Constructivism

Citation: Hess, David J. 1997. *Science Studies: An Advanced Introduction*. New York: NYU Press. Supplemental Lecture 2: “Constructivism.” www.davidjhess.org.

Although there were some attempts to develop the sociology of scientific knowledge prior to the 1970s, most notably the work of Ludwik Fleck, as a research field the sociology of scientific knowledge emerged during that decade and partly in response to Merton’s theory of norms. Michael Mulkay argued that Merton had confused the ideology of scientists with their norms and that scientists tend to articulate for strategic purposes the norms that Merton had described. To some degree, Robert Merton recognized the failure of scientists to conform to general institutional norms, and the problem led him into the study of norms and counternorms. Although there are behavioral norms in science (they can be found wherever strong sanctions apply, such as for plagiarism), Mulkay suggested that the general norms that Merton had delineated were an occupational ideology that scientists used for political purposes, such as to defend their autonomy. However, his critique involved a much deeper issue that the topic of norms. Mulkay also suggested a methdological shift from the study of how norms guide action to how scientists construct rationales for their beliefs and autonomy.[1]

Mulkay's criticism established the groundwork for what is often called the sociology of scientific knowledge (SSK) and sometimes also described as social constructivism or constructivism. The sociology of scientific knowledge focused on what is the "content" of science, or the theories, methods, design choices, and other technical aspects of science and technology. Karin Knorr-Cetina and Michael Mulkay used the term "methodological internalism" to describe the focus on content and the study of how "the 'internal' practices of the scientific enterprise constitute the focus of inquiry" (1983: 6). Unlike the more American and quantitative sociological tradition that developed from Merton's work into the study of career attainment, this area of STS research was mainly European and qualitative.

By selecting scientific knowledge as an object for sociological inquiry, the stage was set for a turf battle with philosophers that culminated in claims that the SSK was irrational, relativist, antiscience, and so on. Although there are some statements among the sociologists of scientific knowledge that might be used in support of such claims, on the whole the field was making a more modest extension of conventionalist philosophy to explore the ways in which social negotiation shaped the decisions that scientists made when vetting empirical claims, methodological differences, and theoretical frameworks. Furthermore, the descriptive problem was ultimately separable from the prescriptive issue of how knowledge claims such as theory choices should be made under ideal conditions.

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1. On critiques of norms by British sociologists of scientific knowledge, see Barnes and Dolby 1970, Mulkay 1976. Although Merton is generally considered to have studied science as an institution and not scientific knowledge, he did address the sociology of scientific knowledge in some of his essays. Hargens also commented to me that when the British critique of norms appeared, the Americans' reaction was puzzlement because they had already rejected Merton's analysis of science based on norms.

The Strong Program and Interests Analysis

In the mid-1970s a group of researchers in Edinburgh developed some of the founding documents in the new sociology of scientific knowledge. The group included David Bloor, Barry Barnes, David Edge, and Donald MacKenzie, as well as Steve Shapin and Andrew Pickering. The Edinburgh "school" at that time can be divided into two main approaches to the sociology of scientific knowledge: Bloor's strong program and interests analyses.[2]

In *Knowledge and Social Imagery,* originally published in 1976, Bloor articulated the basic tenets of the strong program in the sociology of scientific knowledge. The principles are well known and discussed in various sources (including *Science Studies: An Advanced Introduction*), and need not be reviewed here. The symmetry principle has ended up having the most pervasive influence on science studies, partly because it is directly opposed to the asymmetrical ways in which scientists often account for the outcomes of controversies. As Mulkay and Gilbert showed, scientists see their own interpretations as based on evidence and logic, whereas those of their opponents as based on social influences: I have reason and evidence, and my opponents have interests. But the symmetry principle was also a source of ongoing elaboration and transformation. Wiebe Bijker and Steve Woolgar characterized the intellectual history of the sociology of scientific knowledge and technology in terms of progressive extensions of the symmetry principle: from Merton's symmetry between science and other social institutions to Bloor's symmetry in the treatment of true and false knowledge to later

2. The history of the sociology of scientific knowledge predates the events of the 1970s. See Bachelard 1986, Bernal 1969, Fleck 1979, Hessen 1971, Mannheim 1952.

developments that argue for symmetry between science and technology, the analyst and analyzed, humans and machines, and the social and the technical.[3]

Because the strong program proposed a similar approach to the sociology of scientific knowledge and other forms of knowledge, it has sometimes been misconstrued as endorsing epistemological relativism. However, Bloor's goal was to articulate a naturalistic approach to knowledge. He argued that the causes of scientists' beliefs about true and false knowledge include both social factors and the responsiveness of the material world to experimental interventions. The point was that the latter was subject to interpretation and therefore not as definitively influential in the resolution of controversies as a simplistic or naive empiricism would have us believe.[4]

Although the impartiality and symmetry principles opened up some analytical possibilities, they foreclosed others. Because the strong program insisted on a descriptive and analytical role for the social scientist, it tended to reduce interest in the role of the social scientist as a participant in controversies. Obviously, social scientists generally do not possess the expertise to engage in scientific controversies by producing new experimental evidence, but we can intervene in scientific controversies by making evaluative statements about how the arguments of one side of a controversy were rejected without due consideration. Thus, one evaluative role for the social scientist might be to open up a space for discussions about the need for more research. The weak program of Daryl Chubin and Sal Restivo and the feminist philosophy of Helen Longino offered attempts to engage in a higher level analysis that provided

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3. Mulkay and Gilbert 1982, Bijker 1993, Woolgar 1992. The reflexivity principle received a little attention during the late 1980s with some studies that experimented with textual forms. See Ashmore 1989, Woolgar 1988.
4. Bloor 1999, Yearley 2005. Responsiveness to experimental interventions could be interpreted in different ways, depending on one's tilt toward empiricism or conventionalism.

a role for sociologists or philosophers to engage in a normative evaluation of the sides of the controversy. My essay "STS and the Development of Ethnography" develops this idea of how a methodological symmetry can be combined with a second or higher-level asymmetry, and I think this point is consistent with Bloor's approach.[5]

Another problem in the strategy of impartiality and symmetry was the discovery that even when one intended to be impartial and symmetrical, the stance would likely be interpreted as a threat by the politically more powerful side of a controversy. This finding has become known as the "captives of controversy" problem. In other words, in cases of controversy neutral analyses in the strong program tradition will tend to be captured, usually by the outgroup. The "winning" side of a controversy wants to preserve a view of the controversy as having been closed, especially as a result of the winning side's better evidence and reasoning. In contrast, the "losing" side or advocates of a marginalized perspective may find that a symmetrical, impartial analysis helps to reopen the controversy and support their calls for more research.[6]

Notwithstanding the two limitations, the view that the stance of a social scientist should begin by approaching the positions of different scientists in a neutral and fair way has been a fruitful methodological prescription for a wide range of approaches in the sociology of scientific knowledge. It plays a similar role in the sociology of scientific knowledge to that of cultural relativism in cultural anthropology, that is, the idea that the ethnographer attempts to understand the cultural practices of the field site from the perspective of the people who inhabit it. The methodological principle can be treated as independent of a later, higher level of analysis that makes a moral or epistemological judgment about the perspectives studied.

5. Chubin and Restivo 1983, Longino 1990, Bloor 1991. See also Yearley 2005 on Longino.
6. Scott, Richards, and Martin 1990.

One of the first attempts to translate the impartiality and symmetry principles into empirical research was the study of interests and science. In the interdisciplinary STS context, "interests" is another multivocal term that requires some unpacking. According to classical Marxism, some sciences (e.g., nineteenth-century political economy) encode in a technical language the values and ideology of a class (e.g., the capitalist class). One can describe this relationship as the influence or expression of class interests. However, this influence can only be seen clearly when an alternative is posed, such as Marx's alternative version of political economy. Once the point of comparison is established, then it is possible to explore the ideological valences of different positions in the scientific field. Marx's class analysis of interests is encapsulated in his argument that the ruling ideas of the day are the ideas of the ruling class, but a broader application of the idea is that the interests of any kind of social category (men, women, the rich, developed countries, a specific scientific network) shapes its ideas.[7]

The leading example of interests analysis is the study of statistical controversies in early twentieth-century Britain by Donald MacKenzie, with whom Barry Barnes shared some coauthoring. The study explored a controversy between two statisticians (and to some degree their affiliated networks), Karl Pearson and his student George Yule. MacKenzie and Barnes found that the Pearson group was associated with the biometric and eugenic laboratories of University College London, whereas Yule's following was more in the Royal Statistical Society. Although the controversy clearly involved a contest between networks of statisticians, MacKenzie took the argument beyond the framework of competing networks within a research field. He suggested in a tentative way that the intellectual positions of the different networks were connected with conflicts between the professional class and the established upper class.

7. Barnes 1977, Barnes and Shapin 1979, Marx 1977, Marx and Engels 1973.

Pearson was an advocate of eugenics and Fabian socialism, both of which were programs that were seen to benefit the professional class, whereas Yule was a conservative aristocrat with no interest in eugenics. In the Marxist tradition, the assertion went beyond the claim that there was a correlation of scientific and ideological positions to a claim of causality. In other words, the background of class conflict in some way shaped the positions of the statistics controversy. [8]

The analysis of interests was much more precise (and methodologically precarious) than the general causal relationship that Boris Hessen had drawn in his earlier Marxist analysis. He suggested only that broad changes in technology and society, such as the advent of the steam engine, had created technological problems that in turn led to the formation of new research fields, in this case the physics of thermodynamics. In other words, Hessen's form of structural analysis only suggested that broad societal changes (which he explained in Marxist terms) created the conditions of possibility for scientific change. In contrast, MacKenzie's approach was much more fine-grained. It linked a difference between specific theories at a single time period to class interests.[9]

Although there was a cluster of studies that suggested a shaping role for class interests in the polarities of scientific controversies, the analysis of interests fell out of favor. In a flurry of publications that emerged in response to the statistics study, a central objection involved the problem of how macrosociologial class interests were transformed into scientific views. Critics suggested that the analytical approach reduced scientists to rather flat, puppetlike characters who were shaped by exogenous interests rather than a complex set of contingencies and motivations. "Instead of norms," complained Steve Woolgar, "we now have interests" (1981b: 375). Indeed, Woolgar's critique of interests analysis followed that of Mulkay for Mertonian norms, and like

8. Barnes and MacKenzie 1979, MacKenzie 1983.
9. Hessen 1971.

Mulkay he proposed an agency-based account of the scientific field to replace a structural account. In this agentic methodology, the concept of interests shifted from an explanatory variable to an effect of scientists' action. Barnes and MacKenzie provided responses to the criticisms in this complex set of exchanges, but the controversy weakened the appeal of interests analyses in SSK circles.[10]

The agency-based critique of interests theory is still widely influential in the sociology of scientific knowledge, and its rejection is almost an article of faith. In effect, the critics reversed the arrow of causality from social structure to scientific agents, but they did not offer any alternatives for scholars who wish to study the problem of how social structure shapes intellectual positions. An inversion occurred rather than extension, amplification, or complex sense of interactive causality that the Marxist tradition approximated with the term dialectics of action.

There were other possibilities for accomplishing a structural analysis of scientific knowledge that did not fall victim to the problems associated with interests analysis. Certainly the critique of interest theory never refuted the broad historical sociology of knowledge associated with Marxist and feminist scholars, who argued that the broad priorities of scientific research fields were aligned with dominant social groups. Furthermore, there is also a quantitative solution to the problem of the influence of interests. One could define class background as an independent variable, and the position in the controversy as either in favor of Yule or Pearson would be the dependent variable. Other independent variables would then be measured for each statistician's view on the controversy, such as the statistician's network

10. See Barnes 1981; Callon and Law 1982; MacKenzie 1981, 1984; Woolgar 1981a, 1981b; Yearley 1982. Kim 1994, 2009 makes a more realist-oriented critique. Yearley 2005 also has a more detailed summary of the controversy, in which he was also a participant, than what is provided here.

position, problem areas, educational background, gender, and so on. In this way, one could begin to make an inference about the relative power of class interests to explain a scientist's position in the controversy. Although the approach is possible in principle, the data may not have been available. As a result, one would have to use a qualitative, comparative method to study the confounding variables that might have affected the position of statisticians who had positions in the controversy.

Another approach would be to ask the scientists involved about their perceptions of how their political ideologies and class position shaped their scientific views. Although one could survey all of the scientists involved in a controversy, it is more likely that ethnographic or semi-structured interviews would be more revealing for this subtle issue. However, because Pearson and Yule were both "equally dead," to use a phrase of Yule, the solution could only be approximated by close analysis of whatever personal documents were left behind in the archives. MacKenzie did his best with the documents that were available, but for historical documents it is difficult to use the method unless one could find the actors themselves describing the controversy in terms of their own class interests. As a result, the problem tended to motivate scholars to shift more to ethnographic methods, where it would be easier to ask scientists directly questions about their understandings of social shaping factors, either internal or external to the scientific field. Of course, when asked about interests, scientist would likely engage in an asymmetrical analysis. In other words, they might deny any extrafield interests for themselves but welcome the analysis for their opponents, so a researcher would need to piece together a story based on the ways in which interests are constructed.[11]

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11. For the founding laboratory studies, see Knorr-Cetina 1981, Lynch 1985, Latour and Woolgar 1986 (orig. 1979), Restivo and Zenzen 1982. Collins and Pinch 1982 (partly a laboratory study and partly a controversy study) and Traweek 1988 (a comparative study of

There were at least two other solutions to the problem of how broader social structural factors shape positions in a scientific controversy. One could adopt an agency-based approach and look at how extrafield agents affected the structure of the research field through control of funding and funding preferences. This approach would work best in the least autonomous research fields, and it would also be complicated by the capacity of scientists to influence the preferences of funders. In other words, one could study the interests of funders who shape the intellectual interests of scientists, but at the same time one would need to examine how scientists are able to shape the interests of funders.

A second approach is through cultural analysis. Here, at roughly the same time that the interests analyses were being rejected, Bourdieu developed an analysis of the philosophical field that recognized both the autonomy of the field and the capacity for scientists to translate extrafield meanings into scientific meanings. The mode of analysis relied on structuralism and was accomplished by charting homologies between scientific concepts, methods, or positions in a controversy and those of extrafield ideological positions. This type of analysis actually became much more widespread in the cultural studies of science, and in *Science and Technology in a Multicultural World* I suggest that "cultural constructivism" could provide a solution to the problem of imputation.[12]

My broader point is that although there were possible solutions to the problems raised by the critics of interests analysis that did not involve giving up the concern with the relationship between extrafield social structure and intellectual positions in a scientific controversy, the sociology of scientific knowledge shifted away from structural explanation toward agency-based

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laboratories) are sometimes also included as the first generation of laboratory studies. For a review, see Doing 2008.
12. Bourdieu 1981.

approaches. Sometimes described as microsociological, the approaches were not wholly limited to microsociology, and as a result the general rubric of “agency-based” frameworks is more accurate. The approaches tended to avoid the problem of how extrafield structures shaped the intellectual and social positions within the scientific field, and to the extent that they rejected structural explanation, they were limited. In my view there was an over-correction that occurred with the rejection of interests analyses. The constructivist critics threw the baby of structural analysis out with the bathwater of a rather specific form of it. As a result, a much more complicated study of the interaction of structure, agency, and meaning tended to be reduced to an emphasis on how agency produces structure and interprets meanings. Nevertheless, they did bring a necessary counterbalance to purely structural accounts, and they provided some basic insights and concepts that have been highly influential. Many of the basic concepts that people associate with “STS” are based on the theoretical frameworks of agency-based constructivism. The following sections will consider three of the most influential approaches.

EPOR

The work of Harry Collins and Trevor Pinch (originally known as the “Bath School”) shared with interests analyses the focus on scientific controversies, but the theoretical framework drew attention to the role of a small network of scientists in negotiating an outcome to controversies. Collins accepted the symmetry and impartiality principles of the strong program as methodological heuristics, and he viewed the scientific controversy as an important site for the sociology of scientific knowledge. As he argued, the controversy involved neither normal science nor a scientific revolution. In other words, controversies represent a type of science in which scientists and other actors undertake significant changes that stop short of an extensive consensus shift or scientific revolution. Collins's empirical program of relativism (EPOR) had three stages: (1) demonstrating the "interpretive flexibility" of experimental results, that is, their capacity to be subject to more than one interpretation; (2) analyzing the mechanisms by which closure is achieved; and (3) linking the mechanisms of closure to the wider social structure. Although the EPOR designation is not well known today, the translation into technology studies as SCOT, the social construction of technology, is broadly influential in the field.[13]

The third stage of this otherwise microsociological framework represented continuity with the problem defined by interests analyses, and in this sense Collins's approach represented a synthetic framework that is capable of balancing agency-based analysis with structural analysis. However, the third stage tended to remain a less central focus of the EPOR studies than the first two stages. His focus on attention was on how scientists negotiate and strategize rather than how broader social conditions shape their negotiations and strategies.[14]

To understand the mechanisms of closure in the second stage, Collins focused on the "core set" of experts and laboratories, in effect a temporary network of individuals and laboratories. He argued that the core set is not a group, because often the disagreements are so strong that the actors do not interact much socially. However, the core set has the capacity to negotiate the closure of the controversy. Social negotiation is necessary because of the lack of capacity of evidence to determine the outcomes of controversies.

Based on the concept of negotiation by the core set, Collins extended the philosophical arguments associated with the underdetermination thesis by exploring the problem of experimental replication. If one asks scientists how a controversy should be resolved, they will

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[13] Bijker et al. 1987.

14. Collins 1983. The focus on controversies and closure in the EPOR model is the leading but not only approach to the topic. I discuss some other approaches in the book *Science Studies: An Advanced Introduction*.

probably respond in an empiricist fashion and say that they would design an experiment to resolve the controversy. Furthermore, for the experiment to be credible, it must be replicable. The answer reflects a naive empiricism that is unaware of the various qualifications and counterarguments raised by the conventionalist tradition. But as we saw from the discussion of Duhem and Kuhn, even conventionalists would argue that evidence could and should play some role in theory choice. Collins did not deny the importance of evidence; rather, he built on conventionalist arguments by emphasizing that evidence is subject to interpretation. As a result, he challenged a simplistic model of replication as a purely algorithmic model, and he showed instead that the very definition of what constitutes a replicable experiment requires social negotiation, informal knowledge, craftlike technical skills, and interpretation.[15]

Collins introduced the term "experimenter's regress" to describe a problem that can emerge in negotiation among the scientists of the core set. Advocates of an empirical claim or theoretical position can explain a failure to replicate an experiment as due to the failure of the experiment, not the theory-fact claim that is being advanced. In other words, the replication was an incompetent copy of the original experimental protocol. In contrast, the scientists who interpret the failure to replicate as a failure of the knowledge claim can argue that the experiment was a competent copy that provides evidence against the original claim. To meet the criticism that their replication was flawed, they can then design another experiment to address the problems raised by the defenders of the knowledge claim, but the defenders might still disagree over the design and interpretation of the results of the second experiment, and hence it would be possible to enter into the experimenter's regress. In the case study of gravitational waves, Collins found that to obtain closure (at least for the majority of scientists involved), nonevidential factors

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15. Collins 1983, 1985.

such as strong rhetoric and the circulation of a paper on "pathological" science were necessary. In other words, even the conventionalist criteria of simplicity, consistency, and so on were inadequate to close the controversy.[16]

Collins's work does not end with the analysis of the closure of controversies. In his later work, he showed that where there are winners and losers, the losers may decide to continue on with their "cinder" of rejected science. However, the losers face long-term problems of enrolling other supporters and passing on their research programs to a next generation. By following the gravitational wave controversy over the long term, Collins was able to show how old controversies can be revitalized in new laboratories and with new methodologies. A full account of his approach is available in his 870-page magnum opus, *Gravity's Shadow*.[17]

Collins's work is significant for the interdisciplinary conversations of STS because he shows that the prescriptive models of theory choice criteria offered by the philosophers do not completely describe how decisions are made in science. The philosophers' discussions instead might be thought of as an idealized model. One might argue that the result is that science is an inherently irrational process, but most STS scholars instead adopt the perspective that scientific rationality is similar to that of other modern professions such as the law. In a courtroom trial evidence and consistency arguments matter, but they so do "extrarational" criteria (such as the rhetorical power of attorneys, exclusionary rules about what counts as evidence, and the negotiation skills of some jury members). The outcome is fallible but not irrational. In other words, the mix of decision-making criteria tends to produce good decisions in courtroom trials at least for a good percentage of cases. In this sense, it can be functional for scientists to use social criteria for theory choice (such as strong rhetoric or network loyalties). One advantage is that

16. Collins 1985.
17. Collins 1985, 2000, 2004.

social criteria allow scientists not to waste time on claims that are weak from the viewpoint of how the dominant networks of a research field view evidence, consistency, and so on. A claim that is at odds with the mainstream of the field and that comes from someone who lacks position in the field can simply be dismissed as the work of a crank, and precious resources do not need to be wasted on refuting the claim. However, the use of social criteria can also mean that some controversies are closed prematurely, and consequently the stage may be set for a reversal of consensus at some later date.

In another extension of the EPOR framework, Collins and Robert Evans developed a research program called studies of expertise and experience, or SEE. The SEE program represents a post-sociology of scientific knowledge program (a "third wave" after functionalist and constructivist sociologies of science) that involves prescriptive analysis of the conditions under which public participation in technical decisions is warranted. Closure of controversies often occurs first in the wider scientific community, which demands or needs closure, than in the core set of scientists involved in a controversy. Controversies can also become visible to the broader public before they have gone through a closure process internal to the scientific field. In cases where technical decisions come before the broader public before closure has been achieved, there is a problem of extension, that is, of who, in principle, should participate in the process of constructing closure. Collins and Evans argued that STS researchers can offer a valuable prescriptive intervention by providing advice to experts and policymakers on the problem of extension. The solution depends on the distinction between technical and political decision-making. Although public participation in political decisions should be broad, the SEE program suggests that in the case of technical decisions public participation is only warranted under limited circumstances. People or groups who can bring either contributory expertise (the capacity to generate knowledge) or interactional expertise (the capacity to understand a research field and interact with experts) to the technical decision-making process should be welcomed, whereas those who cannot should be excluded and told to participate only in broader political decisions. The argument of Collins and Evans was fairly controversial, but because the issues involve the broader STS discussion on publics and expertise.[18]

Actor-Network Theory

Another important post-strong-program theoretical framework was actor-network theory. I am classifying it under the broad rubric of the sociology of scientific knowledge, but Latour rejected the term "social constructivism," and as Bloor suggested, it is not even clear that later formulations of actor-network theory were attempts to engage in sociological explanation. An incipient form of actor-network theory can be found in one of the first laboratory studies, *Laboratory Life* and in a reply by Michel Callon and John Law to the statistics analysis of MacKenzie and Barnes. Like other agency-oriented frameworks that emerged after the demise of interests analyses, actor-network theory maintains interests as part of the analytical framework but inverts the explanatory relationship between actors and interests. In this respect, the starting point is similar to Mulkay's criticism of Mertonian norms and Woolgar's criticism of interests analyses.[19]

Because the literature on actor-networks is diverse and complicated, this brief summary can only represent an initial mapping. To do so, I will focus on three main lines of analysis: the laboratory, the modern constitution, and the performativity of economics. Of the laboratory ethnographies that emerged during the late 1970s and early 1980s, *Laboratory Life* is probably

18. Collins and Evans 2002, 2007.
19. Bloor 1999, Callon and Law 1982, Latour and Woolgar 1986.

the most influential. When one combines this book with Latour's later work on Pasteur's laboratory, it is possible to use the two studies as a first way in to actor-network theory.

In *Laboratory Life*, Latour and Woolgar argued that facts are first qualified with modalities that suggest the conditional nature of the knowledge claim (e.g., "Scientist X believes Y"). As knowledge claims become more widely established, the modalities are deleted and the statements become less specifically qualified (e.g., "As is widely known"), and in the final stage such as textbook knowledge, facts are merely assumed knowledge without any referent to the conditions of their production. Similar to what Derek de Solla Price called a "packing down" of research and Merton and Harriet Zuckerman called obliteration by incorporation, the links to specific laboratories, persons, and inscription devices become lost as the claims of a specific laboratory become more widely accepted. Furthermore, the facts become embedded as assumptions in future research and in technologies that are used both in laboratories and in the broader society. In this sense, they become black-boxed and embedded in heterogenous networks (networks of persons, knowledge, organizations, and technologies). As the networks grow, the facts become more resistant to challenge. A consensus shift is still possible, but it requires the long-term mobilization of a new network that opposes the widely held and used facts of the old one.[20]

Scientists influence the broader society by defining their work as useful to existing interests. For example, Louis Pasteur developed a vaccine that helped farmers who wanted to stop their herds of cattle from dying from anthrax. By developing new knowledge and technologies, scientists actively redefine and transform social interests by making them consistent with the interests of the scientist (or designer). Interests become a consequence of

20. Latour and Woolgar 1986, Price 1986, Merton and Zuckerman 1973.

scientists' work of "enrolling" others, not a cause that shapes scientific practice. In turn, making others "interested" is one of moments of translation that Michel Callon described as part of forming a network. Scientists must first engage in problematization, in which the laboratory becomes an obligatory point of passage: if you go through me, you will solve your problem. In Callon's terms, "We want what you want, so ally yourselves with us by endorsing our research and you will have a greater chance of obtaining what you want" (1994: 52). But in addition to framing the problem in a way that is appealing, the other parties in the network must become caught up in the network (interessement). When the network is successful, the other entities in the network are enrolled, and the laboratory becomes the spokesperson for them. In the slightly different terms of Latour's analysis of Pasteur, scientists must translate the problem of the outside world into the terms of the laboratory, then translate the results of the laboratory research into a knowledge or technology that has effects outside the laboratory. The laboratory becomes a lever that translates the large scale of the outside world into a physically smaller space, then retranslates the results of the laboratory into the larger outside world.[21]

The theory of heterogeneous network construction is broadly consistent with the EPOR model. In other words, the process of deleting modalities and embedding knowledge claims in black-boxed technologies that travel along the "rails" of networks into the broader society could occur alongside the social negotiation among the core set described by Collins. Both approaches focus on the capacity of scientists, either individually or in networks and core sets, to make knowledge. Of course, because networks are heterogenous, evidence still plays a role in the outcome of contests among networks. However, the evidence is subject to interpretive flexibility,

21. Callon 1986, 1987, 1994; Latour 1983, 1987, 1988.

and hence much rides on who has the power to produce a network that agrees with an interpretation.

Although proponents of actor-network theory share a focus on agency-based approaches with other post-strong program frameworks, they also depart from the strong program in significant ways. Latour was critical of social constructivism because, as he argued, society was used as an unproblematized explanatory resource. Latour and other advocates of actor-network theory insisted on an extended symmetry principle that involves symmetry between the natural and social worlds. From this viewpoint both worlds are the effects of the constructive processes of heterogeneous networks. Although the extension of the symmetry principle is appealing on the surface, it entails some problematic consequences. For example, Bloor noted that the strong program principle of symmetry saw both society and nature as resources for social scientists who seek to explain the cause of scientists' beliefs about nature. He argued that the resources of the social sciences were not as inadequate as Latour presumed, as long as one included systems of meaning and classification systems. Furthermore, he argued that the alternative to the strong program version of symmetry that Latour advocated, a study of the "coproduction" of nature and culture, is highly vague: "When it comes to a positive specification we find that the language, in so far as it conveys anything, begins to slip back into the more familiar language of the sociology of knowledge" (1999: 91). For example, Latour's claim that pre-existing interests have disappeared was not credible, because the concept reappeared in other guises:

> Instead of being told about the perceived coincidence of the interests of the hygienists and the inner group of Pasteurians we hear about their "angles of movement." I do not want to quibble over terminology, but do these metaphors really enable us to say

> anything deeper, different, or better than standard talk about interests? I think not (1999: 100).

A perhaps even thornier problem involves the role played by the "agency" of things in the kinds of explanations that an extended symmetry principle offers. For example, Pasteur's microbes or Callon's scallops have "agency" in the formation of the networks. The microbes and scallops have to "cooperate" in order for the network to hold together. In the essay "Epistemological Chicken," Collins and Steve Yearley found that by resting the outcome of a controversy on the agency of things, the result of actor-network theory is "an asymmetrical old-fashioned scientific story" (1992: 314). In other words, actor-network theory can end up defending an account of how scientific knowledge changes that looks very similar to empiricist approaches. For Collins and Yearley, developing the sociology of scientific knowledge in a symmetrical fashion requires a focus on how scientists come to agree on certain facts about scallops, such as the claim that scallops did anchor and then later did not. As Yearley noted in a subsequent discussion:

> Actor-network analysts still have to be able to determine what the scallops (and so on) did in fact do. This is not as simple as it sounds. Scientific controversies turn on what the data truly were. In Pasteur's famous controversy with Pouchet over the spontaneous generation of life, for every experiment in which Pouchet found microscope life in his trial vessels, Pasteur conducted one in which evidence of life did not appear....ANT wishes to argue symmetrically that Pasteur and his allies succeeded (in part) because he was able to enroll the microbes as well as human actors and organizations. But his successful enrollment of microscopic life depends on the correctness of his beliefs about that life, something that was established by the victory of his alliance (2005: 63).

Yearley argued that Latour eventually responded to the criticisms by backing away from such analysis and redefining his approach as a descriptive project of translation akin to ethnomethodology. In contrast, both Collins and Yearley argue for a sociology of scientific knowledge that allows room for social science explanation.[22]

This summary can only serve as a brief introduction to a complicated set of arguments and counterarguments. However, it suggests that there was a significant parting in the ways in the post-interests analysis among science studies scholars who took the strong program as a fundamental point of reference. Bloor, Collins, and Yearley suggested some problems with both a purely linguistic turn based on discourse analysis and the extended symmetry principle of actor-network theory. At stake in the discussions was how sociological the sociology of scientific knowledge would be, but issues in empiricist and conventionalist philosophies of science also were salient in the controversy.

Another shortcoming of actor-network theory involves the problem of why some networks are able to flourish. Because social structure is viewed as an outcome of network construction rather than both an outcome and a conditioning factor, questions about why it is possible for some actors to build large and robust networks, whereas others are unable to do so, are foreclosed. We are left with an agency-based theory that suggests either implicitly or explicitly that some actors are simply better network entrepreneurs. Like other agency-based approaches to the sociology of scientific knowledge, actor-network theory shows that scientists play an active role in constructing interests, but the framework does so by making the inverted assumption that the extrascientific actors are, in effect, enrollment dopes. Thus, the power to

22. Collins and Yearley 1992. A similar argument about the implicit empiricism of actor-network theory appears in Bloor 1999. See also Amsterdamska 1990. Gingras 1995 developed a similar critique and advances field sociology as an alternative. On Latour’s shift toward a project akin to ethnomethodology, see Latour 1999.

study how extrafield agents affect the positions in the scientific field is lost in an asymmetrical analysis of power. Although one might conclude charitably that the analysis of enrollment is a helpful corrective to the problems raised for the Edinburgh school interests analyses, one might also conclude that a fully symmetrical analysis of power would have to pay attention to the asymmetries of power. What is required is not a complete inversion of structural analysis with agency-based analysis, but an integration of the two.[23]

The same objections can be raised with the concept of the "agency" of things. In a parallel formulation of a similar problem, technology studies scholar Langdon Winner drew attention to the politics of design. Winner argued that material entities are themselves outcomes of previous generations of conflict and cooperation, and the outcomes of those social processes are inscribed in the design of things. This is a highly dialectical view of history, in which agents make structures that in turn structure the fields in which agents act. Although new generations of agents can bring about changes in the design of the material world, their action takes place on a stage and with props that are handed down from previous generations. The playing field may be partially malleable, but it is not level. In Winner's approach to the politics of design, the capacity of things to enable and constrain human action is an outcome of previous social conflict; technologies are congealed social relations. In the actor-network theory approach, the capacity of things to make the world is based on their position in a current network. History and power are narrowed down to processual analysis of network dynamics.[24]

Although a more dialectical view of agency and structure was available to actor-network theorists, they rejected it. For example, Callon used the idea of the agency of things to reject the

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23. For example, see Kleinman 2003 for an alternative, more integrated approach to the study of laboratories.
24. Winner 1986.

field sociology of Pierre Bourdieu, because the latter did not take into account the ways in which technological failure or success can influence the outcome of a conflict between groups in society. Callon's analysis of efforts by a group of engineers to build and diffuse electric vehicles in France showed that they failed because the technology was not developed enough to enable commercialization. Engineers at Renault, who supported gasoline-powered vehicles, quickly seized the technological problems with electric vehicles to bring the reform effort to a halt. Callon showed how the concept of a heterogenous network can be useful for understanding the conflict over electric vehicles and its outcome. However, the study could have also asked questions about how social structure affects the capacity of the two heterogeneous networks to be successful. For example, the government could have responded to the technological problems of the electric vehicle by redoubling efforts, perhaps even of a Manhattan Project level, to speed up the pace of technological development. Here, Bourdieu's concepts of the relations among social fields and the importance of the field of power would provide a helpful antidote to a conceptual framework that ignores the shaping influence of both social structure and systems of cultural meaning on a scientific or technical controversy. Although engineers will always encounter reverse salients in their efforts to develop technology, often the reverse salients can be overcome if there are resources available that favor new technology development. Clearly, engineers are agents who partially determine the flow of resources to favorite projects, but other agents also have their own interests that may resist enrollment into the engineers' frames.[25]

A similar shortcoming reemerges in a subsequent wave of studies on performativity, financial technologies, and economic theory. STS studies of economics and finance, led by Callon and MacKenzie, focused on the problem of how economic theories and financial models

25. Callon 1987. On reverse salients, see Hughes 1987.

shape markets. The concept of performativity is interesting partly because it addresses the problem of responses to a failure to perform that was just raised. For example, failures of financial technologies may lead to interventions that in turn restore the "empirical validity" of the financial model. Thus, the models do more than formulate pictures of a financial world; they also conjure it into place and discipline it. Actor-network theory enables an analysis of economic models and theories that are viewed as embedded in heterogeneous arrangements that have different capacities to act depending on their configuration. The approach involves a contrastive analysis of different "agencements" or configurations of economic theory, actors, organizations, and technologies, but it tends to stop short of the analysis of configurations that do not become part of policy and economic practice. Again, the questions of economic inequality and political power that are closely interwoven with the creation, selection, and use of economic models are not central to the analytical framework. In contrast, Philip Mirowski and Edward Nik-Khah suggested an approach that asks who makes economists and what kinds are choices are available among the types of markets and economic processes that policymakers and economists are constructing. They suggest that a more balanced or symmetrical approach to power would explore both the power of economic models to make markets and the role of powerful economic and political agents to shape which economic models become dominant in public policies and business practices.[26]

Notwithstanding the various shortcomings, there are some useful tools that emerge from the study of actor-networks. The concepts of heterogeneous networks, enrollment, and obligatory point of passage have become part of the general vocabulary of the field. Although the strong

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26. Callon 1998, 2007; Callon et al. 2002; Knorr-Cetina 2005; MacKenzie 2006, 2009; MacKenzie et al. 2007, Mirowski and Nik-Khah 2007. For an attempt to mediate the approaches, see Breslau 2007.

form of the agency of things can lead back in empiricist accounts of knowledge and away from a sociology of scientific knowledge, other approaches to the agency of things that offer a better balance of agency and structure, such as Winner's politics of design or Bourdieu's field sociology, could be brought into conversation with the insights of actor-network theory. Thus, an appraisal of actor-network theory could utilize its achievements without being caught by some of its limitations. The main limitation is that the focus on the agency of network entrepreneurs makes it difficult to develop a general theory of which heterogeneous networks get selected for emulation and diffusion and which ones are left in the articles and laboratories of scientists. More generally, one should exercise caution before adopting uncritically a social theory that is one-sided in its attention to agency. Latour's Pasteur is a highly entrepreneurial figure whose laboratory, like the entrepreneurial firm, engages in leverage (fulcrum), strategic alliances (assemblage of forces), strategic opportunities (obligatory points of passage), and innovation (vaccines). The terms are intended to replace traditional social science concepts that have been developed to study social inequality and power (such as class, power, race, and gender), which Latour claims to render obsolete. Whether the elevation of an entrepreneurial figure and a set of concepts that are highly consistent with the business world had anything to do with the rise of neoliberalism raises a question of cultural analysis of the history of STS theory. That question cannot be answered here, but certainly the parallels have not escaped some critics of actor-network theory. What one can say is that because categories such as race, class, gender, colonialism, and industrial power tend to be absent from actor-network analyses, the capacity to analyze the politics of technology in the sense of their relationship to social inequality is weakened. Actor-network theory offers many interesting and valuable insights, but it does not contribute to the problem of in the elusive third stage of Collins's EPOR model, that is, of bringing to the sociology of scientific knowledge a more symmetrical analysis of the asymmetries power.[27]

Social Worlds Theory

Social worlds theory has sometimes been compared with actor-network theory because of the shared shift away from the conventional units of analysis of social theory (such as class, organizations, institutions, national societies, and communities) and shared emphasis on heterogeneous units at microsocial and mesosocial scales. According to Adele Clarke and Susan Leigh Star, social worlds theory does better at enabling researchers to study the "drag of history," but actor-network theory is better at "grasping emergent connections" (2008: 122). Likewise, they suggest that social worlds theory affords a more pluralistic approach to actors, whereas actor-network theory draws attention to the nodes of concentration of power in the networks, such as in the position of Pasteur.[28]

Social worlds theory is derived from the Chicago school of sociology and the symbolic interactionist tradition in American social theory. The early ethnographies in that tradition tended to focus on mesosocial phenomena such as communities, locales, and events. Social worlds are "universes of discourses," that is, of the meaning-making activities of groups of actors who share commitments to something, such as a social movement or a discipline. Actors both construct boundaries among social worlds and create shared meanings and objects that transverse social worlds. As a result, social worlds theory also has some similarities with "discourse analysis" and ethnomethodology. However, it differs from those approaches in several important regards, most

27. For critiques of actor-network theory from a broader cultural and political perspective, see Fuller 2000, Mirowski and Nik-Khan 2007, and my comparison of Merton and Latour with respect to liberalism (in *Social Epistemology*).
28. Clarke 1990.

importantly the focus on work, objects, and activity, and the attention to meso-levels of social scale.

With respect to the focus on work, objects, and activity, Joan Fujimura showed how scientists negotiate disparate demands from different social worlds, such as those of their employer and their disciplines. Scientists must select “doable” problems and theory-methods packages that align different social worlds in a process similar to the enrollments of actor-network theory. When they are successful in constructing the alignments, as in the case of cancer research based on molecular biology in the United States during the 1980s, a “bandwagon” of research, researchers, and institutions can emerge. A bandwagon might be viewed as a particular type of rapid network growth.[29]

Social worlds both construct boundaries and negotiate relationships across them. On this point perhaps the most influential concept from social worlds theory is “boundary objects.” Leigh Star and James Griesemer defined them as objects that “inhabit several intersecting social worlds...and satisfy the informational requirements of each of them” (1989: 393). For example, the specimens collected by museums become boundary objects that enable the cooperation among zoologists, university administrators, patrons, curators, members of scientific clubs, and taxidermists. Subsequently, Geoff Bowker and Leigh Star extended the concept to include “boundary infrastructures” for more institutionalized structures that operate at a higher level of scale than boundary objects. Again, the concept is used to enable the analysis of a network-like entity but one that takes into account different meanings and uses of a technology or material object as it is employed in different social worlds. Standardized packages are like boundary objects, but they change practices across social worlds, such as when a cooperative agreement is

29. Fujimura 1987, 1992.

signed. Star noted that there is a tendency for boundary objects to undergo standardization, but standardization in turn generates residual categories and communities of practice of outsiders, who in turn generate new boundary objects.[30]

Social worlds theory can also be used to develop an analysis of different viewpoints or standpoints on a scientific or technological issue that takes into account multiple social worlds in an arena. In a study of the abortifacient RU486, Adele Clarke and Theresa Montini explored a range of different perspectives from scientists and birth-control organizations to politicians and a government regulatory agency. RU486 was not the same "thing" viewed differently but "different things to different social worlds in the arena" (Clarke and Star 2008: 123). There were also significant differences within social worlds.[31]

As social worlds theory has developed, it has increasingly paid attention to the broader "arenas" in which social worlds interact. Clarke's study of reproductive science and contraceptive technology contrasts the social world of reproductive sciences with the broader "reproductive arena" that included the social worlds of women's birth control advocates, population control advocates, eugenics movements, and philanthropic sponsors. She found that maverick scientists who were located outside university settings with support from the pharmaceutical industry and/or philanthropists accepted the challenge of working on contraceptive technologies. However, in the process they defined women's reproductive technologies in a narrow way that excluded some alternatives that might have placed control of the technologies more in the hands of women (such as more biological and rhythm-oriented methods). A subsequent development of social worlds and arena theory is Clarke's work on

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30. On boundary infrastructures, see Bowker and Star 1999. On standardized packages, see Fujimura 1992. On the cycle of standardization and residual categories, see Star 2010.
31. Clarke and Montini 1993. See also the subsequent discussion of the case in Clarke 2005.

situational analysis, a method that enables researchers to map situations, social worlds and arenas, and positions. In contrast with other types of the sociology of scientific knowledge discussed in this lecture, the approach is more open to the problem of how extrafield agents and structural conditions shape epistemic and social action within the scientific field. In this sense, the concept of arenas can be approximated to that of social fields.[32]

The Coproduction of Nature and Society

In *We Have Never Been Modern*, Latour outlined an argument that builds on actor-network theory and the study of boundaries in science but is analytically distinguishable from it. His work departed from the dominant tradition of studies of Western modernity in sociology and anthropology, which interpreted modernity as a cultural transformation in which institutions were increasingly based on individualism, universalism, and egalitarianism. Instead, Latour suggested that as an epistemic event modernity involved a double separation, not only between the state and science but also between both and the unrecognized world of hybrid entities. In the construction of the nature/culture relationship in early modern Western Europe, science as an institution came to occupy a particularly important place as a social field that excluded political power and instead empowered the processes of group observation among qualified peers to make credible knowledge claims about the natural world. Furthermore, the construction of the quasi-autonomous scientific field also entailed banishing religion from the epistemic machinery of world-making. More precisely, the epistemic functions of the religious field were severely limited to the certification of knowledge as defined by the religious field, such as doctrinal or spiritual knowledge.

32. Clarke 1998, 2005.

In this argument, one can see some hints of influence of previous generations of functionalist and structuralist theory. For example, the functionalist view that modern science emerged due to the achievement of a quasi-autonomous functional system, freed from control by church and state, is consistent with Latour's analysis of the scientific field as constituted by its exclusion of political power. Likewise, the fundamental cultural problem of defining, distinguishing, and mediating the categories of nature and culture that appears throughout the corpus of Lévi-Strauss reemerges here in an analysis of nature, culture, and modernity, but the focus of Lévi-Strauss on nature-culture mediation is replaced by the idea of hybrids. Up to this point, then, Latour's analysis might be viewed as a synthesis of two theoretical traditions.[33]

But for Latour modernity as a cultural order is different from the focus on differentiation in Durkheim, bureaucratic universalism in Weber, or even the expansion of scale among the dominant classes in the Marxist and world systems theory traditions. He argued that the modern cultural order is based on a division of the world into nature, which science represents, and humans, which the state represents, and that the division leaves unrecognized the proliferation of hybrids, or what the STS literature would call sociotechnical systems, heterogeneous networks, and other mixes of the social and technical. In effect, the modern cultural order rests on a "purification" of hybrids into the natural and social worlds. The failure to recognize the world of hybrids renders problematic theoretical frameworks in the social sciences that would draw lines of causality from society to scientific knowledge. That work includes the social constructivism of the EPOR and SCOT programs and, more to the point, the historical study of Steve Shapin and Simon Schaffer, whose analysis of the controversy between Robert Boyle and Thomas Hobbes was the basis for Latour's theoretical critique of modernity. For Latour, the whole conceptual

33. Lévi-Strauss 1969, Ben-David 1971.

apparatus of the modern social sciences—class, gender, social structure, power, and so on—was problematized as the product of the modern constitution. He agreed with Shapin and Schaeffer that knowledge is constructed, but he argued against sociological analyses that would draw lines of causality from social structure to scientific knowledge. This line of argumentation is familiar in STS theory because it essentially repeats the arguments of Mulkay and Woolgar discussed above. In *We Have Never Been Modern*, structural analysis of any form was again made problematic because, so Latour argued, it maked society primary or unconstructed. Instead, Latour argued that there is a need for a new vocabulary that is founded on the world of hybrids and networks. Such knowledge would start from awareness of the previously invisible linkages found in heterogeneous networks. For example, we may look at a room and see people and things, but we tend not to see the complex heterogeneous networks that link bodies, clothes, chairs, tables, computers, and so on. The project of an amodern study of nature and society would focus on hybrids.[34]

Latour's intellectual strategy, to deconstruct various widely used conceptual oppositions and render them obsolete, was raised to a new level in his anthropology of modernity. In a review of Sharon Traweek's *Beamtimes and Lifetimes*, at the time one of the primary reference texts for the emerging anthropology of science, he accused her of taking for granted the distinction between society and nature that is found in a tradition of anthropological analysis that draws on the work of Emile Durkheim and Marcel Mauss and was influential for Bloor as well. Although one side of Durkheim's work, on solidarity and suicide, was a forerunner of functionalist theory, his work on religion and especially the work of his nephew Mauss on cosmology were forerunners of anthropological structuralism. The latter related cosmological

34. Shapin and Schaffer 1985, Latour 1993.

and social distinctions but, in Latour's view, kept them separate as pre-existing systems that were mapped onto each other. In a similar vein, Latour interpreted Claude Lévi-Strauss as maintaining an unjustified distinction between nonmodern and modern societies in his discussion of the engineer and bricoleur. Although one might accept the criticism of Lévi-Strauss for the essay on the engineer and bricoleur in the book *The Savage Mind*, Latour did not explore the more complex analysis of nature and culture found in *Totemism*, in which Lévi-Strauss borrowed from Saussurean linguistics to show how natural and social categories are coconstituted. However, going down that path would have undermined Latour's distinction between modern and "premodern" societies, because one could easily move from the coconstitution of natural and social orders in nonliterate societies to those in modern societies. There are many examples of the latter intellectual move in the structuralist anthropology of the 1970s, of which the concluding chapters of Marshall Sahlins's *Culture and Practical Reason* are perhaps the most well-known. This work was undoubtedly known to Traweek, who studied anthropology as well as history. Rather than follow out what structuralist anthropology actually accomplished, Latour suggested that the only way to explore the proposition of the coconstitution of nature and society is via the analysis of hybrids in heterogeneous networks. In effect, actor-network theory becomes the obligatory point of passage for a social theory of modern society that avoids the trap of "seeing double," that is, of taking for granted an epistemic division between natural and cultural orders that is embedded in the categories of the natural and social sciences.[35]

Latour's analysis is bold and provocative but ultimately highly flawed. As suggested above, a first problem is the reading of structuralism in anthropology that underacknowledges the role of parallel analyses of nature-culture mediation and "hybridity," even in modern societies.

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35. Bloor 1999; Durkheim 1965; Durkheim and Mauss 1967; Latour 1990; Lévi-Strauss 1963, 1966; Sahlins 1976; Traweek 1988.

However, a second problem comes more from theoretical currents in sociology that would raise a skeptical question about any social theory that requires banishing conceptual categories such as class, gender, race, colonial status, and power from the lexicon of social science. Certainly, reminders to use such terms with clear definitions and awareness of their conceptual and empirical limits are valuable for social scientists. But it is possible to recognize hybridity, heterogeneity, or sociotechnicality without throwing away basic concepts of social structure.[36]

A third problem emerges when one questions the extent to which the epistemic foundations of the modern cultural order actually obscured awareness of hybridity to the extent that Latour suggested. If one accepts the basic idea behind Latour's argument (and that of others, such as Bourdieu and Durkhiem), that the modern order consisted in the construction of social fields that were both more differentiated and more autonomous than in previous historical eras, then one might ask what other social fields emerged and what their relations were to the scientific field. In addition to the political field with its social science knowledges, the scientific field with its knowledge of nature, and the religious field with its theological and spiritual knowledge, there was also an increasingly autonomous economic field with its knowledge of the flows of sociotechnical goods and services. When one takes into account the emergence of a field of relatively autonomous economic activity as part of the modern constitution, then one can see that the modern order explicitly created both an epistemic recognition of hybridity and a zone in which hybrids were valued, produced, created, and even studied and theorized. One might argue that the world of products, commodities, and even their fetishism is not the same as hybrids in a Latourian sense, and it may be possible to build a case for some differences. However, when one is looking at the epistemic dimensions of modernity as a cultural order, the

36. Kleinman 2003, Marx and Engels 1973.

modern order not only brings into being a proliferation of capital goods, commodities, products, and so on but also creates a means for apprehending them via the epistemic functions of economic field, that is, the emergent science of political economy and later of economics. Alongside the social contract theorist, modern natural philosopher, and the other characters of the scientific field emerge the capitalist, inventor, professional, and wage-laborer in the economic field. Of course, if one accepts this line of argumentation, then one is quickly led to question the pretension of this version of actor-network theory to sweep aside five centuries of social theory, and instead one is invited to compare actor-network theory with other currents in the sciences of things, from the philosophy of technology to political economy and economics.

The connections between hybridity and commodities again suggest an affinity between actor-network theory and modern economics, this time not through the figure of the scientist as entrepreneur but through the figure of the hybrid or sociotechnical as the product. Pasteur invents a hybrid entity, a vaccine that traverses nature-culture boundaries and destabilizes a cultural and social order, but that trajectory is well-recognized in the annals of business history as a story of invention and innovation. One begins to see in actor-network theory certain elective affinities with the theory of the firm, innovation, and business history. Such theories rest comfortably in business schools, where questions of social inequality, public ownership, social justice, and the structural conditions of poverty are relatively marginalized from the field of social science inquiry.[37]

To follow this argument out to its conclusion, one would also need to pose a Latourian question for the argument: if hybridity is not the “unconscious” of the modern cultural order that we might think it is, then what is? An alternative approach to the unconscious of the modern

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37. For a related analysis that situates both actor-network theory and modern microeconomics with respect to the development of operations research, see Mirowski and Nik-Khah 2007.

epistemic order would be to explore the other knowledges that we associate today with terms such as local knowledge, lay knowledge, women's knowledge, and so on. Uncodified and informal, those knowledges circulate in the interstices within and among formal organizations. From this perspective, the modern cultural order is defined less by the exclusion of hybrids than by the exclusion of other knowledges and the construction of the unscientific negative knowledges. The fundamental epistemic division is not the double division between the natural and political worlds, and between those worlds and that of hybrids. Instead, it is between other knowledges and the formally codified and institutionally reproduced forms of knowledge (political, economic, scientific, and theological) that comprise the world of the formally "known," including the great divisions between this-worldly knowledge (politics, products and technology, nature and society) and other-worldly knowledge (religious doctrine and experience). Thus, one might accept the basic lines of Latour's argument—that a modern cultural order was created on the basis of epistemic exclusions—but develop it in a more deeply anthropological sense that ultimately also opens the STS field to the kinds of topics that will be discussed later. The line of analysis that I am suggesting is quite similar to that of Sandra Harding, who asks the provocative question, "Who is the 'we' who have never been modern?" (2008: 45). Here, one can begin to develop a much more critical anthropology of modernity that enables attention to excluded standpoints and scientific ignorance as central to the sociology of scientific knowledge.[38]

One may or may not accept this line of argument, and in any case a cultural appreciation of actor-network theory can be separated from a pragmatic evaluation of its usefulness for studying some social science problems. There is a place for the study of technology through the

38. See Harding 2008.

lens of sociotechnical arrangements or heterogeneous networks; the lens can enable insights that might be missed with other analytical frameworks that are less resolutely focused on the politics of design and material culture. The point of the discussion is to enable a pragmatic use of actor-network theory, preferably within a more complete social theory, in ways that do not result in succumbing to its spell of novelty and its absence of attention to the dialectics of social structure and social action.

At this point it is sufficient to leave that possibility open (with a promise to return to the issue later) and to move on to another response to Latour's provocative arguments that occur more from within the sociology of scientific knowledge. In a sympathetic reading, Sheila Jasanoff built on Latour's analysis to argue for the importance of coproduction in science studies. She recognized the problems raised by critics of actor-network theory, including the institutional problem of why some networks are more highly contested than others and why some people benefit more from some kinds of networks. That problem suggests a need for greater attention to structural analysis in science studies. However, she followed constructivist critiques of interest theory by accepting the arguments that sociological analysis must also attend to how interests arise, change, and are sustained. She also rejected structural accounts of technology, which she argued are exemplified by the work of Langdon Winner and David Noble, because "social formations such as capital or class are held to be off limits for analysis and not available for reconfiguration in new attempts to solve 'problems of knowledge'" (2004b: 31). Likewise, she criticized Evelyn Fox Keller because she did "not, in a fully symmetrical, coproductionist move, consider the construction of 'gender' itself as a powerful ordering category within the varied knowledge cultures of modernity" (35). Jasanoff's framework of coproduction focuses on four pathways: making identities, making institutions, making discourses, and making representations. In the constructivist tradition, the use of the term "making" suggests an overall attempt to focus on issues of how agency affects structures. The approach is both consistent with Latour's constructivism but also more analytically open. The term "coproduction" suggests the dialectics of structure and agency, even if the emphasis on "making" suggests a primacy of agency.[39]

Having read and taught Winner, Noble, and Fox Keller (and worked with Winner) for many years, I do not find their work to be as monolithically structural as Jasanoff suggested, but let us assume for the moment that one can find instances in which at least some STS researchers some of the time view social structure as the unmoved mover and also fail to pay attention to the agency of actors. In that case, constructivist accounts of social structure such as those of Latour and Jasanoff are valuable reminders that good social theory should take into account the problem of historical agency that can modify various types of structure, including social and material. However, constructivist corrections especially of the actor-network variety tend to go too far. They do not provide the analytical tools for apprehending what Kleinman calls the "obduracy" of social structures, the pervasive inequality of power relations, the lower capacity of agents located in subordinate field positions to challenge and change their fields of action, and the opposing capacity of more powerful human agents to incorporate and transform those challenges. For many social scientists, including ones who think of themselves as STS researchers, the capacity to address those questions is not only a desirable, but a central, feature of a social theory. For us, a good social theory should strike a balance between recognition of the obduracy of social structure and its continuing reproduction and alteration through the social action of historically situated agents.

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39. Jasanoff 2004b, Noble 1984, Keller 1985, Winner 1986.

Furthermore, a good social theory should also take into account the webs of cultural meaning that historical agents both spin and work within. Even if Jasanoff may be simplifying the structuralism of scholars more deeply influenced by feminism and Marxism, her comparative research on national policy differences provides some restoration of the balance of structure, meaning, and agency that was lost in the rejection of interests analysis. However, for an explicit theorization of how one can achieve a balanced approach, Bourdieu's field theory, even with its significant shortcomings with respect to the culture concept and the analysis of long-term historical change, offers a solid basis on which to build theory. To that end, I have joined with a few others who would like to see some recuperation of Bourdieu in STS, not for the purpose of enforcing an orthodoxy, but instead for the purpose of rebalancing the structure, agency, meaning triad after decades of emphasis on agency.[40]

40. Jasanoff 2004a.

References

Amsterdamska, Olga. 1990. “Surely You Are Joking, Monsier Latour.” *Science, Technology, and Human Values* 15(4): 495-504.

Ashmore, Malcolm. 1989. *The Reflexive Thesis*. Chicago: University of Chicago Press.

Bachelard, Gaston. 1986. *The New Scientific Spirit*. Boston, MA: Beacon Press.

Barnes, Barry. 1977. *Interests and the Growth of Knowledge*. London: Routledge.

_____. 1981. “On the ‘Hows’ and ‘Whys’ of Cultural Change.” *Social Studies of Science* 11(4): 481-98.

Barnes, Barry, and R. G. A. Dolby. 1970. “The Scientific Ethos: A Deviant Viewpoint.” *Archives Européenes de Sociologie* 11: 3-25.

Barnes, Barry, and Donald MacKenzie. 1979. “On the Role of Interests in Scientific Change.” In Roy Wallis (ed.), *On the Margins of Science. Sociological Review Monograph No. 27*. Keele, Staffordshire: University of Keele. Pp. 49-65.

Barnes, Barry, and Steven Shapin, eds. 1979. *Natural Order*. Beverly Hills: Sage.

Bend-David, Joseph. 1971. *The Scientist’s Role in Society: A Comparative Study*. Chicago: University of Chicago Press.

Bernal, John. 1969. *Science in History*. Cambridge, MA: MIT Press.Bloor, David. 1991 *Knowledge and Social Imagery*. 2nd edition. Chicago: University of Chicago Press.

_____. 1999. “Anti-Latour.” *Studies in History and Philosophy of Science Part A*. 30(1): 81-112.

Bijker, Wiebe. 1993. “Do Not Despair: There is Life after Constructivism.” *Science, Technology, and Human Values* 18(1): 113-38.

Bijker, Wiebe, Thomas Hughes, and Trevor Pinch, eds. 1987. *The Social Construction of Technological Systems*. Cambridge, MA: MIT Press.

Bloor, David. 1991 *Knowledge and Social Imagery*. 2nd edition. Chicago: University of Chicago Press.

_____. 1999. “Anti-Latour.” *Studies in History and Philosophy of Science Part A*. 30(1): 81-112.

Bourdieu, Pierre. 1975. “The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason.” *Social Scientific Information* 14(6): 19-47.

_____. 1981. *The Political Ontology of Martin Heidegger*. Stanford, CA: Stanford University Press.

_____. 1990. “Animadversiones in Mertonem.” In Jon Clark, Celia Modgil, and Sohan Midgil (eds.), *Robert Merton: Consensus and Controversy*. New York: Falmer Press. Pp. 297-301.

Bowker, Geoff, and Leigh Star. 1999. *Sorting Things Out: Classification and Its Consequences*. Cambridge, MA: MIT Press.

Breslau, Daniel. 2007. “Pure and Natural Markets: Designing Incentives for Investment in the Restructured Electricity Industry.” Paper presented at the annual meeting of the American Sociological Association, New York. http://www.allacademic.com.

Callon, Michel. 1986. “Some Elements of a Sociology of Translation: Domestication of the Scallops and Fishermen.” In John Law, (ed.), *Power, Action, and Belief*. Sociological Review Monograph No. 32 (University of Keele). London: Routledge. Pp. 196-233.

_____. 1987. "Society in the Making: The Study of Technology as a Tool for Sociological Analysis." In Wiebe Bijker, Thomas Hughes, and Trevor Pinch (eds.), *The Social Construction of Technological Systems*. Cambridge, MA: MIT Press. Pp. 83-103.

_____. 1994. "Four Models for the Dynamics of Science." In Sheila Jasanoff, Gerry Markle, James Peterson, and Trevor Pinch (eds.), *Handbook of Science and Technology*. Thousand Oaks, CA: Sage. Pp. 29-63.

Callon, Michel, and John Law. 1982. "On Interests and their Transformation: Enrollment and Counterenrollment." *Social Studies of Science* 12(4): 615-25.

Chubin, Daryl, and Sal Restivo. 1983. "The 'Mooting' of Science Studies: Research Programmes and Science Policy." In Karin Knorr-Cetina and Michael Mulkay (eds.), *Science Observed*. London: Sage. Pp. 53-83.

Clarke, Adele. 1990. "A Social Worlds Adventure." In Susan Cozzens and Thomas Gieryn (eds.), *Theories of Science in Society*. Bloomington, IN: Indiana University Press. Pp. 15-42.

_____. 1998. *Disciplining Reproduction: Modernity, American Life Sciences, and "the Problems of Sex."* Berkeley: University of California Press.

_____. 2000. "Maverick Reproductive Scientists and the Production of Contraceptives, 1915-2000+." In Ann Saetnan, Nellie Oudshoorn, and Marta Kirejczyk (eds.), *Bodies of Technology*. Columbus, OH: Ohio State University Press. Pp. 37-89.

_____. 2005. *Situational Analysis: Grounded Theory After the Postmodern Turn*. Thousand Oaks, CA: Sage.

Clarke, Adele, and Theresa Montini. 1993. "The Many Faces of RU486: Tales of Situated Knowledges and Technologial Contestations." *Science, Technology, and Human Values* 18(1): 42-78.

Clarke, Adele, and Susan Leigh Star. 2008. "The Social Worlds Framework: A Theory/Methods Package." In Edward Hackett, Olga Amsterdamska, Michael Lynch, and Judy Wajcman (eds.), *The Handbook of Science and Technology Studies*. Cambridge, MA: MIT Press. Pp. 113-138.

Collins, Harry. 1983. "An Empirical Relativist Programme in the Sociology of Scientific Knowledge." In Karin Knorr-Cetina and Michael Mulkay (eds.), *Science Observed*. Thousand Oaks, CA: Sage. Pp. 85-113.

_____. 1985. *Changing Order: Replication and Induction in Scientific Practice*. Beverly Hills: Sage.

_____. 1994. "A Strong Confirmation of Experimenter's Regress." *Studies in the History and Philosophy of Science* 25(3): 493-503.

_____. 2000. "Surviving Closure: Post-Rejection Adaptation and Plurality in Science." *American Sociological Review* 65(6): 824-845.

_____. 2004. *Gravity's Shadow: The Search for Gravitational Waves*. Chicago: University of Chicago Press.

Collins, Harry, and Robert Evans. 2002. "The Third Wave of Science Studies: Studies of Expertise and Experience." *Social Studies of Science* 32(2): 235-296.

_____. 2007. *Rethinking Expertise*. Chicago: University of Chicago Press.

Collins, Harry, Robert Evans, and Michael Gorman. 2007. "Trading Zones and Interactional Expertise." *Studies in the History and Philosophy of Science* 39(1): 657-666.

Collins, Harry, and Trevor Pinch. 1982. *Frames of Meaning: The Social Construction of Extraordinary Science*. London: Routledge.

_____. 1998. *The Golem: What You Should Know About Science*. Cambridge, UK: Cambridge University Press.

Collins, Harry, and Steven Yearley. 1992. "Epistemological Chicken." In Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: University of Chicago Press. Pp. 301-326.

Doing, Park. 2008. "Give Me a Laboratory and I will Raise a Discipline: The Past, Present, and Future Politics of Laboratory Studies in STS." In Edward Hackett, Olga Amsterdamska, Michael Lynch, and Judy Wajcman (eds.), *The Handbook of Science and Technology Studies*. Cambridge, MA: MIT Press. Pp. 279-295.

Durkheim, Emile. 1965. *The Elementary Forms of the Religious Life*. New York: The Free Press.

Durkheim, Emile, and Marcel Mauss. 1967. *Primitive Classification*. Chicago: University of Chicago Press.

Fleck, Ludwik. 1979. *Genesis and Development of a Scientific Fact*. Chicago: University of Chicago Press.

Fujimura, Joan. 1987. "Constructing Do-able Problems in Cancer Research: Articulating Alignment." *Social Studies of Science* 17(2): 257-93.

_____. 1992. "Crafting Science: Standardized Packages, Boundary Objects, and 'Translation.'" In Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: University of Chicago Press. Pp. 168-211.

Fuller, Steve. 2000. *Thomas Kuhn: A Philosophical History for Our Times*. Chicago: University of Chicago Press.

Gingras, Yves. 1995. "Following Scientists Through Society? Yes, but at Arm's Length!" In Jed Buchwald (ed.), *Scientific Practice: Theories and Stories of Doing Physics*. Chicago: University of Chicago Press. Pp. 123-148Hessen, Boris. 1971. *The Social and Economic Roots of Newton's Principia*. New York: Howard Fertig.

Harding, Sandra. 2008. *Sciences from Below: Feminisms, Postcolonialities, and Modernities*. Durham, NC: Duke University Press.

Hughes, Thomas. 1987. "The Evolution of Large Technological Systems." In Wiebe Bijker, Thomas Hughes, and Trevor Pinch (eds.), *The Social Construction of Technological Systems*. Cambridge, MA: MIT Press. Pp. 51-82.

Jasanoff, Sheila. 2004a. *Designs on Nature: Science and Democracy in Europe and the United States*. Princeton, NJ: Princeton University Press.

_____. 2004b. "Ordering Knowledge, Ordering Society." In Sheila Jasanoff, (ed.), *States of Knowledge: The Co-Production of Science and Social Order*. New York: Routledge.

Kim, Kyung-Man. 1994. *Explaining Scientific Consensus: The Case of Mendelian Genetics*. New York: Guilford.

_____. 2009. "What Would a Bourdieuian Sociology of Scientific Truth Look Like?" *Social Science Information* 48(1): 57-79.

Kleinman, Daniel. 2003. *Impure Cultures: University Biology at the Millennium.* Madison, WI: University of Wisconsin Press.

Knorr-Cetina, Karin. 1981. *The Manufacture of Knowledge*. New York: Pergamon.

Knorr-Cetina, Karin, and Michael Mulkay. 1983. "Introduction: Emerging Principles in Social Studies of Science." In Karin Knorr-Cetina and Michael Mulkay (eds.), *Science Observed*. Beverly Hills: Sage. Pp. 1-17.

_____. 2005. “How are Global Markets Global? The Architecture of a Flor World.” In Karen Knorr-Cetina and Alex Preda (eds.), *The Sociology of Financial Markets*. Oxford, UK: Oxford University Press. Pp. 38-61.

Latour, Bruno. 1983. “Give me a Laboratory and I will Raise the World.” In Karin Knorr-Cetina and Michael Mulkay (eds.), *Science Observed*. Beverly Hills: Sage. Pp. 141-173.

_____. 1987. *Science in Action*. Cambridge, MA: Harvard University Press.

_____. 1988. *The Pasteurization of France*. Cambridge, MA: Harvard University Press.

_____. 1990. “Postmodern? No, Simply Amodern! Steps Toward an Anthropology of Science.” *Studies in the History and Philosophy of Science* 21(1): 145-171.

_____. 1993. *We Have Never Been Modern*. Cambridge, MA: Harvard University Press.

_____. 1999. “On Recalling ANT.” In John Law and John Hassard, eds., *Actor Network Theory and After*. Blackwell. Pp. 15-25.

Latour, Bruno, and Steve Woolgar. 1986. *Laboratory Life: The Social Construction of Scientific Facts*. 2nd edition. Princeton, NJ: Princeton University Press.

Lévi-Strauss, Claude. 1963. *Totemism*. Boston: Beacon Press.

_____. 1969. *The Raw and the Cooked*. New York: Harper and Row.

Longino, Helen. 1990. *Science as Social Knowledge*. Princeton, NJ:: Princeton University Press.

Lynch, Michael. 1985. *Art and Artifact in the Laboratory*. London: Routledge.

_____. 2005. “Science and Technology Studies on Trial: Dilemmas of Expertise.” *Social Studies of Science* 35(2): 269-311.

MacKenzie, Donald. 1981. “Interests, Positivism, and History.” *Social Studies of Science* 11(4): 498-501.

_____. 1983. *Statistics in Britain*. Edinburgh, UK: University of Edinburgh Press.

_____. 1984. “Reply to Yearley.” *Studies in the History and Philosophy* 15(3): 251-59.

_____. 2006. *An Engine, Not a Camera: How Financial Models Shape Markets*. Cambridge, MA: MIT Press.

_____. 2009. *Material Markets: How Economic Agents are Constructed*. Oxford, UK: Oxford University Press.

MacKenzie, Donald, Fabian Muniesa, and Lucia Siu. 2007. “Introduction.” In *Do Economics Make Markets? On the Performativity of Economics*, ed. by Donald MacKenzie, Fabian Muniesa, and Lucia Siu. Princeton, NJ: Princeton University Press. Pp. 1-19.

MacKenzie, Donald, and Barry Barnes. 1979. “Scientific Judgment: The Biometry-Mendelism Controversy.” In Barry Barnes and Steve Shapin (eds.), *Natural Order*. Thousand Oaks, CA: Sage. Pp. 191-210.

Mannheim, Karl. 1952. *Essays on the Sociology of Knowledge*. Oxford, UK: Oxford University Press.

Marx, Karl. 1977. *Capital*, volume 1. New York: Random House.

Marx, Karl, and Frederich Engels. 1973. *The German Ideology*, part on, ed. C. J. Arthur. New York: International Publishers.

Merton, Robert. 1973. “The Normative Structure of Science.” In Robert Merton (ed.), *The Sociology of Science*. Chicago: University of Chicago Press. Pp. 267-278.

Merton, Robert, and Harriet Zuckerman. 1973. “Age, Aging, and Age Structure in Science.” In Robert Merton (ed.), *The Sociology of Science*. Chicago: University of Chicago Press. Pp. 497-559.

Mirowski, Philip, and Edward Nik-Khah. 2007. “Markets Made Flesh: Performativity, and a Problem in Science Studies, Augmented with Consideration of FCC Auctions.” In

Donald MacKenzie, Fabian Muniesa, and Lucia Siu (eds.), *Do Economics Make Markets? On the Performativity of Economics*. Princeton, NJ: Princeton University Press. Pp. 190-224.

Mulkay, Michael. 1976. “Norms and Ideology in Science.” *Social Science Information* 15(4/5): 637-656.

Mulkay, Michael, and Nigel Gilbert. 1982. “Accounting for Error: How Scientists Construct their Social World When They Account for Correct and Incorrect Belief.” *Sociology* 16(2): 165-183.

Price, Derek de Solla. 1986 *Little Science, Big Science...And Beyond*. New York: Columbia University Press.

Prpec, Katarina. Restivo, Sal, and Michael Zenzen. 1982. “The Mysterious Morphology of Immiscible Liquids: A Study of Scientific Practice.” *Social Science Information* 21(3): 447-473.

Sahlins, Marshall. 1976. *Culture and Practical Reason*. Chicago: University of Chicago Press.

Scott, Pam, Evelleen Richards, and Brian Martin. 1990. “Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies.” *Science, Technology, and Human Values* 15(4): 474-494.

Shapin, Steve, and Simon Schaffer. 1985. *Leviathan and the Air Pump*. Princeton, NJ: Princeton University Press.

Star, Susan Leigh. 2010. “This is Not a Boundary Object: Reflections on the Origin of the Concept.” *Science, Technology, and Human Values* 35(5): 601-617.

Star, Susan Leigh, and James Griesemer. 1989. “Institutional Ecology, ‘Translations,’ and Boundary Objects.” *Social Studies of Science* 19(3): 387-420.

Traweek, Sharon. 1988 *Beamtimes and Lifetimes.* Cambridge, MA: Harvard University Press.

Winner, Langdon. 1986. *The Whale and the Reactor*. Chicago: University of Chicago Press.

Woolgar, Steve. 1981a. “Critique and Criticism: Two Readings of Ethnomethodology.” *Social Studies of Science* 11(4): 504-14.

_____. 1981b. “Interests and Explanation in the Social Study of Science.” *Social Studies of Science* 11(3): 365-94.

_____. 1992. “Some Remarks about Positionism: A Reply to Collins and Yearley.” In Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: University of Chicago Press. Pp. 327-342.

Woolgar, Steve, ed. 1988. *Knowledge and Reflexivity*. Thousand Oaks, CA: Sage.

Yearley, Steven. 1982. “The Relationship Between Epistemological and Sociological Cognitive Interests: Some Ambiguities Underlying the Use of Interest Theory in the Study of Scientific Knowledge.” *Studies in the History and Philosophy* 13(4): 353-88.

_____. 2005. *Making Sense of Science: Understanding the Social Study of Science*. Thousand Oaks, CA: Sage.