, Politics and Publics in the Neoliberal Age
Political Power and Social Theory, Volume 27, 193–221

ISSN: 0198-8719/doi:10.1108/S0198-871920140000027015

practice innovations and larger environments provides insights into how change ripples across fields, as well as the ongoing contention and dynamism within even relatively stable fields.

Among the variety of approaches taken by the new political sociology of science (Frickel & Moore, 2006), field theory is prominent (Albert & Kleinman, 2011; Berman, 2012a; Hess, 2012). But different versions of field theory have been developed, and additional variations continue to be proposed. In the sociology of science, both Bourdieusian field theory (Albert & Kleinman, 2011; Camic, 2011; see also Albert and McGuire, 2014) and organizational field theory (Berman, 2012a, 2012b; Owen-Smith, 2011; Swan, Bresnen, Robertson, Newell, & Dopson, 2010) have been used.

Recently, Fligstein and McAdam (F&M) (2011, 2012) have made a high-profile attempt to integrate and move beyond existing versions of field theory. Their framework, which explicitly draws on both Bourdieu and the organizational tradition, but also uses insights from economic sociology, historical institutionalism, and the social movements literature, has ambitions to build a common foundation for thinking about how social action leads to the creation and transformation of mesolevel social orders: that is, fields.

This paper explores the potential of that framework by applying it to an empirical case familiar to sociologists of science, and which I have elsewhere (Berman, 2012a, 2012b) used organizational field theory to explain: the changes in US academic science that have taken place since the 1970s, which have variously been characterized as "mode 2" (Gibbons et al., 1994), a "triple helix" (Leydesdorff & Etzkowitz, 1996), "academic capitalism" (Slaughter & Leslie, 1997), "asymmetric convergence" (Kleinman & Vallas, 2001), and "neoliberal" (Lave, Mirowski, & Randalls, 2010).

Renarrating this story using F&M's framework, and then explicitly comparing it with my own, shows the strengths and limitations of each of these approaches to field theory, as well as highlighting ways that the two might be fruitfully extended or combined. In particular, F&M bring two major advantages over other variants of field theory: their conceptualization of fields as nested and overlapping, and their focus on the relationship *between* fields as a main source of change. They also bring a much-needed attention to power that is central to Bourdieu but tends to be played down in organizational field theory. But their framework, at least as presented in

A Theory of Fields, also has two limitations that the empirical case of US academic science highlights: an overemphasis on field-level strategic action and a focus on transitions from one settled state to another.

A conversation between the two approaches points to places where the strengths of each can be combined. In particular, the organizational approach to fields has increasingly developed tools for explaining change from the bottom up and change initiated outside the field (Berman, 2012b; Greenwood, Díaz, Li, & Lorente, 2010; Purdy & Gray, 2009; Smets, Morris, & Greenwood, 2012). These insights can be used in a way that is compatible with F&M's framework, yet goes beyond its present form. At the same time, F&M draw needed attention to the fact that bottom-up and externally driven field change may actually reflect relationships between nested, overlapping, or adjacent fields. Thus considering multiple versions of field theory can help us think more deeply about nature of the link between fields, a promising area for further research.

The paper develops this argument in five parts. First, it provides a short empirical introduction to the changes in US academic science over the past few decades. Second, it reviews major field-theoretic approaches and how they suggest we should think about those changes. Third, it retells the story of the transformation of US academic science using F&M's framework. Then it more briefly examines that story through my own lens, with particular attention to the points of difference between my version and the one suggested by *A Theory of Fields*. Finally, it explicitly compares the two approaches: their strengths, weaknesses, and points of overlap.

THE CHANGING FIELD OF US ACADEMIC SCIENCE: ACTORS, POWER, BELIEFS, AND PRACTICES

Any empirical description of a field must identify the major actors within it, how power and resources are distributed among them, what they understand the purpose of the field to be, and what kinds of action they perceive to be legitimate. Over the last thirty-five years, the field of US academic science has seen the perceived purpose of the field change substantially, as have accepted practices within it. The distribution of power and resources has evolved more moderately, and there has been little change in the composition of the major actors within the field. The rest of this section, based on a summary of previous research (Berman, 2012a), sketches changes and continuities in each of these areas.

Adoption of New Beliefs

From 1978 to the present, a new set of assumptions about how science gets into practical use, and the role played by universities in that process, has been widely adopted within US academic science. These include the following:

(1) Scientific inventions often originate in academia but should be moved into the private sector as quickly as possible for the most efficient development of applications.
(2) A system of incentives, such as patent rights and ownership stakes in startup companies, is needed to encourage both academic scientists and industry to work toward these ends.
(3) As the source of many scientific inventions, academic scientists have a unique role to play in the process of technology transfer.
(4) Universities have a public responsibility to encourage and facilitate this process.
(5) Doing so is important not only because it will get science rapidly into use, but also because it will maximize science's contribution to economic growth and competitiveness.

This way of thinking about the purpose of academic science has partially displaced other views. For example, one alternative belief that has become less common is that the purpose of academic science is simply to produce knowledge. From this perspective, while society supports scientific research partly because of its effects on the world, the best way to ensure those effects is to insulate science and allow it to develop on its own. Identifying and facilitating practical applications should not be a priority of academic scientists. Another alternative view is that society supports academic research to solve specific problems identified by government, like defense needs or health issues. Both of these understandings can still be seen within universities but have lost influence in comparison with the perspective that emphasizes technology transfer and economic impact.

Adoption of New Practices

This new sense of the field's purpose is reflected in a variety of practices focused on realizing the economic value of academic science. These were once uncommon and of questionable legitimacy but have spread and gained acceptance over the last several decades. They include the patenting and licensing of scientific inventions and the creation of university-sponsored spinoff companies, faculty entrepreneurship, the creation of research parks and startup incubators to encourage university–industry relationships, the establishment of a greater variety of university–industry partnerships, and the development of incentives for faculty who pursue activities aimed at technology transfer, such as applying for patents. More generally, a variety of routine scientific activities have been tweaked to play up their potential for commercial applications or their impact on economic development.

Evolving Sources of Power

While beliefs and practices have evolved significantly in US academic science, power within the field still rests on the same sources: scientific authority and economic resources. Scientific authority is conferred upon individuals by professional communities within science. It is produced through publication in high-status journals, through association with high-status peers and institutions, and through professional acknowledgment in the form of citations and awards that recognize the production of scientific knowledge valued by the community. The list of universities, journals, and professional associations most important for conferring scientific authority has much overlap with the list from fifty years ago.

There are also significant continuities in the providers of economic resources. In particular, government, led by the National Institutes of Health (NIH), the National Science Foundation (NSF), and the Department of Defense (DOD), still pays for about two-thirds of academic R&D, down from about three-quarters in 1978. The modest decrease has been made up not by industry funding, which has increased from 4% to 6% of the total, but by academic institutions themselves (which provide 20% of all support today, vs. 14% in 1978) (National Science Board, 2012, Appendix Table 5-2).

There have been moderate, but important, changes in how both scientific and economic capital are acquired in the field. First, while it is more or less the same group of institutions that confer scientific authority, there has been some evolution in what kinds of contributions those institutions value. In particular, academic scientists and their institutions see applied, commercially relevant research as having greater worth now than they did several decades ago, when many saw doing such research as selling out (Boyer, 1994).

Second, the sources of economic capital have also changed in some ways. NIH has become more important to the resource environment, providing 39% of all US academic R&D funding today, versus 29% in 1978. Federal agencies other than NIH, NSF, and DOD have become less important: they once supported 19% of academic research but now fund only 6% of the total (National Science Board, 2012, Appendix Tables 5-2, 5-3). But more critical than the mix of agencies that support academic science is the evolution of their priorities. NSF, for example, was once exclusively oriented toward support for fundamental research, but today focuses on "programs that help drive future economic growth, global competitiveness, and the creation of high-wage jobs for American workers" (National Science Foundation [NSF], 2013, p. 1). Thus while the sources of support have evolved only modestly, the broader shift toward a model of science focused on technology transfer and economic development means that research that can be framed in such terms has a funding advantage, even when support is not explicitly targeting such projects.

But Similar Players and Rules

While new beliefs and practices are quite visible in academic science, and the distribution of resources has changed in some ways, the major actors, and types of actors, in the field are largely the same. A few additional universities – for example, Georgia Tech and the University of California, San Diego – have entered the top tier of scientific institutions. Some new professional organizations, like the Association of University Technology Managers, have become visible. The congressional committees and White House offices that supervise science policy have evolved. But for the most part, the actors important to the field today would be quite recognizable to anyone who was involved with it in the 1970s.

Similarly, while there have been important laws passed, like the Bayh–Dole Act, that have changed specific rules governing the field, there has been no dramatic rethinking of what agencies will provide support for scientific research, or of the role of peer review in making those decisions. Thus if one wants to explain changes in the field of academic science, the most important ones to explain are the evolution of beliefs about the field's purpose, how it works, and how it relates to the larger world, along with the emergence of specific practices tied to those beliefs.

SURVEYING FIELD THEORIES OF CHANGE

The evolution that has taken place in US academic science is important, well-examined, and lends itself to a field-theoretic approach. But different versions of field theory, while overlapping, suggest different ways of thinking about how fields change.

Organizational theory provides one framework for thinking about fields (DiMaggio & Powell, 1983; Greenwood, Suddaby, & Hinings, 2002; Leblebici, Salancik, Copay, & King, 1991), and I have used this framework elsewhere to understand the changes in US academic science (Berman, 2012a, 2012b). Bourdieusian field theory (Bourdieu, 1995, 1988; Bourdieu & Wacquant, 1992) provides an alternative, and sociologists of science have also drawn on Bourdieu (Albert & Kleinman, 2011; Camic, 2011; Cooper, 2009), though not specifically to explain *why* fields change. F&M's recent work (2011, 2012) attempts to build on both the organizational tradition and Bourdieu while going beyond them by incorporating insights from recent work in social movements, historical institutionalism, and economic sociology.

For Bourdieu, fields are primarily spaces of struggle for dominance, with their own stakes, rules, and forms of capital. In the field of science, the game is about acquiring scientific capital and getting to determine what counts as "good science" (Bourdieu, 1981 [1975], 2004). Fields have varying degrees of autonomy; that is, they differ in the extent to which internal actors, versus external actors with economic and political power, get to define the rules of the game and the form of capital that has value within it.

Within Bourdieu's framework, field-level change can be understood in two ways. First, fields change as the result of struggles between different groups of actors within the field. Each wants to see its own attributes defined as the legitimate form of capital. And each will work, in alliance with groups in other, connected fields, to change the evaluation criteria to favor its own form of capital. Second, field change can result from hysteresis – a mismatch between a relatively durable habitus and changing environmental conditions that can trigger unpredictable reactions (Bourdieu, 2000). There has recently been a move toward applying Bourdieu to the sociology of science (see particularly Albert & Kleinman, 2011). But no one has yet used Bourdieu specifically to explain the changes in US academic science in recent decades (though see Albert & McGuire, 2014; Lave, 2012; McGuire, 2011 for related work), and this paper does not try to fill that gap.

Like Bourdieu, organization theory sees fields as structured, relational social spaces in which actors play a common game with agreed-upon rules. But while organizational field theory was inspired in part by Bourdieu's work (Mohr, 2005), there are important differences between the two (see Dobbin, 2008; Emirbayer & Johnson, 2008; Vaughan, 2008 on the intersection of Bourdieu and organization theory). Compared to Bourdieusian approaches, organizational theories unsurprisingly pay more attention to the role of organizations in structuring fields. Less obviously, though, they also focus much less on power (Emirbayer & Johnson, 2008). Instead, they have particularly emphasized institutional entrepreneurship (Hardy & Maguire, 2008) and social movement dynamics (Schneiberg & Lounsbury, 2008) as sources of change.

More recently, however, organizational field theory has shifted its focus toward types of change that are more evolutionary and incremental, and less strategic and intentional (Purdy & Gray, 2009). Much of this work focuses on the emergence and diffusion of new practices (Lounsbury & Crumley, 2007; Smets et al., 2012) and on how competing and hybrid institutional logics create possibilities for change (Greenwood et al., 2010; Greenwood, Raynard, Kodeih, Micelotta, & Lounsbury, 2011; Thornton, Ocasio, & Lounsbury, 2012).

My own explanation of the changes in US academic science is grounded in, and illustrates, this recent scholarship. Following Friedland and Alford (1991), I begin with the assumption that major institutional logics are broadly available in modern society. I further assume that in any field some people are always experimenting with new practices and that such practices instantiate one institutional logic or another. But while multiple logics are always available, some will be stronger than others in a particular field: they will be more visible, and practices based on them will be easier to reproduce. When the field's environment changes, it may become easier for practices based on alternative logics to take hold and spread. If such practices become common enough, the alternative logic may gain strength in the field more broadly (Berman, 2012b).

This, I argue, is what happened within the field of US academic science. Before the 1970s, one could find individuals experimenting with practices based on market logic – practices that saw the value of science in its potential to create economic value – even at a time when other logics that valued science for its own sake or for its service to the state were stronger. In the late 1970s, though, the environment beyond the field – in particular, the preferences of policymakers – made it easier for market-logic practices to take hold and created resources that helped them to do so. The result was the gradual, but profound, strengthening of market logic within the scientific field, which took place even though there was no entrepreneurial or social-movement-like project aimed at changing the field. This is consistent with other recent work in organization theory that emphasizes the interaction between individual innovation, multiple logics, and the broader environment (see Thornton et al., 2012, chapter 6 for a review).

F&M, however, want to go beyond the organizational conception of fields, critiquing it for an inability to explain change other than through the "great man" of the institutional entrepreneur, a lack of attention to fields' larger environments, and inattention to power (2012, pp. 28–29). Instead, they propose their own synthesis, focusing particularly on field emergence and field transformation, rather than on periods of relative stability. Their framework emphasizes seven concepts: (1) fields as, first and foremost, spaces of strategic action; (2) the role of incumbents, challengers, and governance units; (3) social skill and the existential functions of the social; (4) the broader field environment; (5) exogenous shocks, mobilization, and the onset of contention; (6) episodes of contention; and (7) settlement (F&M, 2012, pp. 8–9). F&M see fields as structured by rules set up to favor, and defended by, incumbents, but which challengers are always trying to change to favor themselves.

So far, this is compatible with both Bourdieu and organization theory. But in their explanation of change, F&M emphasize the relationship among fields to a greater extent than other field theories. This relationship is the most common source of change: a crisis in one field ripples out through all the others it is linked to (F&M, 2012, p. 19). The larger of these ripples create opportunities for "episodes of contention" that can lead to field-level change. In an episode of contention, challengers identify a threat to incumbents or an opportunity to advance their own interests. They mobilize their resources and use innovative forms of collective action to leverage the threat or opportunity. Incumbents seek to defend the status quo, and both challengers and incumbents work "to mobilize consensus around a particular conception of the field" (F&M, 2012, p. 22). An episode of contention ends when a new settlement is reached, often with the intervention of state actors: either a restoration of the status quo ante or the institutionalization of a new set of rules and norms favoring a new set of incumbents.[1]

Thus F&M's theory seems well-suited to explaining why change took place in the field of US academic science. It would suggest we look for exogenous shocks leading to an initial disruption of the field, perhaps coming from "fields upon which the strategic action field is dependent" (F&M, 2012, p. 99); challengers identifying this disruption as an opportunity and using their resources and social skill to promote a conception of the field more focused on commercial applications of science; incumbents defending the status quo; and eventual imposition of a new settlement, most likely enforced by the state.

In *A Theory of Fields*, F&M apply their framework to two empirical cases of field transformation: US racial politics and the US mortgage market. In each case, they break their analysis into five moments: (1) "strategic action field: before," (2) "exogenous shocks," (3) "crisis and contention," (4) "settlement and the new strategic action field," and (5) "effects on other strategic action fields" (F&M, 2012, p. 115). In the section that follows, I use this approach to tell the story of the changing field of US academic science, drawing on the research reported in Berman (2012a). The resulting narrative is incomplete but is also generative of new insights into both the empirical case and the question of how fields change more generally.

THE TRANSFORMATION OF THE STRATEGIC ACTION FIELD OF US ACADEMIC SCIENCE

Strategic Action Field: Before

The field of US academic science was in a fairly settled period from the end of World War II to the late 1960s. It was growing rapidly, of course, but that was part of the settlement: both policymakers and scientists assumed that academic science would receive double-digit budget increases every year. In this "social contract for science" (Guston, 2000), it was also assumed that academic science would serve defense purposes, and that universities would in general be responsive to the needs and preferences of the federal government (Berman, 2012a, pp. 35–37).

But one final assumption, possibly the most important of all, was also built into the settlement. Both policymakers and scientists believed that unrestricted federal support for basic research would, by virtue of serendipity, lead to practical applications (Swazey & Reeds, 1978, pp. 5–8). This assumption was ironic, given than the settlement itself grew out of the success of the Manhattan Project, the largest applied science effort of all time. Yet it was fundamental and widely shared. This was a golden period for academic science, a time of rapid expansion (National Science Board, 2010, Appendix Table 4-3) and significant autonomy.

The postwar settlement must be understood in the context of the Cold War. Defense was *the* central concern of American politics, a fact reflected in the prominence of the DOD, the Atomic Energy Commission, and NASA among university funders at the time. In 1960, nearly half of universities' federal R&D dollars came from these three agencies (NSF, 2003, Table B). Physicists, who had been central to the Manhattan Project, sat at the top of the scientific hierarchy, and high-level science advisers came disproportionately from the physics community (Kevles, 1978).

Yet the "pure science" part of this deal was less obvious. It had been wrangled by scientists in the postwar years, written into *Science – The Endless Frontier* and institutionalized in the NSF (Kleinman, 1995). While NSF, which explicitly supported basic science, provided only about 10% of universities' federal research dollars in 1960 (NSF, 2003, Table B), the principle was also upheld by DOD's Advanced Research Projects Agency, which supported "blue sky" research on the assumption that the big defense-relevant breakthroughs were unpredictable (Richard J. Barber Associates, 1975), and even, to a surprising extent, NIH (Endicott & Allen, 1953). With the nuclear bomb still recent, and in an era of expanding budgets, this view was rarely challenged.

This postwar settlement served university scientists very well. Although money flowed particularly freely in defense-related fields, budgets were generally expansionary. Departments were hiring new faculty. Scientists got to decide what kinds of knowledge were valuable. And as late as 1967, scientists thought that 15% annual growth in funding was a reasonable target for the foreseeable future (Greenberg, 1967).

Exogenous Shocks

But things were about to change. As F&M would predict, the changes that disrupted this settlement were largely external to the field of US academic science. Prominent among them was a tightening of the federal budget as the postwar economic expansion slowed and the United States became embroiled in Vietnam (Berman, 2012a, p. 36). The end of the 1960s saw not 15% annual increases in the federal science budget but slight declines (NSB, 2010, Appendix Table 4-3). After the extended period of growth, scientists perceived these cuts as quite painful (Geiger, 1993, p. 246).

Budget pressures also led to more questions about "blue-sky research." In 1966, President Lyndon Johnson suggested in a speech that he might push NIH in a more applied direction (Walsh, 1966). The Mansfield amendment declared that defense funds would be restricted to research with "a direct and apparent relationship to a specific military function or operation" (Public Law 91-121). Even NSF saw a 1968 amendment that authorized it, for the first time, to support applied research (Belanger, 1998, p. 76). All these changes called into question the settlement's assumption that if the federal government simply provided support to scientists and let them decide what to do with it, the rest would take care of itself.

Another external crisis, the escalation of student protests against the Vietnam War (Geiger, 1993, pp. 230–242), was also putting strain on the field's settlement in the late 1960s. As the gap between campuses and Washington became more visible, some members of Congress demanded that universities better control their students and threatened to cut funding, including science funding, if they did not (Boffey, 1969; Walsh, 1969). Some scientists, too, protested against the role science had come to play in the military–industrial complex (Moore, 2008). The resistance of both students and scientists helped to undermine the settlement's assumption that universities would be responsive to government, including military, needs.

Crisis and Contention

This combination of federal budget retrenchment, pressures for more applied research, and campus unrest effectively destabilized the postwar settlement that had served scientists so happily for two decades. Scientists and universities began to complain that "deep budget cuts ha[d] undermined their ability to do effective research" and that they "fear[ed] an unprecedented financial crisis" (Boffey, 1968, p. 340; 1970, p. 555). They called repeatedly for a return to the status quo ante, defending the assumptions of the old settlement:

> It may … be an undesirable distraction to its rigor and sharpness of focus for the research worker himself to be too sensitive to the unpredictable implications of his own work … . To place the burden of such justification [of practical utility] on individual projects would be the surest possible way of stifling the most creative, the least predictable advances in scientific understanding. (US Senate, 1967, pp. 8–9)

But scientists were slow to perceive just how much things had changed, and the field's incumbents seemed ill-positioned to develop a new vision of the field. By the mid-1970s, federal science budgets were still flat, DOD had largely stopped supporting blue-sky research, and NSF found even its strongest supporters criticizing its lack of concern with practical applications (House, Senate Split, 1977; Richard J. Barber Associates, 1975;

Smith & Karlesky, 1977). Other groups, however, were setting the stage for what would eventually resolve itself into a new settlement, and a couple of these were able to use this period of crisis to advance their own interests, as F&M suggest would happen.

One such group was working to start companies that would commercialize recombinant DNA (rDNA) technology. An easy-to-replicate method of recombining DNA was published in 1973, and within a few years a small number of academics, venture capitalists, and other investors were trying to develop applications. Public fears about the dangers of rDNA, however, as well as sharp boundaries between academia and industry were threatening their efforts (Berman, 2012a, pp. 62–71). This group wanted to see academics participate in the commercialization of rDNA research, with the ultimate goal of creating a new industry.

A second loose group wanted to promote university patenting. Motivated by a belief that patents were necessary in order to get scientific inventions into use more rapidly, some of its members were university-based and others were located in government agencies. In the mid-1970s, they formed a professional association, the Society of University Patent Administrators, which became, in F&M's language, an internal governance unit within the field of US academic science (Berman, 2012a, pp. 100–106).[2] This group hoped to change the rules and norms around patenting scientific inventions in the field of US academic science.

A third group, however, would have important effects on the field of academic science even though it was not trying to use the crisis in that field to its own advantage. In fact, it was located in another field entirely and was responding to its own field-level crisis. This group was made up of scientists and executives who managed industrial R&D operations for large corporations. The economic conditions of the 1970s, and increasing competition from Japan and Germany, were threatening the field of R&D-intensive firms. Leaders of those firms' research arms diagnosed this crisis as resulting from a failure of technological innovation and began working to draw the innovation issue to the attention of policymakers and the media (Berman, 2012a, pp. 51–54). While they were not targeting the field of academic science, their efforts would, indirectly, prove important to it.

Settlement and the New Strategic Action Field

During the late 1970s and early 1980s, the terms of a new settlement were laid out. The old order, which assumed that government would support academic science with ever-increasing amounts of money, that useful results would automatically follow, and that universities would be responsive to government preferences, came to an end.

The new order rested on a different conception of how academic science served the public interest and why government should support it. In this vision, the purpose of academic science was to create technological innovations. These innovations would lead to the creation of new companies, or result in products that would make existing companies more competitive, spawn whole new industries, and expand the economy. This process would not occur effortlessly. Universities would have to exert themselves actively to encourage commercially promising inventions and transfer them to industry. In conjunction with this change, practices like industry collaboration, entrepreneurship, and patenting became more prevalent, and those who produced science with commercial potential gained power and resources in the field (Berman, 2012a, pp. 30–32).

This new settlement was not driven by either of the two groups that were trying to use the period of crisis to advance their own interests, although they were indeed able to advance their causes – academic scientists did help establish the biotechnology industry, and the field's rules changed to encourage universities to patent their inventions. The group that proved most important to the transformation of academic science, however, the scientists and executives working for R&D-intensive companies, was the group that was not trying to change academic science at all.

To understand how that could be the case, one must realize that the conception of science that underpinned this new settlement came from in the policy field, not the academic field, and was advanced by the group representing R&D-intensive firms. These firms, which were struggling in the economic environment of the 1970s, developed a narrative that explained their own troubles and identified solutions to them. They argued that a crisis of technological innovation was causing them to lose global economic leadership. This crisis was caused by government policies, and changes in those policies – including cutting taxes, strengthening patent rights, deregulation, and loosening antitrust enforcement – would stimulate innovation and restore the position of US industry (Berman, 2012a, p. 54).

Congress went on to make all those changes, to a greater or lesser extent, in the late 1970s and early 1980s. But the leaders of these firms did not ask Congress to involve universities in solving the problem of innovation. Instead, as R&D-intensive firms convinced policymakers that innovation was both economically critical and in crisis, the policy environment became friendlier to academic practices that treated science as something of potential commercial value – as an input into the economy (Berman, 2012a, pp. 54–57). Policymakers made a variety of decisions that had incremental but pervasive effects on academic science, and activities like patenting, faculty entrepreneurship, and industry collaboration became more common. Increasingly, incumbents within the field of academic science began to use the language of innovation and the metaphor of academic science as an economic engine as ways to talk about its purpose (Berman, 2012a, pp. 154–156).

Unlike in some more radical episodes of change, the field's incumbents did not change dramatically as the new settlement stabilized. While some internal governance units, like the Association of University Technology Managers, became more visible, for the most part the new settlement saw the same agencies supporting university research, and the same organizations representing universities and scientists' interests, as the old.[3]

Within those organizations, however, leadership often evolved to become friendlier to the new conception of the field. NSF, for example, saw a director appointed who was much more closely attuned to the economic impact of science, and who created programs that supported university–industry collaboration (US House, 1978). The agency also saw a growing number of engineers move into leadership positions, which further aligned it with the new model (Belanger, 1998). Similarly, while the new conception did not originate with university leadership, some university administrators, like Harvard's Derek Bok, embraced it particularly early, and used it to their advantage (Bok, 1981).

Incumbents had some ambivalence to parts of this shift. For example, the American Association of Universities chose not to support the 1980 Bayh–Dole Act, which allowed universities to patent their inventions, because it was unsure whether universities should be involved in patenting (S. Steinbach, interview by author, March 31, 2005). But in general, the new settlement did not take hold when challengers displaced incumbents, but when incumbents accepted a new role for academic science because doing so was preferable to allowing the unsettled period to continue.

Effects on Other Strategic Action Fields

This new settlement was institutionalized in a variety of ways. One was through laws: the Bayh–Dole Act, for example, allowed universities to patent their inventions and encouraged them to do so, based on the assumption that providing the incentive of patent rights would move scientific inventions into the marketplace more quickly (Berman, 2008). Another was through funding patterns. NSF, as just mentioned, began to support university–industry research in the late 1970s but also expanded its support for engineering dramatically during the 1980s (Belanger, 1998). Individual states also began to support university–industry collaboration with the goal of driving regional economic development (Berman, 2012a, pp. 135–139). Yet another vehicle for institutionalization was universities' acceptance of new kinds of activities, like faculty entrepreneurship and industry collaboration. While such practices were once actively discouraged in academia, universities came to value their role in connecting academic research with commercial applications, in no small part because policymakers were calling for more such connections (Berman, 2012a, pp. 154–156).

But the new settlement in the field of US academic science also had external effects. It created new fields: the expansion of technology transfer activities within universities, for example, gave birth to a field of professionals devoted to managing such activities and who had an interest in promoting them. The creation of state programs that supported university–industry collaboration with the hope of achieving economic development spawned strategic action fields organized around the question of whom such programs would fund, and to what ends. One might even credit the new settlement with launching the entire biotech industry.

The new settlement also, as F&M would predict, disrupted adjacent fields to a certain extent. For example, because academic science was moving toward closer relations with industry, the young biotech field tended to blur the boundaries between the two. As a result, biotech retained many of the values of academic culture, such as relative openness and a flatter organizational hierarchy (Smith-Doerr, 2004). This shift in values in turn seeped into overlapping fields like the pharmaceutical industry, contributing to what Kleinman and Vallas (2001) have called the "asymmetric convergence" of the cultures of university and industry science.

The cycle of disruption and settlement that took place in US academic science between the late 1960s and the early 1980s did not result from a rupture as sharp or as widespread as the one seen in the field of US racial politics or the US mortgage market, the cases that F&M use to illustrate their approach. Nor did the cast of players change so radically. But the basic dynamics they identify – of external shocks disrupting the old settlement, of a period of crisis and contention, and of a new settlement emerging and bringing relative stability to the field – can clearly be seen at work.

Nevertheless, there are aspects of this transformation that F&M's framework overlooks, or cannot account for. Specifically, the new settlement in

US academic science cannot be explained as the result of challengers taking advantage of a period of crisis to remake the field's rules in ways that favored themselves. It also suggests a better-defined transition from crisis to settlement, and a clearer resolution of contention, than actually took place in this case. In the section that follows, I return to my own field-based analysis of this case, using it to highlight these other parts of the story, and to sketch another way of thinking about the causes of change.

EVOLVING INSTITUTIONAL LOGICS AND PRACTICES IN THE FIELD OF US ACADEMIC SCIENCE

F&M believe that field-level change typically happens when a group of challengers takes advantage of a period of turmoil, itself usually caused by events external to the field, to promote a new conception of the field and new rules governing it that will favor themselves over the previous incumbents. But while some groups tried to take advantage of turmoil in US academic science to forward their own interests in specific ways, they did not try to reshape the field as a whole. Nevertheless, the field *was* ultimately reshaped. How, then, do we explain this change, if it was not the result of strategic action by challengers?

I approach this question by focusing on the coevolution of institutional logics, local practices, and the field's larger environment. From this perspective, the central change in US academic science is the growing influence of the institutional logic of the market, which sees science in terms of its economic value, relative to the institutional logic of science, which sees knowledge as important for its own sake. I assume that these logics coexist but are instantiated in specific practices which may be more or less common or successful in a particular field at a particular moment.

Focusing on the prevalence of competing logics as instantiated in practices provides another way to think about field-level change in cases where there is no obvious project to promote a new conception of the field or new rules governing it. The previous section indicated that a new conception of the field of US academic science came from the policy field. But that narrative is still blurry around the question of how policymakers' beliefs about the economic importance of science and technology actually impact the field of academic science. In the rest of this section, I renarrate this story using the language of logics and practices, which allows us to bring this process into clearer focus.

The Era of Science Logic and its Close

In the field of US academic science, multiple logics have long been visible. These include market logic, which sees the purpose of science in its ability to create things that have economic value in the marketplace; the logic of science, which values the production of knowledge for its own sake; and the logic of the state, which sees science's worth in the service it can potentially provide to the government.

It is possible to identify some practices based on any of these logics at any moment during the last century. However, science logic was relatively dominant during the period of stability and expansion sketched in the last section as the "postwar settlement." The logic of the state was also quite visible during this period, but practices based on the logic of the market could be seen as well (Berman, 2012a, pp. 26–29). Nevertheless, the prominence of science logic meant that scientists had a great deal of autonomy in setting their own research agenda and priorities during this period, and could count on government support to expand from year to year.

Faculty who participated in science-logic practices, like seeking out grants to support curiosity-driven research, found the environment very favorable. Funding was rapidly increasing at a variety of federal agencies, and even the more applied agencies were quite friendly to basic research. The atmosphere was supportive for universities that wanted to expand their PhD programs, for example, or to reduce teaching loads so that faculty could devote more time to self-directed research. Both cultural attitudes and the resource environment made science-logic practices easy to reproduce and to expand.

At the same time, market-logic practices, including research parks, industrial affiliates programs, and industrial extension offices, were all visible and spread to a certain extent. But while they did not, in fact, experience the cultural resistance one might expect in an environment dominated by the logic of science, they did suffer from a scarcity of resources that could potentially support them, and such practices did not thrive (Berman, 2012a, pp. 23–35).

As already discussed, however, by the late 1960s this period was coming to a close as the deal between universities and the state that it rested on became increasingly untenable. By the early 1970s, it was harder to find resources for science-logic practices. Observers were declaring a "recession" in academic science, and the path out of that recession was unclear (Boffey, 1970; Semas, 1973).

The Policy Field Changes the Resource and Regulatory Environment

In the late 1970s, the resource and regulatory environment of academic science would change in ways that would start to favor market-logic practices instead. This happened, as suggested in the previous section, when policymakers, encouraged by representatives of large R&D-intensive companies, came to embrace the idea that technological innovation was critically important to the economy. But while this argument gained bipartisan popularity during a time of economic malaise, what policymakers should *do* to encourage innovation was less clear. Many policy possibilities were loosely compatible with the idea of improving innovation, and people of varying political stripes proposed changes ranging from the highly interventionist to the strongly free-market. By late 1979, "seventy-five or eighty" bills purporting to improve technological innovation were under consideration by Congress (Innovation, 1979).

The popularity of the innovation idea did not result in a coherent new policy direction. But it did give a boost to a variety of proposals from across the political spectrum that could somehow be argued to strengthen technological innovation. These ranged from decisions about what court should adjudicate patent lawsuits, to what the tax rate should be on capital gains, to whether pension funds could invest in venture capital (Berman, 2012a, pp. 159–161; 2012b, pp. 278–285). Some of these, like the Bayh–Dole Act, which allowed universities to patent government-funded research, directly targeted universities. But most did not. Nevertheless, by encouraging activities that treated scientific research in terms of its economic value, their collective effect was to change the broader environment in ways that encouraged the growth of market-logic practices in academic science.

Market-Logic Practices Spread in US Academic Science

In this new environment, such practices began to thrive. Activities like faculty entrepreneurship in the biosciences and the patenting of university research became common, and organizational forms like the university–industry research center diffused. While each of these practices predated policymakers' interest in encouraging technological innovation, they had, in the past, encountered significant barriers that limited the extent of their spread. Would-be biotech entrepreneurs lacked venture capital, patenting of government-funded research was restricted, and little support was available for university–industry collaborations (Berman, 2012b, pp. 273–277).

But the changed policy environment removed those barriers. For example, while breakthroughs in recombinant DNA technology took place in 1973, and the first modern biotech company, Genentech, was founded in 1976 by a UCSF professor and a venture capitalist, scientists had started only a few small biotech firms by 1978. Despite the obvious promise of the technology, such entrepreneurship was restricted: less by the cultural objections to faculty entrepreneurship, though those did exist, than by impending legislation that would strictly regulate recombinant DNA research for safety reasons, and by an abysmal venture capital environment (Berman, 2012a, pp. 62–69).

Those limitations were removed, however, by a variety of policy decisions, each of which was shaped by the new attitude toward innovation that was sweeping Washington in the late 1970s. Proposals to restrict rDNA research, which were seen as "inevitable" in early 1977 (Academy Opposes, 1977), were removed from the table as the debate was reframed around the question of whether such regulation would stifle an emerging high-tech industry (Berman, 2012a, pp. 62–69). The venture capital environment was transformed by new rules that allowed pension funds, which controlled \$300 billion of capital, to invest some of that into new ventures (Board of Governors, 2007, p. 67), and by a large cut in the capital gains tax (the top rate was lowered from 49% to 25%). These decisions, too, were shaped by new arguments about "the necessity of a broad, coordinated approach to improving innovation" – in this case, by providing an environment that was friendlier to risk capital (US Senate, 1979, pp. 1–2).

The result of this change in the resource and regulatory environment was an explosion in the number of faculty founding biotech firms and in the massive growth of the small handful that had already been established. By mid-1980, the four oldest startups had a paper value of an astonishing \$500 million (Wade, 1980), and after Genentech's late-1980 IPO, its cofounder, UCSF biochemist Herbert Boyer, was worth \$65 million (Cole, 1980). By the end of 1981, at least 80 biotech startups had been created (US Congress, 1984, p. 93), with each firm typically involving academic scientists not only as cofounders but as members of scientific advisory boards and consultants (Kenney, 1986).

The case of biotech entrepreneurship is illustrative of how policymakers' new ideas about the economic impact of technology innovation changed the environment of academic science in ways that encouraged market-logic practices. But it is not the only example. A similar pattern can be seen in the spread of university patenting and in the takeoff of university–industry research centers (Berman, 2012a, pp. 94–145). And even market-logic practices that had existed since the 1950s and 1960s but limped along for lack of adequate resources, like research parks and industrial affiliates programs, found themselves rejuvenated by the new appreciation for science that could demonstrate an economic impact (Berman, 2012a, pp. 150–151).

As restrictions on market-logic practices – both legal and cultural – declined, and as more resources became available for science done with the purpose of realizing economic value, market logic became both more visible and more widely used in the field of academic science. While science-logic practices were still quite common, as were those based on the logic of the state, their relative influence declined as it became more challenging to find the resources to support them. The multiple logics would continue to coexist and compete as they had in the past, so calling the shift one from settlement, through crisis, to new settlement seems to overstate the extent of the change. Nevertheless, by the end of this period a new balance among these multiple logics, one that would prove relatively stable, was established and persists to the present.

TOWARD A (MORE) UNIFIED FIELD THEORY?

Testing the ability of F&M's framework to explain a familiar empirical case demonstrates both its value and its limits. Doing so in conversation with an explanation based on the coevolution of institutional logics, practices, and environments can help address the blind spots of each approach.

F&M's theory brings additional value to organizational explanations of field change. Their attention to the relationship between fields, which goes beyond most of organizational field theory, is particularly welcome. If one accepts F&M's argument that the relationship between fields is the most common source of change, as a crisis in one field ripples out into all the others it is linked to (F&M, 2012, p. 19), directly addressing that relationship is critically important. Their framework also returns much-needed attention to power and resources. While power has always been central to Bourdieusian field theory, it too often disappears from organizational field theory, with its cognitive-cultural focus.

The case of US academic science, though, also points to some limits to F&M's explanatory framework. In particular, they focus on the relationship between challengers and incumbents as the proximate cause of field-level change. While they do not say that field-level change *must* result from challengers trying to impose a new settlement on a disrupted field, both cases that *A Theory of Fields* examines in detail reflect such a process. In the field of US racial politics, the civil rights movement was promoting a new vision of legal racial equality that eliminated segregation. In the field of the US mortgage market, the government-sponsored enterprises (GSEs) Fannie Mae and Freddie Mac were trying to encourage a reconceptualization of the mortgage market that was based on securitization (F&M, 2012, pp. 114–163). But in academic science, change did not result from such a strategic, intentional, field-level project, and F&M do not tell us how to explain change in such cases.

F&M's focus on cases of fairly dramatic field transformation – on the transition from settlement, through crisis, back to settlement – also tends to downplay the extent to which the ongoing contestation within fields can lead to evolutionary but meaningful change. Here, there is a tension within *A Theory of Fields*. On the one hand, the authors clearly want to emphasize the constant jockeying and politicking that go on within any field. On the other, they write about very dramatic cases of field-level change and do not tackle the dynamics of this ongoing, more-or-less routine struggle for advantage head on. The changes in academic science were not nearly so dramatic as those in the field of US racial politics or the mortgage market, nor were the boundaries between "old settlement," "crisis," and "new settlement" very clear. Yet shift within the field was nevertheless important, and merits explanation.

Focusing on the coevolution of logics, practices, and environment, though not without limits of its own, is one way to sidestep these issues. It is less a competing explanation for field-level change than a way to explain a different type of field-level change. In fact, I actually began my study of the changes in US academic science looking for a challenger analogous to the ones F&M describe, and it was my failure to find one that led me to other explanatory strategies (see Berman, 2012a, pp. 8–12, for a discussion). My inability to locate field-level challengers caused me to look for other factors that encouraged the development of practices associated with the new regime. What I found was that a diverse group of policy decisions was important in encouraging practices like patenting and faculty entrepreneurship, but they were all shaped by policymakers' new level of concern with the economic impact of technological innovation.

Working backward, I discovered that this concern had increased due to the efforts of representatives of large R&D-intensive companies. Those efforts changed the policy environment in ways that encouraged policies that tried to use science and technology to drive the economy. This, in turn, shifted the resource and regulatory environment of academic science in ways that allowed the bottom-up growth of small, local innovations that leveraged science for explicitly economic purposes.

Thus change that was driven by factors external to the field of academic science – resulting from a project initiated by, and aimed at, members of a different field – affected the environment in ways that facilitated the bottom-up evolution of academic science. But because the change was not intended, F&M have no way to address it and cannot help in locating its source.

This actually helps to answer a question that F&M clearly think is important: how ripples from a strategic action project in one field can contribute to the transformation of another. Field-level change is not only the result of challengers' efforts to change fields. It can also result from the collective effects of local innovations in the context of a changing environment. F&M's incumbent/challenger explanation of field-level change effectively describes one type of transformation, as their empirical cases illustrate. But US academic science suggests another type: cumulative, bottom-up change in practices resulting from the intersection of local innovations and shifts in the field's larger environment. The question of which path is more common, or under what circumstances each one can be found, is largely empirical.

I suspect that the dynamic visible in US academic science may be relatively common. For example, F&M address the transformation of the US mortgage market, but consider a slightly speculative account of the transformation of the field of US student loans. Here, too, what was once a small, stable market for government-backed loans has become a gigantic industry with public and private branches organized, like the mortgage market, around securitization. To the best of my knowledge, nobody set out to transform the field of student loans in the same way that Fannie Mae set out to transform the mortgage market. Yet the field of student loans also suffered from the same external disruptions, in the form of stagflation and a weak economy, as the mortgage market did in the late 1970s and early 1980s.

If the GSEs and investment banks were inventing securitization and related practices like tranching in the nearby field of the US mortgage market, and if they were trying to change policy to remove limits on securitization, then the changes could also have affected conditions for actors in the field of student loans. Actors in the student loan field, probably also experimenting with securitization, would have found that legal barriers had been removed. Their experiments, then, would have been more successful and could have led to a new settlement in the field of US student loans, even if no one actively sought to restructure that field. This account is hypothetical but suggests how the dynamics visible in academic science might also be visible elsewhere.

Thus greater attention to the coevolution of logics, practices, and environments can help us to understand a broader spectrum of field-level changes. This suggests in particular three topics worth exploring more fully as part of an effort to flesh out the connections between a logics-and-practices approach and F&M's theory of fields.

First, we should better specify *how* change in one field is tied to change in a subfield or adjacent field. While I have criticized F&M's overemphasis on strategic action as an explanation for change, I perhaps underemphasize it in a particular way. Though I explain field-level change as the cumulative result of the expansion of local practices, I do not pay much attention to how the originators of those practices were themselves acting strategically in subfields, or attempting to create new fields. While these innovators were not trying to transform academic science as a whole, they did have strategic goals: creating a subfield of university technology transfer, establishing a field of biotechnology firms. It would be useful to think more explicitly about how strategic action aimed at changing a subfield, or establishing a new field, can affect a superordinate field.

Second, we should identify the conditions under which one sees a challenger-incumbent path to change versus change resulting from the spread of new practices. The change in academic science, for example, did not involve a complete restructuring of the field or the replacement of its major players. Might bottom-up evolution, then, be associated with more moderate episodes of change, while more dramatic transformations result from intentional projects to reshape a field? Could bottom-up change typically be an indirect effect of an intentional project in an adjacent or superordinate field? More broadly, are there completely different paths to field transformation not captured by either of these two dynamics?

Finally, we should think about how challenger-incumbent and bottom-up change might coexist and interact. In academic science, challenger-incumbent dynamics did not to explain why the field changed as a whole. But sometimes bottom-up evolution can coexist with projects aimed at changing the field. For example, in the case of US racial politics, F&M note how the mid-1950s rupture in the national field both encouraged the Montgomery bus boycott and also gave it great importance. Doubtless the civil rights leaders who organized the boycott wanted change in the national field. But they were acting much more locally, though in ways that had broad impact because of the field's larger environment (F&M, 2012, p. 127).

In conclusion, field theory is an approach with great potential for the political sociology of science, and F&M's version of it is a real contribution, particularly by virtue of its focus on the relationship between fields and its insistence on attention to power as well as culture. But as US academic science shows, its explanation of how fields change cannot be applied to every empirical case. While F&M have some valid critiques of institutionalism-based organizational field theory, recent work along these lines addresses the complexity and multiplicity of the logics that organize fields while also offering space to address power and resources. Drawing on the strengths of both approaches has the potential to produce something more generative for the political sociology of science, and for sociology more generally, than either approach on its own.

NOTES

1. I am bracketing F&M's explanation for the emergence of new fields, although that explanation is a significant part of their larger project.
2. Internal governance units are organizations "charged with overseeing compliance with field rules" (F&M, 2012, pp. 13–14) and serve as the liaison between one strategic action field and another, particularly the state (p. 78).
3. The Association of University Technology Managers, or AUTM, represents professionals who deal with universities' intellectual property, spinoff companies, and other technology transfer activities.

ACKNOWLEDGMENTS

Many thanks to Mathieu Albert, Neil Fligstein, Scott Frickel, David J. Hess, Dan Hirschman, and Abby Kinchy for their useful feedback and suggestions, and to participants in the June 2012 Political Sociology of Science Workshop at the University of Wisconsin for providing the impetus to write this paper. This work was supported in part by the Richard B. Fisher Membership of the Institute for Advanced Study.

REFERENCES

Academy opposes local DNA research rules. (1977). *Science & Government Report*, May 15, p. 8.

Albert, M., & Kleinman, D. L. (2011). Bringing Pierre Bourdieu to science and technology studies. *Minerva*, *49*(3), 263–273.

Albert, M., & McGuire, W. (2014). Understanding change in academic knowledge production in a neoliberal era. In S. Frickel & D. J. Hess (Eds.), *Fields of knowledge: Science, politics and publics in the neoliberal age* (Vol. 27, pp. 33–57). Political Power and Social Theory. Bingley, UK: Emerald Group Publishing Limited.

Belanger, D. O. (1998). *Enabling American innovation: Engineering and the National Science Foundation*. West Lafayette, IN: Purdue University Press.

Berman, E. P. (2008). Why did universities start patenting? Institution-building and the road to the Bayh–Dole Act. *Social Studies of Science*, *38*(6), 835–871.

Berman, E. P. (2012a). *Creating the market university: How academic science became an economic engine*. Princeton, NJ: Princeton University Press.

Berman, E. P. (2012b). Explaining the move toward the market in academic science: How institutional logics can change without institutional entrepreneurs. *Theory and Society*, *41*(3), 261–299.

Board of Governors of the Federal Reserve System. (2007). *Flow of funds accounts of the United States: Annual flows and outstandings*, 1975–1984. Washington, DC: Federal Reserve.

Boffey, P. M. (1968). Budget paradox: Spending holds even, yet researchers are hurt. *Science*, *162*(3851), 340–342.

Boffey, P. M. (1969). Campus unrest: Riots bring danger of punitive backlash. *Science*, *164*(3876), 161–165.

Boffey, P. M. (1970). Recession in science: Ex-advisors warn of long-term effects. *Science*, *168*(3931), 555–557.

Bok, D. (1981). Business and the academy. *Harvard Magazine*, May–June, pp. 23–35.

Bourdieu, P. (1981 [1975]). The specificity of the scientific field and the social conditions of the progress of reason. In C. Lemert (Ed.), *French sociology: Rupture and renewal since 1968* (pp. 257–292). New York, NY: Columbia University Press.

Bourdieu, P. (1988). *Homo academicus*. Stanford, CA: Stanford University Press.

Bourdieu, P. (1995). *The rules of art: Genesis and structure of the literary field*. Stanford, CA: Stanford University Press.

Bourdieu, P. (2000). *Pascalian meditations*. Stanford, CA: Stanford University Press.

Bourdieu, P. (2004). *Science of science and reflexivity*. Chicago, IL: University of Chicago Press.

Bourdieu, P., & Wacquant, L. J. D. (1992). *An invitation to reflexive sociology*. Chicago, IL: University of Chicago Press.

Boyer, H. W. (1994). Recombinant DNA research at UCSF and commercial application at Genentech. In S. S. Hughes (Ed.), *Oral history conducted by S. S. Hughes*. Berkeley, CA: Regional Oral History Office, The Bancroft Library, University of California.

Camic, C. (2011). Bourdieu's cleft sociology of science. *Minerva*, *49*(3), 275–293.

Cole, R. J. (1980). Genentech, new issue, up sharply. *New York Times*, October 15, p. D1.

Cooper, M. H. (2009). Commercialization of the university and problem choice by academic biological scientists. *Science, Technology & Human Values*, *34*(5), 629–653.

DiMaggio, P. J., & Powell, W. W. (1983). The iron cage revisited: Institutional isomorphism and collective rationality in organizational fields. *American Sociological Review*, *48*(2), 147–160.

Dobbin, F. (2008). The poverty of organizational theory: Comment on: 'Bourdieu and organizational analysis'. *Theory and Society*, *37*(1), 53–63.

Emirbayer, M., & Johnson, V. (2008). Bourdieu and organizational analysis. *Theory and Society*, *37*(1), 1–44.

Endicott, K. M., & Allen, E. M. (1953). The growth of medical research 1941–1953 and the role of public health service research grants. *Science*, *118*(3065), 337–343.

Fligstein, N., & McAdam, D. (2011). Toward a general theory of strategic action fields. *Sociological Theory*, *29*(1), 1–26.

Fligstein, N., & McAdam, D. (2012). *A theory of fields*. Oxford, UK: Oxford University Press.

Frickel, S., & Moore, K. (Eds.). (2006). *The new political sociology of science: Institutions, networks, power*. Madison, WI: University of Wisconsin Press.

Friedland, R., & Alford, R. R. (1991). Bringing society back in: Symbols, practices, and institutional contradictions. In W. W. Powell & J. P. DiMaggio (Eds.), *The new institutionalism in organizational analysis* (pp. 232–263). Chicago, IL: University of Chicago Press.

Geiger, R. L. (1993). *Research and relevant knowledge: American research universities since World War II*. New York, NY: Oxford University Press.

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P., & Trow, M. (1994). *The new production of knowledge: The dynamics of science and research in contemporary societies*. London: Sage.

Greenberg, D. S. (1967). LBJ's budget: Lean fare set forth for research and development. *Science*, *155*(3761), 434–435.

Greenwood, R., Díaz, A. M., Li, S. X., & Lorente, J. C. (2010). The multiplicity of institutional logics and the heterogeneity of organizational responses. *Organization Science*, *21*(2), 521–539.

Greenwood, R., Raynard, M., Kodeih, F., Micelotta, E. R., & Lounsbury, M. (2011). Institutional complexity and organizational responses. *Academy of Management Annals*, *5*(1), 317–371.

Greenwood, R., Suddaby, R., & Hinings, C. R. (2002). Theorizing change: The role of professional associations in the transformation of institutionalized fields. *Academy of Management Journal*, *45*(1), 58–80.

Guston, D. H. (2000). *Between politics and science: Assuring the integrity and productivity of research*. Cambridge, MA: MIT Press.

Hardy, C., & Maguire, S. (2008). Institutional entrepreneurship. In R. Greenwood, C. Oliver, R. Suddaby, & K. Sahlin-Andersson (Eds.), *The Sage handbook of organizational institutionalism* (pp. 198–217). Thousand Oaks, CA: Sage.

Hess, D. J. (2012). *Good green jobs in a global economy: Making and keeping new industries in the United States*. Cambridge, MA: MIT Press.

House, Senate Split on authorization for NSF. (1977). Science & Government Report, May 15, p. 5.

Innovation: Some positive steps. (1979). *Business Week*, November 26, p. 96.

Kenney, M. (1986). *Biotechnology: The university-industrial complex*. New Haven, CT: Yale University Press.

Kevles, D. J. (1978). *The physicists: The history of scientific community in modern America*. Cambridge, MA: Harvard University Press.

Kleinman, D. L. (1995). *Politics on the endless frontier: Postwar research policy in the United States*. Durham, NC: Duke University Press.

Kleinman, D. L., & Vallas, S. P. (2001). Science, capitalism, and the rise of the "knowledge worker": The changing structure of knowledge production in the United States. *Theory and Society*, *30*(4), 451–492.

Lave, R. (2012). *Fields and streams: Stream restoration, neoliberalism, and the future of environmental science*. Athens, GA: University of Georgia Press.

Lave, R., Mirowski, P., & Randalls, S. (2010). Introduction: STS and neoliberal science. *Social Studies of Science*, *40*(5), 659–675.

Leblebici, H., Salancik, G. R., Copay, A., & King, T. (1991). Institutional change and the transformation of interorganizational fields: An organizational history of the US radio broadcasting industry. *Administrative Science Quarterly*, *36*(3), 333–363.

Leydesdorff, L., & Etzkowitz, H. (1996). Emergence of a triple helix of university-industry-government relations. *Science and Public Policy*, *23*(5), 279–286.

Lounsbury, M., & Crumley, E. T. (2007). New practice creation: An institutional perspective on innovation. *Organization Studies*, *28*(7), 993–1012.

McGuire, W. L. (2011). *Constructing quality in academic science: How basic scientists respond to Canadian market-oriented science policy – A Bourdieusian approac*h. Ph.D. dissertation, School of Public Health, University of Toronto.

Mohr, J. W. (2005). *Implicit terrains: Meaning, measurement, and spatial metaphors in organizational theory*. Santa Barbara, CA: Department of Sociology, University of California. Retrieved from http://www.soc.ucsb.edu/ct/pages/JWM/Papers/ImplictTerrains.htm. Accessed on February 12, 2014.

Moore, K. (2008). *Disrupting science: Social movements, American scientists, and the politics of the military, 1945–1975*. Princeton, NJ: Princeton University Press.

National Science Board. (2010). *Science and engineering indicators 2010*. Arlington, VA: National Science Foundation.

National Science Board. (2012). *Science and engineering indicators 2012*. Arlington, VA: National Science Foundation.

National Science Foundation. (2013). *FY 2012 performance and financial highlights*. Arlington, VA: National Science Foundation.

National Science Foundation, Division of Science Resources Statistics. (2003). *Federal funds for research and development, detailed historical tables: Fiscal years 1951–2002*. Arlington, VA: National Science Foundation.

Owen-Smith, J. (2011). The institutionalization of expertise in university licensing. *Theory and Society*, *40*(1), 63–94.

Purdy, J. M., & Gray, B. (2009). Conflicting logics, mechanisms of diffusion, and multilevel dynamics in emerging institutional fields. *Academy of Management Journal*, *52*(2), 355–380.

Richard J. Barber Associates, Inc. (1975). *The advanced research projects agency, 1958–1974*. Springfield, VA: Defense Technical Information Center.

Schneiberg, M., & Lounsbury, M. (2008). Social movements and institutional analysis. In R. Greenwood, C. Oliver, R. Suddaby, & K. Sahlin-Andersson (Eds.), *The Sage handbook of organizational institutionalism* (pp. 648–670). Thousand Oaks, CA: Sage.

Semas, P. W. (1973). "Deep recession" is seen hitting basic research. *Chronicle of Higher Education*, January 8, p. 1.

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

Smets, M., Morris, T., & Greenwood, R. (2012). From practice to field: A multi-level model of practice-driven institutional change. *Academy of Management Journal*, *55*(4), 877–904.

Smith, B. L., & Karlesky, J. J. (1977). *The state of academic science: The universities in the nation's research effort*. New York, NY: Change Magazine Press.

Smith-Doerr, L. (2004). *Women's work: Gender equality vs. hierarchy in the life sciences*. Boulder, CO: Lynne Rienner Publishers.

Swan, J., Bresnen, M., Robertson, M., Newell, S., & Dopson, S. (2010). When policy meets practice: Colliding logics and the challenges of "Mode 2" initiatives in the translation of academic knowledge. *Organization Studies*, *31*(9–10), 1311–1340.

Swazey, J. P., & Reeds, K. (1978). *Today's medicine, tomorrow's science: Essays on paths of discovery in the biomedical sciences*. Washington, DC: US Department of Health, Education, and Welfare.

Thornton, P. H., Ocasio, W., & Lounsbury, M. (2012). *The institutional logics perspective: A new approach to culture, structure, and process*. Oxford, UK: Oxford University Press.

US Congress, Office of Technology Assessment. (1984). *Commercial biotechnology: An international analysis*. New York, NY: Pergamon Press.

US House, Committee on Science and Technology, Subcommittee on Science, Research, and Technology. (1978). *1979 National Science Foundation Authorization*. Washington, DC: US Government Printing Office.

US Senate, Committee on Government Operations, Subcommittee on Government Research. (1967). *Research in the service of man: Hearings*. Washington, DC: US Government Printing Office.

US Senate, Select Committee on Small Business, Subcommittee on Antitrust, Consumers, and Employment. (1979). *Small business and innovation*. Washington, DC: US Government Printing Office.

Vaughan, D. (2008). Bourdieu and organizations: The empirical challenge. *Theory and Society*, *37*(1), 65–81.

Wade, N. (1980). Cloning gold rush turns basic biology into big business. *Science*, *208*(4445), 688–692.

Walsh, J. (1966). NIH: Demand increases for applications of research. *Science*, *153*(3732), 149–152.

Walsh, J. (1969). Campus unrest: Congress ponders federal sanctions on universities. *Science*, *165*(3888), 46–49.