The Dynamics of Research Fields

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The concept of a research field can refer to anything from a vast discipline such as biology to a small network of researchers who follow and evaluate each other's research. The study of research fields is continuous with the constructivist project of the problem of how knowledge is shaped through social negotiation, but it tends to look at a slightly higher scale of analysis in contrast with the focus on how specific knowledge claims are advanced, debated, and accepted or rejected. Derek de Solla Price (1963) extended Robert Boyle's seventeenth-century term "invisible college" to refer to informal groups of scientists who work on related problems with shared methods. The concept of invisible colleges was important for STS research in subsequent decades because it was a forerunner of research on networks. Likewise, attention to personal networks was also crucial for a group of "specialty studies" that tracked the formation of new disciplines.[1]

1. Crane referred to invisible colleges as "communication networks." See Crane 1972, Chubin 1983.

In one of the earliest studies of specialty formation, Joseph Ben-David argued that the relations between academic and applied research fields were important in the development of new research fields. In a study of bacteriology and psychoanalysis, he found that preliminary discoveries were made by practitioners, and initially professional researchers rejected their findings. However, when academic scientists moved into the applied context, adopted the new problems, and used generally accepted methods, they established new disciplines. As a result, he hypothesized that the interaction between applied and academic research fields was one factor that contributed to the formation of new research fields.[2]

In another study, Ben-David and Randall Collins explored the origins of experimental psychology, and they presented another set of factors that contributed to specialty formation known as role hybridization. They argued that when opportunities are reduced in a high-status field, scientists may move into a lower-status field. When the old research field is a higher-status field, the migrants may be unwilling to give up the identity of the old research field. Consequently, they experience role conflict, which they can resolve by importing the methods and techniques of the old research field into the new one. In the case of the origins of experimental psychology, higher-status scientists (physiologists) left their home discipline to colonize a less competitive and lower-status field (philosophy) that had more opportunities. Their cross-disciplinary migration resulted in the formation of a new field (experimental psychology). Ben-David and Collins also identified the three stages of specialty formation: forerunners, founders, and followers. They distinguished role hybridization from idea

2. Ben-David 1960, Edge and Mulkay 1976.

hybridization, in which ideas from different fields are brought together, but there is no new academic or professional role.[3]

Nicholas Mullins built on the Ben-David and Collins model but modified it in significant ways in his case studies of molecular biology and subfields in American sociology. In the case of molecular biology, there was an initial migration of physicists into biology during the 1930s, but they did so for reasons of intellectual challenge rather than because of blocked career opportunities. Furthermore, although the capacity to generate students was important in Mullins's work as well, he did not track the specialty formation entirely in terms of "descent" relations between founders and their students and followers. Instead, he developed an alternative, four-stage model of specialty group formation that included paradigm groups, the communication network, cluster, and finally the specialty or new discipline. In the case of molecular biology, the first stage involved a loose group of scientists who were interested in studying phage (viruses that kill bacteria) as a way of solving the problem of genetic information transmission. At the communication network stage, there were increased connections among the scientists working on the problem and a corresponding decrease in independent scientists. At the cluster stage, the scientists became more self-conscious about their patterns of communication, and they tended to spend time together in places such as Cold Spring Harbor. A shared lifestyle among mentors and students, such as camping trips led by Max Delbrück at California Institute of Technology, helped foster group solidarity. At the specialty stage, the field emerged as a recognized discipline, with meetings, journals, training institutions, and a formal organization.[4]

In a subsequent study of radio astronomy, David Edge and Michael Mulkay argued that the underlying pattern linking the various studies of specialty formation is the exchange of ideas

3. Ben-David 1991, Ben-David and Collins 1966.
4. Mullins 1972, 1973a, 1973b.

and scientists across different research fields. In some cases, an applied scientist might make a discovery that was significant for a scholarly research field, and the academic research field would then investigate and develop the discovery. In other cases, innovation occurred due to the migration of scientists across research fields, either from one academic research field to another or from an academic to an applied setting. However, Edge and Mulkay also found some significant differences between their case of radio astronomy and the role hybridization model of Ben-David and Collins. The identity of radio astronomers was much more flexible and variable than the psychologists whom Ben-David and Collins studied, and there was no attempt to create a new role or to import methods from other fields into the emergent field of radio astronomy for purposes of prestige or identity with a higher-status research field. Instead, the field of radio astronomy was quite respectable, and there was an ongoing exchange of theories and methods with physics. Furthermore, the structural conditions that guided the formation of the specialty network did not include limited career opportunities.[5]

Edge and Mulkay also found differences between the case of radio astronomy and the phage group studied by Mullins. Whereas the phage network was controversial within biology, and recruitment of students was difficult, there were no such barriers for radio astronomy, and the growth of the research field was rapid and continuous. Radio astronomy also went through a different sequence of stages, and, building on the work of John Law, they suggested that one possible explanation for the differences in stages may be the type of research field (theory-based, problem-based, and technique-based).[6]

After the blossoming of specialty studies during the 1960s and 1970s, research interests shifted to other topics. The highly variable histories of each research field suggested that it was

5. Edge and Mulkay 1976.
6. Edge and Mulkay 1976, Law 1973.

unlikely that a general model would be developed to cover all histories of the formation of new research fields in any detail. However, work on related topics has continued. Research subsequent to the specialty studies has tended to emphasize the relations with institutional factors outside the scientific field, a problem area that connects well with the study of the commercialization of science. The increasing emphasis on research that has commercial applications has led to the creation of many new research fields, such as biotechnology and information technology. Changes in the funding patterns for all research, not just university-based research, have not only reshuffled prestige hierarchies among scientific research fields but also led to the creation of new research fields.

Furthermore, the commercialization of science and the growing complexity of policy problems (such as climate change) have increasingly led to the development of interdisciplinary research fields and collaborations, and a literature has emerged on interdisciplinary scientific research. Whereas traditional scientific disciplines are generally closely associated with university-based research departments and are characterized by a problem area that does not necessarily have immediate applications (such as the basic sciences), interdisciplinary fields tend to be located in institutes and programs, and they often are more oriented toward problem areas with commercial, military, or policy applications. Because the interdisciplinary centers often emerge in response to new priorities in extramural funding, they may have access to higher levels of funding than traditional, disciplinary funding, but they are also more contingent on extramural funding and more vulnerable to assaults on professional autonomy. Furthermore, the interdisciplinary fields tend not to have graduate programs with doctoral degrees, which was a central characteristic of discipline formation in the Mullins model, and instead they recruit students from other fields. Stephen Turner has suggested that the existence of doctoral degrees and departments that hire students with those degrees (an "internal market" for doctoral degrees) is a central feature of disciplinary power. Hence, interdisciplinary fields are a somewhat different beast from disciplines in formation.[7]

Field Theory

One of the most influential approaches to the study of the dynamics of research fields is Pierre Bourdieu's field theory. It has become broadly influential in other subfields of sociology, but it has not acquired equivalent influence in STS. Pierre Bourdieu's 1975 essay, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," influenced the emerging sociologies of scientific knowledge, but over time Bourdieu's influence in the sociology of scientific knowledge waned. One explanation is that he did not remain consistently involved in science studies; instead, his major research projects during the 1980s and 1990s focused on the French educational system. Shortly before his death in 2003, he published on the sociology of science, but at that point he felt both misappropriated and misunderstood. This section therefore engages in a recuperative project of exploring what Bourdieu offered for science studies.

In the article mentioned above, Bourdieu described the scientific field as a system of relations of conflict and alliance among agents who struggle to accrue a field-specific form of capital. The agents of the scientific field share a particular interest in the monopoly of scientific authority, that is, the social power to speak legitimately on matters of scientific fact and expertise. In this sense, scientists have a collective interest in defending the field's relative autonomy from extrafield influence. But Bourdieu also argued that at an individual level of

7. Frickel 2004a, Turner 2000, Weingart and Stehr 2000.

scientists or laboratories, the goal is to accumulate symbolic capital, which in the scientific field takes the form of recognition by a scientist's competitor-peers. It is accumulated by investing in problems that are recognized as important by the dominant agents in the field. Although symbolic capital in the scientific field (or scientific capital) can only be bestowed by other scientists, it can be converted into other forms of capital.

Up to this point, the analysis is fairly standard in STS, linking together work on the importance of the quasi-autonomy of the scientific field with the functionalist insights into the reward system, but utilizing a conceptual framework that enables the important insight of the convertibility of different types of capital that was missing in the functionalist accounts of Merton, Hagstrom, and colleagues. However, Bourdieu also drew out an important implication for the study of the accumulation of capital and power in science: those who achieve a dominant position in the field can then impose a definition of the field (its problems, methods, and theories) on the other agents in the field. Thus, part of what is at stake in the struggle to accumulate the symbolic capital of scientific authority is the capacity to impose a consensus on the field and to represent that consensus outside that field. In contrast with functionalist accounts of science, Bourdieu therefore brings questions of power and position into the center of his analytical framework.

By leaving room for the study of the alignments of positions within the scientific field and those outside it, Bourdieu also opened up a way of exploring interfield relations (such as the influence of the economic field on the scientific field). His principal approach to the study of interfield relations involved a cultural analysis of homologies of meaning between fields. The analysis of interfield homologies provides a solution to a problem that was raised by the criticisms of the Edinburgh interests analysis but left unsolved by agent-based sociologies of scientific knowledge. The concept also provides the basis for a higher level asymmetry in the sociology of scientific knowledge in the sense that Bourdieu suggested that sociology can help to identify instances of "misrecognition" of extrafield cultural categories and ideology. As a result, field sociology provides a tool for distinguishing the uses of science as a "justificatory ideology" from more neutral uses.

Interfield homologies do not just arise sui generis; they are transmitted through the habitus, which is similar to the culture concept in American anthropology but focused on fields, social classes, and other social divisions. It is, in Bourdieu's terms, a practical sense or the "sense of the game," the often unconscious knowledge that allows agents to operate competently in a field. But individuals also acquire a trajectory habitus based on their life course, and they carry it with them from one field to another. Bourdieu used the concept of the habitus partly to ward off criticisms from other scholars that his discussion of capital and strategies flattens the scientist into a rational actor who pursues in a mechanistic way the accumulation of scientific and other forms of capital. The concept of habitus also enabled him to hold in dynamic tension the balanced analysis of structure, meaning, and agency that plagues other social theories. Because the structure of the scientific field at any given point in time is an outcome of previous strategies of accumulation, agency and structure are brought together in a dynamic relationship in which structure of the field and the habitus of the agent conditions the possibilities of agents' strategies, but the outcome of the choice of strategies affects the structure of the field and the habitus. The approach is similar to the balance of structure and agency suggested by the concept of co-production, except that the power of pre-existing structure to condition the possibilities of present action is more clearly highlighted.[8]

8. Bourdieu and Wacquant 1992.

Although it is possible for a field to take the form of a perfect monopoly or perfect competition, the general structure of a scientific field is inequality in the levels of symbolic capital among agents. The dominant agents are committed to conservation strategies, whereas the subordinate (or dominated) agents adopt strategies of succession or subversion. The various intellectual positions advanced and the state of ongoing revision or revolution of widely held ideas defines the underlying "doxa" of presuppositions that are assumed in the disputes between orthodoxy and heterodoxy in the field. Depending on the degree of autonomy of the field, the doxa may be more or less influenced by the interests of the dominant agents in other social field. However, for a scientific field to maintain its legitimacy, it must appear disinterested.

The most influential of the laboratory ethnographies, *Laboratory Life*, utilized field sociology in the important analysis of the concept of credibility. Latour and Woolgar found Bourdieu's concept of symbolic capital to be useful in applying "economic arguments to noneconomic behavior" (1986: 230). However, they disagreed with Bourdieu's focus on scientific productivity by arguing that it is "unclear why scientists should be interested in each other's production" (206). Instead, they proposed that there is a market for information, in which scientists have a demand for the work of other scientists because they use that information in their own research. Explicitly likening the work of a scientist to that of a small corporation, they argued that scientists invest in a cycle of growing or declining credibility.

The concept of a cycle of credibility that accumulates or declines over time builds on and improves Bourdieu's convertibility thesis that symbolic capital can be converted into other forms of capital, such as the financial capital of research funding. However, after *Laboratory Life* the influence of field sociology was less evident in the sociology of scientific knowledge. In Latour's subsequent studies of Pasteur, the idea of an agonistic field was still evident, but his focus was on one scientist and the extrafield relations with farmers, hygienists, and the state rather than intrafield relations of conflict and cooperation. Latour also dropped the economic metaphors of Bourdieu in favor of military metaphors, and the translation of external interests into the scientific field became an explicit, emic project of the applied scientist Pasteur rather than an analytical, etic task of the social scientist. Latour's analysis of network building was broadly compatible with field theory, but it tended to downplay the structural aspects of power relationships both within and between fields. As a result, the potential for a critical analysis of the ideological dimensions of sciences was lost.[9]

Karin Knorr-Cetina also originally showed some interest in field sociology, but she, too, moved away from it. In another influential laboratory study published two years after *Laboratory Life*, she raised two major objections to Bourdieu's approach. In an argument somewhat parallel to the objection raised by Mulkay against Merton's use of norms, she suggested that scientists' use of economic metaphors need not be taken at face value; they may simply be using an available and widely understood cultural repertoire to both rationalize and interpret their experience. Instead, she suggested an analysis of different types of "resource relationships" that include social position, financial resources, citations, and prizes. Although that approach was similar to the different types of capital and the convertibility thesis discussed by Bourdieu, she raised a more significant objection: Bourdieu restricted his scope of analysis to the scientific field. Instead, she argued that resource relationships transverse the boundaries of science, funding, and the media in "transscientific fields." Bringing together her criticism with the analysis of Pasteur, the two studies pointed to one weakness in Bourdieu's original formulation of field sociology for science: he tended to remain focused on the scientific field itself rather than

9. On Pasteur, see Latour 1983, 1988.

interfield relations. However, his work on Heidegger and subsequent studies of French higher education did address interfield relations.[10]

After the initial engagements, work in the sociology of scientific knowledge became more focused on networks and the semiotic methods that had been developed in the laboratory ethnographies. The concept of an agonistic scientific field that is related to other social fields and requires an analysis of institutional power was replaced by heterogeneous networks and agency-based analysis of social strategies for constituting knowledge. There were some significant exceptions, such as the conflict sociology of Randall Collins and Sal Restivo, but on the whole field sociology did not have the same traction in STS that it had in sociology.[11]

Bourdieu argued that his approach offered distinct advantages over most of the approaches in both the institutional sociology of science and the sociology of scientific knowledge. He was critical of Merton's approach for reasons similar to those raised by the sociologists of scientific knowledge, but he also criticized some of the leading conceptual frameworks in the sociology of scientific knowledge. With respect to the interests analysis of the Edinburgh school, he found that it engaged in a "short-circuit effect" that directly related scientific knowledge to "general social and economic conditions" (1990: 298). He also was opposed to intellectual moves that separated out interactionist and semiological perspectives from social structure and institutional power. His concern with the shortcomings of semiological or cultural analysis extended even to approaches that attempted to bridge the analysis of power and discourse. Although he was a friend of Michael Foucault and respected his colleague's contributions, Bourdieu argued, "Foucault refuses to look anywhere except in the 'discursive

10. Knorr 1977, Knorr-Cetina 1981. On Bourdieu's work on interfield relations, see Bourdieu 1981, 1998.
11. Collins 1975, Collins and Restivo 1983.

field' for the principle that will elucidate each of the discourses inserted in it" (1991: 11). He thought that the "field of stances in itself and for itself" should instead be analyzed with respect to the "field of positions" (ibid.).

In work subsequent to his original essay on the scientific field, Bourdieu developed his approach to the study of science in several important ways. He explored the tensions between the producer and consumer poles of the field, an approach that provided another way of studying interfield relations in addition to the cultural analysis of homologies. Although Bourdieu originally developed the analysis in his studies of the artistic field, Mathieu Albert has expanded on his work and applied it to the scientific field. In general, the most prestigious positions in a research field are granted to scientists who have made fundamental contributions to the field; in other words, they are part of the producer pole of the field in which research is for consumption by other producers. In contrast, scientists who produce for extrafield consumers such as governments or industries accrue financial advantages and general acknowledgement, but they suffer from lack of peer recognition. Their symbolic capital may as low as their financial capital is high. One can expand on the idea to develop an analysis of career pathways that is of relevance for the study of the commercialization of science. Given the convertibility of capital, Bourdieu noted that those who are weakest in symbolic capital "may appeal to external powers to enhance their strength, and even sometimes to triumph, in their scientific struggles" (2001: 58). Although direct interference would likely be controversial or even unsuccessful because scientists in the research field would reject the assault on autonomy, less direct forms of support can include obtaining extrafield sources of financing, institutional position, and public credibility.[12]

For example, scientists who experience marginalization dynamics (perhaps because they pursued a subversion strategy in their research field) may find themselves pushed out of the dominant networks of the producer pole of the field and driven toward the consumer pole, where other types of funding are available for more applied projects. The high-prestige scientists at the top of the producer pole of a research field may dismiss the challenger's move as shifting into applied research and therefore as losing position. Applied research is largely irrelevant to producers because it is outside the high-status research game of the producer pole. However, scientists who pursue a diversified strategy may find that investments in the consumer pole of the field provide positive feedback into the position in the producer pole of the field. The financial and social capital obtained from success in the consumer pole can be reinvested in resources that lead to an improved position in the producer pole. Like the cycle of credibility explored by Latour and Woolgar, there is reinvestment of different types of capital, but unlike the cycle of credibility, field position is central to understanding the reinvestment strategy.[13]

In *Science of Science and Reflexivity* (based on Bourdieu's last lecture at the Collège de France), he also extended the field sociology of science by analyzing the types of scientific capital in more detail than in his earlier work. Specifically, he used distinctions among the symbolic capital of recognition (such as citations and prizes bestowed by peers), the cultural

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12. Albert 2003, Bourdieu 1996. Bourdieu borrowed the terminology that Lévi-Strauss had used for kinship analysis (generalized and restricted exchange) but modified it to describe types of scientific production. Where the requirements for entry are high (such as high levels of technical knowledge) and the field is therefore highly autonomous, he argued that the producers tend to produce for each other in a system of restricted production and exchange. In contrast, in the less autonomous (or "heteronomous") fields of generalized production, scientists tend to produce for extrafield consumers. See Bourdieu 1991.
13. Latour and Woolgar 1986.

capital of accumulated knowledge and scientific resources, and the temporal capital of control of organizational positions, funding, and material resources. Bourdieu likened the different types of capital to stacks of tokens that represented both the outcome of previous strategies and the resources available for current ones. The concept of field-based capital need not be restricted to Bourdieu's definitions, and in fact it has undergone subsequent elaboration. For example, Steven Wainwright and colleagues subsequently defined scientific capital as an amalgam of economic (financial resources), social (personal relationships), cultural (legitimate knowledge), and symbolic (recognition) capital. They also found it useful to introduce other categories of capital, such as "ethical capital," that were especially relevant to the field that they studied.[14]

In the same book Bourdieu expressed dismay with his reception by sociologists of scientific knowledge: "My article has been the object of many borrowings, whether overt or disguised—one of the most skilful ways of concealing these being to accompany them with the critique of an imaginary text against which one can sometimes argue exactly what the criticized text put forward" (2001: 36). His main objection to laboratory studies was the failure to situate laboratories in a structure, that is, the scientific field. He likened the laboratory ethnography to the village monograph in anthropology, and he argued instead that it would not be possible to understand its properties without also analyzing its position with respect to other laboratories and the broader scientific fields in which they are situated. The criticism might apply to *Laboratory Life*, but it does not apply well to Latour's subsequent study of extrafield relations in *The Pasteurization of France* and Knorr-Cetina's focus on transscientific fields in *The Manufacture of Knowledge*. In fact, it might be fairer to say that all sociologists were struggling with the post-interests analysis problem of how to analyze extrafield relations.

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14. Wainwright et al. 2009. For an additional discussion of types of capital, see Hong 2008.

However, Bourdieu had another, broader criticism that more directly speaks to the distinctive feature of field sociology and its still unrecognized potential in science studies. With respect to *Laboratory Life*, he suggested that Latour and Woolgar approached science as a "literary activity" (2001: 28), and he returned to the concerns mentioned above for his 1991 article on science studies, in which he had criticized the shortcomings of a strictly semiological or culturalist approach:

> The semiological vision of the world, which induces them to emphasize the traces and signs, leads them to that paradigmatic form of the scholastic bias, textism, which constitutes social reality as text (in the manner of some ethnologists, such as Marcus and Fischer, or even Geertz, or some historians, who with the "linguistic turn" at about the same time, started to say that everything is text). Science is then just a discourse or fiction among others, but one capable of exerting a "truth effect" produced, like all other literature effects, through textual characteristics such as the tense of verbs, the structure of utterances, modalities. (2001: 28)

Bourdieu followed with a discussion of *The Pasteurization of France*, which he argued portrayed Pasteur as "a semiological entity who acts historically," with the result that the sociology of science is reduced to "alliances and struggles for symbolic 'credit'" (2001: 29). In its place, Bourdieu suggested a field sociology of "positions and dispositions," which he claimed would also avoid the reductionism of a Machiavellian or economic rational actor. In other words, an approach to the sociology of science that focuses just on strategies and accumulation (actor-network theory) is like a field sociology that focuses only on various types of capital but ignores the other two main conceptual points of reference: the field (positions) and the habitus (dispositions).

Whatever merit Bourdieu's arguments have from the perspective of social theory, from a political perspective his critiques in *Science of Science and Reflexivity* managed to challenge Foucauldians, "cultural critique" anthropologists, and constructivist sociologists of scientific knowledge. The targets of his criticism were located in leading departments, where a significant proportion of the leaders of the next generation of STS was trained. It is perhaps unfortunate that Bourdieu made the argument in the form of what would inevitably be interpreted as a polemic; the strategy might be understood in the context of a last lecture series, but it was not necessarily the best way to encourage a reappraisal of his work. Responses were predictable. His book was, as Sergio Sismondo suggested in a review, "burdened...by Bourdieu's sense that S&TS took a wrong turn thirty years ago; this is a claim for which he does not mount a convincing and sustained argument" (2005: 2). Likewise, in a review published in the STS journal most closely associated with constructivist sociology of scientific knowledge, he is accused of "righteous wrath," "perfunctory" readings, and adopting "the classical definitions of science without questioning them" (Mialet 2003: 614, 619).[15]

The rejection of Bourdieu seems unfortunate. There is much room for integrating the concepts of a field, convertibility of capital, habitus, and interfield relations into a synthetic sociology of scientific knowledge, institutions, and culture. Field sociology provides a way of studying both the durability and malleability of structure, the mechanisms by which extrafield relations become translated into intellectual and social positions within the scientific field, the problem of why some networks grow and prosper while others wither on the vine, and how cultural imaginaries travel across fields through the mechanism of the trajectory habitus. Given the potential for field sociology to solve some of the problems recognized in both the sociology

15. See also Gieryn 2006.

of scientific knowledge and sociology of scientific institutions, it is possible that a next generation of social scientists might reappraise the dismissal of Bourdieu. Indeed, there are some signs that a recuperation is occurring, such as a special issue of *Minerva* on Bourdieu and STS that appeared in 2011.[16]

Power and Field Positions

Given the success at transforming official evaluation functions (such as university admissions for undergraduates) in more egalitarian directions, but the failure to maintain successful starting percentages for historically excluded social categories such as women, attention has turned from formal mechanisms of evaluation to its informal mechanisms. Much evaluation involves loyalties to scientific networks that share intellectual commitments. Scientists may think of their assessments as objective and universalistic, but those who receive negative evaluations may view the decision as a reflection of the biases of the dominant network of a research field. When students have powerful mentors with large networks, they may have access to more favorable evaluations because there is a greater chance that their publication submissions, tenure files, or grant applications end up in the hands of allies, students, or friends of their mentors. Conversely, scientists who do not match the dominant demographic of a scientific research field may also tend to choose mentors, graduate programs, research problems, conceptual systems, and/or methods that are outside the dominant networks of the research field. In turn, the intellectual concerns of the scientist may affect their choice of location for a graduate program and their acceptability to prestigious departments upon graduation.

Because most research fields have a mainstream (a dominant network or an interlocking set of dominant networks), the processes of evaluation also constitute processes of

16 Kleinman and Albert 2011.

marginalization. The leaders of the field and their students are not only professors in the leading graduate research departments but also editors of journals, officers of professional associations, and members of prize committees. Researchers who work outside the mainstream may be well-tolerated, especially if they play helpful service roles in the field and simply opt for different problem areas rather than different conceptual systems and methods. However, some scientists also choose to configure their research in ways that is more challenging to the dominant networks of the research field. An example is the work of cancer researchers who believe in the fundamental role of nutrition in both the prevention and treatment of cancer. Although research on nutrition for cancer prevention is fairly mainstream, in the field of cancer therapeutics research the mainstream approaches focus on surgery, radiation, and pharmaceuticals. Nutrition is generally considered too vague and weak to be anything other than an adjuvant to mainstream therapies.

As Harry Collins has argued, in research fields where there is a strong network of mainstream researchers (what he calls a strong "core group"), such as in physics, it may be possible to appear tolerant of alternative approaches by maintaining a relatively open policy for publication and funding. In other words, if the mainstream is cohesive, it can maintain marginalization by simply ignoring the challenger, but it can afford to keep the formal gatekeeping mechanisms of publication and funding evaluation relatively open to the challengers. In contrast, he suggests that in fields where the mainstream itself is divided among different intellectual positions and networks, such as often occurs in the social sciences, or where the informal mechanisms have been exhausted, then the use of formal mechanisms such as blocking publications and funding will become more prevalent.[17]

17. Collins 2000.

The fundamental mechanism for maintaining the marginal status of challengers and networks with unorthodox views is to ignore them. Mentors and senior figures in the dominant networks simply send cues to students and junior colleagues that the work of a selected scientist or group of scientists is unimportant, uninteresting, or unfounded. One might think of this approach, following the studies of Herbert Marcuse, as a version of "repressive tolerance." In other words, the dominant networks officially tolerate different viewpoints, but they also ignore them. But there is another dimension to dismissal: cooptation. The work of the challengers can be both dismissed and officially published and even funded, but it can also be appropriated and translated into the categories and methods of the dominant networks. The latter can occur without citation or acknowledgement, at least during the lifetime of the scientists involved. For example, in cancer research, there are several examples of heterodox researchers whose work later was selectively adopted and transformed into the mainstream, including therapies that were changed slightly to become patented pharmaceuticals. William Coley advocated a bacterial etiology for cancer and a vaccine-based therapy. Although that part of his work was dismissed, he has since been recuperated as the "father of cancer immunology."[18]

There are various ways that research and researchers can be rendered invisible and marginal. One strategy is the citation blockade, a set of practices that establish a boycott on citing the marginalized research and on assigning it as readings for graduate students. The citation blockade can also appear in evaluation processes, where the gatekeeping process of journal editors may require that scientists drop the "irrelevant" research and citation network as part of the price of publication. A second and related strategy is the citation tax, or requiring that a proposed publication include a requisite number of citations from the dominant network, or

18. Marcuse 1969. On Coley, see Hess 1997.

that the work be reframed in terms that are consistent with the dominant network. Thus, even if a challenger publication is not rejected outright, it can be transformed and repositioned through the imposition of the citation tax. It should be said that such strategies may not appear political to the dominant networks, who simply see the challenger's intellectual position as wrong-headed and the challenger's social position as an outward confirmation of the assessment. In other words, in a process that Pierre Bourdieu has described as "symbolic violence," the dominant networks of a research field can, in effect, blame the victim by viewing the outcomes of marginalization as justified by poor research methods and problem choices.[19]

Another set of informal strategies of marginalization involves intellectual and social stigmatization. For example, work may be included in journals and conferences, but signals can be given that indicate its questionable status. In the medical field, advocates of nutritional treatments for cancer have sometimes found their work published but accompanied by a commentary that undermines it. A proposed article may be accepted for publication only on the conditions that it is shortened or "demoted" to the status of a research note. Likewise, conference organizers can allocate marginal conference time to marginalized researchers (early morning on the first day or late afternoon on the last day). In one case, I found that a panel discussion (in a conference on chiropractic medicine, no less) on journal publication politics to which I was invited was so controversial that it was taken off the conference program and allowed only as part of a post-conference "rump" session. Professional associations may opt to keep marginalized scholars off prize committees, editorial boards, and governing boards, and to the extent that they are given service duties, it may be in less visible and potentially less damaging

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19. I am not aware of any research that uses the concepts of citation blockade and tax that I am introducing here, but in my own experience in the social sciences they are relatively common. On symbolic violence, see Bourdieu 2001.

roles in the organizations. Finally, at social events, marginalized researchers can experience shunning, where groups of people turn their heads away from the marginalized scholar and pretend that they do not see him or her, or when they make plans for dinner that do not include the marginalized researcher.

When the challengers are highly visible and more difficult to ignore, more overt strategies of containment become necessary. As Collins noted, the primary formal strategies are publication and funding blockage. Publication blockage goes beyond what I call citation blockades and citation taxes; it means that the articles are no longer accepted in the leading publications of the research field. Collins noted that scientists may still be able to publish in peer-reviewed journals, but they are pushed out to secondary journals that are not as well read. When funding blockage occurs, he notes that scientists may shift to "Pascalian" funding, that is, high-risk, high-return funding that is outside the peer-reviewed system of government grants. Military organizations, private foundations, or even venture capitalists may support the research with their discretionary funds, but they may also attach strings to the funding. When Pascalian funding ends, researchers may be able to persist by using the basic infrastructure available in their universities, but the option depends greatly on the capacity to internalize the cost of maintaining a laboratory through personal funds and departmental resources. Thus, we might hypothesize that in fields with a low cost of research, there is greater potential for the persistence of marginal researchers and hence a more diverse intellectual terrain.[20]

When it is no longer possible to ignore a challenger, overt attacks are more likely to emerge. Here we enter into a terrain that can remain on an impersonal plane of controversies over data and methods. Jason Delbourne has described this situation as "agonistic engagement"

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20. Collins 2000.

among scientists. Direct agonistic engagement may begin with attacks from lower-status members of the dominant networks. On this point, C. Wright Mills noted that the challenger may be assigned to “a minor but upcoming member of the clique who has not published much” so that the challenger is placed “in a position of less importance than if it is assigned to an eminent scholar” (1959: 112). If the challenger takes the bait and responds, the decision has the effect of reinforcing the dominant network’s assignment of the challenger to a subordinate position in the field by getting caught in a debate with a relatively junior or less important scholar. If the challenger does not respond, the unanswered review can be used as fodder for the argument that the dominant network’s view of the challenger is correct. The double bind strengthens the marginalization process. However, when the stakes are higher, the attacks also come from higher-status members of the dominant networks.[21]

My interviews with parapsychologists suggest that marginalized researchers have thought through various strategies to address the stigma attached to their intellectual positions and mitigate the risk of direct attacks. First, they perform collegial functions and focus on specific areas of technical disagreement. This “sweetly reasonable” strategy, to use the terms of one interviewee, can facilitate social integration of marginalized scientists and also facilitate consideration of the challenging views. Second, marginalized scientists may diversify their research portfolios to include areas of research that are more in tune with those of the dominant networks. Thus, not all of the research may be viewed as challenging and marginal. Third, marginalized scientists may look for cracks in the dominant network, that is, people who are more tolerant of different views, and attempt to find allies in the more powerful dominant

21. Delbourne 2008, Mills 1959.

networks. Those strategies can work where research is controversial, but suppression has not become fully materialized.[22]

Some marginalized scientists may choose not to downplay attacks such as agonistic engagement or adopt a mitigation strategy, and instead they may confront the attacks directly. They may be able to draw some support from senior researchers in their home institution or the broader research field. However, they may also need to go outside the research field to broader networks of scientists or even students and publics. By doing so, the scientists shift the terms of the controversy from a contained debate over data and methods to a much more public dispute over the politics of science. Because a public debate creates risks for the autonomy of the research field, the strategy may create additional stigma for the marginalized scientist for airing dirty linen in public.[23]

Just as challengers can shift the terms of the controversy to a public arena and a dispute over scientific misconduct, so can the opponents of the challenger. When opponents shift from attacks on methods and a strategy of marginalization based on publication and grant blockage, they broaden the attacks to the terrain known as intellectual suppression. Brian Martin, the founder of this subfield of research, suggests that suppression tends to occur especially when scientists adopt research programs that are directly in conflict with powerful military or industrial interests. Thus, whereas the mechanisms of repressive tolerance and agonistic engagement tend to be more prevalent for intrafield controversies, the mechanisms of suppression tend to be more prevalent when there is extrafield involvement. More specifically, suppression often occurs when the research, teaching, or public statements of intellectuals threaten the vested interests of a corporation, government, or even profession. (Here, the concept

22. Hess 1992.
23. Delbourne 2008.

of interests can be used with clarity, because it is attached to a specific actor, such as a corporation.) The direct mechanisms of suppression involve denying funds or work opportunities; blocking appointments, tenure, promotion, courses, and/or publication; preventing free speech; dismissal; harassment; blacklisting; and/or smearing of reputations. The indirect mechanisms involve implied sanctions, a general climate of fear, or pressures for conformity. Usually complaints are made to a person’s supervisor rather than to the person directly. Martin and colleagues also distinguish suppression from repression, which involves physical violence such as beatings, imprisonment, torture, and murder; and from oppression, which they define as an institutionalized lack of justice or freedom. Martin’s later work then charted out the subsequent mechanisms of backfire, or outrage that occurs when instances of suppression become visible, either within a research field or for a broader public. In turn, dominant networks and elites can then engage in strategies of containment of backfire, such as covering up the event, stigmatizing the target, reinterpreting the event, and holding official inquiries.[24]

In addition to confronting responses of agonistic engagement and suppression, marginalized scientists also face a long-term problem of reproduction. The “Planck effect” that Thomas Kuhn popularized (the idea that in some controversies, closure only occurs when the rejected researchers in a controversy retire and die) is based on a fundamental observation that controversies in science can linger for decades. When there is no closure, the reproduction of the challengers becomes crucial for their survival. Collins suggested that the advocates of parapsychology and Vitamin C as a cancer therapy were able to secure a toehold in universities that enabled some long-term viability for the networks. In contrast, in the case of the

24. Martin et al. 1986; also Martin 1996, 2007, 2010.

gravitational wave controversy, the primary advocate was not able to recruit a next generation of supporters.[25]

Even relatively well-positioned networks that are far from marginalized and have recruited the next generation of graduate students may become marginalized due to degrees of reproductive success. To understand the problem (what one might think of as the reproductive fitness of a network), it is helpful to use an analogy. In the game of *Monopoly*, the goal is to place one's hotels on the most expensive properties, such as Boardwalk and Park Place, to ensure the highest possible rent from any visitors who land on the spots. In turn, the high rents can be reinvested in the acquisition of new properties and in the investment of hotels on those properties, which in turn increases the rents collected. An increasing returns (or cumulative advantage) cycle sets in that leads eventually to monopoly. In a similar way, the objective of the scientific field from the perspective of reproduction is to place one's students on the Boardwalks and Park Places of the research system. As Val Burris shows in a field analysis of academic departments, the prestige of departments is explained less by its productivity patterns than by its hiring patterns. Prestige is an outcome of departmental social capital, and its reproduction through hiring patterns accounts for the long-term stability of departments as well as the association between prestige and departmental size.[26]

In some national university systems, the level of resource availability is relatively even across the more and less prestigious universities, and hence the goal of placing students in a prestigious department has a lower impact on the reproductive capacity of a network. However, in the United States and some other countries (to some degree in France, as Bourdieu showed), the most prestigious universities also tend to have the highest level of temporal resources, such

25. Collins 2000, Kuhn 1970.
26. Burris 2004.

as research institutes, funded lecture series, endowed professorships, graduate student financial support, faculty financial support, and well-equipped laboratories. The resources facilitate the capacity of a scientist to hold conferences, invite guests, mentor students, sponsor postdocs, and otherwise set agendas and cultivate networks that not only enhance the scientist's individual capital but also can shape the direction of a research field. The resources are especially important in fields with relatively low sources of extramural funding, where they can help researchers to attract the best students, but they are important across all fields.[27]

To the extent that a scientist occupies the Boardwalk and Park Place of the university system or the broader system of research institutions, the scientist is able to attract the best students and has a competitive advantage in placing those students in the next-generation position of Boardwalks and Park Places. In contrast, a scientist located in a less prestigious institution with lower resources will watch with chagrin as many of the best incoming students are lost each year to institutions with better resources and as the scientist's own students fail to obtain jobs in the more prestigious research universities. There are exceptions. Some brilliant students come out of less prestigious institutions, and they may work their way up the institutional hierarchy to overcome cumulative advantage dynamics. Exceptions notwithstanding, one needs only to posit a statistical relationship to see that scientists located in subordinate institutional positions will find that their students will tend to be located in the less prestigious programs and less prestigious institutions. In turn, the students may have students (the "grandchildren" of reproductive system), but the process will repeat itself. Furthermore, the students of students in secondary positions may end up not being located in graduate programs that can produce students, and consequently there can be a third-generation die-off. They may

27. Bourdieu 1998.

also find that unlike their advisors, the second and third generations lack the illusio (the belief in the value of the field) to remain in the research game, and they may exit. However fertile their extrafield careers may be as activists, advisors, or administrators, they are infertile with respect to the reproduction of the challenger network. In effect, the Matthew and Matilda effects of the career attainment literature play themselves out across the generations due to the politics of what might be called secondary reproduction. Merton’s identification of the reward system as a site for the study of power in science was therefore insightful, but he did not follow through with the study of how the structures of career attainment can coincide with the intellectual divisions of the field by dominant and subordinate networks. In other words, the crucial element of the analysis of power within the scientific field, which would have transformed Merton’s functionalism into a political sociology of the reward system, was missing. The problem of career attainment can involve not only a failure of individuals to accumulate advantage but also a failure of subordinate networks to maintain long-term reproductive capacity.

Extrafield Relations

Increasingly STS researchers have broadened the field concept by paying attention to extrafield relations. The political sociology of science studies the institutional matrix of the scientific field, specifically the relations with the political, industrial, and civil society fields. Because our approach to the topic is discussed in detail in the essay “Science and Neoliberal Globalization” discusses the issues in more detail, I will not go into it more here. However, there is also the broader issue of the relationship between different types of knowledge, specifically the knowledges of the scientific field and that of lay peoples. This topic includes the vast field of the interactions of Western and non-Western knowledges, and considerable work by historians and anthropologists has documented the importance and extent of the interactions in the history of modern science. Even today, there is a continuing incorporation of other knowledges via the interactions of doctors and patients, designers and users, governments and citizens, and managers and employees. In these fields a wide range of methodologies—focus groups, participatory design, surveys, observation, fieldwork, and interviews—enable the designers of technologies and products to draw on the lay knowledge of users to improve design. In the process, designs that may have originated with a limited modal user in mind can be expanded to incorporate new user categories. An example is universal design, which incorporates the concerns of the disabled, arthritic, elderly, children, and women into designs that were originally oriented toward a modal healthy, ambulatory, adult male. The universalizing of design in technology is similar to the strengthening of objectivity, to use Sandra Harding's phrase, that occurs in a research field that diversifies its social composition and, in the process, incorporates other knowledges. Although the professional knowledges of product and system designers is not the same as scientific knowledge, the professions serve as mediators that systematize and identify lay knowledge that become part of the applied sciences at the consumer pole of the scientific field. Because there can be an interaction between a research field and an associated technological field, participatory and user-centered design may become associated with strong objectivity in a related scientific field. In other words, as the design of the systems improves, the theorists of the research field extract principles of system design, thus translating the lay knowledge and increasing their research field's breadth and depth of knowledge.[28]

Diversification of the social composition of science creates the conditions for different perspectives to emerge in the scientific field that can lead to consensus shifts or expansions of

28. Harding 1992.

inquiry that strengthen objectivity. Standpoint theory is the most developed framework in science studies that explores how the diversification of the social composition of a research field can lead to a transformation of scientific knowledge. According to Harding, standpoint theory emerged during the 1970s and 1980s among feminist scholars who shared the common argument that a feminist approach to scientific research should start from a basis in women's lives. She generalizes standpoint theory as follows: "When marginalized groups step on the stage of history, one of the things they tend to say is that 'things look different if one starts off thinking about them from our lives'" (2008: 115). Translating this insight into field sociology, when persons with a trajectory habitus based on subordinate positions in other social fields migrate into the scientific field, they tend to bring new perspectives to the dominant networks of research agendas, conceptual frameworks, and methodologies. The argument is also similar to that of the role-hybridization thesis of Joseph Ben-David and Randall Collins, but it is different in two crucial regards. Whereas role-hybridization explores migrations of high-status scientists across scientific fields into lower-status fields, the trajectory habitus includes knowledges based on extrafield positions and experience in subordinate positions in other fields.[29]

Several qualifications are necessary for understanding standpoint theory and the related concept of an extrafield trajectory habitus. First, the importance of excluded standpoints is likely to vary across research fields. One might expect that feminist standpoints would be most important as a corrective to biases in the research field when the research problems themselves include issues of gender in the social and natural worlds, such as occurs in some subfields of sociology, physiology, endocrinology, and primatology. The standpoints of women may be less important for the theoretical and methodological development of more formalized knowledges

29. Ben-David and Collins 1966.

such as the statistics of multiple regression. However, even here one might find that differences emerge that are associated with gender. For example, the difference between ordinary least squares regression and logistic regression (or between forms of statistical analysis that include "dummy" or binary variables, itself an interesting term, and those that require interval variables) may have relationships with gender.

Second, there is also a reminder that flanking effects of standpoint can occur, as Harding has suggested in her postcolonial qualification of feminist standpoint theory. The great vulnerability (which is also a strength) of feminist standpoint theory is that once one begins to look at one marginalized standpoint, many others soon proliferate. The effects are so powerful that they may account for the ambivalence with which a younger generation of feminist STS scholars have taken up explicitly feminist themes in contrast with general studies that include race, class, and colonialism. In other words, one generation's subversion strategies may appear to another as complicit with the field's conservation strategies.[30]

Even accepting these potential qualifications, there is an important descriptive claim with policy implications: opening the scientific field to greater social diversity, and with it the potential questioning that occurs based on previously excluded standpoints, produces greater diversity of scientific opinion that ultimately leads to an improvement in a research field's capacity to represent the social and natural worlds. This argument bears similarities with forms of democratic theory that emphasize the value of inclusion as mechanisms for improving the success of political decisions. In this sense, affirmative action and internationalization for the scientific field is more than an issue of social fairness. It also has implications for understanding the conditions for reducing the ideological bias, either conscious or unconscious, of research

30. This last point emerges out of a conversation with Harding and her response to a question that I had for her.

fields and for strengthening the "objectivity" of the intellectual autonomy of the scientific field. This point enables a connection between Harding's analysis of strong objectivity and Bourdieu's argument that the self-interested competition for symbolic capital in the scientific field can also generate distinterested knowledge. The point is also similar to Karl Popper's argument that debate and disagreement in the scientific field, rather than the collective marching to the drumbeat of the Kuhnian paradigm, is necessary for the ongoing vitality of the field. Here, competition and debate are important, but so is opening the field to a diverse set of social addresses.[31]

Before one rushes into policy prescription, however, there are some important caveats that need to be raised. In the book *Inclusion*, Steven Epstein explored how reformers both inside and outside the biomedical field attempted to diversify the standard of biomedical research from a focus on an "average" white, male body. In the United States, the movement was largely successful in the sense that biomedical research has come to accommodate biological differences among kinds of human bodies. However, the success also has created the paradoxical problem of enabling explanations of unequal health status for women and men, or among ethnic groups, that reduce health disparities to biological differences. The outcome is not inevitable, and it is possible to develop frameworks for studying health disparities as complex outcomes of both biological and social variables. To reduce the possibility of a rebiologization of race and gender, Epstein suggested using terms such as "health disparity populations," which might emerge as a second-order response to the problem created by the solution to the problem. Thus, although Epstein's research is not by any means evidence against the value of opening up the social

31. Bourdieu 2001, Harding 1992, Popper 1963.

composition of the scientific field (and of its problem areas) to greater diversity, it suggests that the results of such changes are by no means straightforward.

Conclusion

Attention to the dynamics of scientific fields makes it possible to bring various perspectives in a more synthetic and historical approach that pays attention both to the making of knowledge and the making of dominant and subordinate positions both within and outside the scientific field. There are institutional relationships between the scientific and other social fields, such as the growing focus on commercialization in scientific research funding, but there are also epistemic relationships that range from the continued interchanges between scientific and lay knowledges to the less obvious effects that come from the inclusion of previously excluded standpoints in the social composition of research fields.

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