Background Frameworks in Science and Technology Studies

Citation: Hess, David J. 2012. *Science Studies: An Advanced Introduction*. New York: NYU Press. Supplemental Lecture 1: “Background Frameworks in Science and Technology Studies.” www.davidjhess.org.

Prior to the 1970s, the study of scientific knowledge was largely the province of philosophers, who tended to explore the topic from a prescriptive perspective that utilized empirical material on a selective basis. For example, they studied the problems associated with the concept of causality and different grounds for choosing among theories and hypotheses. Although some sociologists considered the problem of scientific knowledge, for the most part sociologists of science prior to the 1970s were concerned with science as a social institution. Likewise, the anthropology of knowledge and thought rarely addressed modern, cosmopolitan science, even though it produced a rich body of research on non-Western and non-modern forms of knowledge. As a result, there was a relatively neat division of labor among the disciplines.

The division of labor was disrupted by the growth of the sociology of scientific knowledge during the 1970s and the infusion of anthropological methods into science studies via the cultural history of science and the ethnography of modern science and technology. Understanding the background of intellectual traditions across the three fields prior to the 1970s is essential for understanding some of the deeper methodological and conceptual currents that have characterized the field since then. Consequently, this lecture focuses on three strands of thought in mid-twentieth-century philosophy, sociology, and anthropology: respectively conventionalism, functionalism, and structuralism.

Conventionalism

For those who are interested in the social studies of science and technology, it is possible to enter the field without much knowledge of the neighboring discipline of philosophy. However, there were some discussions in the philosophy of science that are important to the STS field as a whole. Of greatest importance for the sociology of scientific knowledge were "conventionalist" criticisms of empiricism. Because those criticisms served as a point of reference for some of the studies of the sociology of scientific knowledge that were published during the 1970s and 1980s, it is important for STS researchers to have a basic understanding of the arguments that emerged.

Many scientists, even today, hold at least some views about science that could be characterized as empiricist. Within philosophy, the term includes a wide range of philosophical projects stretching back at least to the seventeenth century (such as the work of John Locke, who is more widely recognized for his social contract theory of government), but the focus here will be on twentieth-century empiricism. Empiricist philosophy is generally associated with the Vienna School that flourished before World War II and with contemporary philosophers in Great Britain. Because there are many flavors of empiricism and because individual philosophers changed their views over time, it is best to begin an "ideal type" of an empiricist. An "ideal type" does not necessarily match a single historical person, but it is a useful heuristic for understanding a body of related characteristics that are often found together in the beliefs of individuals.

The ideal-typical empiricist holds several of the following views: 1) because the language used to describe observations is different from that used for theory, theory and observations are distinguishable; 2) it is possible to choose between theories based purely on evidence, such as by designing a crucial experiment; 3) the context of discovery (how one creates or finds new knowledge) is quite different from the context of justification (how one makes reasoned decisions about knowledge claims); 4) the only statements that are scientifically meaningful are ones that can be in principle be verified (or at least falsified) by making observations; and 5) one arrives at theories by a process of induction from data. Putting the ideas together, an ideal-typical empiricist would claim that theoretical terms such as thermal energy are scientifically meaningless because they do not directly describe observations, such as temperature measurements. As long as the two theories are empirically equivalent or "evidentially indistinguishable," then the choice between them is not scientifically meaningful. For the ideal-typical empiricist, theories are instruments or calculating devices, but they do not reach beyond observations to provide insight into the hidden structures of the world.

There are many flavors of empiricism. An empiricist might adopt a position of realism as opposed to instrumentalism with respect to the claim that theoretical concepts have no value beyond calculation. In this context, the term "realist" would refer to someone who believes that theoretical concepts actually point to the deep structure of reality, and in this sense theory plays a more significant role in science than that of a calculating device. For a realist on this issue, theory allows scientists to leap ahead of the data and achieve insights that cannot be explained by an instrumentalist view of theory. A realist could defend the view by arguing that often theoretical terms (such a viruses or electrons) later become observables when new technologies are developed. Although a realist might reject the instrumentalist interpretation of theories, he or she might otherwise accept the principles outlined above that are associated with empiricism, especially the importance of experimentation in resolving disputes in science.

A second partial break with the principles of empiricism could involve the issue of induction. One might argue that theories are derived largely from deduction from a theoretical system rather than induction from observations. For example, the influential philosopher Karl Popper believed that scientists jump to conclusions, conjecture a hypothesis, and then try to defend it or refute it through observations. Furthermore, they do not proceed by confirming theories or laws but only by failing to falsify them. Over time they become increasingly convinced of the correctness of their views. Thus, although Popper believed in deduction, he also defended a type of verificationism that has become known as the principle that theories are scientific if they are falsifiable.[1]

A much more complete departure from the ideal-typical empiricist is described here as "conventionalism." The view was first developed by French scientists and philosophers who were influenced by the rationalist tradition of continental philosophy, and to some degree the difference between empiricism and conventionalism corresponds with other cultural differences of intellectual and institutional styles that can be found between Anglophone and continental countries. Conventionalist arguments were developed in the dialogue that took place between the history and philosophy of science, and the arguments of conventionalists influenced the highly read historian and philosopher Thomas Kuhn.[2]

---

1. For a concise comparison of Carnap and Popper, see Hacking 1983. On realism, see Boyd 1991a, 1991b. As Tom Nickles pointed out in a comment on this section, there was an important element of conventionalism in Popper's thought, because he thought that observations were established by agreement among scientists.
2. Although conventionalism is usually identified with Pierre Duhem, it may be more accurate to view Jules Henri Poincaré as the originator of the position. See Gillies 1993.

Again, it is useful to construct an ideal-typical conventionalist. The basic idea behind conventionalism is to draw attention to the role of definitions, or conventions, in the making of scientific knowledge. Their role can involve differences of scale. For example, Kuhn's emphasis was on the importance of the broad paradigm, which establishes fundamental definitions, methods, and problem areas within which empirical research occurs. Others might focus on smaller units of bundles of science and evidence, such as a research program or theory.

Two specific theses associated with conventionalism are underdetermination and theory-ladenness. As a first approximation, the underdetermination thesis of Pierre Duhem and W. V. Quine holds that evidence alone is not enough to provide adequate grounds for choosing among theories. Because evidence is subject to what sociologists of scientific knowledge later called "interpretive flexibility," advocates of a particular theory or group of theories can often, if not always, come up with ways of reinterpreting negative evidence so that they do not have to reject their theories. In other words, a broad or general theory (and especially the broad network of theories, research problems, and accepted knowledge that Kuhn called a paradigm) can be maintained in the face of contradictory observations provided that an adjustment is made in the auxiliary hypotheses or subunits of a theoretical system. An auxiliary or ad hoc hypothesis is a modification in a theory in the face of a refuting instance in order to cover that refuting instance but no further problem. For example, when astronomers found that the motion of planets did not form perfect circles, they did not have to reject the theory that planetary motion was based on circles. Instead, they added epicycles, or the little circles within the planetary orbits, which were used in early models of the solar system.[3]

3. Duhem 1982; Quine 1980; Laudan 1977, 1990; also Hesse 1980. It is helpful to keep in mind the difference between ad hoc theorizing and post hoc analyses. The latter are statistical analyses made after data have come in and reported in the discussion section of a paper. Often post-hoc

Empiricists argue that the underdetermination thesis is not as damaging to their approach as first appears. In practice it may be possible to design experiments in which auxiliary hypotheses are specifically tested as a proxy for broader theoretical commitments. Thus, advocates of two different theories may negotiate among themselves methods for testing auxiliary hypotheses. When repetitions of experiments with tests of different auxiliary hypotheses all support one theory, the advocates of the other theory or group of theories will tend to shift allegiances. Hence, to some degree a controversy can be resolved through empirical evidence. However, as Harry Collins has argued, one can see the way in which this empirical resolution of the underdetermination thesis leads to the sociology of scientific knowledge, because there is an irreducible element of social negotiation in deciding what counts as evidence and how it is interpreted. In other words, the art of agreeing on something that approaches a crucial experiment, not to mention its design and interpretation, cannot be reduced to an algorithm. There is an irreducible human element of negotiation. In this sense, evidence in science is always interpretable.[4]

The point leads to the second main principle associated with the conventionalist criticism of empiricism: the thesis of theory ladenness. Some version of the argument was defended by Duhem and later by Paul Feyerabend, Thomas Kuhn, and Norwood Hansen. It is now generally accepted that theories shape, constrain, or color both the interpretation of observations and the application of methods. For example, measuring devices used to describe observations are often

---

analyses are performed to find out why negative results were obtained. As a clearly labeled exploratory exercise for future hypothesis generation and research, they can be useful. However, when used with post-hoc explanations and when multiple post-hoc analyses are made on the same data, post-hoc analyses are considered methodologically unsound. When post-hoc analyses are not labeled and misleadingly reported as “pre-hoc” hypotheses in the results section, they are fraudulent. The term Duhem-Quine thesis may be a bit of misnomer because Duhem’s and Quine’s versions were different. See Gillies 1993.

4. Collins 1983, 1985, 1994.

the congealed outcome of previous research that in turn was based on theoretical assumptions about what can be measured and how it should be measured. Thus, a conventionalist rejects the ideal-typical empiricist's strict divisions between theories and observations, or between theoretical and observational language. An empiricist could reply that in most cases the conditioned nature of observations is not strong enough to prevent theory choice based on observations obtained in research protocols designed to evaluate or test competing theories. As Richard Boyd argues, theory dependence of methods is "now universally accepted," and "most recent innovations within the empiricist tradition have arisen from attempts to accommodate the fact that scientific methods are theory-dependent while continuing to deny that knowledge of unobservable theoretical entities is possible" (1991a: 8).

An extreme form of theory ladenness was developed by Thomas Kuhn as the incommensurability thesis, although Feyerabend also defended the idea and it has antecedents in the work of Rudolf Carnap. Originally the thesis held that the advocates of different paradigms live in such different worlds that their theories are mutually unintelligible, but over the years the thesis shifted to the problem of translatability across general theories. Another reconstruction of the thesis is that the earlier theory is not encompassed by the later one and cannot be derived from the later one. That is clearly not always the case—twentieth-century physics theory largely subsumes that of the seventeenth-century—and philosophers have consequently refined the thesis. In social studies of controversies, the incommensurability thesis has not proven very useful because theoretical opponents often are quite adept at understanding the terms of the opposing side. They are also able to bracket the translation of theoretical differences and move to direct replication of procedures and protocols. In reply to this problem with the incommensurability thesis, a conventionalist who supports the thesis could argue that although there is negotiated commensurability among theories of relatively low empirical scope, there is still incommensurability among grand theories or paradigms.[5]

However, a defender of the strong form of incommensurability faces a problem: it can result in a picture of science in which there are no rational grounds for theory choice. Scientific change, at least at this broad level, may be more like a conversion process than a change of minds led by argument and evidence. For example, the physicist Max Planck commented that changes of broad theoretical allegiances are often associated with a shift of generations. As Kuhn suggested, an older paradigm becomes extinct as an older generation dies off. His use of the "Planck effect" argument led some to accuse him of irrationalism and mob psychology. However, in a statement that appeared to contradict those who accused him of characterizing at least some aspects of scientific change as an irrational process, Kuhn defended a loose set of prescriptive criteria for theory choice that included accuracy, consistency, scope, simplicity, and fruitfulness. The term "accuracy" suggests a role for evidence, but the other terms suggest a role for non-evidentiary but "rational" criteria. In contrast, "nonrational" criteria can be understood as nontechnical considerations such as allegiances to a network of allies.[6]

Although Kuhn wavered on what incommensurability entailed, his ideas were generally consistent with those of the ideal-typical conventionalist. The conventionalist rejects purely empirical grounds as inadequate for theory choice but does not embrace nonrational criteria as a prescriptive alternative. For example, Duhem argued that a confirmation criterion could be

---

5. Carnap 1953, 1991, 1995; Kuhn 1970. On Kuhn and incommensurability, see Earman 1993; Hoynigen-Huen 1993; Kuhn 1989, 1993; Lakatos and Musgrave 1970; Sankey 1994. See also Hacking 1983, who distinguished among topic-incommensurability, dissociation, and meaning-incommensurability, and Fuller 1988, who distinguished between textual and ecological incommensurability. On Kuhn more generally, see also Fuller 1992, 2000; Nickles 2002. On understanding opposing sides, see MacKenzie and Barnes 1979.
6. Kuhn 1977, Planck 1949.

maintained for a body of theories as a whole that must approximate the totality of empirical laws or generalizations. For specific theories, theory choice criteria should include internal consistency and consistency with other theories. Furthermore, Duhem did allow for a degree of disconfirmation to settle a dispute between competing theories. In a description of theory change that sounds similar to Kuhnian paradigm shifts, he wrote, "We may find it childish and unreasonable for the [scientist] to maintain obstinately at any cost, at the price of continual repairs and many tangled-up stays, the worm-eaten columns of a building tottering in every part, when by razing these columns it would be possible to construct a simple, elegant, and solid system" (1982: 217). The frequently used example is the shift to a heliocentric view of the solar system that assumes simple elliptical orbits instead of the whistles and bells of epicycles, deferents, and equants. In this comment, Duhem also seemed to be adding simplicity and elegance as prescriptive theory choice criteria to his more general criterion of consistency. In turn, the simplicity criterion has served as an entrée for a realist account of theoretical terms.

To some degree Popper also admitted nonempirical criteria for theory choice. For example, he agreed with using the criterion of simplicity, although he interpreted it as having to do with the boldness of an empirical claim. He used the theory of gravity as the example: "The new theory should proceed from some *simple*, *new*, *and powerful*, *unifying idea* about some connection or relation (such as gravitational attraction) between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitational mass) or new 'theoretical entities' (such as field and particles)" (1963: 241). Note that Popper's use of simplicity as a theory-choice criterion takes for granted the empiricist principle that the new theory will explain the same set of facts as a rival theory. He also adds that the new theory must also have some new and testable consequences. Thus, in developing his criteria for theory choice, Popper narrowed falsification to successful, new predictions. In this sense his views were empiricist, but he also attached them to the idea that the new theory should be a simple and unifying idea, which is a nonempirical or conventionalist criterion for theory choice.

An influential empiricist reply to the conventionalist challenge was developed by Imre Lakatos, whose work was in some ways situated between that of Kuhn and Popper. Lakatos accepted the argument that theories coexist within an ocean of anomalies and therefore are not easily rejected even in the face of potentially refuting instances. However, unlike Kuhn he proposed a methodology of scientific research programs that he called a sophisticated falsificationism. Lakatos argued that the "basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a 'research program'" (1978: 110). He suggested that the supporters of a research program could defend its central theoretical propositions and survive empirical refutation of the auxiliary hypotheses, which could generally be replaced. Thus, the criteria for theory choice have to focus on the broad research program, and he proposed that one could distinguish between "progressive" and "degenerative" research programs based on their capacity to generate successful, surprising predictions. Similarly, Larry Laudan defined progress as an increasing ability to solve problems, which could be either empirical, such as resolving an anomaly, or conceptual, such as when a theory is either internally inconsistent or inconsistent with another accepted theory. The arguments of Lakatos and Laudan incorporated the conventionalist criticisms of ideal-typical empiricism but blended them in a modified or sophisticated empiricism.[7]

In the end, the general conclusion that a social scientist might draw from the exchanges among philosophers over theory choice criteria is that observations and methods are theory laden

7. Lakatos 1978, Lakatos and Musgrave 1970, Laudan 1977. See also the appraisal in Rouse 1996.

and that it would be naive to believe that choices among broad research programs can be reduced to simple experimental refutations such as a single crucial experiment. Awareness of conventionalist arguments is valuable for social scientists so that we do not think about our own practice in ways that are innocent of conventionalist arguments. The sociology of scientific knowledge extended conventionalist arguments by showing that in practice there is considerable social negotiation among scientists and that factors other than empirical evidence, simplicity, consistency, and fruitfulness affect decisions. The other criteria can include personal commitments to a research program, loyalties to members of a network, and cultural and political meanings associated with a new theory. Those other criteria are not “rational” in the sense of being based on the kinds of criteria that either empiricists or conventionalists would want to accept. At this point, philosophy and social studies part ways, because the philosophical problem here is to determine how theory choice criteria should proceed under the assumption that it is based on rational criteria. In contrast, social scientists and historians are more interested in how theory choice actually occurs. The finding that it is not an entirely rational process—that other criteria enter into the decision-making process—is a descriptive rather than prescriptive claim. But it may have prescriptive implications, because somehow science seems to work even if it does not follow a philosophical model of rationality. It works either because the social factors that influence decisions are themselves functional or because the role of rational criteria is adequately influential.

After the 1960s the philosophy of science continued to develop, with debates emerging on general issues such as naturalism and realism. Although philosophers continued to work at least partly in the world of prescription, they became more historical and descriptive, and their work also became more coterminous with high theory in specific research fields, such as physics, biology, economics, and cognitive science. As those changes occurred, the problem of deriving prescriptive approaches to theory-choice criteria and research program progress became less salient. Discussions among philosophers of science take place largely in a separate intellectual field from the social studies of science. However, the debates involving what I am calling conventionalism became an important point of departure for the sociology of scientific knowledge. As a result, it is important for students of STS to have a rudimentary understanding of the field. The understanding also helps to situate the work of Thomas Kuhn, which is often the most read (and misunderstood) work for scholars, scientists, and students who lack a basic knowledge of STS.

Functionalism

A second important theoretical framework that served as a point of reference in subsequent generations of STS was functionalism. The term “structural-functionalism” is sometimes used (such as by A.R. Radcliffe-Brown and Robert Merton), and the usage points to the connections between social structures and institutions on one side and their functional role in society on the other. However, for the present purposes only the term “functionalism” will be used.

There are many ways to define functionalism, and views toward functionalism changed among various sociologists of science. However, three basic elements comprise much of functionalist thought: 1) a social system, such as science, can be studied as a quasi-autonomous system composed of structures and functions; 2) the social system is governed by a cultural system of norms and values (and a related cultural categories); and 3) a system of rewards and sanctions channels individual interests into outcomes that are functional for the system as a whole. Functionalism was widely accepted during the middle decades of the twentieth century, and its theoretical roots can be traced to Emile Durkheim's work on solidarity, Max Weber's studies of legitimacy and religion, the nineteenth-century utilitarian philosophers, and in some cases early twentieth-century psychologists and economists. In sociology Talcott Parsons's version of functionalism dominated the field for a while and influenced Robert Merton as well. Through the Social Relations Department of Harvard and the students of that program, Parsons influenced scholars in both sociology and anthropology, including even postfunctionalist forms of anthropology such as interpretive or symbolic anthropology. For example, Clifford Geertz's influential essay "Deep Play: Notes on the Balinese Cockfight" developed an interpretive analysis of ritual that was organized along the lines of Parsonian functional systems.[8]

As for some other functionalist accounts of society, for Parsons the task of social theory was to explain the problem of order and survival, in other words what it takes for a society to keep from falling apart, to reproduce itself, and to satisfy the basic requirements of adaptation to the external environment of the natural world and other societies. The same basic problem can also be applied at a smaller scale to the study of social institutions and organizations. Parsons's general theory of action was based on four basic functional requirements: pattern maintenance, integration of units, goal-attainment, and adaptation. In human systems, the four functions correspond respectively to the cultural system, the social system, the personality system, and the organism. The functions are organized recursively; thus, within the social system the four functions correspond with the maintenance of institutionalized cultural patterns (as in schools

8. Merton's other teachers included George Sarton, Pitirim Sorokin, and L. J. Henderson. See Crothers 1987. See also Dukheim 1964, Mullins 1973b, Geertz 1973, Parsons 1966, Weber 1978.

and churches), the societal community (as in kinship and social structure), the polity, and the economy. In turn, subsystems can be broken down according to the same functions.

Robert Merton was both a very influential sociologist in general and the most influential of the functionalist sociologists of science. He was not a grand theorist like Parsons; he is known instead for having defended theories of the middle range that were more relevant to empirical research problems. Merton also used different labels at different points in his career for his theoretical approach, but again the use of the term "functionalism" here is sufficient for introductory purposes.

The background of functionalist theory is helpful to understand one of Merton's major contributions to the sociology of science: his description of what has come to be known as "Mertonian norms." The description rests on a functionalist account of norms and values in society, and it also assumes a modernist vision of science as a self-regulating social system. Values connect the social and cultural systems, whereas norms are primarily social. As Parsons wrote, norms "have regulatory significance for social processes and relationships but do not embody 'principles' which are applicable beyond *social* organization, or often even a particular social system" (1966: 18). Perhaps a clearer description of the difference between norms and values is provided by the sociologist Anthony Giddens:

> Values are abstract ideals, while norms are definite principles or rules that people are expected to observe. Norms represent the "dos" and "don'ts" of social life. For instance, monogamy—being faithful to a single marriage partner—is a prominent value in most Western Societies. In many other cultures, a person is permitted to have several wives or husbands simultaneously. Norms of behavior in marriage include, for example, how husbands and wives are supposed to behave towards their in-laws (1991: 34).

In the essay "The Normative Structure of Science," originally published in 1942 as "Science and Technology in a Democratic Order," Merton described the ethos or cultural system of science as comprised of norms and values, with institutional values legitimating norms that are expressed as prescriptions, proscriptions, preferences, and permissions. He then distinguished two types of norms. Technical norms include adequate and reliable empirical evidence, logical consistency, and systematic and valid prediction. That type of "norms" is quite similar to the prescriptive theory choice criteria developed by philosophers, except that Merton was studying them descriptively as one type of norm that guided part of the work of scientists. The other category of norms, what Merton calls "institutional imperatives," is quite different. The word "imperatives" seems unnecessarily confusing here; probably he could have used the word "norms" without any loss of meaning, and indeed these imperatives have come to be known as "Mertonian norms."[9]

Merton envisioned complementarity between technical and institutional norms, and the distinction allowed philosophers to pursue their work as a separate endeavor from the sociological analysis of functional social norms in science as an institution. The "Mertonian" or social norms of science as an institution include universalism, communism (in the sense of sharing one's research results), distinterestedness, and organized skepticism. The third and fourth norms point to the social condition of autonomy that is necessary for the existence of the scientific field. Other norms have been added, such as individualism, and Merton's original formulation has been clarified, but for the present purposes the four basic norms are sufficient.[10]

The idea of norms underlay Merton's later work in the sociology of science, but he also refined his analysis. In 1957, when he published his presidential address in the *American*

---

9. Merton 1973c.
10. Merton 1973c. On other norms, see Barber 1952, Zuckerman 1977.

Sociological Review on priorities in scientific discovery, the analysis of norms remained part of his framework, but the address marked a transition toward a concern with the study of the reward system, which in the 1960s became directed toward the analysis of status attainment and stratification. According to Joseph Ben-David, a central publication that marked the transition was Warren Hagstrom's book *The Scientific Community*, which emphasized how scientists produce research in exchange for recognition from their peers. Hagstrom viewed science as a system of competition for recognition; in other words, scientists "give" away their research for their peers to consume, but the gift is not completely free, because they hope to receive recognition in return (such as in the form of citations or prizes).[11]

Merton's 1957 paper developed a dynamic picture of the conflicting relations among norms and values. He argued that scientists held two conflicting values, such as originality, "which leads scientists to want their priority to be recognized," and humility, "which leads them to insist on how little they have been able to accomplish." In turn, the conflict among values leads to "ambivalence toward priority" (1957: 647). The strong cultural emphasis on originality can also lead to deviant practices or dysfunctionality in science, such as fraud. In subsequent writing, Merton developed his theory of cultural dynamism through conflicting norms and/or values in his concept of sociological ambivalence, which describes the situation of contradictory demands that people in general, including scientists, face as a result of conflicts among values,

11. On the historical importance of the Merton's 1957 essay for the sociology of science, see Hargens 1978, Ben-David 1978. I am indebted to Lowell Hargens and Ed Hackett, who provided helpful background historical information. Hagstrom compared the exchange of information for recognition with the gift exchange systems studied by anthropologists for precapitalist societies. See Mauss 1967. As Bourdieu later suggested, one could also think about the exchanges using the metaphor of capitalist accumulation. Bourdieu 1975.

statuses, and roles. The concepts of conflict and ambivalence enabled Merton to accommodate empirical material that showed widespread violation of the norms outlined above.[12]

Although Merton's work was influential, criticisms of his theory of norms came from several quarters. One set of criticisms came from the emerging sociology of scientific knowledge during the 1970s. This network of researchers developed the view that Merton had mistaken the ideology of science for its practice. In other words, scientists used a discourse of norms for specific purposes both internal to the scientific field and externally when justifying their autonomy to funders and administrators.

A second set of criticisms emerged more from within the sociology of science and the implications of Merton's theory for research on career attainment and social fairness in science. Much of this research focused on universalism, and I have discussed this and related literatures in the institutional sociology of science in the second chapter of *Science Studies: An Advanced Introduction*. Cumulative advantage theory—the idea that those who start graduate school in elite institutions with top mentors have a lifelong career advantages that increases over time—is important because it provides an instance of apparently nonuniversalistic processes in science as an institution. Let us consider the example of two college students, one of whom obtains a Ph.D. with a well-known mentor at a top-ranked department and the other obtains a Ph.D. from a lesser known mentor at a lower-tier department. Even if the second student produces more publications as a graduate student, the first student may end up with a better first job, which in turn entails access to greater resources and prestige. The career of the first student therefore is fast-tracked in a pattern that can lead to increasing success, whereas that of the second student may develop at a much slower pace. Of course, the basic issue that the difference in career attainment raises is

12. On ambivalence and role conflict, see Merton 1973a, 1976; Mitroff 1974.

whether or not the outcome is fair. If the first student has a higher level of talent to start with (and is rewarded for the talent by getting into a better graduate school and having a more prestigious mentor), then one might argue that the system is in some ways fair or "universalistic." If, on the other hand, students in the first student's position tend to belong to a privileged demographic (such as white and male), then the system might in fact be coding for "particularistic" criteria.

The differences in the pattern of advancement became the starting point of a vibrant career attainment literature that has important policy implications with respect to affirmative action, parental leave, and other issues involving the workplace for scientific researchers. For the present purposes, it is only necessary to underscore the idea that at least one set of norms and counternorms (universalism and particularism) generated an important and longstanding controversy in the literature on science as an institution. As occurs in many scientific controversies, there was no clear victory of critics or defenders in the controversy, and instead of closure the controversy tended to be reframed. Certainly, the research revealed that there were situations in which universalism was more or less approximated.

A third criticism of Merton's approach to norms involved the general assumptions associated with functionalist theory. Because functionalism tends to focus on the problem of how systems achieve and maintain stability over time, it tends to underplay conflict and the capacity for marginalized groups to exert transformative effects on social systems. The focus on systems also exaggerates the autonomy of the scientific field and underplays the influence of social movements, governments, and industry on fundamental issues such as choices among research agendas. A variety of frameworks has been developed to address more carefully the problem of conflict and power in science. For example, the new political sociology of science has drawn attention to the need to study the commercialization of science, the role of scientism in regulatory policy, and the effects of social movements on science. Often that work draws on the field sociology of Pierre Bourdieu.[13]

Structuralism

Structuralism has had an influence mainly on the cultural studies of science and technology. The term "structuralism" should be distinguished in this context from its use in sociology, where it may be applied to approaches that emphasize the causal shaping role of social structure, such as class, race, or gender conflict. In this book, the term "structural" will be used for approaches that emphasize social structure as a shaping factor in sociological explanations, whereas "structuralist" will be reserved for the meaning of the term in anthropological and cultural studies of science. The distinction is complicated by the existence of structuralist analyses of social structures, such as semantic analyses of kinship relations. Yet even in that case, there is a difference between a structuralist analysis of social structures, which emphasizes their cultural meaning, and a structural analysis, which emphasizes social structure as both a cause and effect of social action.

Claude Lévi-Strauss developed the culture concept by applying the structuralist theory of linguistics to kinship, myths, and other social phenomena. As anthropologists use the culture concept, it refers to the underlying categories of meaning, including "values" or normative meanings, that are shared, contested, and distributed across a group of people. Thus, a "cultural" approach to social life focuses on understanding what action, institutions, texts, and so on mean to people; in effect, it involves looking over their shoulders and trying to understand the world

13. On the new political sociology of science, see Frickel and Moore 2006.

from their point of view. But cultural analysis is not restricted to the point of view of a category of people selected for analysis. From that starting point the cultural analyst constructs broader systems of meanings, much as a linguist constructs a grammar after first learning to speak a language. Furthermore, just as the analysis of language is not restricted to its uses in literature, so the analysis of systems of meaning is not restricted to the areas of society that is popularly called the "cultural" sphere (such as religion, the arts, and the intellectual field). Rather, much as there are specific vocabularies for different walks of life, there are networks of meaning (or "cultural systems") that are specific to social fields as well as ones that cut across social fields.

Lévi-Strauss was one among many anthropologists who used the model of language as a way of thinking about culture. He followed the linguistic model of breaking down phonemes (basic sound units that listeners hear, such as the "p" and "b" of "pit" and "bit") into phonetic features that linguists generalize from observations of phonemes (such as plus or minus voice, or plus or minus aspiration). When translated into anthropology, the distinction of phoneme and phonetic feature became known in some quarters as emic versus etic types of analysis. Emic analyses represent the categories of the local culture (what things mean to the people in question), whereas etic analyses are categories produced by the social scientist based on comparative analysis of emic categories. More broadly, structuralism in anthropology provided a methodology for analyzing culture as a system of semantic oppositions that are sometimes called binaries or binary oppositions. Semantic oppositions are meaningful only in the context of a system of similarities and differences. For example, we understand a "cat" to be a cat because it is not a dog, a pig, or a cow. In the strong form of this approach, some anthropologists even argued that semantic meanings were determined by linguistic categories.

The structuralist analysis of the constitution of meaning systems predates later constructivist accounts of the coconstruction of social and technical entities and the coproduction of nature and society. In classical structuralist linguistics of Ferdinand de Saussure, the articulation of two sets of differences, sound and meaning, coconstitutes both. Lévi-Strauss extended the Saussurean framework in his analysis of totemism to suggest that in a similar way the articulation of social and natural differences coconstitute both as systems of meaning. The argument can also be found in a less developed form in the work of Emile Durheim and Marcel Mauss. This is not to say that it was the only way that social and natural differences were coconstituted, because one semantic code can be mapped onto many other semantic codes. To be more accurate, we might say that any specific mapping of two codes partially coconstitutes both.[14]

The strategy of mapping codes onto each other is widely used in the cultural analysis of science, but the formal, linguistically-oriented methodology of Lévi-Strauss gradually fell out of favor during the 1980s. One reason why Lévi-Straussian structuralism is considered outdated is because of its failure to theorize relationships between actors and structures. It can be used to study issues of social change and power, but those questions were not central in his corpus. Another factor in the decline of Lévi-Straussian structuralism was the rise of deconstruction and other approaches to cultural interpretation that focused more on ambiguity, contradiction, and paradox. Nevertheless, even though more fluid, interpretive approach emerged, often its underlying conceptual framework owed a heavy debt to structuralism. Structuralism continued to influence anthropology and cultural studies by providing a method for interpreting the meaning of action, texts, and institutions by translating codes or, more loosely, mapping meanings.

14. Durkheim 1965, Durkheim and Mauss 1967, Lévi-Strauss 1963, Saussure 1966.

Furthermore, structuralism had a significant influence on feminist science studies. As Lévi-Struass showed, one can interpret categories of nature and culture in myths by mapping them onto codes of cooking. In most circumstances, raw food is natural and cooked food is cultural. Gender in many societies mediates between codes of nature and culture, and the general argument was raised that cultural codes often involve the symbolic equation that women are to nature as men are to culture. Although Lévi-Straussian analysis has been faulted for androcentrism, the basic method of interpreting the coding of nature/culture divisions against gender divisions remained influential. Furthermore, the general strategy of mapping one set of distinctions onto others is fundamental in most cultural analyses.[15]

As in the philosophy of empiricism and the sociology of functionalism, there are significant criticisms of structuralist analysis that have been highly influential. An important line of thinking challenges a form of semiotic analysis that is based on oppositions or distinctions that may mask less visible semiotic hierarchies. For example, the categorical distinction of man and woman also exists as a semantic hierarchy in which the "man" is the encompassing term and woman is the supplemental term, to use phrases borrowed respectively from Louis Dumont (encompassment) and Jacques Derrida (supplementarity). The hierarchies are also nested, such as the use of the term "woman" to encompass both "woman" as the adult female and "girl" as the child female. Deconstructive interpretations provide many reading strategies that can surmount some of the assumptions of a formalist structuralist analysis based on binaries without consideration of hierarchy. One of the most important deconstructive approaches is to show how an apparently neutral opposition is a hierarchy, which in turn can be inverted by showing how the supplemental term can be reinterpreted as the conditions of possibility of the first. A major

---

15. Lévi-Strauss 1969, MacCormack and Strathern 1980, Ortner 1974.

use of this strategy is in feminist cultural studies, which provide new readings of old texts that bring out an overlooked standpoint such as the submerged perspective of women scientists or of laypeople. The strategy can also be used more broadly in science studies through attention to marginalized research programs and researchers.[16]

Although some science studies scholars are influenced by the deconstructionist philosophy and methodology of Derrida, in general the more prominent successor programs to Lévi-Straussian strucuralism are the "poststructuralist" work of Michel Foucault and Pierre Bourdieu. Both scholars borrowed heavily from structuralist methods, especially in their early work that compared the play of similarities and differences of meaning, or patterns of "homology," across social fields. In the *The Order of Things* Foucault compared the underlying categories of three research fields known today as economics, linguistics, and biology to show how at different time periods there were underlying patterns in the way knowledge was conceptualized and organized across all three fields. In effect, he constructed a "grammar" of the underlying cultural system of those sciences in specific time periods. He referred to the underlying order of the fields as their episteme, or an "epistemological field...in which knowledge, envisaged apart from all criteria having reference to its rational value or to its objective forms, grounds its positivity and thereby manifests a history which is not that of its growing perfection, but rather that of its conditions of possibility" (1970: xxii). The study of the changes in the episteme, which he referred to as an "archaeology" of knowledge, was different from the history of a research field. The task of the archaeologist of knowledge was to show the lateral connections among research fields at a given time as well as the consistent ways in which

16. Dumont 1980, Derrida 1974. To be clear, deconstructive analysis is also part of a tradition of philosophical skepticism, and consequently the field is much more complicated than a reading strategy.

research fields underwent changes from the Renaissance to the early modern period to the nineteenth century. For example, Foucault described the early modern or classical episteme as follows:

> The whole classical system of order...is unfolded within the space that is opened up inside representation when representation represents itself...Language is simply the representation of words; nature is simply the representation of beings; need is simply the representation of needs. The end of classical thought—and of the episteme that made general grammar, natural history, and the science of wealth possible—will coincide with the decline of representation (1970: 209).[17]

At first glance Bourdieu's concept of the habitus—"a system of largely unconscious, transposable, generative dispositions, which tends to generalize itself" (2001: 41)—seems quite similar to that of Foucault's episteme. However, the habitus is much more focused on the unconscious dispositions that enable an agent to act competently in a field of action. The disciplinary habitus includes the nomos, a way of seeing the world that is shared by the discipline and different from that of other disciplines. Bourdieu distinguished between a habitus of disciplines, trajectory (both within a field and outside it), and position. As a result, the concept is not only much more concerned with unconscious and embodied practices but also much more individualized than that of the episteme. He was especially interested in class differences in taste that could be translated into academic or scientific differences within an intellectual field. Whereas Foucault's concept of the episteme involves a horizontal structuralist analysis across

17. Another line of influence from cultural analysis is the study of scientific cultures, such as the work of Knorr-Cetina 1999 on comparative disciplinary cultures. See also my book *Science and Technology in a Multicultural World*, where I discuss various approaches to cultural analysis in STS.

research fields, Bourdieu's studies of taste and distinction involved a vertical structuralist analysis across social fields in terms of class or other social divisions.

The concept of the habitus enabled Bourdieu to carry out an analysis of the scientific field that explored the relations between scientific concepts, methods, and problem areas and the extrafield trajectory of scientists. Whereas Foucault's work on epistemes remained restricted to the underlying epistemic structures of the scientific field, Bourdieu developed an approach to cultural analysis that enabled the tracking of extrafield influence on the scientific field. A scientist brings to the scientific field semantic categories and dispositions for action that are linked to a biographical origin that can be categorized by a social address in terms of class, ethnicity, gender, nationality, and time period. Those categories and dispositions are sometimes expressed in a translated form as differences among the concepts, methods, and problem areas that are meaningful only in terms of their position in a research field. By attending to relations between the biographical or trajectory habitus and epistemic differences within an intellectual field, Bourdieu opened the door to an analysis of the interfield relations. An example of his method is the analysis of the homologies between Heidegger's philosophy and the political ideologies of his time. Rather than arguing that Heidegger intentionally attempted to import political meanings into his intellectual work, or that there was a political selection process that operated directly on the intellectual field, he showed that there is a correspondence of meanings, a homology, between philosophical and political categories. He suggested that some research fields have a "false autonomy" and rely on the "misrecognition" of their disinterest with respect to other fields.[18]

18. Bourdieu 1981, 1990.

The structuralist legacy in science studies is a set of methods that enable researchers to explore nonobvious patterns and generative structures of meaning for the scientific field. Both Foucault and Bourdieu used a comparative method to make the invisible visible, and the approaches were complementary. Whereas the analysis of epistemes remained restricted to comparisons across research fields within the scientific field, the analysis of misrecognition studied homologies between the intellectual field and the political field but did not explore homologies across scientific research fields. Although the difference may seem trivial, and certainly it invites a synthesis, it also reflected a somewhat different philosophical approach to the relationships among social scientists, science, and society among two influential scholars who were colleagues and often political allies.

Conclusion

The arguments associated with critiques of empiricism, functionalism, and structuralism did not disappear with the emergence of the sociology of scientific knowledge, cultural studies of science, and other approaches that emerged during the 1970s. The first studies of laboratories and controversies made explicit reference to conventionalist arguments. However, the sociologists of scientific knowledge use the arguments to advance a descriptive, empirical claim: because there are competing ways of making “rational” choices among interpretations of data and theories, science advances through a complicated process that involves social negotiation.

Likewise, the critique of universalism influenced both feminist science studies and a subsequent generation of research on stratification and scientific careers. Because the scientific field is presumed to be meritocratic, one would expect that inequalities in science would be based on universalistic criteria such as the volume and quality of research. However, there appear to be numerous ways by which "functionally irrelevant statuses"—social criteria that ideally do not play a role in a purely meritocratic institution, such as race and gender—find a way into patterns of career attainment, evaluation, and productivity. The questions raised by functionalist accounts of science therefore did not disappear, but became embedded in a broader literature on science as an institution.

Finally, the critique of structuralism should not be taken to mean that its influence disappeared. Both Michel Foucault and Pierre Bourdieu drew heavily on structuralism, especially during the early phases of their career. They also went beyond structuralism by attending to power, practices, and, in Bourdieu's case, the agency-structure relationship. The structuralist influence is evident in a subsequent generation of feminist and anthropological studies of biotechnologies that explore the shifting construction of cultural categories such as life, death, and nature.

References

Barber, Bernard. 1952. *Science and the Social Order*. New York: MacMillan.

Ben-David, Joseph. 1960. "Roles and Innovations in Medicine." *American Journal of Sociology* 65(6): 557-68.

_____. 1971. *The Scientist's Role in Society: A Comparative Study*. Chicago: University of Chicago Press.

_____. 1978. "Emergence of National Traditions in the Sociology of Science: The United States and Great Britain." In Jerry Gaston (ed.), *Sociology of Science*. San Francisco: Jossey-Bass. Pp. 197-218.

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.

_____. 1991. "The History of Scientific Reason." *Sociological Forum* 6(1): 3-26.

_____. 1993. *The Field of Cultural Production*. New York: Columbia University Press.
_____. 1996. *The Rules of Art: Genesis and Structure of the Literary Field*. Stanford, CA: Stanford University Press.
_____. 1998. *The State Nobility: Elite Schools in the Field of Power*. Stanford University Press.
_____. 2000. *Pascalian Meditations*. Stanford, CA: Stanford University Press.
_____. 2001. *Science of Science and Reflexivity*. Chicago: University of Chicago Press.
Bourdieu, Pierre, and Loïc Wacquant. 1992. *An Invitation to Reflexive Sociology*. Chicago: University of Chicago Press.
Boyd, Richard. 1991a. "Confirmation, Semantics, and the Interpretation of Scientific Theories." In Richard Boyd, Philip Gasper, and J.D. Trout (eds.), *The Philosophy of Science*. Cambridge, MA: MIT Press. Pp. 3-35.
Boyd, Richard. 1991b. "Observations, Explanatory Power, and Simplicity: Toward a Non-Humean Account." In Richard Boyd, Philip Gasper, and J.D. Trout (eds.), *The Philosophy of Science*. Cambridge, MA: MIT Press. Pp. 349-378.
Carnap, Rudolf. 1953. "Testability and Meaning." In Herbert Feigle and May Brodbeck (eds.), *Readings in the Philosophy of Science*. New York: Appelton-Century-Crofts. Pp. 47-92.
_____. 1991. "Logical Foundations of the Unity of Science." In Richard Boyd, Philip Gasper, and J.D. Trout (eds.), *The Philosophy of Science*. Cambridge, MA: MIT Press. Pp. 393-404.
_____. 1995. *An Introduction to the Philosophy of Science*. Mineola, NY: Dover Books.
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.
Crothers, Charles. 1987. *Robert K. Merton*. London and New York: Tavistock.
Derrida, Jacques. 1974. *Of Grammatology*. Baltimore: Johns Hopkins University Press.
Duhem, Pierre. 1982. *The Aim and Structure of Physical Theory*. Princeton, NJ: Princeton University Press.
Dumont, Louis. 1980 *Homo Hierarchicus*. Chicago: University of Chicago Press.
Durkheim, Emile. 1964. *The Division of Labor in Society*. New York: The Free Press.
_____. 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.
Earman, John. 1993. "Carnap, Kuhn, and the Philosophy of Scientific Methodology." In Paul Howich (ed.), *World Changes: Thomas Kuhn and the Nature of Science*. Cambridge, MA: MIT Press. Pp. 9-36.
Fleck, Ludwik. 1979. *Genesis and Development of a Scientific Fact*. Chicago: University of Chicago Press.
Foucault, Michel. 1970. *The Order of Things*. New York: Vintage.
_____. 1976. *History of Sexuality. Volume 1*. New York: Vintage.
_____. 1979. *Discipline and Punish*. New York: Vintage.
_____. 1980. *Power/Knowledge: Selected Interviews and Other Writings.* New York: Pantheon.

_____. 2008. *The Birth of Biopolitics: Lectures at the Collège de France, 1978-1979*. London: Palgrave Macmillan.
Frickel, Scott, and Kelly Moore, eds. 2006. *The New Political Sociology of Science: Institutions, Networks, and Power*. Madison, WI: University of Wisconsin Press.
Fuller, Steve. 1988. *Social Epistemology*. Bloomington, IN: University of Indiana Press.
_____. 1992. "Being There with Thomas Kuhn: A Parable for Postmodern Times." *History and Theory* 31(3): 241-75.
_____. 2000. *Thomas Kuhn: A Philosophical History for Our Times*. Chicago: University of Chicago Press.
_____. 2011. "The New Behemouth." *Contemporary Sociology* 39(5): 533-536.
Geertz, Clifford. 1973. *The Interpretation of Cultures*. New York: Basic Books.
Giddens, Anthony. 1979. *Central Problems in Social Theory*. New York: Macmillan.
_____. 1991. *Introduction to Sociology*. New York: W.W. Norton.
Gillies, Donald. 1993. *Philosophy of Science in the Twentieth Century*. Cambridge, MA: Blackwell.
Hacking, Ian. 1983. *Representing and Intervening*. Cambridge, UK: Cambridge University Press.
Hagstrom, Warren. 1965. *The Scientific Community*. New York: Basic Books.
Hargens, Lowell. 1978. "Theory and Method in the Sociology of Science." In Jerry Gaston (ed.), *Sociology of Science*. San Francisco: Jossey-Bass. Pp. 121-139.
_____. 1988a. "Further Evidence on Field Differences in Consensus from the NSF Peer Review Studies." *American Sociological Review* 53(1): 157-60.
_____. 1988b. "Scholarly Consensus and Journal Rejection Rates." *American Sociological Review* 53(1): 139-51.
_____. 2004. "What is the Mertonian Sociology of Science?" *Scientometrics* 60(1): 63-70.
Hesse, Mary. 1980. "Duhem, Quine, and a New Empiricism." In Harold Morick (ed.), *Challenges to Empiricism*. Cambridge, MA: Hackett Publishing Co. Pp. 208-229.
Hoyningen-Huene, Paul. 1993. *Reconstructing Scientific Revolutions*. Chicago: University of Chicago Press.
Knorr-Cetina, Karin. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, MA: Harvard University Press.
Kuhn, Thomas. 1970. *The Structure of Scientific Revolutions*. 2nd edition. Chicago: University of Chicago Press.
_____. 1977. *The Essential Tension*. Chicago: University of Chicago Press.
_____. 1989. "Possible Worlds in the History of Science." In Sture Allen (ed.), *Possible Worlds in the Humanities, Arts, and Sciences*. Berlin: W. de Gruyter. Pp. 9-32.
_____. 1993. "Afterwords." In Paul Howich (ed.), *World Changes: Thomas Kuhn and the Nature of Science*. Cambridge, MA: MIT Press. Pp. 311-341.
Lakatos, Imre. 1978. *The Methodology of Scientific Research Programmes*. Cambridge, UK: Cambridge University Press.
Lakatos, Imre, and Alan Musgrave, eds. 1970. *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge University Press.
Laudan, Larry. 1977. *Progress and Its Problems*. Berkeley: University of California Press.
_____. 1990. *Science and Relativism*. Chicago: University of Chicago Press.
Lévi-Strauss, Claude. 1963. *Totemism*. Boston: Beacon Press.
_____. 1966. *The Savage Mind*. Chicago: University of Chicago Press.
_____. 1969. *The Raw and the Cooked*. New York: Harper and Row.

MacCormack, Carol, and Marily Strathern, eds. 1980. *Nature, Culture, and Gender*. Cambridge, UK: Cambridge University Press.

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.

Mauss, Marcel. 1967. *The Gift*. New York: W.W. Norton.

Merton, Robert. 1957. "Priorities in Scientific Discovery: A Chapter in the Sociology of Science." *American Sociological Review* 22(6): 635-59.

_____. 1970. *Science, Technology, and Society in Seventeenth-Century England*. New York: Howard Fertig.

_____. 1973a. "The Ambivalence of Scientists." In Robert Merton (ed.), *The Sociology of Science*. Chicago: University of Chicago Press. Pp. 383-412.

_____. 1973b. "The Matthew Effect in Science." In Robert Merton (ed.), *The Sociology of Science*. Chicago: University of Chicago Press. Pp. 439-459.

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

_____. 1973d. "The Puritan Spur to Science." In Robert Merton (ed.), *The Sociology of Science*. Chicago: University of Chicago Press. Pp. 228-253.

_____. 1976. *Sociological Ambivalence and Other Essays*. New York: The Free Press.

_____. 1984. "The Fallacy of the Latest Word: The Case of 'Pietism' and Science." *American Journal of Sociology* 89(5): 1091-1121.

_____. 1987. "Three Fragments from a Sociologist's Notebook: Establishing the Phenomenon, Specified Ignorance, and Strategic Research Materials." *Annual Review of Sociology* 13: 1-28.

_____. 1988. "The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property." *Isis* 79(4): 606-23.

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.

Mitroff, Ian. 1974. "Norms and Counternorms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists." *American Sociological Review* 39(4): 579-595.

Mullins, Nicholas. 1972. "The Development of a Scientific Specialty: The Phage Group and the Origins of Molecular Biology." *Minerva* 10(1): 52-82.

_____. 1973a. "The Development of Specialities in Social Science: The Case of Ethnomethodology." *Social Studies of Science* 3(3): 245-73.

_____. 1973b. *Theories and Theory Groups in Contemporary American Sociology*. New York: Harper and Row.

Nickles, Thomas, ed. 2002. *Thomas Kuhn: Philosophy in Focus*. Cambridge, UK: Cambridge University Press.

Ortner, Sherry. 1974. "Is Female to Male as Nature Is to Culture?" In Michelle Rosaldo and Louise Lamphere (eds.), *Woman, Culture, and Society*. Stanford, CA: Stanford University Press. Pp. 67-88.

Parsons, Talcott. 1966. *Societies: Evolutionary and Comparative Perspectives*. Englewood Cliffs, N.J.: Prentice-Hall.

Planck, Max.1949. *Scientific Autobiography and Other Papers*. New York: Philosophical Library.

Popper, Karl. 1963 *Conjectures and Refutations*. London: Routledge.

Quine, W. V. 1980. "Two Dogmas of Empiricism." In Harold Morick (ed.), *Challenges to Empiricism*. Cambridge: Hackett Publishing Co. Pp. 46-69.

Rouse, Joseph. 1996. *Engaging Science: How to Understand its Practices Philosophically*. Ithaca: Cornell University Press.

Sankey, Howard. 1994. *The Incommensurability Thesis*. Aldershot, UK: Avery.

Saussure, Ferdinand de. 1966. *Course in General Linguistics*. New York: McGraw-Hill.

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

Weber, Max. 1978. *Economy and Society: An Outline of Interpretive Sociology*. Berkeley, CA: University of California Press.

Zuckerman, Harriet. 1977. "Deviant Behavior and Social Control in Science." In E. Sagarin, *Deviance and Social Change*. Thousand Oaks, CA: Sage. Pp. 87-139.