

Neoliberalism and the History of STS Theory: Toward a Reflexive Sociology

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Abstract:

In the sociology of science and sociology of scientific knowledge, the decline of functionalism during the 1970s opened the field to a wide range of theoretical possibilities. However, a Marxist-influenced alternative to functionalism, interests analysis, quickly disappeared, and feminist-multicultural frameworks failed to achieved a dominant position in the field. Instead, functionalism was replaced by a variety of agency-based frameworks that focused on constructive or performative processes. The shift in the sociology of science from Mertonian functionalism to the post-strong program, agency-based sociology of scientific knowledge has parallels with the broader shift in political ideologies from social liberalism to neoliberalism. The argument is made in a way that is cognizant of the criticisms raised against interests analysis and avoids the "short circuit" of class imputation. Instead, the approach defends the potential for a more integrated approach to the structure-agency-meaning triangle in STS via the use of field sociology.

Note. The following essay was developed from invited comments made at a special session on neoliberalism and STS at the Japanese Society for Science and Technology Studies in Tokyo, August, 2010. The special session, organized by Professor Hidetoshi Kihara, discussed the demise of Marxist and related theoretical frameworks in STS in Japan. This paper addresses the issue more generally for STS as a field, with a focus on neoliberalism and STS theory in northern Europe and the Anglophone countries.

Although scholars have raised questions about the failures of neoliberal policies for decades, the continuing effects of the global financial crisis that began in 2007 have stirred a broad public questioning of neoliberal policies and their effects on economic stability and social fairness throughout the world. The term "neoliberalism" will be used here to mean both public policies and economic thought that have guided a transition in many of the world's economies toward the liberalization of trade and financial markets, the privatization of public enterprises, and the retrenchment of government commitments to social programs. Some of the broader public questioning of neoliberalism has translated into a scholarly literature on neoliberalism, including an emergent literature in the field of Science and Technology Studies (STS). Within STS, studies of neoliberalism have been largely associated with a growing and significant body of research on the commercialization of the academy (e.g., Lave et al. 2010, Slaughter and Rhodes 2004, Vallas and Kleinman 2008). Work in the political sociology of science and technology has also included a wider focus on the role of neoliberal ideologies in technology regulation and the changing relations between research communities and social movements (e.g., Hess 2011, Kleinman and Kinchy 2007, Moore et al. 2010). As STS researchers begin to study the problems associated with neoliberalism, science, and technology, there is a need for a reflexive inquiry into the underlying conceptual frameworks of the STS field itself and the possibility that the some of the dominant

conceptual frameworks of the field are inflected by decades of neoliberal thought. The reflexive inquiry developed here will explore the potential for what Pierre Bourdieu called “misrecognition,” or the not entirely visible ways in which positions in the political field have homologies with those in the intellectual field (Bourdieu 1981).

Functionalism, Fairness, and Ideology

In the early 1970s, functionalist (or structural-functionalist) approaches to the study of science were highly influential. Although there were many social scientists working in that line, the leading figure was arguably Robert Merton (1973). His sociology rested on an analysis of science as a quasi-autonomous intellectual field governed by a reward system in which scientists bestowed upon each other prestige (and funding and institutional positions) based on the quality of their research. He allowed for particularistic considerations to enter into decision-making apparatus of the reward system, and much of the subsequent research on the reward system that developed from his work focused on the extent to which rewards were allocated based on universalistic or particularistic criteria (e.g., Cole 1987, Long and Fox 1995).

Radical scholars, including Marxists, have often criticized functionalist theory for emphasizing the self-correcting nature of social systems and failing to attend to conflict, including class conflict. The failure to attend to conflict also led to an under-recognition of the potential for societies to undergo abrupt transformation, especially when led by historically disempowered groups. As a result, radical scholars viewed functionalism as having an implicitly conservative social bias. However, this type of criticism was not very precise because it tended to lump together a range of liberal ideologies as “conservative.” In the case of Merton the sociology of the sociology of science might be more precisely directed at the implicit social liberalism of his thought. The term “social liberalism” is understood here as the equivalent of the term “social democracy” in Europe; in the American context it refers to the Democratic Party politics that dominated the country’s decision-making from the New Deal in the 1930s to the Great Society in the 1960s. As a network of political ideology and policies, social liberalism involved some protections for labor, the poor, and the elderly in the form of Keynesian and welfare-state policies.

To understand the thesis that Merton’s sociology of science was homologous with social liberalism, Merton’s portrayal of Darwin provides a good starting point. Merton tended not to develop deep case studies, but in the influential article based on his 1957 presidential address before the American Sociological Association, he lingered somewhat on Darwin to portray a person who was deeply conflicted over how to respond to Wallace’s anticipation of his theoretical claims. The conflict was, for Merton, an exhibition of the power of norms in science: “Humility and disinterest urge Darwin to give up his claim to priority; the wish for originality and recognition urges him that all need not be lost” (1973: 306). Although Merton saw this conflict in terms of norms, it was also a question of distribution of a scarce resource: credit. For Darwin, the resource should be distributed on grounds of fairness, that is, universalistic grounds. If a perception were to arise among scientists and historians that the allocation of credit to Darwin was based on particularism, such as would be entailed if Darwin were to block Wallace’s publication, then the credit could shift from Darwin to Wallace, or at the minimum Darwin’s reputation as a scientist, particularly with respect to the norms of honesty and humility, might be tarnished. As Merton reports, Darwin’s colleagues resolved the conflict by arranging to have both papers read at the Linnean Society. Merton notes that the decision to have the two papers read simultaneously does not affect scientific knowledge, but it does affect the institution of science: “It is the social *institution* of

science and individual men of science that would suffer from repeated failures to allocate credit justly” (1973: 306).

This passage is suggestive of a concern that runs throughout Merton’s work on science; his exploration of the reward system draws attention to the distributive aspects of science, that is, the question of who gets what and the problem of the grounds of fairness for unequal distribution. It leads to what is arguably his most influential work in the sociology of science, the study of cumulative advantage and its relationship to the norm of universalism. Ultimately his research became the basis of a rich literature in the sociology of science on the extent to which science is universalistic or particularistic with respect to gender and other characteristics outside the scientific field (Long and Fox 1995). Merton and his colleagues such as Jonathan Cole tended to defend the functionality of apparent particularism such as cumulative advantage and gerontocracy in science.

This was no mere academic debate; at stake was the intellectual basis of policies such as affirmative action and the general reform of science to make it a more fair and universalistic institution. In Merton’s conceptual framework, the universalistic, functional, and democratic were opposed to the particularistic, dysfunctional, and totalitarian. As he wrote, “Democratization is tantamount to the progressive elimination of restraints upon the exercise and development of socially valued capacities. Impersonal criteria of accomplishment and not fixation of status characterize the open democratic society. Insofar as such restraints do persist, they are viewed as obstacles to the path of full democratization” (1973: 273). To be a functional institution, science must be fair with respect to extrascientific statuses such as gender and race. Merton supported students who wanted to work on gender and inequality in science (Epstein 2003), but his support of universalism in science also led him to criticize African American scholars who argued that only they could do good African American history. He found such views to lead down a road that ended in solipsism, and he compared them to the equal and opposite view that only outsiders such as Gunnar Myrdal could understand a social controversy such as American race relations. Instead, in a move that might be compared with Harding’s formulation of strong objectivity, he wrote, “Insiders and Outsides, unite. You have nothing to lose but your claims” (1973: 136; Harding 1992).

Merton’s work in the sociology of science often displayed a similar tension, in which he studied distributive issues such as patterns in the allocation of credit but also sought to explain how apparently unfair outcomes, such as the apportionment of more credit to senior authors in cases of multiple discovery, were often necessary for the institution of science to retain functional stability. Likewise, he argued in favor of the universalism of science but also recognized that particularism in the form of functionally irrelevant statuses could undermine universalism. This view of the scientific field is homologous with a widely shared view in society about the conditions under which inequality could or could not be perceived as just. In a social liberal regime, merit-based inequality was just, whereas inequality based on heredity and prejudice was not.

Merton was very much attuned to issues of inequality as well as opportunity not only in his social theorizing but in his reflections on his life history. He grew up as Meyer R. Schkolnick in a family of Jewish immigrants from Eastern Europe in a low-income neighborhood of South Philadelphia (Merton 1994). But he did not see his life as deprived; instead, his autobiographical memories focus on opportunities that surrounded him, such as local libraries and concerts. In a Bourdieusian moment of reflection on his trajectory habitus, he noted, “The seemingly deprived South Philadelphia slum was providing a youngster with every sort of capital—social capital, cultural capital, human capital, and, above all, what we may call public capital—that is, with every sort of capital except the personally financial” (1994: 10). Politically, he described himself as a socialist

during college, a perspective that he suggested was not different from “many another Temple College student during the Great Depression” (1994: 14). But as a social scientist Merton was more frequently described as a liberal rather than a Marxist or socialist (Crothers 1987). A life trajectory that moved from the slums of Philadelphia to the pinnacles of academic social science involved a habitus that enabled him to see distributive issues but also to emphasize the potential for achievement-based inequality based on the social liberal ideal of equality of opportunity. His habitus was consistent with the dominant political ideology of social liberalism, in which distributive issues of the rights of labor, women, the poor, ethnic minority groups, small businesses, communities, and the environment were legitimate areas for state intervention in the economy, but such issues were addressed within a framework of reform that enabled both equality of opportunities and inequality of outcomes through regulated competition. As Merton wrote, “Under changing conditions, new technical forms of organization must be introduced to preserve and extend equality of opportunity. The political apparatus may be required to put democratic values into practice and to maintain universalistic standards” (1973: 273).

Structural Adjustments

It is now possible to offer a reading of the post-Mertonian history of STS. A watershed publication was Michael Mulkey’s 1976 essay on Mertonian norms. The 1970s represented a crucial decade in the history of global ideology. On September 11, 1973, Salvadore Allende was overthrown, and with his overthrow the great experiment in South American democratic socialism came to an end. Likewise, in 1975 New York City underwent a kind structural adjustment program in response to its bankruptcy (Harvey 2005). However, one should not make too much of historical parallels at this point; the intellectual field is relatively autonomous and undergoes transitions that are articulated in the terms set by the field.

The basic thesis of Mulkey’s essay was that science as an institution was not governed by norms, and Merton mistook the ideology of scientists—the rationales and rhetoric that they display for purposes of justification before funders and publics—with the actual practice. For American sociologists of science the response was puzzlement. As one very senior sociologist commented to me, their reaction was, “Where have these guys been?” In other words, for American sociologists there had not been much use of norms for over a decade; the focus on attention was on the reward system. Even Merton had recognized the complexity of an analysis using norms by working with the ambiguities associated with norms and counternorms. To the extent that concern with norms remained important in the sociology of science, it was mainly focused on the issue of universalism and particularism. In turn, that problem was less about how much individual scientists internalized norms and more about the properties of the reward system in terms of fairness and egalitarianism.

Mulkey proposed an alternative problem for the sociology of science: how scientists perform and construct ideologies. In other words, a structural-functional problem in which a social system governs individual action through socialization and a reward system would be replaced by an agency-oriented analysis in which individuals perform norms. The locus of attention would then shift from the study of systems to construction and performance. Attention to issues of fairness and particularism would disappear. The field would later undergo a corresponding a name change, from the sociology of science to the sociology of scientific knowledge. (Some American sociologists of science today even today refuse to use the term “STS” because it is seen as something different from the sociology of science.)

The fundamental theoretical statement of the new approach came in the widely read book *Knowledge and Social Imagery* (Bloor 1976). The strong program itself was flexible enough to allow for structural explanation (that is, the study of the effects of social structural differences, such as class, race, and gender on intellectual positions); indeed, as it later became clear in David Bloor's critiques of actor-network theory, he preferred a sociological approach that preserved the potential for the explanatory role of social structure (Bloor 1999). The first main historical implementation of the strong program was interests analysis, and the paradigm case was Donald MacKenzie's study of statistics in Britain (MacKenzie 1978, 1983). The study implemented a method that used the scientific controversy as the unit of analysis, and it showed how a technical dispute over appropriate definitions of statistics was related to differing social networks of statisticians. But the study also went beyond a microsociological account of the constructive work of networks to a structural argument. In other words, MacKenzie argued that networks in turn were in some way shaped by associations with the interests of two classes: a rising professional class associated with Fabian socialism and eugenics and a declining aristocracy with notions of noblesse oblige and concern with poverty.

In an important theoretical event for the history of STS, interests theory was heavily criticized in an extensive exchange in the journal *Social Studies of Science*, which became (and remains) the main venue for constructivist studies. The critics included advocates of alternative conceptual frameworks such as ethnomethodology and an incipient form of actor-network theory (Callon and Law 1982; MacKenzie 1981, 1984; Woolgar 1981a, 1981b; Yearley 1982). The critics drew attention to the problem of assigning a causal relationship between structural conflicts in society (in this case class conflict) and the different intellectual positions of advocates of a controversy. Steve Woolgar drew explicit parallels between Marxist interests and functionalist norms as two flawed structural accounts of the sociology of scientific knowledge. As he memorably complained, "Instead of norms we now have interests" (Woolgar 1981b: 375). Woolgar's criticism of interest theory followed that of Mulkay for Merton, and again the alternative view was to study how agents actively perform or construct their worlds rather than how their worlds shape them. Furthermore, to explore that problem, social scientists would need detailed interviews with the participants in the controversy to explore how broader social ideologies and politics affected their intellectual positions. Because the data for that type of question were often not available in the archives, ethnographic methods such as laboratory studies could be used to explore those questions.

An alternative to interests analysis that attempted to integrate and balance structural, cultural, and agency approaches was available at that time. Field sociology enabled both a more robust view of the relative autonomy of the scientific field and the possibility of identifying extrafield influences on scientific thought, while also retaining an analytical balance among social structure, the agency of actors, and systems of symbolic meaning (Bourdieu 1975). Furthermore, Bourdieu's study of Heidegger provided a methodology for studying the ideological influence of the political field on the intellectual field while also recognizing the relative autonomy of the intellectual field (Bourdieu 1981). By employing methods borrowed from structural anthropology, he could point to a chain of semiotic homologies that connected categories of distinction in the political field with those in the intellectual field. In the process, he could show how those chains of association could be ignored and forgotten, so that the ideological dimensions of intellectual positions were unrecognized. He argued that the approach was superior to the "short circuit" of the Edinburgh interests analyses, but like interests analysis, field sociology preserved the capacity to study the question of how political

ideology influences intellectual distinctions in the quasi-autonomous scientific field (Bourdieu 1990).

However, the emerging work in constructivist STS developed a reading of Bourdieu that avoided the challenge of his study of Heidegger, and it focused instead on the weaknesses of his analysis of capital. In *Laboratory Life*, Bruno Latour and Steve Woolgar included a “Bourdieuian” chapter that built on his concept of an agonistic field. Their central argument against Bourdieu and also the functionalist Warren Hagstrom (1965) was that their economic models of science could not explain why scientists read each other. Consequently, the “demand” side of science must be included, that is, the way in which scientists use each other’s work as a basis for their own research claims. The result was a model of cycles of investment. Latour and Woolgar maintained the concept of the field but shifted the central metaphor from capitalist accumulation to warfare, a metaphor that was continued in Latour’s subsequent book, *The Pasteurization of France*, where heterogeneous networks replace the concept of the field.

In a book published two years after *Laboratory Life*, Knorr-Cetina (1981) raised two major objections to Bourdieu’s approach, a view that represented a more negative position than her prior discussion of Bourdieu (Knorr 1977). In an argument somewhat parallel with the objection raised by Mulkay against Merton, 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 (Knorr-Cetina 1981: 83). Although the variety of resource relationships was not inconsistent with the diverse types of capital that Bourdieu studied, she faulted Bourdieu for restricting his scope of analysis to the scientific field. Instead, resource relationships appear to transverse the boundaries of science, funding, and the media in “transscientific fields.”

Somewhat later, Michel Callon (1987) used a case study of the failure of electric vehicles in France to reject the Bourdieu’s field sociology. The case study described the efforts to develop electric vehicles by a group of engineers associated with electricity companies. When the efforts ran into technical problems, the engineers at Renault who supported gasoline-powered vehicles used the technical problems to support their efforts to stop the reform. Callon argued that Bourdieu’s field sociology would point to the role of cars in consumer culture, the prestige value of gasoline-powered cars, and the potential resistance from consumers who might not find electric vehicles to fit in well with their sense of class distinction. Callon suggested that field sociology would predict the failure of the electric vehicle but for the wrong reasons, whereas attention to heterogeneous networks would provide a more accurate account.

In all three criticisms of field sociology, the objections raised could be surmounted. For example, the concept of cycles of credit could be viewed as an advancement of the understanding of the accumulation processes in the scientific field that builds on Bourdieu’s argument that social and intellectual struggles are deeply intertwined, and that one form of capital can be converted into another. Likewise, the concept of resource relationships is arguably consistent with the idea that agents accumulate different forms of capital and that they can have positions in more than one field at a time. The concept of transscientific fields might also be interpreted as an improvement on the more field-specific analysis that Bourdieu had developed at the time, but it was also consistent with Bourdieu’s subsequent discussions of interfield relations and the position of the intellectual field in the field of power (1998). With respect to Callon, one can accept the general point and grant him some ground for the value of the concept of a heterogeneous network, but one can use a field analysis to explore why

the arguments of one group of engineers carried the day over others. In other words, a possible outcome might have been for the government to invest even more money into electric vehicle development in response to the perceived technical failure.

When STS turned away from field sociology, it lost the potential for a successor program to both functionalism and the class analysis of the Edinburgh interests school that retained the benefits of both while avoiding their shortcomings. To some degree the loss of field sociology can be explained by Bourdieu's own intellectual trajectory. He did not engage the STS field directly by writing on science and attending STS meetings. Instead, he focused more on the French educational system. When he did finally engage STS more directly at the end of his life, he was highly critical of it, and reviewers from the STS community were equally disparaging of his last work in the sociology of science (Bourdieu 2001, Gieryn 2006, Mialet 2003, Sismondo 2005).

Agents and Entrepreneurs

During the 1980s and 1990s a group of methods and conceptual frameworks sprouted up on the grave site of functionalism and interests analysis: ethnomethodological studies of discourse, laboratory studies of the construction of scientific facts, actor-network studies, social worlds analysis, and the empirical program of relativism (which later was extended as the better-known social construction of technology program). The research programs that became the mainstream of STS theory shared a family resemblance by focusing on how scientists as agents made their world. The shared problem, along with some shared belief in the symmetry principle of the strong program, became the *doxa* for the field. The agent-oriented alternatives to structural analysis focused on interpretive flexibility, enrollment, boundary construction, social negotiation, and performativity. The accounts have the strength of enabling a fine-grained analysis of fact construction and technology design, but alternative approaches, such as feminist, institutionalist, and field sociologies, became marginalized as the subordinate networks of the research field.

The mainstream research programs were far from a hardened body of immutable principles. They changed over time, and the proponents of different approaches were often involved sometimes heated controversies. Laboratory studies gradually fell out of favor, and attention shifted to the construction of expertise in public fora. Some of the conceptual frameworks, such as discourse analysis and actor-network theory, tended to view society as an epiphenomenal outcome of agents' discourse and work, a point that more sociologically oriented scholars found problematic (e.g., Bloor 1999, Yearley 2005). Furthermore, the more sociological empirical program of relativism of Collins (1983) created analytical space for the study of the social shaping of outcomes of controversies by broader societal divisions. Although in practice the research that resulted from Collins's program tended to focus on the microsociology of the negotiations among a core set of participants in a controversy, some of the subsequent studies in the social construction of technology studied groups in society that became relevant for the closure of a technological controversy (Bijker et al. 1987). Likewise, social worlds theory also expanded to include the study of arenas, power, and social movements, and Clarke developed a method (situational analysis) that is in some ways parallel with field sociology (Clarke 2005, Clarke and Montini 1993, Clarke and Star 2008).

As a result, attention to structural analysis (that is, the ways in extrascientific fields such as industry, state, and social movements affect the scientific field, which in turn shapes the extrascientific fields) did not disappear entirely, but it tended to be pushed out to the subordinate networks of the field, to feminists, Marxists, Foucauldians, political sociologists, and students of career attainment. Thirty years after the demise of

the interests analysis school, much of mainstream STS research takes for granted an allergy to structural accounts in the sociology of scientific knowledge and technology. For example, a mention of interests at the 2010 joint meeting of the Society for Social Studies of Science and Japanese Society for Science and Technology Studies prompted a brief lecture by a senior STS scholar on how interests as explanatory resources have been debunked, and it was better to see interests as outcomes of agents' action.

So how does one approach the complicated and even treacherous reflexive issue of the relationship between STS theory and neoliberalism? First, one should be wary of the "short circuit" of interests analysis, which could explain a controversy in an intellectual field (here, between what might be termed structural and constructivist perspectives) as the effect of differences in the political and economic fields. In this sense one accepts the shortcomings raised by both Bourdieu and the constructivist critics of MacKenzie's statistics analysis. However, rather than simply ignore the need for structural analysis, one utilizes other methods to approach the issues, such as the study of the semiotics of misrecognition that Bourdieu had suggested.

Second, one must be clear that the relationship between extrafield political ideology and theoretical concepts and methods within a social scientific field often exists at an implicit cultural level, akin to what Foucault (1970) called epistemes and Bourdieu (2001) called habitus. Because the relationship is often implicit, it is not always consciously recognized by the social scientists. Indeed, my sense of interacting with and learning from STS social scientists over many decades is that most are social liberals or even democratic socialists, and they reject at least some of the changes in our economies, governments, and universities that are associated with neoliberal political ideologies. Thus, the analysis of the implicit neoliberal inflection of agency-based STS theory should maintain a distinction between the implicit cultural meanings of STS theory and the explicit politics of STS scholars in their role as citizens.

Third, one can develop this reflexive sociology of STS while also accepting the epistemic claim that agency-based frameworks provide insights that may not be as readily visible in functionalist, Marxist, or even field sociological accounts. In other words, the agency-based frameworks do provide a wealth of insights into specific kinds of problems in the sociology of scientific knowledge. The exercise proposed here is not a case of debunking. One can recognize the epistemic value of agency-based frameworks for some problems but also argue that they are less well-suited to study the problem of the ideological valences of the intellectual field, one needs a methodology that is closer to field sociology or cultural analysis.

Fourth, in exploring the relationship between agency-based frameworks and neoliberalism, one is in effect using the symmetry principle of the strong program. In other words, one might think of the history of STS as involving a longstanding and not always explicit controversy between a dominant network of agency-based frameworks and subordinate networks that retained a greater interest in structural explanation and extrafield influence on the scientific field. Any sociological explanation of the intellectual positions, their relationships, and the outcome of theoretical controversies would need to be symmetrical. In other words, there are ideological associations with functionalism (as indicated above) and structural approaches, such as Marxism and feminism, in addition to associations proposed for agency-based frameworks. In this sense an analysis of the possible linkages between neoliberalism and agency-based frameworks is part of a broader project of exploring the ideological valences of all frameworks in the STS field.

With those four cautions in mind, I will focus on the low-hanging fruit of actor-network theory, specifically the paradigm case of Pasteur as described by Bruno Latour (1988). No disrespect is intended for the scholar, for he has produced a body of provocative work that has left an indelible impact on the STS field. Rather, the analysis

is intended to suggest some underlying cultural resonances that might help explain the attraction of actor-network theory and its avatar, Louis Pasteur, to scholars across a range of social science and humanities fields.

The parallels between actor-network theory and neoliberalism have already been recognized by other scholars (e.g., Fuller 2000), but I wish to suggest here a method that is based on the field sociology of misrecognition. To do so, it is necessary to begin with cultural analyses of neoliberalism, that is, studies that define neoliberalism not simply as a network of policies and ideologies (or even economic theories) but also as a body of practices that become embedded in everyday routines, practices, and habitus. Here, the work by Bourdieu on neoliberalism, which focused on developing a critique of neoliberal policies and an analysis of policy field transitions, is probably less helpful than that of Foucault and scholars influenced by his work (Bourdieu 2003, 2005; Foucault 2008; Ong 2006). One of the central findings of the cultural analyses of neoliberalism is the importance of entrepreneurship as central value and practice. In a world in which long-term employment is precarious, government welfare floors are declining, organizations crave innovation, retirement funds are individually managed, and hedge-fund managers rule the world, everyone's life becomes a story of entrepreneurship, intrapreneurship, or social entrepreneurship. Obviously, the term is used broadly to include a configuration of practices and ideals that are based on the idea of self-responsibility and creative self-fashioning. To overstate, the heroes and celebrities of the day are entrepreneurs more than citizens, Steve Jobs more than Martin Luther King, Jr.

Unlike Merton's Darwin, Latour's Pasteur does not fret over norms and values, nor does he show much concern with priority or recognition within the scientific field. He is more interested in another kind of recognition, one associated with a public figure who ends up "pasteurizing" France: his technologies are widely accepted, and his name is memorialized on street signs. Merton's Darwin is a product of what Bourdieu would call the producer pole of the scientific field (Bourdieu 1991, Albert 2003). He is primarily concerned with his standing among other scientists, and hence he wishes to have priority, but he does not want to receive the rewards associated with priority if he achieves them through particularistic means, such as by blocking Wallace's access to publication. Latour's Pasteur is more of a product of the consumer pole; he is engaged in applied science and working on a highly public stage with tremendous political and economic stakes for industry and the state. This figure is, I would suggest, recognizable to students of science in the late twentieth and early twenty-first centuries, where molecular biology and computer science have become high-status and high-stakes sciences largely due to industrial linkages and technology transfer. In this sense, Latour's Pasteur is a figure who is consistent with the conditions of scientific work in the late twentieth and early twenty-first century. Of course, the fact that Pasteur is a biotechnologist from another time period also serves to remind us that discussions of neoliberalization and the scientific field must resist simple historical formulations.

But Latour's Pasteur is a neoliberal figure in another, deeper way. The laboratory is a small-scale enterprise that enables the scientist-entrepreneur to accomplish a scale shift akin to that of a small business that becomes a corporate titan. As Pasteur's network grows, he accumulates prestige, influence in the political field, and more financial resources for his science. His career follows a "sideways movement" (94) in which he is always innovating, then moving on to a new field, in the classic trajectory of the serial entrepreneur. But the sideways movement is also an upward trajectory in a field; his shifts from one problem to the next entail a heterogeneous accumulation of wealth, knowledge, and fame (that is, various forms of convertible capital). Pasteur may not found a pharmaceutical company and get rich, and the absence of such an element in the story suggests a strategy for describing a difference between the consumer-

oriented pole of science in the nineteenth century and today, but the trajectory from crystallography to fermentation and biochemistry is also one in which the network accumulates “force.”

Latour’s Pasteur is a model of the scientist as entrepreneurial capitalist, of a scientist who remakes the world not just via a theoretical or empirical breakthrough but also through technology transfer. The model of the entrepreneurial scientist was formalized in *Laboratory Life* (Latour and Woolgar 1986) in the chapter on cycles of credit, which used economic metaphors such as “cashing in” on credit. In apparent contrast, the *The Pasteurization of France* is written in the key of military metaphors, a code that is maintained consistently throughout the text. But the traffic of military and economic metaphors is widespread and easily found in any textbook of business strategy and marketing, and the transposition between the two keys is not hard to make. Occasionally, the metaphors become overtly economic, as in the description of Pasteur’s career, in which he faces “a crucial economic question,” is “captured an entire industry,” “capitalize(s) on the attention of an educated public,” and moves on to a “new economic problem” (pp. 68-69). Had Pasteur been portrayed more consistently as an entrepreneur who uses leverage (the fulcrum, p. 34), forges strategic alliances (“capturing” and assembling forces, pp. 41 and 111), recognizes strategic opportunities (obligatory points of passage, p. 44), tests the market (trials of strength, p. 115), and finds that competitors eventually emulate him (strategic reversal, p. 135), the book would probably have been less appealing rhetorically. Likewise, had the leveling of the human and the nonhuman been accomplished via one of the similar leveling categories of economic theory—capital, commodity, or product rather than “actant”—the text would have drawn attention to the elective affinities of this antisociology with rational-actor theory, and it would have raised questions about both its politics and its originality.

One might argue that Latour’s Pasteur generates distributive effects, and consequently the issue of attention to *cui bono* issues does not clearly distinguish Merton and Latour. As Latour writes, “Some lose their places (the engineers, the microbes, the public authorities); others gain their places (the Pasteurians, the hygienists)” (1988: 56). But Latour’s concern with distributional issues involves more the effects of the Schumpeterian creative destruction of entrepreneurialism. In contrast, the Mertonian concern with the allocation of credit within the scientific field and the intrusion of extrafunctional statuses into the purportedly universalistic processes shifts attention to another type of distributional issue that is more characteristic of social liberalism: exclusion and prejudice.

The elective affinity with neoliberalism in Latour’s analysis of Pasteur appears even more clearly in the subsequent generation of studies of economics and performativity. It is not necessary to cover the terrain in detail; the connections between performativity theory and neoliberalism have already been made elsewhere (e.g., Mirowski and Nik-Khah 2007, Mirowski and Sent 2008). Since the late 1990s the STS field has turned to the social studies of economics and financial technologies, and it has produced a body of literature on the ways in which economic theories and financial technologies make or shape markets (e.g., Callon 1998, MacKenzie 2006, 2009). Again, the reverse arrow of causality, the problem of how economic and political elites shape the economics profession and economic theory (Marx’s old question) is there but not central to the mainstream conversation (e.g., Babb 2004, Fourcade 2009). As a result, the mainstream of the STS studies of finance lacks the capacity to develop a critical analysis of neoliberalism as economic theory and financial technology, let alone to provide the imagination for alternative economic theories and financial technologies.

Conclusion

Field sociology—that is, an approach to social theory that attends to the political structure of quasi-autonomous social fields and the trafficking of influence among fields through capital conversion and the translation of cultural meanings—provides a more complete methodology for exploring the problems of neoliberalism, science, and technology than either Mertonian functionalism or agency-based theories. The alternative suggested here lacks a totem, an equivalent of Darwin or Pasteur, and perhaps it is just as well. If I were asked to choose a third biologist, it would likely be a nineteenth-century version of George Washington Carver or Barbara McClintock. Even better, to take Merton's caution about insiders and outsiders seriously, one might select an insider-outsider. Antoine Béchamp, a relatively well-positioned French biologist comes to mind, for a variety of reasons of subversion with respect to both Darwin and Pasteur: his emphasis on bacterial pleomorphism with its Lamarkian associations that today are being rediscovered in research on microbial DNA exchange; his emphasis on host defenses rather than microbial vectors as crucial factors in disease (a view that Pasteur is rumored to have recognized on his deathbed, when he said, "The terrain is everything"); and the legacy that led to an alternative pathway in twentieth-century medicine, such as bacterial vaccines and etiologies of cancer (Hess 1997). Although from a presentist scientific perspective anchored in today's knowledge of biology, Béchamp is a much more questionable figure, from a sociological perspective his subordinate position in the scientific field is valuable because it enables a comparative perspective on the dominant networks of the field. When one begins with the comparative perspective afforded by a subordinate position in a research field, the implicit assumptions of the dominant networks are easier to see, and when that becomes possible, the deeper cultural meanings and ideological valences become more easily recognizable.

Furthermore, field sociology offers something beyond a cultural analysis; it also suggests the need to look at the relations between institutional and social positions. The contrast of Merton's Darwin and Latour's Pasteur only suggests the beginning of a reflexive sociology of the STS field. The analysis initiated here could be deepened by exploring in more detail ideologies associated with organizational homes and positions, such as the Bureau of Applied Social Research at Columbia University or the Center for the Sociology of Innovation at the Ecole des Mines (the latter a task that Fuller 2000 has suggested). Elsewhere, I have suggested a quantitative approach to the field of the history and social studies of science and technology that relates academic positions to intellectual positions in terms of interest in race, class, labor, gender, democracy, labor, environment, and sustainability (Hess 2012). One might also take very different starting points, such as the concept of tacit knowledge and its relationship to interpretive flexibility. For example, Mirowski (2004: 55-78) has argued that Polanyi's (1998) use of tacit knowledge was closely related to his critique of the social planning associated with socialist J.D. Bernal, and his concern with the antidemocratic potential of social planning was shared by his colleague Friedrich von Hayek, the forerunner of neoliberalism. However, the uses of tacit knowledge may have lost some of their political meaning when taken up in the late twentieth-century sociology of scientific knowledge, so one can only raise a question here for further research.

One might also ask why the critique of structural explanation associated with interests analysis did not lead quickly to embracing field sociology or some other conceptual framework that retained a role for structural analysis but corrected the short-circuit of interests analysis. A possible explanation might involve a comparative analysis of underlying epistemic changes in the social science field, in which changes that occurred in STS during the 1980s were similar to changes that occurred in related research fields. For example, Geertz's interpretive anthropology drew attention to ritual

and performativity, which the next generation turned into a reflexive analysis of the ethnographic text as a rhetorical performance (Geertz 1973, Clifford and Marcus 1986). Both culture and ethnography became performative, and with the change the field also shifted toward literary criticism and away from the social sciences. Certainly in anthropology the increased interest in performativity was associated with the reflexive critique of the field's compromised history with colonialism, but that critique was not articulated in a way that drew attention to a social scientific inquiry into the economic and military conditions that enabled colonialism and neocolonialism to work. Like the STS constructivists, cultural anthropologists managed to engage in a kind of epistemic radicalism, to feel radical for slaying the dragon of colonialism in the form of the functionalist monograph, while at the same time distancing themselves from the Marxist tradition in the social sciences.

Today, as STS research has increasingly engaged problems of public policy, and likewise as neoliberalism has become increasingly contested in a wide range of countries and among diverse social movements, the need for an understanding of power in society may create the historical conditions for a revitalization of interest in structural analysis. It is likely that as the academy becomes more commercialized, the spaces for overtly Marxist analyses will continue to shrink, but frameworks that depart from a theoretical base of Marxist and culturalist structuralism, such as the work of Bourdieu and Foucault, have shown traction among younger generations. For example, there are signs that field sociology and institutional perspectives more generally are receiving renewed attention in STS, and citations to the concept of field are growing in sociology more generally (Frickel and Moore 2006, Kleinman and Albert 2012, Sallas and Zavisca 2007). It is my hope that growing attention to the need for more balanced approaches to structure, culture, and agency in STS and related research fields will provide the field with a better basis for studying the problem of neoliberalism, science, and technology. The new approaches may also help to contribute to reconceptualizing historical agency away from a Marxist unitary force, such as the proletariat or the socially responsible scientist, to a more complex understanding of the role of coalitions among justice-oriented social movements and between them and scientists.

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