Merton’s “Norms” in Political and Intellectual Context

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Abstract

*Merton’s two papers on the norms of science were written in a period of intense political activity in science, and responded to this context, using conceptual tools from classical sociology and Harvard thinking of the time. The basic reasoning was Weberian: science and politics each had a different ethos. One target was the Left view of science as a model for society. Another was the view of the American Left that complex societies required regulation, but that science should be free of control. Merton pictured science as already intensely policed, but threatened by the conflict between its special ethos and potential democratic demands, and requiring protection. This was a “liberal” argument, but Merton used the language of the Left to present it.*

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Merton’s two papers on the norms of science, “Science and the Social Order” ([1938]1973) and the more famous text, originally (and obscurely) published in a short-lived, Harvard-dominated specialty journal as “A Note on Science and Democracy” (1942) and reprinted as “Science and Democratic Social Structure” in *Social Theory and Social Structure* ([1949, 1957]1968) and “The Normative Structure of Science” in *The Sociology of Science* (Merton, 1973), were based neither on previous sociology of science (which was dominated by his nemesis William Fielding Ogburn) nor on Merton’s previous writings on Puritanism and science. They were, rather, a response to the political “crisis” over science at the time, a crisis over issues that had been formulated most forcefully by scientists on the Communist Left in Britain, but were also being expressed in petitions directed at the Nazi uses of science and the actions of the Nazi regime toward controlling science. His main cited sources were in this literature, and the anxieties he expresses over the relation of science to democracy, are variants of the concerns of this literature, particularly the concerns in the American petitions. Nevertheless, some of what Merton has to say has recognizable roots in sociology, and is written in a way that is “sociological,” that is to say politically opaque. My concern in this paper will be to locate Merton and his paper in this political context, explain the relation between Merton and the British and American scientific Left, and to show how Merton’s own position was “liberal” without either overtly challenging the position of the Left or embracing the arguments of the critics of the Left view of science.

The “Crisis” of the Late Twenties and Thirties
A Short Chronology

The issues which were addressed by Merton in these papers arose in the course of a specific series of events in the thirties. The story of the period has often been told, so what follows in this section is a very compressed chronology of the key events, rather than an interpretation of their intellectual significance. That will follow in later sections.¹ Merton himself refers to this background in both papers ([1938]1973, 254-66; 1942, 115), and it is essential for an understanding of the figures he cites and comments on.

By the late twenties, concerns were being expressed, especially in Britain, about the impact of science on society. In 1927, at a meeting of the British Association for the Advancement of Science, The Bishop of Ripon had called for a decade-long moratorium on scientific discovery, to enable society to catch up and to allow for a reconsideration of its social consequences (Burroughs 1927, 32).

In 1931 a fully developed Marxian account of science, which had developed in the Soviet Union quite independently of “Western Marxism,” and was sponsored at the highest level of the Soviet ideological apparatus by Nikolai Bukharin, emerged on the scene, at an international congress of historians of science in London. The Soviet scholars produced a volume of articles applying the basic ideas of historical materialism to the history of science. The book and the dramatic event of its presentation had a profound and galvanizing effect on British discussion of science (Delegates of the USSR 1931).² 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. Moreover, as Michael Polanyi paraphrased Bukharin’s views on the basis of a personal conversation, the distinction between pure and applied science “made in capitalist societies” was a sham and an ideological construction “due only to the inner conflict of a type of society which deprived scientists of the consciousness of their social functions, thus creating in them the illusion of pure science” (Polanyi, 1939, 176).

In the following years, there was a plethora of writing in this vein by scientists and Left-wing science journalists, much of it in the form of “popular” writings on science from a Marxist point of view. This included works like Mathematics for the Million (Hogben [1937]1940; see also Hogben, 1938; Hyman Levy, The Universe of Science (1933), A Philosophy for a Modern Man (1938), and Modern Science: A Study of Physical Science in the World Today (1939), as well as similar works by Crowther, An Outline of the Universe (1931) The ABC of Chemistry (1932) and Haldane, Science and Human Life, Science and Everyday Life (1940). These books combined popular expositions of science or the history of science with materialist interpretations of the historical reasons for scientific advance.

The books typically included chapters which predicted the future of science in society. The basic reasoning is well represented in a statement by Levy:

Capitalism has run its course. In using science for its own ends it has created the possibilities of a greater future than it, with its structure, is competent to bring to fulfillment. Its own internal contradictions have driven it to war against itself.....That is
the dilemma in which the scientist to-day finds himself. At once the product and the servant of a dying culture, he holds in his hands the key to the making of a new. (1939, 100)

A closely related literature developed during the depression under the heading “the frustration of science,” which focused on the idea that science could solve the economic problems of the present if it was only given the opportunity, and which attempted to counter the widespread sense that technological advance was a basic cause of unemployment (Hall, 1935; Stern, 1937a, b). All of the Marxists in this discussion were “frustrationists,” but not all frustrationists were Marxist.

Discussions of the social responsibility of science also developed rapidly. Josiah Stamp, in his Presidential Address at the 1936 meeting of the British Association for the Advancement of Science, called on scientists to consider the social responsibility of scientists ([1936]1937), and the editor of Nature in many of his editorials during this period promoted this and related themes, including the responsibility of scientists for preventing war (1938, 723-4).

During the period after 1933, under the Nazis, scientists who were Jews were expelled, and a loud campaign against the “Jewish influence” in science was mounted. A paper by a German scientist, Johannes Stark, originally published in a Nazi journal, was translated and published in Nature (Stark, 1938). The publication of Stark’s paper focused the anxiety of scientists and the Left about Nazism, and prompted a huge response (Loewenstein n.d.).

The British and American Scientific Left

The British Left had many members who was more openly Communist than in the American Left, and the British Left in general was more openly dismissive of the language of “democracy” as well as, of course, of the language of “liberty” and “freedom.” Daniel Hall, one of the authors that Merton encountered as a reviewer for Isis, put it as follows:

“Democracy” is a word that has become almost meaningless; no modern state is democratic in the original sense of the word; the mere weight of numbers and size makes it impossible that every citizen can participate in government. Regarded en masse, the citizens are becoming passive recipients of government that is in the hands of a relatively small administrative class; the real question is how and by whom those officials are controlled (Hall1937, 712).

This reflected a sense of the particular problems of Britain, which, he believed, came down to efficiency: a system of parliamentary rule that was efficient in the nineteenth century was a visible failure. “For nine years the Waterloo Bridge hung broken-winged across the Thames, a startling reproach in the very heart of Empire to our inability to make a decision” (1937, 714). Hall suggested that it was this inability that led the younger people “impatient of our representative government and is turning them on the one hand to Fascism and on the other towards Communism” (1937, 714). Mussolini “in a short time carried through great schemes of social amelioration of the land and of public health, and ...has introduced a new standard of efficiency into Italian life. This, indeed, is the crux– efficiency” (1937, 714).

Hall made the contrast between democracy, and its inability to make a decision, and
planning. The kind of planning he envisaged was “not to be understood as that drive of
government to put the nation on a brand new track, such as the ‘Five Years Plan’ of the Soviet or
the ‘Four Years Plan’ of Germany,” which he took to be too authoritarian and redolent of the
“ant and bee community” for the British (1937, 715). He called instead for a form of expert
consultation that integrated decision-making for national purposes. This was common currency
among the scientific Left.

The issue of “freedom” arose explicitly in the writings of the overtly communist Left.
Bernal, for example, in his pivotal book *The Social Functions of Science*, writes that

The great change, of which we are now witnessing the first stages, between a social life
based on traditional techniques and one based on science will certainly be reflected in an
entirely different attitude towards freedom. The freedom of the nineteenth century was a
seeming thing. It was an absence of a knowledge of necessity. Its basis lay in social
relations through a market. In liberal theory every man should be free to do what he liked
with his own, buy or sell, work or idle. In fact he was tied by the iron laws of economics:
laws socially produced but taken as laws of nature because they were not understood. In
an integrated and conscious society this conception of freedom is bound to be replaced by
another—*freedom as the understanding of necessity*. (1939, 381, emphasis in the original)

Negative freedom, in the distinction made famous by Isaiah Berlin, which was in any event
illusory, was to be replaced by the positive freedom to contribute to society. This vehement
denunciation of freedom led to a specific reaction, which included the defense of the autonomy
of science, an argument that science could not be “planned” by Michael Polanyi but flourished
only where it operated with spontaneous co-ordination like that of a market, and the claim that
planning, in the phrase of Hayek, placed us on the road to serfdom. Hayek also contributed a
still-valuable historical critique of *The Counterrevolution of Science* (1955), which traced these
ideas to their Saint-Simonian and Comtean roots and to the hubris of *L’École Polytechnique.*

Popper, whom Hayek was a sponsor of, developed a discursive account of science as more or
less analogous to liberal debate, which also undermined the notion of planning.

In the United States, matters were, on the surface at least, very different. “Freedom” and
“democracy” were terms that all sides employed and affirmed, including the Communists, who
for the most part (and in science organizations, such as he American Association of Scientific
Workers) operated in popular front organizations and did not identify themselves publically as
such. Thus the response in the United States to this collection of issues 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 (for a history of these, see

The differences in viewpoint are there in the texts, but discerning them from the texts
alone requires more hermeneutical subtlety than there is space for in short article. One example
will suffice. Here is John Dewey, whose role will be discussed shortly, on “liberty”:

...the ends which liberalism has always professed can be attained only as control of the
means of production and distribution is taken out of the hands of individuals who
exercise powers created socially for narrow individual interests. The ends remain valid.
But the means of attaining them demand a radical change in economic institutions and the
political arrangements based upon them. These changes are necessary in order that social
control of forces and agencies socially created may accrue to the liberation of all individuals associated together in the great undertaking of building a life that expresses and promotes human liberty ([1935, 1936] 1946, 125).

The idea that political arrangements are based on economic institutions is standard base-superstructure thinking, and the picture of the economy as “social” powers expropriated by narrow individual interests is equally Marxist. But Dewey was on the anti-Communist Left, fresh from running the Trotsky assassination inquiry, and in response to the issues discussed here founded the explicitly “anti-totalitarian” Committee for Cultural Freedom, which had few scientists among its members and identified Germany, Italy, Spain, and Russia as exemplifications of the totalitarian idea. This committee had the effect of splitting the popular front, which split even more deeply, and especially in Left science organizations, over the term “peace” at the time of the Hitler Stalin pact when “peace” became part of the Cominterm line (Kuznick). A simple hermeneutic in the face of these complexities is this: see how the Soviet Union is referred to or placed in the discussion. A leitmotif of these discussions, more evident in the British context, where “planning” had a greater intellectual hold on the non-Communist Left, is the problem of reconciling “freedom” with means to social ends, such as planning, that involved regulation and coercion.

Science as an Anomaly: A Problem for the American Left

A key manifesto of the American Left was a large petition circulated by Franz Boas, entitled “Manifesto on Freedom of Science.” This document, published December 1938, denounces the Stark paper with the phrase “any attack on freedom of thought in one sphere, even as non-political a sphere as theoretical physics, is in effect an attack on democracy itself,” and rejects the claim that “theory leads to a crippling of empirical research,” one of Stark’s theses, describing it as “tantamount to a denial of the whole history of modern physics” (Boas 1939a, 4). This appeal to free speech had political significance on the Left, as it was a signal of red-baiting; its use by a popular front group with significant communist leadership was a tactic for the preemption of its appropriation for red-baiting tactics that would divide the Left.

But using “freedom” against Stark produced a theoretical issue that was particularly acute for the American Left, which appears very directly in an address by Boas “at a meeting sponsored by the Lincoln’s Birthday Committee for Democracy” in 1939 (the organization soon became the American Committee for Democracy and Intellectual Freedom, discussed at length in Kuznick 1986, 195-226). Like Bernal, Boas embraces, though in carefully veiled language, the socialist regulation of life, and treats this as a consequence of the complexity produces by science itself. Science, however, and more generally the “domain of thought,” was to be exempt from this regulation.

The restrictions which we accept as unavoidable consequences of the inventive genius of mankind and the size of our population do not extend to the domain of thought. Even if we wanted to we could not maintain absolute individualism in social and economic life, but it is the goal to which we strive in intellectual and spiritual life. It took us a long time to free thought from the restraints of imposed dogma. This freedom has not by any means been achieved completely. The thoughts of many are unconsciously or consciously so restrained, and attempts at the forcible repression of thought that run counter to accepted
tenets of belief are still too frequent. A bigoted majority may be as dangerous to free thought as the heavy hand of a dictator. For this reason we demand fullest freedom of expression, so that our youth may be prepared for an intelligent use of the privileges and duties of citizenship (emphasis supplied, 1939b, 1).

Boas was a man of the left, and this is in a familiar code, the language of false consciousness and the euphemistic use of “restriction” for the exercise of state coercion. The position, however, was intellectually awkward. On the one hand, he acknowledges “the success of totalitarian states.” On the other, he embraces liberal notions of civil liberties and the term individualism (at least as it applied to science) which Dewey, incidentally, routinely attacked ([1935, 1936] 1946, 118-121). Boas, in contrast, wraps himself in the flag, as when he says, “democracy as conceived in our constitution and as expressed in our daily life is a treasure that we are determined to guard under all circumstances” (1939b, 1).

The position was intellectually awkward. Why should science be exempt from the general principle that social advance required greater regulation? Why don’t the same considerations—complexity and the end of self-sufficiency, also apply to science, with the same implications, namely that science needs to be socially controlled? Why is public opinion so dangerous to science? Isn’t the essence of democracy the rule of the many? And shouldn’t science be made to serve the public good— isn’t “absolute individualism” in science a pathology as ruinous as it is in economic life, as Bernal held? And shouldn’t the same principle, namely democratic control through planning, apply to science as it must elsewhere, as Bernal argued?

The British Left was more consistent. For Bernal, social regulation was to apply equally to science and to the economic sphere: both were to be subject to “planning” and freedom was, as noted above, to be redefined. Bernal, indeed argued that Science was Communism:

In its endeavor, science is communism. In science men have learned consciously to subordinate themselves to a common purpose without losing the individuality of their achievements. Each one knows that his work depends on that of his predecessors and colleagues, and that it can only reach its fruition through the work of his successors. In science men collaborate not because they are forced to 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 goal. (1939, 415-16)

David Hollinger’s question “what happened to the idea of science as a political model for society” can be answered by comparing Dewey, Bernal, and Boas (1983). As is so often the case, “Cold War” ideas and movements have their origins in the struggle against fascism. Bernal said that “already we have in the practice of science the prototype for all human common action”(1939, 415-16). Dewey, in contrast, spent the decade complaining about the failure of scientists to fulfill what he too called “the social function of science,” ([1936]1946, 177) because science “is still something that a group of persons, called scientists, do; something they do in laboratories, observatories, and places of special research. It is far from being the temper of mind with which human beings individually and collectively approach the problems that confront them” ([1936]1946, 173). Dewey was opposed in other respects as well. He inveighed against the understanding of science as technology, a pillar of Bernalism and the British Left, as well as against expert rule ([1936]1946, 174), quoting Bertrand Russell making the point that there was an especially large gulf between the “temper” of “practical experts who employ scientific technique”—“a temper full of a sense of limitless power, of arrogant certainty, and of pleasure in
the manipulation of human material” ([1936]1946, 173). Against this background, Boas’s call for science to be regarded as separate and distinct from the rest of society, his insistence that “individualism” and “freedom” be preserved there even when it could not be in the rest of society, stands out all the more as an alternative to either understanding of science.

What was the case for exceptionalism, for treating science as different? In Britain, it was made by Polanyi in a breakthrough paper on the autonomy of science (1941). In the United States it was made primarily by a group of Harvard commentators on science, notably James Bryant Conant, who objected to the idea that “science” had, as Dewey thought, a special “temper” that could be taken out of “laboratories, observatories, and places of special research” to supply answers to the problems of politics and life.

Enter Merton

Merton had been a protégé, an “unruly apprentice” as he later put it, of the historian of science George Sarton (1985). While still a student, Merton had written eight book reviews for Sarton’s journal Isis, and was especially responsible for “social aspects of science” (Sarton to Merton, quoted in Merton 1985, 479, cf. Miles 1975). Reviewing for Isis could not have failed to give Merton a good overview of these issues, and in fact he reviewed the major writers on the Left, including the authors of a book edited by Daniel Hall (Merton 1936a; mislabeled in his review as edited by Frederick Soddy who merely contributed an introduction), Stern (1937a, b), Hogben (1939), Bernal (1941). But what did Merton himself bring to this discussion that was distinctive? And how did it relate to what his contemporaries said about the same topics? One can approach this problem by identifying some of the contexts, both institutional and intellectual, in which Merton worked in the thirties. Four seem particularly relevant, and are reflected in the texts, as I shall note.

The “Classical” Sociological Tradition

The first is the sociological tradition itself, especially what we now think of as classical sociology. Neither text, “A Note” nor “Science and the Social Order,” is laden with references to the classics. But there is an immense problem about Merton’s attitude towards the classics, which I can only indicate. Part of the problem is connected to Merton’s own obsession with the problem of originality, evidenced by his relentless retracing of such figures of speech as “standing on the shoulders of giants,” his account of the “foreshadowings” of future sociology of science in his dissertation on Puritanism (Cohen 1990, 334-371) and his attack on “adumbrationism,” which he defines as “the denigrating of new ideas by claiming to find them old through nimble reconstruction” (1977, 114). Suffice it to say that this is a warning to his future interpreters which at the same time raises a question about what debts he is attempting to conceal. Yet Merton was steeped in the classics, especially Weber, Durkheim and Simmel, and two of the three appear in “A Note” in the thinnest of guises.

Weber of course has pride of place in Merton’s writings on science. Occupational statistics are used as evidence of the influence of Protestantism in the opening pages of The Protestant Ethic. Merton’s teacher and mentor Pitirim Sorokin commented during the writing of the thesis in 1934 that it made exactly the same mistakes as Weber and Troeltsch (Cohen 1990,
Merton had already cited an obscure passage of Weber’s “Objectivity in Social Science and Social Policy in 1937 (cf. 1973, 109), and Weber is central to Merton’s “The Unanticipated Consequences of Purposive Social Action” (1936b). So Merton’s engagement with Weber during this period is ongoing. Weber’s “Wissenschaft als Beruf” is specifically concerned with science. In this text Weber invites us to “recall Schwammerdam’s statement, ‘here I bring you proof of Gods’ provenance in the anatomy of a louse’,” which Weber notes shows “what the scientific worker, influenced (indirectly) by Protestantism and Puritanism, conceived to be his task” (1946, 142). What does Merton say about Weber’s lecture? In the two essays, he said nothing. Later, referring to a 1974 paper by Tenbruck, he notes “that it can now be seen to imply, to those attuned to implications of his ideas, a number of basic problems in what would develop as a sociology of science” (1977, 14). Merton’s core notion of ethos appears in Weber repeatedly, and particularly in The Protestant Ethic, where it is used to deal with the early capitalists’ distinctive orientation toward work. But in Merton’s “A Note on Science and Democracy” the reference is to Sumner.

The relevance of the lectures on the “Callings” of science and politics to Merton’s theme in the two Merton papers is nevertheless striking. Weber’s point in the lectures is that “the qualities that make a man an excellent scholar or teacher are not the qualities that make him a leader to give directions in practical life, or more specifically, in politics” (1946, 150), and that outsiders cannot understand the strange passions of the scholar. Merton’s similar point is that the ethos of science is alien to the democratic public. In the first of Merton’s papers, “Science and the Social Order,” the list of special virtues of scientists, which included “intellectual honesty, integrity, organized skepticism, disinterestedness, impersonality”– ([1938]1973, 259), closely matches Weber, who speaks of “the plain duty of intellectual integrity,” uses the term integrity often (1946, 155, cf. 146, 156), denounces the idea of personality in science (1946, 137), and his discussions of whether an anarchist can teach law or whether a devout Catholic and Freemason would teach a course on religious history in the same way (1946, 146) are models of the notion of disinterestedness. And although Weber does not use the term organized skepticism, the idea is there, when he says that “the primary task of a useful teacher is to teach his students to recognize inconvenient facts” (1946, 147).

Simmel was another frequent source for Merton’s sociological writings outside of science. He seems also to appear in “A Note,” also without acknowledgment, in the most striking passage in the text, where Merton characterizes scientists as “subject to rigorous policing, to a degree perhaps unparalleled in any other field of activity” (1942, 124). Elsewhere Merton discusses what he calls “observability” in the terms of Simmel’s characterization of the gentleman in his account of the sociological character of aristocracies.

“There is also an absolute limit [upon numbers] beyond which the aristocratic form of the group can no longer be maintained. The point at which it breaks down is determined in part by external, in part by psychological circumstances. If it is to be effective as a whole, the aristocratic group must be ‘visible or observable’ by every single member of it.” ([1949, 1957]1968, 373)

This was close to Merton’s view of the “exacting scrutiny of fellow-experts” that provides the social mechanism enforcing the “public and testable character of science” (1942, 124).

The Harvard View of Science
The second context is the Harvard view of science. Merton learned about science at a moment when much was being written about and taught about science at Harvard. At the end of the decade, Cambridge-Boston scientists were at the center of the scientists’ political movement. Merton was aware of this context: he later wrote of the “successive microenvironments at Harvard” concerned with understanding science as an activity (1977, 86). The specific Harvard context for the discussion of science was thus especially rich and highly relevant to Merton’s own thinking. Two key figures, L. J. Henderson and the President of Harvard, James Bryant Conant, were especially relevant. But it is still a mystery as to what Merton got out of it. Henderson wrote relatively little, but was evidently a master lecturer, and his Pareto seminars were a major intellectual event on campus (Heyl 1968). Henderson himself wrote on science in ways that prefigure Kuhn’s notion of paradigm, using the notion of “conceptual schemes” (Henderson in Barber [1941-42]1970). Henderson, Merton and Parsons repeatedly refer to Alfred North Whitehead’s Harvard lectures of the twenties on science. There is some interesting evidence of Conant’s views, but there is a temporal problem with it. Conant published his two major public statements of his view of science after the war. Conant asked whether it is “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 that “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). The reference to the crowd of witnesses sounds very close to Merton’s notions of policing and observability as the basis for the demand for disinterestedness (1942, 124). And it is striking that like Merton he does not refer to objectivity and epistemic values, and dismisses the idea that individual character rather than social circumstance is decisive.

Was this in the Harvard air Merton breathed? We know that Conant took an interest in Merton, who tells the story of being invited to lunch by Conant, as a graduate student, to discuss Puritanism and science (Merton and Gaston 1977, 85). How much, and what, did Merton derive from Conant or Henderson? Conant was an “opportunity liberal” who promoted education reform in such popular writings as “Education for a Classless Society” (1940) and opened up Harvard to poor but talented students. These concerns appear in “A Note” under the heading of universalism. But much of Conant, notably that which reappears in Kuhn, has no echo in Merton, despite the fact that Merton later taught out of Conant’s collection of case studies.

There were also many other Harvard sources. W. D. Cannon, a sometime Pareto circle participant and inspirer of Parsons with respect to equilibration and homeostasis, had made a famous declaration complaining about financial support for science in 1936 in Moscow (Kuznick, 1987, 154-55, where he was a guest of his friend, the lavishly funded Pavlov, and was an inveterate petition signer, as were such Left wing scientists as Harlow Shapley. All were drawn into the issues raised by Communist dominance of popular front type organizations publically committed to “democracy,” yet resisted being drawn into Dewey’s CCF (Kuznick 1987 passim). Sarton himself was a constant participant, who was consulted about tactics in relation to the signing of the petition against Stark (Kuznick 1987, 184-5). Merton, as an “expert” on social issues in relation to science, who had commented in “Science and the Social Order” (1938), a paper delivered in 1937 at the AAAS, on the apathy of most American
Scientists, was, at Harvard, front and center as a spectator to this frenetic activity.

The Sociological Context of the Thirties

The third is the specific context of sociological thinking of the thirties, as Merton would have experienced it. Whatever his deviations from it may have been, Merton was highly attuned to the young Parsons and his concerns. Henderson, who was in effect the leading (and certainly the most powerful) “sociologist” at Harvard, had a special interest in the physician-patient relationship, which Parsons himself took on as a basic illustration of the problem of the nature of the professions. But Parsons also focused on the different, Durkheimian, problem of the integration of the professions into the larger social system. “Professionalization” was to become the key substantive idea of mid-century American sociology, of its understanding of social integration, of modernity, and provided its primary substantive bulwark against the economic point of view, by giving sociology its own special topic that is irreducible to economic considerations.

What was Merton’s relation to these concerns? The evidence of his later work is that it was a very close relation. A very large proportion of Merton’s actual empirical research related to professions. In the forties and fifties he did studies of medical schools, engineers, intellectuals in bureaucracies, and an astonishing number of studies of nursing. His own statement of his interests in science in the thirties places this problem at the top of the list: “How did the institutionalization of science as a field of activity develop in seventeenth-century England in competition with other fields of occupational interest?” (1977, 20).

But Merton was never as caught up as Parsons in the notion of the professions as the modern form of moral integration, and the difference shows up very clearly in the contrast between Merton’s norms essay and the parallel Parsons essay on science, presented at the 1946 AAAS meetings (which ignored Merton’s paper). Parsons assimilates science to the model of integration, at the level of values:

One of the most fundamental aspects of the values of science is universalism. Truth in the nature of the case cannot be parochial. It cannot be confined in its validity to one section of the population. It is therefore, as a value, likely to be integrated with other universalistic values, such as the valuation of achievement in terms of performance rather than, for instance, the valuation of status in terms of origin by birth (1946, 216).

The similarities to Merton’s formulation of the norm of universalism are clear. But Merton drew a different lesson. Merton’s point in the two papers on science was precisely the opposite of Parsons: that the contrast between science and the democratic public placed science under threat. This kind of consideration later appears in Merton’s work under the heading of ambivalence. And instead of making a Durkheimian, Parsonsian point about the norms of science, Merton used the notion of ethos to make Weber’s point about the differences between the ethos of science and those of ordinary political life, and the consequent need of science for “autonomy.”

In the 1938 essay, this appears as a specifically political point, in political language: autonomy is a itself a political term, and Merton underlines the political meaning of his claim with the comment that
In a liberal society, integration derives primarily from the body of cultural norms toward which human activity is oriented. In a dictatorial structure, integration is effected primarily by formal organization and centralization of social control. ([1938]1973], 265) Although Merton did not make this explicit, the lesson applied both to science and society: each was governed by its own ethos, and thus each were “liberal.” This is parallel to the argument developed by Polanyi, who put it in the language of conservatism: that science and democratic politics were each strongly traditional, but separate and distinct traditions.

The Political Context

The fourth is the political context, much of which has been discussed above: Merton’s problem of science and democracy was the one described by Boas. Bernal’s Social Function of Science of 1939, had a large importance to his own thought (which Merton acknowledged in his acceptance speech after being given the Bernal prize, and is the obvious source of Merton’s idea of “communism” as a norm of science. But there were specific career linkages as well, one of which is accessible through published sources, his connection with Joseph Needham. Although Merton was of course no historical materialist, there was a loose affinity between Merton’s Puritanism argument and the general idea, promoted by the British academic Left, that science was propelled by external, social forces. The significance of Merton’s argument, however, underwent a radical change after the Putney Green debates were discovered and published in the late thirties. These showed that there was a strong radical element in Puritanism, that of the diggers and levellers, that was suppressed by Cromwell’s lieutenants. The publication had an important effect on the non-Communist British left. Needham cited Merton’s dissertation in the introduction to a book by “Henry Holorenshaw”– in fact a pseudonym for Needham himself, which praised the levellers as the men who

first saw the vision of the co-operative social commonwealth, where the iniquity of class should for ever be swept away,” made the specific point that “no less than the men of property, the levellers realized the social importance of science, and foresaw the part it would one day play in human welfare (Needham 1939, 5-6).

Merton thus became Needham’s ally in making this (very dubious, but empowering) case, and Merton was deeply grateful for Needham’s support, which included a favorable review (Merton 1977, 115). Thus Merton had good reason for being circumspect in his political pronouncements: his potential and actual benefactors, such as Conant and Needham, came from more than one side of the spectrum.

Conclusion

How did Merton’s text relate to these contexts? Here is a brief summary, norm by norm.

Communism: The notion of “Science as Communism” is Bernal’s (1939, 415). Merton emphasizes anti-secrecy, a theme from Bernal (1939, 150-1, cited directly by Merton, 1942, 122), and the incompatibility of the scientific ethos with technology understood as property, a theme of the “frustration of science” debate, taken up by Bernal and ‘Red’ Bernhard Stern, among others.
Universalism: The term is Parsons’ but the idea of internationalism of science had been strongly promoted by W. D. Cannon. The discussion involves two ideas: that truth claims are to be established in accordance with pre-established impersonal criteria, and that there are no national boundaries to science. This was a staple of the literature and manifesto writing in response to Stark, which Merton himself cited (1942, 118). Also, however, it includes the idea that science ought to open to all talent, a theme of Bernal, but also of Conant. These meritocratic ideas appear in Merton under the label of “democratization” (1942, 120).

Disinterestedness. Weber is uncited, but Merton cites Parsons (1939) on professions, and comments that “The distinction between institutional compulsives and motives is of course a key conception of Marxist sociology,” meaning that not personal qualities, but role requirements were determinative (1942, 124). Merton shifts his emphasis away from Weberian virtues in the “Note,” leading to the Simmelian policing/visible gentlemen model of social control in science.

Organized skepticism. Merton’s discussion reflects the manifestos and writings of the thirties (such as Boas, 1939a). He uses the term “the suspension of judgment” popularized by Whitehead at Harvard, to characterize the distinctive stance of science which potentially conflicts with democratic publics over such topics as sacred symbols, and points to past conflicts with organized religion and to the issue of dogma. There is an allusion in the last sentence to of the section to “totalitarian society” and its anti-rationalism and centralization of institutional control. This clearly applied to Nazism, which is mentioned in a reference on the same page. The burning question on the American Left, however, was whether the Soviet Union belonged in the same category. “Totalitarian” was generally used to include it. But in the rhetorical practice of the time the qualifier Merton adds, “anti-rationalism,” implicitly excluded the Soviet Union, at least to its supporters.

Merton’s language in 1938 was political: governance by an ethos rather than central control is “liberal.” Emphasizing the mutual strangeness of the ethos of science to that of politics is also liberal. But he formulated the difference by employing the terms “communism” and “police,” political terms antithetical to liberalism, to characterize the internal social life of science. This removed the anomaly produced by Boas’s claim that society required regulation but science needed absolute individualism. The new argument was that science did not need outside policing because scientists policed themselves so well already, according to their special ethos. Thus did Merton produce a liberal argument for the autonomy of science, in the rhetorical clothing of the Left, but opposed both to Bernal and Dewey.

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Notes

2. The story of these events and their impact is well told in Werskey (1978).
3. Stark had a long prior record of hostility to English and American physics, dating to WWI, which his later anti-Semitism extended to the Jews. Ironically, the text had much more impact in the West than in Germany. The Nazis paid little attention to him and shortly after this text appeared his influence on German physics vanished as well (Loewenstein, n.d.)