ALTERNATIVE PATHWAYS IN SCIENCE AND INDUSTRY

Activism, Innovation, and the Environment in an Era of Globalization

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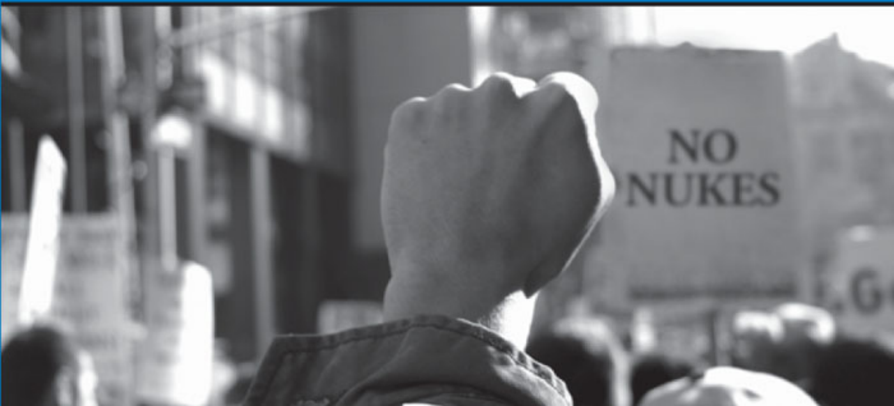

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Retheorizing Scientific Change

In order to develop a conceptual framework for understanding the role of alternative pathways in industrial innovation, it is necessary to begin with existing theories of science, then develop an understanding of how alternative pathways interact with scientific change and industrial innovation. As a social institution, science is enormously important, because it sets the stage of modern politics by circumscribing the horizons of the possible. It proclaims authoritatively what is and can be the case, and it grounds normative projects of public policy and technological innovation in a realism of the possible and impossible. Scientists need not make policy recommendations to be politically influential. By drawing the lines between the true and the untrue, as well as between the possible and the impossible, they can eliminate from consideration normative proposals that are based on assumptions deemed invalid and futures deemed impossible. Although the legitimacy of science as an institution depends on its claim to be relatively innocent from direct influence by political positions, the autonomy of the scientific field is precarious. The scientific field and the doxa that it produces is more like a carefully tended garden than a wilderness preserve. Increasingly, the crucial question for the garden of science in an era of globalization is "Who decides what plants are grown?"[1] To answer this question, it is necessary to begin with a simple but workable definition of "elites." I use the term to refer to networks of people and organizations that control investment decisions and policy making. Though interconnected, they are divided by industrial affiliations and institutional positions, so that political and industrial elites often have sharp internal divisions, and political elites may also clash with industrial elites. Because elites are often divided, they tend not to "breathe together" (literally, to conspire) in a simplistic manner that is evoked by the phrase "ruling class." Often, political and economic elites see their actions as representing the best interests of society, and often they can turn to the elites of the social and natural sciences to support their interpretations of what is possible and impossible.

However, elites are also responsive to pressure from below in the form of consumer preferences, opinion polls, elections, and social-movement action. As a type of elite the leaders of scientific fields are in a delicate position; their research fields undergo profound external influence from economic and political elites in the form of funding preferences for research agendas, but the scientific fields also undergo less profound influence from social movements and general public opinion. Scientists are sensitive to both even as they defend their field's autonomy. The dynamics of scientific fields and their position in society will be the topic of theoretical exploration in this chapter and the next. In order to develop a conceptual framework for addressing the problem adequately—that is, in a way that does not reduce science to ideology but that also escapes from the micropolitics of networks and knowledge construction—it is necessary to revisit the field of science studies and begin with some of its basic arguments.

The Problem of Undone Science

Because political and economic elites possess the resources to water and weed the garden of knowledge, the knowledge tends to grow (to be "selected") in directions that are consistent with the goals of political and economic elites. When social-movement leaders and industry reformers who wish to change our societies look to "Science" for answers to their research questions, they often find an empty space—a special issue of a journal that was never edited, a conference that never took place, an epidemiological study that was never funded—whereas their better-funded adversaries have an arsenal of knowledge to draw on. I call this "the problem of undone science." From the perspective of the activists and reform-oriented innovators, the science that should get done does not get done because there are structures in place that keep it from getting done.[2]

The prioritization of research tends to create huge pockets of undone science that result in the systematic nonexistence of selected fields of research. Where is the university that has all of the following: an electrical engineering department that focuses on distributed and off-grid renewable energy; schools of architecture and urban planning that focus on sustainable design for low-income neighborhoods; a school of agriculture oriented toward sustainable local agriculture; a department of chemistry that works closely with chemically exposed communities and develops green chemistry alternatives; a school of business administration that focuses on developing employee-owned, locally owned, and cooperative businesses; a psychiatry department that explores mind-body therapies as replacements for pharmaceuticals; and a biochemistry department that focuses on food-based neutraceuticals? Although such departments and research clusters exist here and there, and if put together in one place would probably make an interesting and powerful university with unpredictable new synergies, they have not been selected as the dominant research fields and problem areas.

The point need not be overstated. Pierre Bourdieu once noted that the state has a left and a right hand; that is, there are ministries dedicated to issues such as education and welfare and those dedicated to commerce and defense. It is also the case that the university has a left and a right hand. Universities will probably always continue to have a left hand that educates students in citizenship and prepares some for careers of public service. One tends to find such departments and schools among the humanities, social sciences, and professional schools oriented toward the functions of the welfare state, such as schools of public health and social work. But in the current era of globalization, the transition of the research university into an engine of regional development implies that the right hand will tend to become much larger and stronger, and the spaces for developing alternative research fields may become narrower. Even in the left-handed schools and departments within the university, one finds trends toward an orientation to the needs of industry. As a result, the problem of undone science is likely to increase rather than to diminish.[3]

The politics of undone science appear not only in decisions surrounding funding priorities but also in the controversies that envelop the knowledge-making process. Because some methods and equipment cost more than others, the dominant networks tend to have access to the most expensive methods and equipment, and as a result well-funded networks can drown out the alternatives not only through gross productivity but also through access to the preferred methods and the disciplinary institutions that enforce definitions of what is better science. Methods in science are somewhat akin to lawyers in the justice system; the wealthy often have access to the best lawyers, so they have an easier time winning their case. However, knowledge and truth are not infinitely malleable. Because science is fragmented as an institution, it is possible for reformers to go around the consensus of a subfield and recruit advocates and research methods in neighboring fields. In other words, there is the potential for countervailing powers in the scientific field to reduce the dominance of scientific elites. Still, the politics take place in a historical terrain that is increasingly right-handed in the sense of being shaped by the problems, methods, and conceptual frameworks deemed important by industrial and political elites that seek innovative and profitable new products to create jobs, earn foreign exchange, and enhance overall economic and military competitiveness.

Theories of How Knowledge Changes

In order to be of assistance to reformers who use scientific knowledge, social scientists and scientists alike need a theory of science that is not naive with respect both to the epistemic and political authority of science's truth-making machinery and to the often invisible hands that tend the fields of disciplinary knowledge. We need a theory of how knowledge changes in science and of how science is shaped by society, yet we also need a theory that avoids a combination of philosophical relativism and political realism that can reduce scientific knowledge to political ideology. This chapter lays out such a theory by assessing and building on the long interdisciplinary conversation about science that has involved philosophers, historians, and social scientists.

The most basic models of how scientific knowledge changes were developed by philosophers, whose work idealized scientific change in a manner akin to the way that neoclassical economists developed idealized models of markets. Under the empiricist model of science, which in many ways is the "lay" philosophy of working scientists, a sharp division exists between non-observable, theoretical terms and observational terms. Concepts or words that describe unobservables are seen as useful heuristics, or, if one is a philosophical realist, they may present hints of a deep structure of reality that is not yet observable, such as the concepts of viruses and electrons did before machines were developed to transform the theoretical terms into observables. In either case theoretical terms are devices for making generalizations from observations, and science changes when new observations cause scientists to rethink their generalizations. An empiricist may also accept a gradual model of scientific progress based on the subsumption of narrow generalizations or theories by broader ones. If two theories cover the same empirical material, scientists choose between them by finding a point where they predict different observations, then the scientists design a crucial experiment to determine which prediction is more accurate. If the theories are evidentially indistinguishable, a true empiricist will bite the bullet and say there is no ground for choosing between the theories.[4]

Alternative views usually begin with the recognition that science can have other rational grounds for distinguishing among theories, such as consistency with other theories and a combination of internal consistency and simplicity. By connecting theories to each other as much as to observations, the ground is set for recognizing the theory-ladenness of observations and methods. The long tradition of conventionalism, which dates back to the early-twentieth-century French scientists Henri Poincaré and Pierre Duhem, argues that methods and observations are only interpretable within a theoretical system. Furthermore, because theories can be adjusted to new data, it is not easy to design a crucial experiment or decide upon a crucial observation that would allow a clear choice between two broad theoretical systems. In the face of what appears to be contradictory evidence, a defender of an existing theoretical system can make various moves: argue that the methods behind the new empirical evidence are flawed, claim that the interpretation of the data is wrong, or modify a subtheory without jettisoning the broader theoretical system.[5]

A conventionalist approach to knowledge change has the advantage of bringing the model closer to scientific practice and recognizing that new empirical observations do not easily resolve major theoretical controversies, especially when large networks of scientists have substantial intellectual and material investments in existing theoretical systems and associated research programs. Instead, the history of science often narrates stories of scientists who line up behind one theoretical and methodological system in opposition to another group of scientists. Resolution of the controversy is grounded in logical argumentation and displays of evidence, but it also requires negotiation between the sides over what counts as evidence and what methodologies and research designs are considered acceptable. The accumulation of evidence gradually tends to put one side increasingly on the defensive and leads some of the advocates to be persuaded by the opposing view. As Max Planck observed and Thomas Kuhn popularized, sometimes one side must retire or die before the controversy is fully resolved. Moreover, if one side has better access to laboratories and other research resources, it is in a stronger position. However, just as having the funds to hire the best lawyers is helpful in a court trial but does not guarantee the outcome, a network that controls the best laboratories is not guaranteed of winning a scientific controversy.[6]

Perhaps the best-known conventionalist account of science is Thomas Kuhn's book *The Structure of Scientific Revolutions*. In Kuhn's model, scientists in a research field labor under a paradigm, or a specialist research culture of observations, theories, exemplars, methods, and people. The theories of a paradigm do not undergo revision as the result of a single crucial experiment; rather, the paradigm undergoes a slow and steady erosion by the accumulation of anomalies. As new research results and problems come to shake faith in the existing paradigm, an alternative is proposed, and the outcome is resolved according to the conventionalist model of mixes of empirical evidence, consistency arguments, and negotiation of methods and results. When the challenging perspective is triumphant, there is a scientific revolution, and scientists who labor under the new paradigm settle in to work under the conditions of normal science.[7]

The Structure of Scientific Revolutions currently has more than 7,000 citations in the various science citation indices, and some have claimed that it is the most-cited book written in the twentieth century. The Kuhnian model of scientific change was widely influential, and some young scientists read it with the aspiration of becoming the leader of the next revolution rather than a mere cog in the machine of normal science. Social scientists who work on social movements, environmental issues, and other fields that are not directly connected with science and technology studies also tend to talk about science in terms of paradigms and revolutions. However, after half a century a substantial literature has developed to reveal the book's shortcomings. Perhaps the greatest shortcoming is the idea that a specific scientific field is governed by a monolithic paradigm, and likewise the converse idea that some scientific fields, especially the social sciences, are pre-paradigmatic anarchies of immature science. Instead, attention has come to focus largely on the diversity of the theoretical and methodological differences among networks of scientists within a field. Although there are many versions of what has become known as the sociology of scientific knowledge, most of them recognize the intense power of networks and their competition for recognition in science. Scientists within a research field do not all march to the tune of the same paradigmatic piper; rather, research fields are often characterized by disagreement and controversy over empirical claims, proper methods, and conceptual categories.[8]

To formalize the alternative, agonistic view of research fields, a research field is characterized by relations of cooperation and conflict among advocates of different conceptual frameworks, research methods, and problem areas. An individual scientist usually is engaged in more than one research program, that is, a bundle of research that brings together a method and theoretical framework to bear on a research topic. Scientists who work on similar research programs can be said to consist of a specialty network; they are the most competent to review each other's work, and they form what Harry Collins has called the "core set" of actors when a controversy erupts. However, other scientists may have some overlap in expertise with a portion of the bundling of theory, method, and problem area that occurs in a specialty network. As a result, peer review is also possible by neighboring colleagues who hold expertise in a portion of an individual's research program but not the entire program, such as the methods or conceptual frameworks but not the problem area. One can also determine the degree of proximity and therefore to some extent the value of the peer review by assessing how much of a theoretical framework, set of methods, and problem area is shared.[9]

When controversies erupt within networks of proximate colleagues, peer review by non-proximate but neighboring colleagues provides for a system of checks and balances in science, and it provides editors, funders, and other gatekeepers with a mechanism for sorting out disagreements. If a challenger scientist has developed a new method or generalization, or if the scientist has produced a new empirical finding that is not recognized by the specialty network, the challenger scientist has recourse to appeal by attempting to persuade the non-proximate colleagues who share a research culture along one or more of the dimensions. The process is both intellectual and social at the same time; in order to make knowledge change, scientists must mobilize both convincing arguments and convinced colleagues.

This view of scientific change is more or less the way researchers in the field of science and technology studies approach knowledge today. Most empiricists have ceded at least some ground to conventionalist arguments, and most social scientists with a conventionalist orientation have recognized that although knowledge making involves socially negotiated argumentation, scientists must still convince their colleagues with evidence and logical argument, and consequently the epistemological status of scientific knowledge is not equivalent to political ideology. One can take science off its pedestal of a naive empiricism without giving up the claim that scientific knowledge is, like other forms of occupational expertise, generally superior to that of non-experts, at least on topics where the expertise is well developed, empirically grounded, and openly vetted.

In this sense we can say that scientific knowledge is socially constructed. The term is used here not as a philosophical claim but instead as a limited empirical generalization from social-science research, which has found that scientists must vet differences over theories, methods, and the interpretation of observations through a social institution that relies on negotiated assessments among more or less proximate peers. As a result scientists generally must make complex judgments, much like a jury that is evaluating various arguments from the defense and prosecution, rather than resolve disputes through the simplicity of the single, definitive, crucial experiment. The process of understanding how the world works in science is similar to the processes in other rationalized modern professions, such as lawyers establishing the facts of a case, doctors conferring over the diagnosis of a disease, or mechanics assessing different explanations of what is wrong with the car. Although scientific knowledge, like other forms of expert knowledge, is fallible, I will assume that scientific knowledge about the natural world, like social scientific knowledge about the social world, is generally better than that of non-experts—although not always so, as literatures on lay and non-Western knowledge demonstrate—and that the knowledge accumulates, even if it sometimes undergoes theoretical reconceptualization.[10]

Beyond the Empiricist-Conventionalist Debate

Although the philosophically oriented models of scientific change are valuable as a starting point, they are in a sense devoid of content. In other words, it hardly matters if one is talking about physics or psychology; the philosophical and sociological models are concerned with general processes by which a community of experts determines that one theory or observation (or theory-observation-method bundle) is superior to another. The problem is the construction of knowledge, of how knowledge is built up from the basis of previous research and vetted for general acceptance within an expert community of scientists. In this book I am more concerned with the content of scientific fields, that is, the question of which knowledge comes to be selected as deserving attention and which knowledge is considered not worth pursuing. I term this second problem the "selection" of knowledge, in contrast with the construction of knowledge. The term may invite some confusion, because of the widespread association of selection with evolutionary theory. However, one might remember that Charles Darwin began his discussion of natural selection with the human selection of domesticated plants. The concept of selection is understood here in the root etymological sense of choosing (as in the selection of candidates, theories, or products), which can include ranges of choice that are imposed on the less powerful. In my view, the primary question for science and technology studies in an era of globalization is no longer the constructivist question of how scientific knowledge is socially negotiated or shaped, but instead the structural question of what science is selected to be done. To begin to answer that question, we must first turn to the reward system in science.[11]

In the early philosophical and sociological models of scientific fields, research takes place inside a bubble, as if it were socially isolated from society. From that perspective, research fields grow and develop organically from new questions that emerge from new answers. The agenda of research topics flows from basic curiosity about a piece of the world, but the agenda is not influenced by societal demands on science as an institution. This view, based on an assumption of a high level of autonomy for the scientific field, represents a simplified or idealized model of scientific change.

The autonomy assumption represented not only the peace in the feud among empiricist and conventionalist theories of how scientific knowledge changes, but also between the philosophical accounts and the modernist sociological accounts of the reward system. A reward system under conditions of autonomy allocates prestige to scientists based on their ability to solve problems in a field. Robert Merton is generally given credit for having first described the reward system, but Warren Hagstrom's subsequent work drew out two crucial points. First, the reward system has some features similar to pre-capitalist gift exchange. Scientists in the ideal world of pure science give research away, but they do so because they receive recognition in the form of citations, prizes, or general renown among peers. Even though scientists have given away their research, they remain attached to it because it is the source of prestige. As a result, they will defend their work against attack to preserve its scientific aura in the peer networks. Second, as in a gift exchange system scientists also compete for recognition; the very people on whom one is dependent for recognition are also competing for the same recognition. It is as if the athletes competing for an Olympic gold medal also made up the panel of judges. Under such circumstances backstabbing and strategic alliances can be expected, not because the stakes are so low, as is commonly said of academia, but because the system is set up to hold in constant tension relations of cooperation and competition. The primary check on duplicity is provided by the existence of non-proximate and multiple peer review.[12]

In the 1970s two significant theoretical developments began to open up the analysis of the reward system from the autonomy assumption to a framework that is compatible with the study of science and globalization. Bourdieu's convertibility thesis focused on how scientists transform economic resources into the symbolic capital of publications and prestige, and vice versa. He also argued that such conversion strategies were crucial in agonistic struggles over credit. The convertibility thesis became increasingly valuable as historians and ethnographers began to pay more attention to the problem of how funding flows affected research agendas. The other development was Bruno Latour and Steve Woolgar's analysis of how credibility is a crucial factor in the investment cycles of material and symbolic capital. They argued that successive publications give scientists the credibility to gain access to funding that leads to increased research and more publications, which in turn leads to more funding, and so on. The accumulation of reputation or credibility is concretized in the curriculum vitae and can be traded as collateral for material resources for future research. In other words, as the reputation or track record of a scientist increases, it generally becomes easier to obtain larger grants, better positions, higher salaries, lower teaching loads, better graduate students, and better physical research space and equipment. Provided that access to increased inputs results in continuing recognition for the scientist's research, there is what they termed a "cycle of credibility."[13]

The increasing-returns dynamic of the Latour-Woolgar model was anticipated somewhat by Merton's cumulative advantage theory of scientific careers—the idea that in science the rich (in prestige) get richer and the poor get huge teaching loads—but the Latour-Woolgar model suggests that increasing returns would operate regardless of the starting point. Their model also showed how the success of a scientist's publications is related to demand: successful research is cited research, that is, research that is used by other scientists to build their own research. Consequently, as one's own research is embedded in other scientists' research programs, and, as the number of users increases, the cited scientist is increasingly able to withstand challenges. In what amounts to an early version of actor-network theory, the higher the number of positive citations, the more widely accepted a piece of research is, and the more it approaches consensus knowledge. It is not necessary to enter into the question of whether the high use of a scientist's research is caused by the scientist's successful marketing, strong social networks, and impeccable pedigree, or because the research happens to represent a portion of the world in an accurate and novel way. The answer is simply that a mix of the two occurs, and the factors operate synergistically.[14]

The work of Bourdieu and Latour and Woolgar helped to undermine the autonomy assumption by opening up the reward system to include material capital and by showing how in a sense the goal of the scientific game is to accumulate a high mix of both symbolic capital (citations, prizes, prestigious appointments, and successful students) and material capital (grants, laboratory space, equipment, postdocs, and graduate student research assistants). The opening to material capital punctures the bubble of autonomy and shows how knowledge making is contingent on external sources of funding, with the exception of a few fields that have negligible research costs. In other words, funding shapes what science can and will be done as well as what remains undone. However, one must be careful with this argument, because it can turn into a simplistic form of externalist, economic determinism. To further develop a post-autonomist theory of scientific change as knowledge selection, this chapter will analyze how individual scientists select research programs as well as the more general issue of the rise and fall of research fields. Although the discussion may seem technical, it is fundamental for understanding the problem of undone science and the potential for activism and social movements to play a role in knowledge generation and in technological innovation.

The Selection of Research Programs

It is possible to find research fields in which there is little or no controversy, but it is more typical for the research front of a research field to be characterized by controversies over methods, conceptual frameworks, and assessments of what constitutes an important and trivial problem area. However, the existence of controversies does not necessarily imply a level playing field of pluralistic research networks; hence, Kuhn's paradigm concept does flag a condition that is often seen. It is often the case that research fields have one or more dominant networks that control the lion's share of resources in the field. The dominant network is the in-group that has hegemony over the major graduate departments, journals, professional society positions, and grant-awarding institutions. But the position of the dominant network is not stable; it is constantly facing challenges by new networks of researchers who are importing new methods or concepts for a problem, or who are attempting to divert resources for a method or conceptual framework to new problem areas. Although it is possible to find suggestions of a paradigm in the sense of consensus knowledge, it is also the case that at the research front most research fields are characterized by controversy.[15]

Lack of consensus is not a sign of an immature science or a paradigm in the throes of revolution but instead an indication of the vigorous vetting process that occurs through complex linkages of cooperation and competition. Controversy over what the important problems should be, what the best methods are, and how the problems should be defined are all aspects of "normal" science, not, as Kuhn would argue, symptoms of the pre-paradigmatic phase of a research field's history. As a controversy is resolved, the change may not be as simplistic and dramatic as a paradigm change; instead, the challenger networks of theory-method-problem bundles may find themselves incorporated into the dominant networks but also transformed by the incorporation process, often through theoretical and/or methodological syntheses.

In view of the network structure of scientific fields, the process of choosing a research program is both an intellectual and political investment. Consider a scientist who, during the wind-down stage of a significant project or merely during a pause in a busy schedule, steps back to assess what research projects should come next. A wide variety of factors influence the decision, as has been noted as long ago as the work of Hagstrom. The scientist will probably make an assessment of the risk/reward ratios for the new research project, somewhat akin to an investor who is selecting a stock. For example, there may be a hot new growth field that has a lot of buzz, many new entrants, and upward funding curves. As Henry Menard noted, if a scientist makes a significant contribution to a new field, the rewards will be great, and consequently new fields pose great opportunities to young risk takers. However, the field may not pan out, the funding may dry up, and a bandwagon of new entrants may quickly crowd the field. For more established scientists the costs of retooling and the high risks of failure in a new field may make entrance less attractive. In contrast, by returning to an ongoing research program there are lower entry costs (both intellectual and material), and although the returns may not be as great, they are less risky. Still, the "bond" option of scientific investing poses risks of its own; scientists who pursue that option face diminishing returns of a method to the same research topic. Scientists with larger pools of accumulated capital may consequently diversify their research program investments by putting some of their resources into established research programs and some into high-risk, high-return programs, and they can also have the resources to migrate more successfully into high-risk, high-return fields if their original investments pan out.[16]

One of the risks of new fields, especially for junior researchers who are often the first colonizers, is the well-known pattern, first characterized by Merton, whereby less well-known scientists who may have priority will tend to receive less credit than better-known scientists who do not have priority. One can also generalize the argument, as Margaret Rossiter has done, to notice that credit flows not only to more senior scientists but disproportionately to privileged social categories, such as men over women. Young scientists in a new, high-risk field may at first welcome the entry of more senior scientists into the field because they legitimate the field and help open up the doors to greater funding and access to publications, but the entry of the silverbacks can be a mixed blessing for the young founders, because the silverbacks will tend to redefine the field, absorb future credit, and reapportion credit to themselves and their own students over the founders, especially if the founders lack the initial network linkages of the high-prestige late entrants. There can even be a rubbing out of the achievements of the lesser known researchers as credit is absorbed by the better-known researchers.[17]

Two points should be made about the economic model of the decision-making process of the scientist. First, the model can be accommodated to either an autonomist or non-autonomist view of science. In other words, the existence of high-risk, high-reward fields can be an outcome of an internal logic of problem solving, and scientists can capture pools of externally controlled funding for the new field by convincing funders to alter their priorities. However, the general priorities of funding patterns, which in turn are a significant factor in how hot or cold a research field is, are often determined by extra-scientific actors, and the pattern is increasingly away from control of broad agenda setting by scientists, if they ever had such control. Historians of science and funding have documented the effects of extramural funding on research priorities in a variety of fields, from physics, chemistry, and engineering to biology, medicine, and the social sciences. From the viewpoint of the scientist on the ground, the system may appear to exist in an autonomous bubble that develops according to its own internal logic, but when one steps back and follows the money, it is clear that funding flows help some fields to prosper while others wither on the vine.[18]

Second, the economic metaphor of the scientist as a rational actor who assesses risks and benefits before making investments of research effort should be seen as providing a helpful but ultimately incomplete picture. There are many ways in which investment decisions are not rational in the sense of decisions that are based on calculations to optimize recognition and other rewards. One example is the scientist who clings to an old research program, even when the evidence has been overwhelmingly in favor of divestment for a long period of time. As in the financial world there can be unsuccessful and incompetent investors in the scientific world. Another example is the scientist who invests in a research program because of a sense of its societal value. The example is particularly important for developing a structural understanding of the career risks faced by scientists who are aligned with social movements and with social-change values; those scientists may choose to invest in research programs that result in lower prestige and marginalization, if not outright suppression. They are not bad investors as much as people who understand the tradeoff between socially responsible scientific research and career advancement. Scientists who pursue out-of-favor research that is linked to social movements and social responsibility values sometimes attempt to make an alignment between the societal value and funding availability. As Adele Clarke has demonstrated, scientists who work with social movements may also negotiate a "quid pro quo" in which movement and advocacy organizations exchange financial support for a research program that is not what they originally wanted but is closer to the scientist's needs for research that will be valued within the scientific field. A third example of decision making that is not intuitively accommodated to a prestige-optimizing model is the scientist who opts for applied research projects that have high financial rewards, such as licensing opportunities for patents, but much lower status rewards, such as prestigious publications. In the extreme case the scientist is transformed into an inventor and exits from the prestige game of the scientific field. The transformation, which provides a linkage between the scientific and industrial field, can take diverse forms. For example, the scientist may pursue technological research that is funded by a large corporation, or the scientist may engage in technology development in an entrepreneurial setting. Cross-cutting the settings can be a mix of values from pure profit seeking to social and environmental amelioration.[19]

Given the shortcomings of a prestige-optimizing model of scientific decision making, one might argue that choosing a research program could be likened less to an investment decision and more to a decision to move into a new neighborhood or to look for a new job. By shifting research programs, a scientist also shifts the reference specialty network and faces the inevitable adjustments of getting to know new neighbors and colleagues. Depending on how far one moves into the proximate peer networks, one can face increasing tests to prove oneself. In the new specialty network a scientist's accumulated reputation may not matter as much, and one must rebuild relationships with new colleagues. As with any move, the process can entail a range of experiences from liberation from old rivalries and antagonisms to the travails of intellectual hazing from anonymous peer reviewers and citation taxes levied by journal editors who want a specific network cited before accepting a paper for publication.

The Selection of Research Fields

So far my analysis has focused on the issues a scientist may face as an individual when selecting research programs and assessing the option to do research in areas of undone science that is relevant to alternative pathways. However, the decisions of individuals take place within broader historical changes by which whole research fields come into and go out of favor. As funding priorities set by governments, foundations, and private corporations shift, researchers will tend to follow the money at an aggregate level, even if some individuals select problem areas that go against the incentive structure. For example, a problem area with growing government and industrial research funding, substantial startup opportunities in the private sector, departments in the top universities, and high salaries (i.e., nanotechnology research in the United States at the time of writing) will attract many researchers. Because money spent on one problem area of a research field is money not spent on another, the size and shape of problem areas follow the funding flows, and the scientists who come to occupy a backwater will find themselves and their students excluded from the rewards of prizes, large grants, and the circulation of senior hires among elite departments. Where individual careers are evaluated in terms of dollar value of sponsored research at the tenure point, the pressures to move into the mainstream fields will be even stronger. In other words, the aggregate funding flows that shape the status hierarchies within and among research fields exert a downward pressure on the portfolio decisions of individual researchers, just as the decisions of individual scientists about what should be the next problem area for a research front exert upward pressure on the funders' decisions to reallocate portfolios. The availability of funding makes new research possible, but the positive results of the new research encourage funders to increase support. Funding will tend to continue to grow as long as the momentum of positive results continues and as long as political and economic elites target the fields for further development.[20]

Although there is significant historical variation in the growth and decline of specific research fields, there is also some value in developing a general model of the economic dimensions of the cycle. In this section I will develop a general economic model of the growth dynamics of research fields, then relate the model to the problem of undone science. To begin, if the funding rate for a new research field does not keep pace with the growth of scientists who have decided to shift into the field, then in the short term competition for funding and other resources will increase. Likewise, in neighboring research fields that are more established and not undergoing the excitement of a dramatic new breakthrough and/or an increase in funding, scientists will begin to migrate out to join the new growth field, and (other things being equal) competition within the old, declining research field will decline. In the short term, an equilibrium mechanism across the fields tends to adjust the expectations of higher recognition with the realities of higher competition, so that individuals face a tradeoff between high-risk, high-reward fields and low-risk, low-reward fields.

If funders agree that the new research field is worth supporting, they will reallocate their portfolios toward the new research field, and (in the absence of overall increases in funding) they will cut the research in the older neighboring fields. Their action will tend to equilibrate the costs of entry across the research fields over the longer term. The declining research field will now be doubly unattractive: not only will the promise of recognition be lower as the field size decreases, but the competition for funding relative to the new field will have increased due to the cuts in research funding. Likewise, the new field still has the higher promise of recognition, but (again, other things being equal) the increases in funding mean that competitive rate for funding has gone down relative to the old field (even if not in absolute terms). As a result under these conditions, the reallocation of funding accelerates the growth in the new field and the decline in the old field.

Just as a small town becomes a large city, when a research field grows the original research network will mushroom so that it is no longer a small-scale invisible college that met Derek de Solla Price's criterion of everyone knowing everyone else. At the beginning a research field might be somewhat interdisciplinary, with scientists from diverse subfields or disciplines reading each other's work. As the research field grows in size, each of the areas represented by a few scientists, or even one research group, might become occupied by a whole specialty network of research groups. New research groups will enter and bring to the research field slightly different problems, methods, and conceptual frameworks. In short, as the field grows in size, it will become more diverse. New conceptual and methodological frameworks from different disciplinary specialties will be drawn into the problem area. A field with a few competing specialty networks may find itself now having several competing networks as specialists in new research fields enter the field. Old-timers may lament the good old days when conferences were small and everyone knew everyone else, but they may also welcome the growth in prestige and opportunity that the success of the field has brought to them and their students.[21]

However, the promising new field does not remain in boom mode forever. There may be a long-run shift from sponsorship by government and foundation sources to industrial sources. With the change in funding, both research problems and intellectual property regimes shift, and some scientists may find other, more basic research fields more attractive for their reputational goals. Even in the absence of the shift toward technology and product innovation, researchers in the same field will tend to experience diminishing returns unless new methods can be brought to bear on the problems. If a field has already grown to a large extent and become fairly diverse, then (other things being equal) the probability of new methods leading to new breakthroughs will be lower. The field will begin to settle into a steady state (a term I prefer to "normal science") of incremental advances in knowledge. In turn, the field may undergo diminishing rates of citation in journals with lower readerships for each new study. Scientists and especially their students will begin to leave the field as the possibilities of new breakthroughs and status enhancement emerge elsewhere. Eventually, funding will also slow down as new boom cycles emerge elsewhere and the once growing field now becomes an established or even backwater field with declining rates of citation and funding.[22]

It is not necessary to assume that the cycle of growth and decline outlined here applies to all research fields. In mathematics and in some of the social sciences, research costs are low and researchers may follow their personal interests or those of small networks of colleagues. When they are free to follow their own interests, they are likely to spread out into an increasingly diverse number of problem areas. As the size of specialty networks declines, theoretical and conceptual issues lose salience because there is no one to provide alternative views on the same empirical problem. Specialty networks per se cease to exist; there are only more proximate and distant neighbors who share some knowledge of each other's methods, conceptual systems, and topical problem areas. Under such conditions, they read each other's work out of a need to maintain institutional support rather than a sense of building together a collective understanding of a common problem area. This anomic type of research field could, in theory, exist in any field that has low research costs.[23]

One solution to scientific anomie is involution: a small number of researchers read each other carefully and cite each other's work, but the citation pattern is more of a closed cluster than an open network. Their work tends not to be cited outside the narrow research field, and the members of a citation ring may develop special languages and methods that make their work especially difficult for members of neighboring research fields to find their work accessible. Rather than fading off into the overlapping networks of neighboring research fields, in which research in one field has spillover effects on another field, the involuted field has closed in on itself. Sealed off and hermetic, the field will tend to be less likely to produce research of value to activists.

Yet scientific involution is probably less responsible for producing undone science than the general trend for government funding to emphasize research fields that advance national competitiveness and for a growing research field to shift toward applied science and technology.

When a mature research field shifts to an emphasis on patenting and licensing, funding will tend to diversify out from public sources, such as federal government research grants, to industrial support. More generally, when a research field undergoes growth, it requires new resources to fuel the growth, and it will tend to seek funding resources from industry. When industrial funding sources increase, opportunities for alignment with general societal benefit and projects that are coherent with social-movement goals will be reduced, or they will have to be recast in ways that are made compatible with industrial goals.[24]

However, because government research is increasingly tied to industrial innovation goals, even in the absence of increasing amounts of direct support from industry the overall funding portfolio for a research field may be oriented toward technological innovation and industrial competitiveness goals from the outset. Researchers who opt to do work in conflict with the competitiveness goals set by economic and political elites, such as on the environmental and health risks of new technologies, may find limited funding budgets with correspondingly diminished field sizes, citation rates, and opportunities for career advancement. They may still opt to pursue research programs that are aligned with social-change goals, and they may do so by cobbling together limited available funding sources, including their own resources, or by dedicating a portion of their research portfolio to pro bono research. In doing so they are swimming against the tide and creating conditions for their own marginalization. Rather than dedicating all resources toward achieving prestige as a founder of a large and growing research field, to the extent that they dedicate their research resources toward undone science, they have opted for a career path of anonymity. They risk becoming a flounder in a field that may be perceived to be tainted by political interest or merely dismissed as boring because it is not on the cutting edge.

Conclusions

In this chapter I have outlined a theory of scientific change and innovation that is appropriate for an era of globalization. Although the history of science can produce some cases that approximate Kuhnian paradigm revolutions, I have suggested an alternative approach for understanding change and innovation in science. Certainly there are consensus shifts in science, but Kuhn's focus on sudden and dramatic changes in the form of revolutions diverts attention from the deeper and more pervasive dynamic of scientific change: the ongoing differentiation and diversification of scientific fields as new research fields are both created by and the creators of funding sources. Rather than see science as moving in a circle of revolution, normal science, and back to revolution, I draw attention to the deeper pattern of innovation in science that occurs with the differentiation of new research fields.[25]

In contrast with Kuhn's pairing of the scientific revolutionary and the inglorious normal scientist, the drone who does the mop-up work of the revolutionaries and is consigned to the dustbin of history, the framework outlined here suggests an alternative pairing. The founder of a new field recognizes opportunities for innovation, especially when the opportunities are not yet obvious to peers, and the founder colonizes new areas of knowledge when the risks are high. The founder does not cause the growth of the field, but neither does the shift in external funding priorities cause the growth of the field. Rather, there is an interactive process in which the founder is an actor in the structured structuring of the history of the growth and decline of a research field. Although the founders of a research field may eventually lose credit during the secondary succession that involves colonization by the dominant networks of adjacent fields, the founders who do not suffer from obliteration through incorporation will tend to enjoy great prestige in science.[26]

In contrast with the figure of the founder, the flounder chooses to stick with research programs associated with research fields where funding, citations, and number of researchers are stagnant, small, or even declining. The flounder may have good reasons for doing so, including an alignment with a social movement's political goals. Flounders will tend to work in the basements of the funding systems (and often of the research buildings) on topics that their peers and funders consider to be unimportant or to have backwater status. They may have graduated from universities that lack the funding to attract the best students, and they may be faculty members who are condemned to social reproduction by watching with chagrin as even their stellar students achieve positions in second-tier institutions. Flounders may even find that they are alone, or located in an anomic field where peer interest in one's research approaches zero. After recognition of their position sets in at mid career, they may opt to leave the game of science to develop applied research for clients, who may range from private-sector firms to social movements.

Although research conducted by scientists at the behest of social movements (or even research programs that are not aligned with the goals of dominant industries) may cast the scientist in the position of a flounder, it is not the only outcome. Clever scientists may be able to produce an alignment of social-movement goals with goals associated with large, growing research fields and even large industrial corporations. For the intellectually, politically, and strategically brilliant, there is a way out of the dilemma, especially if they are located in a prestigious institution and can leverage the institution's halo effect to put a new topic on the intellectual map. If the scientist is able to solve the dilemma, the research may garner for the scientist both high social prestige outside the scientific field and high recognition within it. Such possibilities exist, and their frequency and conditions for success can be studied empirically. The point here is not to make an empirical claim—that all scientists who choose research programs associated with social-movement goals will become flounders instead of founders, or even that there is a valence for such work to produce flounder status—but instead to outline a theory of the dynamics of a system in which such decisions take place, career risks are experienced, and issues can be conceptualized and studied. Because the system is set up so that certain areas of science will be well tended while others will be left to wither on the vine, the scientific field will develop historically to have large areas of undone science. The pockets of undone science will tend to include knowledge that would be especially valuable to the building of alternative pathways.

corporations, see Barnet and Müüller 1974, Barnet and Cavanagh 1994, and Sklair 2001. On international social movements, see Cohen and Rai 2000 and Della Porta and Tarrow 2005.

17. The claim that globalization has increased inequality should be examined comparatively and across classes. Firebaugh (2003) argues that after 1960 (that is, after one date frequently used to mark the beginning of a period described as "globalization") aggregate between-nation inequality decreased whereas within-nation inequality increased (see also the United Nations Development Program reports, e.g., 1999). A more detailed discussion is offered by Schmitt (2000), who argues that almost all of the increase in the inequality of wages in the U.S. between the late 1970s and mid 1990s was due to a decline in real wages at the lower end of the income scale. For example, according to Schmitt women in the top 10 percent experienced gains, but there were greater losses in real wages for women in the lower 10 percent.

18. My interpretation follows Schmitt 2000 and Williamson, Imbroscio, and Alperovitz 2002 by sidestepping the trade-vs.-technology debate and looking instead at the political economy of globalization as a factor. Unionization is one example of lowered wage bargaining power; those statistics are from pp. 37–38 of Williamson, Imbroscio, and Alperovitz 2002. On job losses and the 2004 statistics, see Bronfenbrenner and Luce 2004.

19. On post-Fordism, see Harvey 1989. On university-industry-government partnerships, see Etzkowitz and Leydesdorff 1997. On import substitution, see Shuman 2000, 2006.

20. On diasporization and the failure of the nation-state in its assimilation policies, see Friedman 2003. On ongoing reconstruction of diasporic identities, see Inda and Rosaldo 2001. On ethnogenesis, see Roosens 1989.

21. Weber (1949, chapter 2) provides a succinct statement of the methodology. Ringer's discussion of methodology (2004, chapter 3) is also helpful on this point.

22. See also Fischer's (1995) discussion of types of policy evaluation. A similar division might be constructed for the natural sciences, such as distinctions among natural history, ecology, ecosystem restoration and design, and philosophies of nature.

Chapter 1

1. On the concepts of doxa and fields, see Bourdieu 1971, 1975, 1977, 1982, 1988, 2001 and Swartz 1997. For similar work on conflict in science, see Collins and Restivo 1983. I borrow significantly from Bourdieu, but his lack of a historical perspective on the late-twentieth-century scientific field led to an overemphasis on autonomy, even though he also developed an incipient critique of the autonomy assumption in science studies.

2. I introduced the concept of undone science in a paper (Hess 1998), then explored it in an electronic publication (Hess 2001) and in a co-authored essay

(Woodhouse et al. 2002). See also Hilgartner (2001) on the unknowable; the philosophers of feminist and postcolonial science studies, e.g. Haraway (1989) and Harding (1992, 1993, 1998), who also focus on the systematic exclusion of categories of knowledge and people from the scientific enterprise; and the new political sociology of science (Frickel and Moore 2006; Kleinman 2003).

3. On the metaphor of the left and right hands, see Bourdieu 1998.

4. For an introductory account that includes a discussion of philosophical realism, see Boyd, Gasper, and Trout 1991.

5. This paragraph provides a synopsis of arguments that, for a more leisurely discussion, can be reviewed in Hess 1997 and in such sources as Collins 1983, 1985, Duhem 1982, Kuhn 1970, Lakatos 1978, and Quine 1980.

6. See Collins 1983, 1985 and Kuhn 1970.

7. I am bracketing discussion of his incommensurability thesis, because I assume that the thesis is not borne out by the actual practice of scientists, who are generally able to understand each other's theoretical differences and often willing to negotiate methods. The incommensurability thesis can ground an argument in favor of epistemic relativism, but Kuhn himself rejected that characterization of his work.

8. I cannot review the huge literature here, but my 1997 book *Science Studies*, though now somewhat dated, provides one way into the various currents of the sociology of scientific knowledge. The accounts by Callon (1986, 1994) and Latour (1987) are the best-known and most influential of a large literature on networks, schools, and research programs.

9. On the discussion of core sets and controversy, see Collins 1983, 1985, 2000, 2002.

10. See, for example, Collins's argument about the irreducibility of replication to an algorithm (1985). It is an important argument, but there are points where Collins seems to push it in favor of epistemic relativism, with which I disagree (Hess 1997).

11. I have found metaphors of variation and selection provocative when applied loosely to science and technology. (See, e.g., Basalla 1988, Campbell 1990, and Knorr-Cetina 1981.) However, because there is a danger of over-naturalizing the idea of selection and consequently of blinding the analysis to issues of power, I emphasize an everyday definition of selection. See also Frickel and Moore 2006, Kleinman 2003, and Fuller 2000a. Fuller's discussion of the democratic control of science emphasizes various mechanisms that could enhance democratic control over the selection of research programs.

12. See Merton 1973 (orig. 1957), Hagstrom 1965, and Mauss 1967. On the general consistency of the autonomy assumption with other scientific systems of the modernist period, which tended to emphasize self-correcting and closed systems, see chapter 4 of Hess 1995.

13. See Bourdieu 1975 and Latour and Woolgar 1986. I am retracing somewhat Knorr-Cetina's (1981) discussion of this trajectory of thinking, but with a purpose of leading up to a broader argument.

14. Again, see Latour and Woolgar 1987. Stated more formally, cumulative advantage theory (Merton 1973) holds that students of famous scientists at top institutions start out at the top of the status hierarchies and tend to do better and better over time than students who start out from lower status positions. On actor-network theory, see Callon 1986, 1994 and Latour 1986.

15. For more detailed critiques of Kuhn and the paradigm concept, see Fuller 2000b and Restivo 1983. For medicine the pattern of dominant networks has also been discussed in the analysis of the "dominant epidemiological paradigm" (Brown, Zavestoski, and McCormick 2001; Zavestoski et al. 2002). See also the concept of "scientific and intellectual movements" (Frickel and Gross 2005), which describes a specific type of the more general politics of research fields.

16. On the choices, see Hagstrom 1965: 78–79. On high-risk fields, see Menard 1971 and Hargens and Felmlee 1984. On shifts on interests in the form of bandwagons, see Fujimura 1987. On diminishing returns, see Rescher 1978. On migration and status, see Ben-David and Collins 1966 and Mullins 1972.

17. On priority disputes and assignments, see Merton 1973. On the Matilda effect, see Rossiter 1993.

18. Leading historical studies of the topic include Forman 1987, Kevles 1997, Kleinman 1995, Kohler 1991, Leslie 1993, and Noble 1977.

19. On the quid pro quo, see Clarke 1998, 2000. On relations between scientists and social movements in general, see Frickel 2004a, 2004b, Hård and Jamison 2005, Hess et al. 2007, and Moore 2006a.

20. See my discussion of the phenomenon in a comparison of angiogenesis and cartilage research in the cancer field (Hess 2006a).

21. On invisible colleges, see Price 1963. On the formation of new fields, see Mullins 1972.

22. On diminishing returns, again see Rescher 1978. In the field that has an increase of specialty networks competing over relatively stable funding sources, the specialty networks will engage in various intensification strategies to meet the increased competition for scare resources. Controversy (akin to warfare in societies that face resource shortages) among the specialty networks may break out. We know that controversies occur at the research front, and we know a lot about how they are maintained, negotiated, and resolved, but we know little about the conditions that are likely to produce high and low levels of controversies. The framework outlined here suggests that controversies will be more likely not only at the research front but when competition for scarce resources (funding, journals, etc.) increases.

23. See also Hagstrom 1964, 1965.

24. For a similar argument (the "finalization" thesis), see Böhme, van den Daele, and Krohn 1976 and Schäfer 1983.

25. For more on the critique of the cyclical view of scientific revolutions, see Fuller 2000b.

26. On obliteration and incorporation, see Merton 1973: 508.

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