2 The Social Study of Science before Kuhn

Stephen Turner

The controversy over Thomas Kuhn's astonishingly successful *Structure of Scientific Revolutions* ([1962]1996), which denied the possibility of a rational account of conceptual revolutions and characterized them in the language of collective psychology, created the conditions for producing the field that became "science studies." The book was the immediate product of an existing tradition of writing about science, exemplified by the works of James Bryant Conant and Michael Polanyi, and the distal product of a literature on the social character of science that reaches back centuries. This literature was closely connected to practical problems of the organization of science and also to social theory debates on the political meaning of science. The basic story line is simple: a conflict between two views of science, one of which treats science as distinguished by a method that can be extended to social and political life, and a responding view that treats science as a distinctive form of activity with its own special problems and does not provide a model for social and political life. Interlaced with this story is a puzzle over the relationship between science and culture that flourished especially in the twenties and thirties. In this chapter I briefly reconstruct this history.

BACON, CONDORCET, AND THE BEGINNINGS OF AN EXPLANATORY INTEREST IN SCIENCE

The *fons et origo* of this discussion is Francis Bacon's vision of a political order in which the class of scientists is given power by an enlightened ruler in his House of Solomon in "The New Atlantis" ([1627]1860–62, vol. 5: 347–413). This vision had a practical effect on the attempts by the Royal Society in London to distinguish itself by its methodological practices and internal governance as a type of political body in relation to the Crown (Sprat, [1667]1958: 321–438; Lynch, 2001: 177–96; Shapin, 1994) and to do the same with parallel institutions elsewhere in Europe (Hahn, 1971: 1–34; Gillispie, 2004). The Victorians assured that Bacon would be best known for his ideas about induction as a method (cf. Peltonen, 1996: 321–24) and, as his major German expositor put it, "how his whole nature was, in every way, instinctively opposed to verbal discussions" (Fischer, 1857: 307). But Bacon's extensive body of writings included not only writings on method but also on "counselors" to the Crown, or experts, on the merits of republics, on the nature of political authority, on the proper internal organization of science, on funding and authority over science, and on collective research.

The fundamental issues of science studies can be teased out of these works, but only with difficulty, because of the intentional absurdity of Renaissance style. The main "political" argument, for example, is presented as a fiction, and like other political works by ambitious office seekers, Bacon's message is shrouded in ambiguities. The basic and most influential claim (though he was far more subtle than this) (cf. Whewell, 1984: 218–47; Fischer, 1857) was that scientific truth can be produced by following a technique of assembling facts, generalizing about them, and ascending to higher level generalizations from them; that following this method precluded contestation and controversy, which were the great evils of "the schools"; that the technique is open to all, or public and democratic, because it "places all wits . . . nearly on a level" (quoted in Peltonen, 1996: 323); that it can and should be pursued collectively or cooperatively; that it requires that the mind be freed of prejudices or assumptions (and perhaps of theories); that something like social science or "civil knowledge" was also possible and necessary; and that kings would be better able to accept counsel on the basis of merit than on the basis of trust of obedient favorites. This now familiar picture of science and its extension to the social world was then novel and radical. Bacon's politics fit with his hostility to contestation, and although his recent admirers (e.g., Peltonen, 1996) have argued that he was not the stereotypic proponent of royal absolutism and unfettered state power that he was once thought to be, Bacon's primary role in the history of political thought has been as the archenemy of Edward Coke, the judge who, as defender of the common law and the rule of law, was a key progenitor of modern liberalism (cf. Coke, 2003). It sharpens and assumes new forms.

The Baconian picture is recast in a recognizably modern form in Condorcet's "Fragment on Bacon's *New Atlantis*" ([1793]1976), and in chapter X of Condorcet's *Outlines of an Historical View of the Progress of the Human Mind* ([1795]1955), which promoted the idea that science was the engine of human progress. Condorcet deals with such issues as scientific rivalry, which he regards as a normal product of the passions of scientists for their work but which can take pathological institutional forms; with the failure to utilize talent, which he regards as a major flaw of the old Regime; with concerns about financing and the forms of scientific association and internal governance, which he resolves with an argument for science's need for autonomy, or freedom from political control; and with the need for scientific knowledge of the social and political world. The argument for autonomy is grounded on the consideration that only scientists have the capacity to govern scientific activity. Though Condorcet believed in the benefits of science and the diffusion of scientific knowledge, he, characteristically, also grasped the contrary idea that there was nothing automatic about the benefits of advances in science, and he concluded that the production of these benefits required state action.

Condorcet's preferred method of extending the benefits of science was education, by which he meant the kind of education that was useful for "citizens" and would enable them to think on their own ([1795]1955): 182).[1] But he also recognized that no educational program would make scientists and citizens epistemic equals.[2] Moreover, education was politically ambiguous: not only did it require the exercise of state power, there was a sense, which he shared with other *Philosophes*, that progress resulted from collective submission to reason and science, each understood as authoritative in its own right. Condorcet attempted to put a nonauthoritarian face on this submission to science and scientists: he expressed a "hope" that the citizens thus instructed would acknowledge the "superiority of enlightenment" of their intellectual betters in choosing leaders ([1793]1976: 283). But this would necessarily amount to a regime of expert rule, with democratic consent.

SAINT-SIMON AND COMTE: SCIENCE REPLACES POLITICS

The implication that social knowledge allowed for the replacement of politics was always the most problematic element of this picture, for it placed science and politics in direct competition. In 1803, in the aftermath of the restoration of French politics and as part of the return to normalcy after the revolution, the section on social and political science of the French Academy was suppressed (Columbia Electronic Encyclopedia, 2001–2004).[3] This action served to draw a line between acceptable science and dangerous science and to reject the extension of science to politics. One consequence was that social and political speculation, and in particular speculation on science and politics, now fell to thinkers outside the academy and on the margins of science, notably Henri de Saint-Simon. Saint-Simon's faith in scientists as the saviors of society (a faith which diminished in the course of his life) was similar to Condorcet's, and he carried forward and generalized similar concerns especially with the problem of the full utilization of talent, making this theme central to his social theory, as expressed by the slogan "Each according to his capacity, to each capacity according to its works" on the masthead of the Saint-Simonian newspaper *Le Globe* (Manuel; 1995: 163).[4]

But Saint-Simon radicalized Condorcet's invasion of the political. His explicitly antipolitical and implicitly antiliberal idea that in the future the rule of man over man would be replaced by "the administration of things" proved to have a long future in the hands of Marxist-Leninism.[5] Politics would vanish, he argued, because social antagonism would disappear in a society in which capacities were fitted to tasks. The theory of "capacities" assumed that capacities were transparent. His model was science. Within science, in Saint-Simon's view, scientific merit was sufficiently transparent that scientists would naturally recognize and defer to greatness in others, allowing for the fulfillment and utilization of talent, and creating within science a natural hierarchy. This was in turn a model for the natural hierarchy of the new scientific and industrial order he envisioned.[6] Saint-Simon's young secretary, Auguste Comte, revised and extended his sketchy but illuminating ideas into a complete intellectual system, Positivism, which provided both a philosophy of science and a model for the relations of science and society, and was also an explicit repudiation of liberalism, which Comte, like most of the advanced continental thinkers of the time, regarded as a transitory historical phenomenon doomed by its overwhelming defects (Comte, [1830–42]1864, [1877]1957).

Saint-Simon was not a methodological thinker, but Comte was. His newly christened science of "sociology" which represented the fulfillment of the dream of extending science to society and politics, required him to reflect extensively on what science was, to classify the sciences, and to give an account of method. His central "discovery," the law of the three stages, which he took to be the core finding of sociology, was a law about the internal development of scientific disciplines: the first stage was the theological or fictitious, the second the metaphysical or abstract, and the third the scientific or positive, in which such metaphysical notions as causation were supposed to disappear, leaving only predictive law (Comte, [1830–42]1858: 25–26). The principle was reflexive, indeed self-exemplifying: sociology was to be the last science to reach the positive stage, and the law predicted that it would do so. Comte never strayed far from the lessons of science as a model. Indeed, the history of science, specifically Joseph-Louis de Lagrange's history of rational mechanics (which explored the filiation and descent of ideas), was the model for the specific "historical" method that he claimed was appropriate for sociology ([1830–42]1858: 496).[7]

The laws themselves were "objective." But in the end, according to Comte's later account, when sociology had reached the positive stage, all the sciences would become subordinate to it, and the relation of all knowledge to the subject, man, would be revealed.[8] At this point the sciences would be the servants of man, by analogy to medicine. Moreover, a fully developed sociology that related all knowledge to the subject would teach the critical anti-individualist lesson of the dependence of each person on others. Sociology would be both policy science and state ideology.

Comte's account of politics was similar to Saint-Simon's but with an even more strident hostility to liberal discussion.[9] Comte ([1830–42]1864, IV: 50ff) expressed his disgust for the idea that everyone should be permitted to have their opinion heard, that the ignorant and expert should be equally empowered, and to "conscience."

> [T]here is no liberty of conscience in astronomy, in physics, or even in physiology, that is to say everyone would find it absurd not to have confidence in the principles established by the men of these sciences. (Comte [1830–42]1864, IV: 44n, trans. in Ranulf, 1939: 22)

Science, in particular the science of sociology, was consequently both model and means for overcoming the "anarchy of opinions" by providing consensus. In contrast, liberal politics and free discussion, from the point of view of the prospect of such knowledge, was no more than the politics of ignorance and pointless dissension.

Comte, to put it simply, had assembled all the elements of a powerful argument to resolve the ambiguities of Condorcet by eliminating its liberal squeamishness about authority and consent. For Comte, the issue was this: if science is correct, and science includes knowledge of the social world and politics, why shouldn't scientists rule over the ignorant, or rule through their control of education? And is not the rule, *de facto* if not *de jure,* of scientists the condition of progress? Is the public's failure to consent to such rule anything other than a failure of scientific education? And if the understanding and recognition of the authority of science are the central condition of progress, shouldn't science be imposed on the ignorant, just as the dogmas of Catholicism had been so effectively imposed in the past (Comte [1830–42]1864, IV: 22, 480; V: 231)? Given his premises, the conclusion was difficult to avoid, and even John Stuart Mill, his admirer who rejected his later work, admitted that as a matter of logic, Comte was correct ([1865]1969: 302).

THE LIBERAL CHALLENGE

Although each of these premises, and the related picture of science they depended on, would be rejected by Comte's critics, a fully coherent response, with an alternative image of science, was slow to develop. The main obstacle to constructing an alternative was the notion of scientific method itself. Although Mill was a paragon of liberalism, he was trapped between his father's faith in free discussion, which he expounded in his famous *On Liberty* ([1859]1978), and his own methodological views, which were centered on the idea that the canons of induction lead to proven knowledge. The canons produced consensus apart from discussion, by the following of rules—even in the social sciences, where their value was limited by the problem of causal complexity. Moreover, Mill was a utilitarian, who believed that moral and political questions resolved into questions of the greatest good for the greatest number. So he was compelled, in the conclusion of Book VI of *A System of Logic,* to say that questions of politics were a matter of practical science, subordinate to the principle of utility (1974). To the extent that this is true, there is much for indoctrination to be about, and little if anything for democratic discussion to be about, a point not lost on his critics (cf. Cowling, 1963).[10]

Mill did not resolve the conflict between science and free discussion. In *On Liberty,* science is simply omitted. In his address to the University of St. Andrews that discusses science, freedom of speech is commended, but for schools of theology, and although science education is discussed at length in this text, it is not mentioned in connection to free discussion. Mill's critique of the later Comte expresses concerns about the practical implementation of the authority of science. He notes that Comte's position relies on the consensus of scientists but that this authority "entrusted to any organized body, would involve a spiritual despotism" (Mill, [1865]1969: 314). But he does not question the notion of consensus itself. Mill's conflict is nevertheless deep: if science is distinguished by the possession of a consensus-producing method, its reliance on human institutions is incidental or inessential and the authority of science overrides free discussion.

There were other important, and less ambivalent, though also less direct, responses. When William Whewell wrote the history of core intellectual advances in science, he also wrote about the difficulties that major ideas had in becoming accepted, which undermined the idea that within science truth was readily recognized and acknowledged (Whewell, 1857; cf. 117–20, 130–33, 150–53, 177–79, 184–88). A section of Buckle's *History of Civilization in England*, one of the most influential works of the nineteenth century, argued that state patronage of knowledge in France had diminished French intellectual life ([1857]1924: 490–516). The idea that science was a product of routinizable methods itself became the subject of an intense debate, much of it critical of Mill. This debate set the stage for a new formulation of the basic Baconian picture of science.

PEARSON AND MACH

Although there are questions about the nature of Comte's influence on the next stage of the discussion, Ernst Mach and Karl Pearson in their writings come into focus as transitional figures between two widely separated bodies of thought: Comte's positivism and the Communist theorists of science of the 1930s. One of the latter, Lancelot. T. Hogben, recalled that his generation had "been suckled on the *Grammar of Science*," Pearson's major text on science (Hogben, 1957: 326, quoted in Porter, 2004: 7). Mach developed and popularized a philosophy of science that was congenial to certain subsequent developments, notably Logical Positivism, and served as a carrier for some key ideas of Comte's (Blackmore, 1972: 164–69).[11] Both had a view of science as "economical" or oriented to "efficiency." Pearson connected this to contemporary ideas of national efficiency, Mach to a movement of scientists led by Wilhelm Ostwald called "energeticism" that opposed the atomic theory and extended the law of conservation of total energy to a normative notion of the economy of energy in social life. This idea also influenced their ideas about the relation of theory to data. Because they thought of theory as economical expressions of data, they were hostile to realistic interpretations of theoretical entities that went beyond the data. The standard view placed them together: "just as Mach opposed the atomic theory, so Pearson fought Mendelism" (Blackmore, 1972: 125; cf. Porter, 2004: 269–70 for a more nuanced view).

The Grammar of Science ([1892]1937) began in this vein, with a discussion of the purpose of science, which Pearson claimed was the same as that of any other human activity: to promote the welfare of human society, to increase social happiness, and to strengthen social stability. Stability was strongly associated with consensus, and as in Comte, science was a model for the achievement of consensus. Yet Pearson appeared to be of two minds about the problem of consensus, as indicated in his phrase "unforced consensus," which reflected both his idea, shared with Mach, that the age of force had ended, and his insistence on consensus as a condition of social stability and that social stability was the ultimate goal of science. The conflict lies in the relation of the two ideal elements of the ideal of unforced consensus. One is that "force" in the form of a scientific hierarchy persecuting scientific heresy would be fatal to progress. The other is that consensus is the primary good that science provides. And for Pearson the scientific method assures consensus without force.

Citizens must, of course, accept the consensus produced by science, and this is where education and popularization come in. Pearson was concerned with the right way to inculcate the scientific, unbiased cast of mind. Merely reading about science did not lead to this result: what did so was the close scientific study of some small area ([1892]1937: 15–16). And one could expect such experience to transfer to the role of the citizen. This would produce consensual politics without coercion ([1892]1937: 11–14).

Although he was a socialist, Pearson was no egalitarian with respect to the hierarchy of scientific talent. The role of the semi-educated citizen was still primarily one of respect for the "Priests" of science.[12] But he also believed, in the phrase of nineteenth century Catholicism, in "no rights for the wrong." Lack of conformity to the canons of legitimate inference, Pearson says, is "antisocial" if it involves believing "in a sphere in which we cannot reason," and there is no "right" to holding false beliefs that lead to negative consequences in matters that are "of vital importance to others" ([1892]1937: 54–55). And he argued that "the abnormal perceptive faculty [i.e., the kind that failed to arrive at the consensual conclusion assumed to be more or less automatically produced by persons with normally evolved perceptual powers], whether that of the madman or the mystic, must ever be a danger to human society, for it undermines the efficiency of the reason as a guide to conduct" ([1892]1937: 120).

Pearson's optimism about the efficacy of the scientific method as a source of consensus was grounded in his philosophy of science. The facts of science for him are perceptual successions, and so the idea of arriving at an unforced consensus on them is plausible. What is controversial is the idea that *political* questions can be resolved into issues of perceptual succession. Pearson's examples of how this should work included Poor Law reform, where "the blind social instinct and the individual bias at present form extremely strong factors of our judgment" (Pearson, [1892]1937: 29), preventing their objective solutions through considerations of national efficiency.[13]

THE PROBLEM OF CULTURE

The thinkers we have considered here, in the line from Bacon to Pearson, had an "extensive" conception of science, one in which science, understood for example as a method, could be applied to something beyond its normal subject matter. Science could be conceived "extensively" in a variety of ways: as incorporating technology and engineering, as including "social" and "mental" sciences, as including the policy sciences, and even as a foundation for ethics, a popular theme in the post-Darwinist period. The nature of science came to be discussed in terms of the essence which carried over. It was in response to this that a "liberal" view of science finally emerged. Pearson, and later the heterodox economist Thorstein Veblen, talked about science and engineering as a cast of mind that carries over from one activity or topic to others, and the theme was deeply embedded in the culture of the time (Jordan, 1994). There was also a strong current of sociological thinking that developed a variant of this thesis. William F. Ogburn's *Social Change* ([1922]1966), one of the most influential works of sociology of the interwar years, which introduced the term "cultural lag," into the language, was akin to technological determinism.[14]

The "cultural" significance of science soon became a hotly contested issue. In the German speaking world, the issue took the form of a discussion of the idea of a scientific *Weltanschauung*. Mach and his successors, including the Logical Positivists and especially Otto Neurath, were interpreted, and sometimes interpreted themselves, as providing a scientific alternative to retrograde *Weltanschauungen* (cf. Richardson, 2003), and Neurath used the term *Wissenschaftliche Weltauffassung*, or scientific conception of the world, to distinguish the scientific alternative from mere "world views" (Richardson, 2003: 68–69). This quest for a scientific conception of the world played a role in German thought analogous to the role that the problem of the replacement of traditional religion had played in British and French thought.

The problem of whether science could provide a *Weltanschauung*-substitute in turn produced an issue about the cultural status and character of science that was highly consequential for what followed, first in the German-speaking world (Lassman and Velody, 1989), and ultimately, as Logical Positivism was imported, in the Anglo-American world. But the discussion also led indirectly to a body of explicitly "sociological" thought about the nature of world views and the causal relations between science and civilization, and ultimately to the "classical" sociology of knowledge of Mannheim and to the development of Marxist accounts of science.

The carry-over thesis answered the question of causal direction in the science-society relation by making science the prime mover. But the question could also be put as follows: Did advances in science, or indeed the phenomenon of modern science itself, depend on cultural conditions? Philosophers, such as Alfred North Whitehead ([1925]1967), and civilizational sociologists, such as Sorokin ([1937]1962) and Max Weber ([1904–05]1949: 110; [1920]1958: 13–31), ran the direction of causality in this other way, from culture to science, seeing features of modern western culture as conditions for the growth of science and the scientific mentality.[15]

The idea of the scientific resolution of policy questions, already formulated by Mill, also played a significant role in this period, in a variety of forms. Fabian socialism in Britain and a huge array of reform movements in the United States, as well as bodies such as the German *Verein für Sozialpolitik*, promoted scientific or engineering solutions to social and policy problems and an "efficiency" movement. The Russian Revolution proclaimed itself to be "scientific" in that it was based on the scientific materialism of Marx and Engels: this was a realization in practice of the extension of science to absorb and obliterate politics. The experience of "War Socialism" in Germany during WWI persuaded many thinkers, notably Otto Neurath, of the practicality and desirability of a planned economy (cf. O'Neill, 1995; Steele, 1981). The issue of the efficacy of planning was to become central to the later literature.

The idea of experiment also served as a political model. John Dewey, in such works as *Human Nature and Conduct*, pronounced the experimental method to be the greatest of human achievements, and he promoted the idea of its application to human affairs, replacing "custom" and attachment to traditions, such as constitutional traditions, as a basis for political action (1922). Yet Dewey distinguished the techniques of science from the spirit: he wanted the spirit, and its creativity, in politics, but not the techniques or the experts that employed them, or the experts themselves, whom he dismissed as specialists and technicians whose work needed to be "humanized" (Dewey, [1937]1946: 33). This reasoning, and the movement it represented, was not attractive to scientists themselves (Kuznick, 1987: 215).

In connection with science, the model of "conceptual schemes," under the influence of L. J. Henderson, became a Harvard commonplace (Henderson, [1941–42]1970). The reception of this way of thinking about science was aided by developments in science and mathematics, such as the discovery of non-Euclidean geometries and the broader recognition that what appeared as physical truth was dependent on nonempirical choices of mathematical structures. This was a thesis developed by Poincaré, but quickly absorbed and underlined by other thinkers, notably the Vienna Circle, and in the extended discussion of the theory of relativity that followed (Howard, 1990: 374–375). The broader relativistic implications of this idea were recognized at the time. When Neurath wrote that the choice of mathematical structures for a theory was not an empirical matter, Max Horkheimer cited the passage as evidence that he embraced hyperrelativism ([1947]1972: 165). This assimilation of scientific premises to "culture" took many other forms as well, for example, in such influential texts as Alfred North Whitehead's *Science in the Modern World* ([1925]1967) and *Process and Reality* ([1929]1978), and even more explicitly in E. A. Burtt's *Metaphysical Foundations of Modern Physical Science: A Historical and Critical Essay* (1927). This was part of a larger and pervasive climate of opinion,[16] shared by Mannheim's sociology of knowledge (though Mannheim specifically exempted science from the subject matter of his "sociology of knowledge") but also by Ludwig Fleck, who used the notion of *Denkgemeinschaft* to account for the problem of the reception of scientific ideas ([1935]1979), an issue that was soon to become central.

This general approach was paralleled in France in a series of historical studies broadly influenced by the French neo-Kantian tradition and phenomenology, which focused on conceptual change and difference, and in particular on conceptual breaks and ruptures. Pierre Duhem was one of the pioneers of this approach, especially for his studies of medieval physics, which he showed to be methodologically sophisticated and coherent, and his holism, which led him to reject the idea of crucial experiments. Later French historians of science, such as Alexandre Koyré, who focused on the scientific revolution, stressed the radical nature of change between the conceptual systems it involved (1957). This austerely presuppositional approach, influenced in his case by Husserl, largely ignored experiment and data as relevant to scientific change. His contemporary, Gaston Bachelard, performed a similar analysis of the transformation represented by Einstein's special theory of relativity (1984). His concept of the "epistemological break" was a means of expressing the interconnected or holistic aspect of such transformations, including their relations to general philosophical outlooks. Georges Canguilhem extended this notion of epistemological breaks in relation to the creation of fields of knowledge, especially, in the life sciences, through concepts of normality (1978). Canguilhem was the reporter for Michel Foucault's dissertation on psychiatry. Foucault extended this reasoning to new topics and new disciplinary fields and to the phenomenon of disciplining itself, thus completing the extension of explanations of the history of science in terms of breaks to the explanation of the history of culture. By focusing relentlessly on theory rather than experiment, technology, and instrumentation, and by its concern with rupture, the French discussion (which of course influenced English-language history of science, particularly with respect to the scientific revolution) simply bypassed the issues that arose in the English and German language discussions of science, not only becoming Kuhnians *avant la lêttre* but using this new understanding of science as a model for the understanding of intellectually organized social life generally.

The English and German discussions arrived at a similar point through a much more tortured route, and the reasons are relevant to the subsequent history. During the early twentieth century neo-Kantianism was in "dissolution," but the dissolution took various forms. Both Heidegger and Positivism provided different approaches to the problem of *a priori* truth, and each undermined the "presuppositions" model (cf. Friedman, 1999, 2000, 2001), as did the later Wittgenstein ([1953]1958, para. 179–80). These criticisms pointed in the direction of a notion of practice or tacit knowledge. Karl Popper attacked the presuppositions model by arguing that presuppositions changed every time a theory changed, and he attacked Mannheim for his idea that identifying presuppositions placed one in a position to "critique." The discussion of conceptual schemes, frameworks, and the like persisted in the history of science during this period, but it was not until the fifties, with N. R. Hanson's Wittgenstein-influenced *Patterns of Discovery* (1958), which undermined the notion of raw observational data, that it came into its own in philosophy proper.

WEBER'S "SCIENCE AS A VOCATION"

The German postwar discussion of the idea of science as a *Weltanschauung* produced an especially important response that did not directly figure in the historical and philosophical literature, but later appeared in the influential "sociological" approach to science developed by Merton. The idea that *Wissenschaft* had a cultural and political task of providing a worldview gained significance as a result of the cultural crisis produced by military defeat. This idea was to receive its classic critique in two speeches by Max Weber: one on "Politics as a Vocation" ([1919]1946a), the other on "Science as a Vocation" ([1919]1946b).[17] In "Science as a Vocation" Weber provided a history of motivations for science from Plato through Schwammerdam's proof of God's providence in the anatomy of a louse, dismissing them all and concluding the list with the brutal comment on "the naive optimism in which science, that is the technique of mastering life which rests on science—has been celebrated as the way to happiness. Who believes in this?—aside from a few big children in university chairs" (Weber, [1919]1946b: 143). These big children would have included Pearson, Mach, and Ostwald, whom he took the trouble to denounce in a separate article, particularly for the utilitarian theory of knowledge Ostwald shared with Mach, who spoke of theories as economizations (Weber, [1909]1973: 414). The brunt of his emphasis in the speech, aside from its anti-utilitarian view of science,[18] was on specialization as a condition for genuine achievement. This also undermined the "extensive" conception of science: the achievements of the specialist do not generalize into lessons about the mastery of life.

The message in the speech on politics was also explicit: "the qualities that make a man an excellent scholar and academic teacher are not the qualities that make him a leader . . . specifically in politics" ([1919]1946b: 150). The aspiring political leader was constrained by the realities of modern party politics and the demands of creating a following, as well as the intrinsic demands of the pursuit of power, demands so onerous that very few people had the personal qualities for such a career. This account of the political sphere—with its emphasis on the necessity of power for the achievement of any meaningful end, as well as its relentless reminders that the means specific to the state is violence and that to engage in politics is to contract with diabolical powers—served to place the sphere of the political beyond the prospect of transformation by intellectuals. And Weber made a particular point about the limitations of the bureaucratic mentality in the face of the demands of politics, thus undermining any thought that politics could be replaced by the administration of things.[19]

HESSEN AND THE TRANSFORMATION OF THE DEBATE

In 1931 the discussion of science was transformed by the emergence of a fully developed Marxian account of science, sponsored at the highest level of the Soviet ideological apparatus by Nikolai Bukharin. Bukharin's own main theoretical work was entitled *Historical Materialism* and opened with these sentences: "Bourgeois scholars speak of any branch of learning with mysterious awe, as if it were a thing produced in heaven, not on earth. But as a matter of fact any science, whatever it be, grows out of the demands of society or its classes" (Bukharin, [1925]1965). A volume of articles applying these ideas to the history of science was produced for an international congress of historians of science in London, and it had a profound, galvanizing effect, especially in Britain (Delegates of the U.S.S.R., 1931).[20] The thesis they presented was in fact a dramatic one that had the effect of incorporating "premises" talk into the Marxian theory of base and superstructure. The major point of this text was to show in detailed case studies that science was also the product of the demands of the time for technological results, that the demands were specific to particular social formations and historical situations, and that "theory" was ultimately driven by technological practice, so that the idea of an autonomous realm of pure science was a sham and an ideological construction (Hessen, 1931).

The British discussion of science had evolved differently than the German one. At the 1927 meeting of the British Association for the Advancement of Science, E. A. Burroughs (1927: 32), Bishop of Ripon, suggested a moratorium on science for a decade to allow for a reconsideration of its social consequences. Josiah Stamp pursued this theme in his Presidential Address at the 1936 meeting of the same association when he called on scientists to consider the social responsibility of scientists ([1936]1937). In this context of social concern and deepening economic and political crisis, a message about science from the Soviet Union, already idealized by British Fabian socialists such as the Webbs, was bound to have an impact.

One of Marx's central ideas was that the revolutionary moment occurs when the conflict between the forces of production and the capitalist class structure and system of economic relations is at its height. One of the central ideas of both the fascists and the Soviets was that of rational planning in the economy and other spheres of life. These ideas had a strong grip on the public and on policy makers during the Depression. In the case of science, a large literature developed on "the frustration of science," the idea that capitalists, incompetent bureaucrats, and politicians stood in the way of the kinds of scientific developments that could overcome the failures of capitalism.[21]

These ideas became the core of a Left view of science, which focused on conditions outside of science, such as the demands of the economy for particular kinds of technology, which either propel or retard relevant scientific development.[22] In line with the Marxist theory of history, the explanations of scientific development were implicitly teleological. But the detailed explanations themselves were novel and quite different from other histories of science, especially when they showed how the development of particular ideas was closely entwined with the technology of the time. A particular favorite was the argument that the availability of slaves in the ancient world and the consequent contempt for "work that could be carried out by slaves" led to Aristotle's failures to recognize relevant facts, such as the fact, known to ancient craftsmen but whose significance was not grasped until Galileo, that water could not be raised more than thirty feet by pumps (Hogben, 1938: 367–68).

The leading Marxist commentators on science argued that the Soviet Union was the one country in which science had obtained its "proper function," as its most important figure J. D. Bernal put it.[23] They viewed the Soviet system as benign and also argued that neutrality was impossible for the scientist, especially in face of the anti-scientific drive of Fascism. They argued further that money for science would flow freely in a rationally organized planning regime rather than a market economy and that "any subject is capable of being examined by the scientific method" (Huxley, 1935: 31) including the economic system and society. They held that history was presently in a transitional phase moving toward a state in which science, understood extensively as implying "a unified, coordinated, and above all conscious control of the whole of social life" (Bernal, 1939a: 409), would abolish the dependence of man on the material world. Its rightful role was to become the conscious guiding force of material civilization, to permeate all other spheres of culture.[24] This claim allowed Bernal to say, echoing Pearson on unforced consensus, that science already is Communism, since it is performing the task of human society, and in the Communist way, in which "men collaborate not because they are forced by superior authority or because they blindly follow some chosen leader, but because they realize that only in this willing collaboration can each man find his goals. Not orders, but advice, determines action" (1939a: 415–16).[25] In practice, as Bernal envisioned it, scientists would be organized into trade-unions which would cooperate with other trade-unions in producing and carrying out the five-year plans.[26]

THE CRITIQUE OF EXTENSIVENESS

Bernal and his comrades understood that the issue that made their position unpersuasive to other scientists was the notion that planning would be applied to science itself. This raised the question of what sort of freedom of inquiry would exist under planning. These were not issues that could be confined to the Soviet Union. Nazi science was not only planned, it was "extensive" in a problematic sense that was also relevant to Lenin's notion that no cultural organization in the Soviet regimes should be autonomous from the party. Under the Nazis, science was expected, though in practice this often meant little, to conform to Nazi ideology. Scientists who were Jews were expelled, and a loud campaign was mounted against the "Jewish influence" in science. A paper by a German scientist, Johannes Stark, originally published in a Nazi journal, was translated and published in *Nature* (Stark, 1938). Stark's paper focused the anxiety of scientists and the Left about Nazism and prompted a huge response (Lowenstein, 2006). The response in the United States, however, was cast in terms of "freedom" and assertions about the link between scientific freedom and democracy, leading to manifestoes and resolutions in defense of science and democracy (Boas, 1938; Merton, 1942: 115; Turner, 2007).

This discussion provided the initial spur to a renewed debate on the autonomy of science. Bernal, mindful of the successes of German planned science, defined the issues in terms of a conflict between freedom and efficiency, a conflict which he thought could be resolved within the framework of planning. But the issue of freedom under planning was to be a theme in a larger and more wide-ranging political discussion.[27] The issue of planning and the problem of the autonomy of science, which were originally distinct, now converged. Robert Merton, who had emerged as a respected figure for his study of religion and the Royal Society, wrote two papers, "Science and the Social Order"([1938]1973) and "A Note on Science and Democracy" (1942), both about autonomy and written with an eye to Nazi science, which extended Weber's cryptic account of science in "Science as a Vocation." Merton described four norms of science: universalism, organized scepticism, "communism" or sharing of scientific results, and disinterestedness. In 1938 Merton noted that this was a "liberal" argument, for, as he put it, in a liberal society integration derives primarily from the body of cultural norms ([1938]1973: 265). Merton's norms were not rooted in, nor even consistent with, the attitudes of the public, which could be expected to resent them. It was for this very reason that science was vulnerable to fascism, which trades on popular antirationalism and places centralized control on science. But conflicts occurred in democracies as well, especially when the findings of science invalidated dogmas (cf. 1942: 118–19). Thus, science and democracy are not compatible unless there is a recognition of the autonomy of science, and such recognition was always under threat by the normal extension of science into new topics, such as social science investigations of areas considered sacred (1942: 126).

These were writings in "sociology" and reflected one of the dominant research concerns of sociology in the period: the professions. Merton stayed away from issues of scientific content and was careful to avoid taking sides between Left and liberal views of science. For the most part, his argument preceded the bitter debate over planning that broke out in the 1940s between Bernal's Social Relations of Science movement and the antiplanning Society for Freedom in Science (McGucken, 1984: 265–300).

The leading intellect of the anti-Bernal group was Michael Polanyi. Polanyi provided, where Merton did not, an argument for the autonomy of science based on the claim that science had no need for political governance in the form of planning because it was already "governed" sufficiently by its own traditions and because the nature of scientific discovery itself could not be rationalized in the fashion assumed by the planners (an argument that turned into an assault on the notion of scientific method itself). Polanyi, like Conant, who made the issue of reception a centerpiece of his view of science, denied that science proceeded by overthrowing theories on the basis of new observations, noting that it often required the assimilation of significant changes in unarticulated background knowledge (e.g., 1946: 29–31). Science was, Polanyi argued, a community as distinct from the sort of "corporate" bureaucratic order that was subject to planning.[28] Planning would destroy the feature of community life that made possible the growth of ideas, which was, for Polanyi, the ability of scientists to freely choose which ideas to pursue.[29] He based his claims about science on an elaborately developed account of the ultimately inarticulable cognitive processes of scientific discovery and the way in which discovery is dependent on local traditions and a special level of community life that honors "scientific conscience" and the use of scientific judgement (1946: 52–66). This was an attack on any mechanical or "logical" account of science.[30]

Polanyi's argument addresses the problem of science and democracy in a novel way that contrasts with Merton's. If science, understood as nonmechanical activity of discovery dependent on inarticulable knowledge, is subject to democratic control it will not flourish. But science, Polanyi says, is not an anomaly for democracy. It is similar in character to other communities, such as the church and the legal profession, which are granted autonomy on the basis of their strongly traditional, self-governing character. Democracy itself, Polanyi argued, is strongly traditional and moreover depends on a tradition "of free discussion" and decisions based on "conscience" (1946: 67) like that of science. So the relation between science and democracy should be one of mutual recognition and respect, from one traditional community to another, consistent with the recognition that the fruits of science can best be gained by granting autonomy to the scientific community (cf. Polanyi 1939, 1941–43, 1943–45, 1946, [1951]1980).[31]

These were abstract considerations. There was also a practical battleground for the Left view of science: education and public understanding. From Condorcet on, the Left view of science education was that the workers should be made to think scientifically through some sort of basic training in science itself. Nor was this merely a pious hope: many British scientists participated in workingmen's educational projects that realized this goal, and the idea is reflected in the titles of the texts written by the key Left thinkers about science in the 1930s, such as *Mathematics for the Million* (Hogben, [1937]1940; see also Hogben, 1938; Levy, 1933, 1938, 1939; Crowther, 1931, 1932; Haldane, [1933]1971, [1940]1975). The critics of this view included James Bryant Conant (1947: 111–12n), who dismissed as a failure the fifty years of applying Pearson's idea that elementary instruction in science would make for better citizens. He reformed science education at Harvard accordingly, with the idea that, instead of engaging in rudimentary exercises, it was better for students to get some knowledge of the nature of science by working through case studies of major changes in "conceptual schemes"—the favored Harvard language—such as the Copernican and chemical revolutions.[32] The set of case studies that was produced for this course (Conant, 1957), which Conant at first taught and which ran for nearly a decade (Fuller, 2000: 183), became the background for Kuhn's *Structure of Scientific Revolutions.* Kuhn himself, who was recruited by Conant as an instructor for the course (Kuhn, 2000: 275–76), wrote the case study of the Copernican revolution, which became his first book (Kuhn, 1957). Conant was equally aggressive in attempting to reform recruitment into scientific careers, which he hoped to make more open and meritocratic, a goal consistent with his "opportunity" liberalism (1940).

Although there are some differences in emphasis between Conant, Merton, and Polanyi, to a remarkable extent they overlap, and Conant and Polanyi are particularly close. Both Conant and Polanyi had a Liberal approach to science in the following sense: they thought it was best to govern science indirectly, by facilitating competition among scientists.[33] But Conant, acknowledging the realities of "big science," thought it was necessary to have a set of major elite universities with massive resources, analogous to major corporations, in order to make this competition meaningful. The argument for extensiveness depends on a reductive account of science, identifying transportable features, such as a "method" with unique intellectual authority. Conant objected to the notion that there was a universal method of science and to the "wide use of the word science" (i.e., what I have been calling extensiveness).[34] Almost any account of science that characterized the activity of science as continuous with nonscientific forms of reasoning, psychology, perception, and forms of organization, and accounted for it as a complex but distinctive amalgam of these features, made science less transportable. Moreover, this style of explanation inevitably conflicted with the more expansive claims of science to intellectual authority.[35]

POSTWAR SCIENCE STUDIES: THE ERA OF DISCIPLINES AND ITS CONSEQUENCES

The response of physicists to the Bomb, the coming of the Cold War, the betrayal of atomic secrets by scientists, the Oppenheimer case, the Lysenko affair[36] (which finally discredited the Soviet model of science), and the rise of an aggressively anti-Stalinist Left[37] transformed this debate. Scientists on the Left turned to the nuclear disarmament movement. The rapid growth of universities in the postwar period also led to a greater focus on disciplinary discourse and consequently narrowing of interest in topics "belonging" to other disciplines.[38] The previously marginal field of philosophy of science became the most prestigious and powerful subfield of philosophy while shedding its past interests in Left wing politics.[39] Much of its energy was taken up with consolidating the standard view of the logical structure of scientific theories.[40] Sociology of science, however, declined precipitously.

A bibliography by Barber and Merton in 1952 defines its literature: an amalgam of Left commentary on science with studies of technology, including Ogburn's *Social Effects of Aviation* (1946), works by scientists and historians with a "social contexts" component, government documents, Polanyi and Conant, and studies of Soviet Science. The sociology of knowledge and Mannheim were intentionally omitted (Barber & Merton, 1952: 143n); Fleck had yet to be discovered. Ogburn was at the end of a long career, Stern was to die in the fifties. In American sociology only three major scholars, Merton, Barber, and Edward Shils, continued to write on science, and Barber, a follower of Talcott Parsons, was the only one of these to do so systematically and to teach the subject. Merton left the field. Aside from the bibliography with Barber (Barber & Merton, 1952) and the introduction to Barber's book (1952), Merton published only one paper on science, on the importance of claims of priority, between 1942 and 1961. Shils became involved with the atomic scientists' movement, became close to Leo Szilard, sponsored the hiring of Polanyi at the University of Chicago, and was involved, along with Polanyi, in the Congress of Cultural Freedom and its Hamburg conference on Science. What he wrote on science was largely restricted to the scientists' movement (1972: 196–203).[41] This interest did lead to a minor classic, *The Torment of Secrecy* (1956), on the inherent conflict between science and security in liberal democracies. His basic formulation of the autonomy of science split the difference between Merton and Polanyi: like Polanyi, he argued that "there is an inner affinity between science and the pluralistic society" (1956: 176), and that the "tradition of the free community of science" grew up independently of modern individualistic liberalism; like Merton, he was concerned with "populistic hostility to science" (1956: 181) which exacerbated the intrinsic problems of political supervision of science.

Parsons, the inescapable "theorist" of this era in sociology, wrote a great deal on universities as institutions, but little on science.[42] Parsons saw science through the lens of his own view of the professions as essential building blocks of modernity, especially by virtue of their embodiment of the normative commitments of modernity, and thus as sharing in the central values of the society (cf. Parsons, 1986). The same thinking informed Barber's *Science and the Social Order* (1952), the first text that was recognizable as a theoretical and empirical overview of the sociology of science. In his 1990 collection of essays on science, Barber argued for "the special congruence of science with several characteristic subsystems of modern 'liberal' societies'" (1990: 40) as well as "the independent rationality of science." The emphasis on the place of science in the social system was, as Barber commented, "Parsonian all the way" (1990: 39).

Despite Barber's *Science and the Social Order*, which was one of the earliest in a long series of works by Parsons's students that were designed to colonize and bring theoretical order to the study of different societal "subsystems," the study of science did not flourish. Merton became a major figure in sociology but not for his writings on science. Like the Parsonians, he wrote on the professions, engineering, nursing, technologists, and medical students. When he returned to writing on science in the1960s, "ambivalence" replaced the conflict between science and society of his 1938 essay, and the model of ambivalence was the reluctance of patients to accept the authority of physicians' advice ([1963b]1976: 26).

In the 1960s and '70s, Merton and his students became associated with the argument that science functioned meritocratically, which was a version of the argument that the autonomy of science ought to be honored, but it was a characteristically depoliticized argument and avoided issues involving the intellectual substance of science in favor of external indicators, such as Nobel prizes and citations, which could be correlated with one another (e.g., Cole and Cole, 1973).[43] Merton barely acknowledged such thinkers as Polanyi.[44] Although Merton himself was partial to the history of science and not narrowly "sociological" in his writing about science, the abstractly quantitative approach of the Mertonian "program" made it largely irrelevant to the discussion of science that Kuhn's *Structure of Scientific Revolutions* was opening up, which was dominated by issues relating to the collapse of the theory-observation distinction that had been central to the standard model of scientific theory of the Logical Positivists.

Kuhn was the intellectual heir of Conant (though also influenced by Polanyi and the Quinean critique of Carnap), but he was Conant with the politics left out. He was nevertheless a genuinely interdisciplinary thinker who had been especially ensnared by the disciplinary divisions of the 1950s. But this situation was quickly changing. Departments of history and philosophy of science were established at London (1949) and Melbourne (1946), and Indiana (1960) and Pittsburgh (1971), and others were to follow. Kuhn was appointed to a comparable position at Princeton.[45] *Minerva* was established in 1962. At Edinburgh, the interdisciplinary unit of Science Studies was established in 1964. A department of history and sociology of science was established at the University of Pennsylvania (1971). The continuities with the older discussion were highly visible. Polanyi's concerns and those raised by the atomic scientists' movement guided *Minerva*. The historian of the Bernal circle, Gary Werskey, was appointed at Edinburgh, a program motivated in part by concerns about explaining how science actually worked, a project parallel to Conant's but this time pursued by veterans of the Social Responsibility of Science movement, the heir to the Social Relations of Science movement (MacKenzie, 2003).

The institutional stage was thus set for the developments that produced "Science Studies." Ironically, among the central intellectual conditions for the rise of science studies was the separation between the disciplines that occurred in the 1950s. Now it represented an opportunity for debate. The rational reconstructions given by philosophers of science and the Popperian model of falsification became targets for sociologists of science, and the agonistic relation that emerged (Zammito, 2004) was to provide the motive force for the revival of science studies as an interdisciplinary field. The conflict between science as an authoritative technique and science as a form of life was to take a new form: initially defined in disciplinary terms as a conflict between philosophy and sociology of science, and eventually in political terms as a dispute over the authority of science and of experts that Bacon himself would have recognized.

Notes

1. "by a suitable choice of a syllabus and methods of education, we can teach the citizen everything he needs to know in order to be able to manage his household, administer his affairs and employ his labour and his faculties in freedom . . . not to be in a state of blind dependence on those to whom he must entrust his affairs or the exercise of his rights; to be in a proper condition to choose and supervise them; . . . to defend himself from prejudice by the strength of his reason alone; and, finally, to escape the deceits of charlatans . . . " ([1795]1955: 182).

2. "When it comes to the institutions of public instruction, and the incentives that it would be their duty to provide to those who cultivate the sciences, they can have only a single guide: the opinion of men enlightened on these questions, which are necessarily foreign to the greatest number. Now it is necessary to be endowed with a superior reason, and to have acquired much knowledge oneself, to be able to listen to this opinion or to understand it well." ([1793]1976: 286).

3. Originally there were three classes of the Academy (physical and mathematical sciences, moral and political sciences, literature and fine arts), but in1803 a decree of Napoleon I changed the division to four (physical and mathematical sciences, French language and literature, history and ancient literature, and fine arts), suppressing the second class (moral and political sciences) as subversive to the state.

4. This was later modified into the more famous Marxist version, "to each according to his needs" (Manuel, 1966: 84; Manuel, 1976: 65).

5. A notion greatly expanded by Lenin's account of the withering away of the state in "The State and Revolution" ([1918]1961).

6. Manuel gives a useful account of Saint-Simon's shifting view of the role of the scientist, which was gradually reduced and subordinated to the industrialist (1960), in part as a reflection of his disappointment at the reluctance of scientists—whom he tellingly denounced for their "anarchism" (1960: 348) for failing to submit to the authority of the general theory he proposed—to join his cause.

7. The idea that the history of mathematics might be the key to the understanding of intellectual progress already appears in Saint-Simon (Manuel, 1960: 345).

8. This was Comte's theory of the subjective synthesis (Acton, 1951: 309).

9. As Manuel explains his reasoning,

> Since men of a class would seek to excel in their natural aptitudes, there could be only rivalry in good works, not a struggle for power. When class chiefs owed their prestige to their control of men, they could fight over one another's 'governed', but since there would be no governors and no subjects, from what source would class antagonism be derived? Within a class men of the same capacity would be striving to excel one another with creations whose merits all members of the class would be able to evaluate. Between classes there could be only mutual aid. There was no basis for hostility, no occasion for invading one another's territory. (Manuel, [1962]1965: 134–35).

10. There is a bitter dispute in Mill scholarship over the question of whether Mill believed in free discussion as such or believed in it only as a means of advancing the opinions he preferred (an interpretation that calls attention to his views on education). Maurice Cowling, in a classic polemic, collects the most damning quotations in support of the view that Mill's notion of "the intelligent deference of those who know much to those who know more" (quoted in Cowling, 1963: 34–35) amounted to a plea for deference to an intellectual and cultural elite. Chin-Liew Ten provides a strong refutation, based on an interpretation of Mill's ethical views (Ten, 1980). These ethical views, however, look very different in the light of the consideration, usually ignored, that Mill regarded politics as applied social science. The argument over whether Mill intended to apply the arguments in *On Liberty* to science is summarized in Jacobs, who denies that he did (Jacobs, 2003).

11. Mach's biographer discusses the commonplace that he is a transitional figure between Comte and Logical Positivism. Mach did not use the term positivist, nor did he acknowledge Comte as a source. But the similarities, especially with respect to their campaigns against metaphysics and religion, their insistence that they are not providing a philosophy but rather a science, and their belief in the unity of the sciences, are telling.

12. Pearson's authoritarianism differed from Comte's. Pearson hoped for the establishment of "poets, philosophers, and scientists" as "high priests" (Pearson, 1888: 20), and for the elevation of "reason, doubt, and the enthusiasm of the study" above the "froth and enthusiasm of the marketplace" (Pearson, 1888: 130–131, 133–134). But he labeled this "free thought" and was careful to describe science itself in terms of freedom. He comments at one point that there is no pope in science ([1901]1905: 60) and observes that doubt is integral to science and part of science's mystery ([1892]1937: 50–51). Skepticism of a certain kind (presumably toward religion) is enjoined ethically: "Where it is impossible to apply man's reason, that is to criticize and investigate at all, there it is not only unprofitable but antisocial to believe" ([1892]1937: 55). Later, out of fear of "scientific hierarchy" based on past achievements, he was to say "science has and can have no high priests" (1919: 75).

13. The main finding of science that most concerned him was the basic law of heredity, that "like produces like," which he applied to the problem of government and national strength by proposing eugenic control over population quality emphasizing the elimination of the unfit, the racially inferior, and stressing the necessity for the reproduction of the best minds. This was consistent with socialism, for him, because he believed that socialism required superior persons ([1901]1905: 57–84).

14. Ogburn's thinking in this book was rooted in an argument that Giddings had derived from Pearson's genetics in which the stabilization of intellectual objects was conceived on analogy to the stabilization of species. The reasoning was used in Ogburn's dissertation on the evolution of labor legislation in American states, which showed how the original diversity of legislation was supplanted by close similarity (1912).

15. Ogburn argued a parallel point in his essay on multiple discoveries and inventions (Ogburn & Thomas, 1922), deriving the argument from Alfred Kroeber (1917): variations in talent were likely to be small and could not account for variations in discovery; what could account for these variations, and at the same time account for the striking phenomenon of multiples, was the variation of cultural conditions. Ogburn's examples of "cultural conditions" were all drawn from material culture, an issue that Merton was to attack him over (1936).

16. Ironically, the term climate of opinion was popularized by the historian Carl Becker in *The Heavenly City of Eighteenth-Century Philosophers* to account for the Enlightenment, and it remains one of the best formulations of the idea that intellectual constructions are the product of tacit assumptions of a particular time (1932: 1–32).

17. The text contains a famous farewell to religion as an intellectually serious alternative, on the grounds that at this point in history religion required an "intellectual sacrifice" (Weber, 1946: 155).

18. An important American critique of Pearson's "social aims" interpretation of science was given by C. S. Peirce, writing as a public scientific intellectual (1901).

19. The term *Wissenschaft* is broader than the English "science" and means any field of organized inquiry. Weber's speeches led to a huge controversy, which in turn contributed to what came to be known as the crisis of the sciences. This became the German language form of a set of issues that was to unfold in a quite different way in the English-speaking world. For an introduction to the German dispute that immediately followed "Science as a Vocation," see Peter Lassman and Irving Velody (1989). Weber's account of the career of the scientist, here and in other texts, emphasized the notion of institutional roles and the inappropriateness of promoting values from within this role. It is worth noting that he had earlier attacked the monarchical socialist economists of the generation before his for wrongly thinking that there were scientific solutions to policy questions. They had, he argued, in a bitter controversy in the prewar period, surreptitiously inserted their own values into the advice, deluding themselves into thinking that they got the policy results from the facts alone. Thus, the effect of the paired accounts was to separate the sphere of science from that of politics, giving the scientist nothing to say to the politician except to instruct him on means, and to strip the scientist of the role of cultural leader. This way of thinking about professions eventually led, through Talcott Parsons's and Merton's emphasis on professionalization, to the American "sociology of science" of Barber and Merton, which was depoliticized. It also led, however, to an explicitly political critique of science in the work of Hans J. Morgenthau, the influential international relations theorist (1946; 1972).

20. The story of these events and their impact is well told in Werskey (1978; on Hessen see Graham, 1985).

21. The texts based on this thesis include Hall (1935) and in the United States the writings of "Red" Bernhard Stern (National Resources Committee, 1937: 39–66), a pioneering medical sociologist and precursor to the sociology of science (Merton, 1957), as well as Ogburn.

22. Much has been written on the key members of this group, J. D. Bernal, J. B. S. Haldane, Hyman Levy, Julian Huxley, Joseph Needham, Lancelot Hogben, and others related to them, such as F. Soddy and P. M. S. Blackett. A brief introduction is Filner (1976).

23. All that the phrase "the dictatorship of the proletariat" meant, according to Julian Huxley, writing in the period of the project of the collectivization of agriculture that killed millions through famine and violence, was that things were administered for the benefit of all (1932: 3).

24. Although Bernal was respectful of "dialectics" (cf. 1939b), the respect was superficial. It would be more accurate to say that he owed his picture of the authority of scientist-experts to Pearson and Fabianism, his picture of the ideal organization of science to the "guild socialism" of Fabian apostate G. D. H. Cole, and, as noted above, his picture of the ideal scientist to the image of the altruistic "new socialist man."

25. Bernal, as Veblen had earlier in his discussion of the likelihood of the creation of a Soviet of Engineers (cf. [1921]1963: 131–51), acknowledged that present scientists were too individualistic and competitive to fully realize the communism inherent in science and pointed to a future, transformed scientist, much in the manner of the New Socialist Man sought by the Soviet Union (1939a: 415).

26. Recall that Condorcet, Comte, and Pearson faced the problem that the condition for their account of science to contribute to progress was that citizens become scientifically educated, if only in a limited way, which required scientists to be, in effect, ideologists whose ideology was authoritative for the rest of society. This was tantamount to rule by scientists, which even enthusiasts such as Bernal regarded as impractical, though preferable, raising the question of whether Bernalism was Pearsonism in the vestments of Communism. Communism was not rule by scientists, though it involved an authoritative ideology that regarded itself as scientific.

27. Friedrich Hayek, in *The Road to Serfdom* (1944), emphasized the inherent conflicts between planning and freedom. Interestingly, in *The Counterrevolution of Science* he traced the issues to Saint-Simon's and Comte's ideas ([1941; 1942–44]1955).

28. The term "community of science" came into standard usage in the 1940s primarily through the writings of Michael Polanyi (Jacobs 2002). Until 1968 Merton did not use the term to describe science, except ironically, noting that "For the most part, this has remained an apt metaphor rather than becoming a productive concept" (1963a: 375).

29. And to benefit from the results of the free choices of other scientists.

30. The Logical Positivists responded by dismissing "the mystical interpretation" of discovery and treating their own views as concerned only with the (logical) context of justification as distraction from the (psychological and social) context of discovery (cf. Reichenbach, 1951: 230–31). This immunizing tactic set them up for the intellectual catastrophe, hastened by Kuhn, that in the 1960s destroyed Logical Positivism—the narrow "logical" view of scientific theory could not account for either conceptual change or the role of necessarily theory-laden "data" (cf. Shapere, 1974; Hanson, 1970).

31. In a widely quoted paper, Hollinger treats the argument for autonomy, for what he calls "laissez-faire communitarianism," essentially as an ideological means of extracting government money without government control (1996). It is notable that Bernal's Left approach to science, though it was self-consciously an attempt to balance freedom and efficiency, involved an even broader scope for self-governance.

32. This reform is extensively discussed in Fuller (2000).

33. This characterization raises the question of their relation to Karl Popper, whose connection with Hayek and defense of liberalism made him an outlier in this group. Popper had a brief flirtation with Polanyi, though the two found themselves uncongenial. In any case they were different kinds of liberals. Polanyi was a competent economist. Both were more interested in the liberal model of discussion and in the analogous "free criticism" of scientific discussion (Popper, [1945]1962: 218; Polanyi, 1946). Neither developed the analogy between liberal discourse and scientific discourse, and if they had done so the relations might have been more evident (cf. Jarvie, 2001). Both are limited forms of discourse, governed by a shared sense of boundaries. Popper's way of bounding science, the use of falsification as a demarcation criterion, may have led him to think that there was no need to locate a supporting ethos or tradition. The difference between the verificationist theory of meaning and falsification distinguishes him from the Logical Positivists. Verification faces out, so to speak, to those forms of purported knowledge that science might hope to supplant or discredit. It is directed at the larger community. Falsification looks in, to the process of scientific discussion that it regulates, and in so regulating makes it into a variant of liberal discussion.

34. One exemplary instance of his critique of extensiveness is his dismissal of the notion of the special virtue of the scientist—argued also in Merton's "Note on Science and Democracy" (1942). Conant puts the point succinctly by asking whether "it be too much to say that in the natural sciences today the given social environment has made it very easy for even the emotionally unstable person to be exact and impartial in his laboratory?" His answer is this. It is not any distinctive personal virtue like objectivity but "the traditions he inherits, his instruments, the high degree of specialization, the crowd of witnesses that surrounds him, so to speak (if he publishes his results)—these all exert pressures that make impartiality on matters of his science almost automatic" (1947: 7). These mechanisms, however, exist only for science proper—not for its extensions into politics, where the scientist has no special claim.

35. Both rejected the idea that scientists had relevant authority over such topics as religion and morals (Conant, 1967: 320–28; Polanyi, 1958: 279–86).

36. The Lysenko episode was a test of the credulity of the Bernal circle and of their willingness to compromise science for politics. The actual background to the events is even more bizarre than could have been known at the time, and Stalin's actions were not, as was often assumed, based on ideas about the class basis of science. Bukharin, discussed earlier, had been convicted in the famous show trial and confession that was the basis for Koestler's *Darkness at Noon* (1941). He appears to have been protecting his family rather than performing, as the expression of the time put it, one last service for the party. Hessen apparently died in the gulag around 1940. Lysenko, who was placed in charge of Soviet agricultural science, had tried to justify his opposition to the gene theory by stigmatizing it as bourgeois science, discrediting the British Communists' defense of Soviet science. Stalin, as it happens, was having none of this and personally crossed out such phrases in the official report Lysenko was to deliver on this issue, and "where Lysenko had claimed that 'any science is based on class,' Stalin wrote 'Ha, ha ha, . . . and mathematics? And Darwin?'" (Medvedev and Medvedev, 2004: 195).

37. Sponsored to a significant extent by the CIA (cf. Saunders, 1999: 1, 167–68).

38. Polanyi's career illustrates this: he continued to be a major participant in discussions of science and continued to write seriously about science in an interdisciplinary sense, publishing in 1958 his magnum opus, *Personal Knowledge: Towards a Post-Critical Philosophy*, a text that combined philosophy with psychology and a vivid picture of the social character of science. But the book was largely ignored by philosophers, who took the view that Polanyi, who of course was a scientist by training, was not a real philosopher.

39. Howard (2000). Jonathan Rée tells this story for Britain: "[T]he *Times Literary Supplement* [1957] tried to reassure its readers that [A. J.] Ayer's hostility to metaphysics was part of the now withered 'Leftist tendencies' of the thirties. In those days, thanks to *Language, Truth, and Logic* [(1936)1952] 'logical positivism successfully carried the red flag into the citadel of Oxford philosophy. . . . But now, at last, philosophy has been purged of any taint of leftism'" (Rée, 1993: 7). For the United States, see McCumber's *Time in the Ditch* (2001). Whether a political explanation of the narrowing of Logical Positivism is needed is open to question.

40. A comment by Einstein on Carnap, written in the course of declining an offer to contribute to a volume on Carnap, puts it thus:

> Between you and me, I think that the old positivistic horse, which originally appeared so fresh and frisky, has become a pitiful skeleton following the refinements that it has perforce gone through, and that it has dedicated itself to a rather arid hair-splitting. In its youthful days it nourished itself on the weaknesses of its opponents. Now it has grown respectable and is in the difficult position of having to prolong its existence under its own power, poor thing. (1953, quoted in Howard, 1990: 373–74).

41. Shils was a translator of Karl Mannheim's *Ideology and Utopia* ([1929]1936). Mannheim, who died in 1947, dedicated himself to a non-Marxist yet antiliberal project of social reconstruction that included the "planning of values," of which Shils thought little. His personal attitude toward Mannheim is revealed in Shils's memoir of Mannheim (1995). With respect to science Shils relied on Polanyi, with whom he was close, and made no attempt to produce an original "sociology of science." Shils did, however, write extensively on intellectuals and on the puzzle of why intellectuals—of which the Left scientists of the 1930s were an example—were so often opposed to the societies that supported them, and he developed a fascination for Indian intellectuals. He also took an interest in the university as an institution. The work of his student Joseph Ben-David, who in the 1960s was to write an introductory text on science for a series of works for students in sociology (1971), reflected this interest in institutional structures and disciplines as institutional forms, and he provided an influential periodization of dominant scientific institutional forms, which was at the same time a history of scientific autonomy and how it was achieved and threatened in each period.

42. Two exceptions are a text written to obtain funding for the social sciences that was known to Parsons' students but not published until much later (Parsons, 1986) and "Some Aspects of the Relations between Social Science and Ethics" (Parsons, 1947), a text that compares to Merton's "Note on Science and Democracy" (1942).

43. The Mertonian reflexive history of the school is given in Cole and Zuckerman (1975).

44. When the idea of the scientific community was revived by Derek Price under the heading "Invisible Colleges" (1963), Merton accepted it enthusiastically, leaving it to his student Diana Crane to concede that the data vindicated Polanyi's earlier insistence on the idea (cf. Crane, 1972).

45. M. Taylor Pyne Professor of Philosophy and History of Science, 1964.

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Olga Amsterdamska
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