

# Michael Polanyi and the Social Construction of Science

Mary Jo Nye

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*Scholars in the field of social studies of science marked the year 2012 as the 50th anniversary of the publication of Thomas S. Kuhn's The Structure of Scientific Revolutions. Kuhn's book is routinely cited as the beginning of a new intellectual movement that jettisoned logical and empiricist accounts of scientific progress in favor of sociological and psychological explanations of scientific practice. In contrast, this essay argues that the roots of the social construction of science lie earlier, in the 1930s, in the political milieu, scientific careers, and intellectual debates of a generation in which Michael Polanyi was a central figure. Crucial elements in the development of Polanyi's philosophy of science are examined, with comparisons to J. D. Bernal, Karl Mannheim and others of their generation, as well as to the younger Thomas Kuhn and to Karl Popper.*

The year 2012 marks the 50th anniversary of the publication of Thomas Kuhn's *Structure of Scientific Revolutions*. Many special conferences have been focusing on Kuhn's ideas and his legacy, just as anniversary conferences in 2008 marked the 50th anniversary of the publication of Michael Polanyi's book *Personal Knowledge*.<sup>1</sup> Kuhn's *Structure* is routinely cited as the beginning of a new intellectual agenda in studies of the history and philosophy of science, coinciding with the social and political unrest of the 1960s and with a skeptical critique of post-World War II and Vietnam-era "Big Science." This intellectual movement jettisoned the methods of the history of ideas and the assumptions of logic-oriented philosophy, including Karl Popper's popular philosophy of critical rationalism, in favor of sociological and anthropological approaches to explaining contemporary and past science. By the 1970s, practitioners of the social construction of science (which became identified under the broader rubric of science studies) were explaining scientific theories as the result of negotiations of competing interests within scientific communities, so that scientists' claims for the value-free objectivity and universal validity of their results became a matter of self-interest rather than reflections of the natural world.

Polanyi scholars are aware of similarities between Kuhn's and Polanyi's philosophies of science, as are scholars in science studies who routinely cite Polanyi's *Personal Knowledge* or *Tacit Dimension*. Some participants in commemorative Kuhn conferences of 2012 have paid attention to Polanyi in their assessments of Kuhn, who was some thirty years younger than Polanyi and drew upon Polanyi's early writings along with many sources. In my recent book on *Michael Polanyi and His Generation*, I demonstrate in detail just how much is owed to Polanyi and to some other scientists of his generation who took the first social turn in writing about the nature of science and scientific practice.

My aim in the book is to explain in detail how the origins of the social construction of science lie in the scientific culture and political events of Europe in the 1920s and 1930s during a period when scientific intellectuals struggled to articulate the nature of scientific knowledge and to strengthen public confidence in science in an era of economic catastrophe, popular socialist and nationalist movements, and the rise of Stalinism and Fascism. I focus in my book on Michael Polanyi as a central figure—in fact THE central figure—in a generation that broke, by the 1940s, with accounts of science either as historical narratives of great men and

great ideas or as philosophical analyses of a universal scientific method based in empiricism and logic. In my book, I did not have the responsibility of writing a biography of Michael Polanyi, because of the excellent biography published in 2005 by Martin X. Moleski with the late William T. Scott, but I was able to weave Polanyi's life and career into a more general history. In this essay, I present an account of the main arguments and themes that run through my book, and I conclude with remarks about the aims and epistemology of the social construction of science by way of comparison to Michael Polanyi's generation and to Thomas Kuhn's *Structure*.

First, it is crucial to understand that Michael Polanyi (1891-1976) was a member of the Central European refugee generation of the 1930s, men and women who were expelled from their home countries for political reasons couched in terms of anti-communism or anti-Semitism. This generation and their role in the emergence of the sociology of science is the focus of the May 2012 issue of the journal *Studies in East European Thought*.<sup>2</sup> The central events of Polanyi's youth were the Great War of 1914-1918 and the Bolshevik Revolution, followed in Hungary by the short-lived communist government of Bela Kun and its overthrow by conservative nationalist Hungarian military and political forces. Polanyi was a member of a sub-group that is identified by historians as the twice exiled generation or the "Hungarian phenomenon." They left Hungary in late 1919 or early 1920 for Germany, Austria, and elsewhere under pressure from a post-revolutionary, conservative regime that adopted an explicitly Christian and nationalist ideology in a Hungarian nation that previously had been a multi-ethnic state. The experience of the Hungarians' first exile is reflected in the Hungarian sociologist Karl Mannheim's description of the "socially unattached intelligentsia" in *Ideology and Utopia*, published in German in 1929.<sup>3</sup>

Michael Polanyi was one of the older members of this generation of Hungarian scientists. They were born around 1900 and included Eugene Wigner, Leó Szilárd, John von Neumann, and Dennis Gábor. Theodore von Karman was ten years older than Polanyi, and Edward Teller was a decade younger. As scientists, they worked in Germany or Austria before the anti-Semitic policies of the Third Reich forced their second emigration elsewhere. The first part of my book focuses on Polanyi's early life in Budapest; the circumstances of his double exiles, his later experiences of McCarthy-era discrimination in the United States, and his reflections on assimilation and Jewish identity. In Budapest, the young Polanyi absorbed a cosmopolitan, liberal, and trans-national European point of view that characterized many assimilated or baptized Jews of early twentieth-century Budapest. His own family upbringing was secular, educated, literary, and scientific with a strong dose of leftwing politics, which he came to reject. The vision of the multiethnic society promulgated among liberal politicians and thinkers of the Habsburg Monarchy was one of the underlying foundations of Polanyi's later descriptions of the "republic of science" and "public liberty."

The Budapest upbringing was crucial in other respects as well. Nowhere in Europe were citizens of Jewish origin more assimilated, secularized, and "westernized" than in Budapest before the Great War. His confrontation with a rise in anti-Semitism in Hungary and Germany forced Polanyi to rethink the meaning of Jewish identity and his relationship to religion and to Christianity. His religious reflections strongly influenced his later philosophy of science. I emphasize, too, that his commitment to Jewish assimilation was part of his commitment to modern science, to the old Hapsburg Empire and to internationalism, and to his political criticism of the kind of radical and revolutionary-left politics that Central European anti-communists came to identify as Jewish. As with many émigrés, Polanyi's experiences led him to acknowledge his potentially marginalized status as cultural outsider and encouraged in him the wide-ranging intellectual exploration and combativeness that helped constitute his originality. In this vein, Arthur Koestler, another of Polanyi's family

friends from Budapest who wound up in England, ruminated on the fates of Hungarians, “the only people in Europe without racial and linguistic relatives in Europe; therefore they are the loneliest on the continent. This . . . perhaps explains the peculiar intensity of their existence . . . Hopeless solitude feeds their creativity, their desire for achieving.”<sup>4</sup>

Another influence on Polanyi’s later notions of the republic of science and the autonomy of science was the long period that he spent working in Fritz Haber’s Institute for Physical Chemistry in Berlin. Arriving in Berlin in 1921 from medical and scientific studies in Budapest and Karlsruhe, Polanyi found himself in a milieu of laboratories, colleagues and co-workers, research seminars, and social groups which he came to think of as unrivaled anywhere on Earth. At that time, the institutes of the Kaiser Wilhelm Society constituted a new kind of organization for scientific research. The existence and funding of the Society relied on a network of financial support from the German national state (Reich), the Prussian state (Land), German industry, philanthropy, and private foundations, but from 1920 until 1933 the Society followed a research mission which was devoted to traditional academic values and to fundamental science. Working within this institution, Polanyi was sheltered to a large extent from pressures to make his own research conform either to pedagogical imperatives of the university or to demands from the state or from industrial employers for immediately useful results. As a result, Polanyi came to see the Haber Institute as a kind of ideal city of science, praising it in that language as early as 1928. I argue that Haber’s Institute became an idealized model for Polanyi’s vision of the need for the independence of science from external control and from demands for social usefulness.

In addition, and crucially, the day-to-day experiences of Polanyi’s long scientific career—in Budapest, Berlin, and Manchester—provided the data and the convictions that informed his rejection of standard accounts of science in the positivist mode, in favor of a sociological and psychological account of the nature of science. I discuss Polanyi’s chemical work in some detail in my book, showing how the general applicability of his views on scientific practice was ensured by the wide range of his scientific researches in the overlapping fields of thermodynamics, X-ray crystallography, chemical kinetics, reaction mechanisms, and quantum theory. Polanyi had many scientific triumphs during his career, especially in chemical kinetics and in modern chemical dynamics, where he is regarded as one of the field’s founding fathers alongside Henry Eyring and Eugene Wigner. Polanyi experienced disappointments, too, for example in his work in surface chemistry and X-ray diffraction studies of fibers and metals. Physical chemists judged his surface theory of adsorption inadequate, preferring the approach of the American chemist Irving Langmuir, and Polanyi’s pioneering work in X-ray diffraction and chemical structure met resistance from colloid chemists. There also were objections to the semi-empirical approach that he and Eyring developed for calculation of the energies of transition-states in simple chemical reactions.

I argue that Polanyi came to see his own successes and setbacks as typical of scientists’ experiences, rather than as unusual, although he tended to underestimate the esteem in which he was held in the broad scientific community. In lectures and essays that he began writing in the early 1940s in Manchester, Polanyi drew upon memories of the everyday routines and ups and downs of his own scientific career in order to develop what he intended to be a novel and controversial description of science as a community of dogmatic traditions and social practices rather than a march of revolutionary ideas and individual genius. He drew upon his laboratory experiences in order to develop an account of the apprenticeship system within science and to describe the “typical” or ordinary scientist who is at the heart of everyday scientific practice (what Thomas Kuhn more famously called normal science). Polanyi’s personal reflection on resistance to some of his own chemical results led him to sociological explanation, rather than logical explanation, for the mechanism by

which scientific priority and recognition are accorded within the structure of scientific authority. He was one of the very first scientists to come to these conclusions in published writing, along with the Polish bacteriologist Ludwik Fleck in 1935 and the British X-ray crystallographer J. D. Bernal in 1939, a point to which I will return.

After more than two decades of a high-profile research and administrative career in physical chemistry, Polanyi turned away from vigorous chemical research in the late 1930s to focus on economics, politics, and philosophy of science. One of the factors in this shift, as I document in my book, is that he was unable to duplicate at Manchester the material culture and intellectual excitement of the science of the Berlin years. Another factor was his contemporary political milieu, as he adjusted to a very different culture than central Europe at a time of immense political turmoil in Great Britain. He arrived in Manchester shortly after the formation of a coalition National Government which was contemplating various measures for central planning in order to combat the problems of the Depression. Discussions were also taking place in the context of the development of strongly centralized, command economies in Germany and the Soviet Union, which greatly distressed Polanyi.

Economics had long been a preoccupation in Michael Polanyi's family. His older brother Karl was a journalist and teacher specializing in political and economic analysis. Karl left Vienna in 1933 for London. Michael's own interest in economics had accelerated in the late 1920s, when he organized an economics dinner seminar for friends and colleagues. In the late 1930s he began reading and writing about the causes of the Great Depression, and he made two films on Keynesian free-market economics which were shown in London, Manchester, Paris, New York and elsewhere from 1938 to the mid-1940s. I argue that economics became a transition between Polanyi's career in chemistry and his vocation in the philosophy of science. He began using economic metaphors as models for the scientific community, describing science as a dynamic order of internally and mutually adjusting spontaneous actions, governed, like the free-market community, by an invisible hand. With this model, he argued against socialist planning schemes and government control of science, whether in the Soviet Union or Great Britain. He rejected, too, the views of his brother Karl that the free-market system is outmoded and had succeeded briefly only because of local conditions of time and place. It is hardly original for me to suggest that Polanyi's economics played a role in his *Minerva* essay of 1962 on the "republic of science." I do emphasize, however, that he had more than passing knowledge of economics, and that he had long been committed to classical Liberal economics in the Austrian tradition of the late Dual Monarchy. Polanyi's economics also shows the influence of debates with his older brother, and I see echoes of Karl's economic history of guild traditions and functionally-organized economic units in Michael's discussions of the nature and structure of the scientific community.<sup>5</sup>

Economics and politics are difficult to untangle, and Michael Polanyi was entangled in their interlocking spheres in England by the late 1930s. Combating British left scientists' sympathy with the Soviet experiment and defeating the social relations of science movement in Great Britain became essential aims in Polanyi's intellectual and political life around 1940. In particular, J. D. Bernal became a prime target of Polanyi's essays and lectures, some of which were collected in Polanyi's 1940 book *The Contempt of Freedom*. Bernal's 1939 book on *The Social Function of Science*, which argued the historical relationship between pure and applied science and the need for scientists to describe the usefulness of their work, fueled Polanyi's writings against science planning in Britain and led him to help found a "freedom in science" counter-movement aimed against the British scientific Left. Yet, what I have found is that while Polanyi and Bernal had unbridgeable differences over Marxism and the Soviet Union (as did Michael with his brother Karl), there are striking similarities in

their views on the operation of social norms of behavior within the scientific community and the inadequacy of a history of science told as the march of disembodied ideas.

Like Polanyi, Bernal shifted talk about science from scientific method and scientific heroes to scientific communities and scientific practice—from the logic of science to the life of science. Science must be understood, Bernal wrote, not as the pious record of works of great men, but “*as an institution*.”<sup>6</sup> He described the training of scientists and networks of scientific communication.<sup>7</sup> He insisted on scientists’ passionate desire to get to the truth and also to get ahead in competition with other scientists, and he laid out a code of moral values and ethical conduct in science in 1939 which closely resembles the American sociologist Robert K. Merton’s description three years later in a 1942 essay on “Science and Democracy,” where Merton defines the scientific norms of universalism, disinterestedness, organized skepticism, and what Merton originally called “communism.”<sup>8</sup> Had Bernal been able to extricate himself from his ideological commitment to Marxism and Stalinism, which had very little practical effect on his science or on the views expressed in *The Social Function of Science*, other than to alienate many of his readers, the immediate reactions to his views would have been substantially different.

In contrast to his political differences with Bernal, there was mostly agreement between Polanyi and the Viennese-born Karl Popper on broad political matters, but not on the nature of science or on the role of skepticism in science and politics. As I discuss in my book, Popper was in England by the mid-1940s, teaching at the London School of Economics. Educated philosophically by his personal discussions and differences with Vienna-Circle logical empiricism, Popper’s philosophical ideas first appeared in English in political writings before the appearance in English of the *Logic of Scientific Discovery*. His main target in the political writings—which first appeared in the 1940s—was the doctrine of historical determinism, with Popper rejecting the notion that one can arrive at historical laws based in observations of historical facts and the discovery of historical rhythms or patterns.<sup>9</sup> In order to argue that history is not a science, Popper had to describe what science is and what scientists do, and in this he used the same arguments in *Poverty of Historicism* and *The Open Society and Its Enemies* as he did in the 1934 German version of *The Logic of Scientific Discovery*.

Karl Mannheim figures importantly in this history. One of Popper’s targets in *The Poverty of Historicism* was Mannheim who, like Popper, was teaching in the 1940s at the London School of Economics. Like Polanyi—and I will return to that point—Popper objected to Mannheim’s advocacy of central planning and social engineering in the 1935 book *Mensch und Gesellschaft im Zeitalter der Umbaus (Man and Society in an Age of Reconstruction)*. Popper identified Mannheim’s vision with a closed society that is fundamentally antithetical to an open society based in the free dialectic of making mistakes and correcting them.<sup>10</sup> Equally erroneous in Popper’s view was Mannheim’s sociology of knowledge in *Ideology and Utopia*, translated in 1936. Popper argued that Mannheim undermined confidence in scientific objectivity by discussing the mental or psychological attitude of the individual scientist and the scientist’s training in distinctive thought-models. Popper accused Mannheim of failing to recognize how intersubjectivity and the public character of scientific debate preserve the objectivity of science.<sup>11</sup>

If, on first glance, Popper seemed to be leaning here toward a social turn, Popper never made the turn. His quest for objectivity centered on critical rationalism, the process of conjecture and refutation, and the eternal under-determination of scientific theories. As for the role of the personal in scientific knowledge, Popper wrote that “subjective experience, or a feeling of conviction, can never justify a scientific statement, and . . . within science it can play no part.”<sup>12</sup> In articles of 2011 and 2012, Struan Jacobs and Phil Mullins used

correspondence to trace personal interactions between Polanyi and Popper, whose philosophical differences became painfully apparent in 1958. Polanyi subtitled *Personal Knowledge* “Towards a Post-Critical Philosophy,” thereby positioning his philosophy against Popper’s critical rationalism. The next year, Popper prefaced the 1959 edition of *Logic of Scientific Discovery* with an attack on an unnamed Polanyi, with Popper ridiculing the notions of tacit and personal knowledge as destructive of rational thought.<sup>13</sup>

Polanyi’s *Personal Knowledge* of 1958 was based on his Gifford Lectures at the University of Aberdeen during 1951-1952, a period in which he also delivered a series of lectures at the University of Chicago, soon published in 1951 as part of the book *The Logic of Liberty*. The book *Personal Knowledge* had been in the making since Polanyi hired Olive Davies in November 1941 as part-time secretary to help with his writing projects. These projects included a book, he told his friends, on scientific life. The final form of the book was at least partly influenced by the mandate of the Gifford Lectures to discuss natural religion or natural theology “without reference to or reliance upon any supposed special exceptional or so-called miraculous revelation.”<sup>14</sup> Polanyi incorporated themes from earlier lectures and essays into *Personal Knowledge*, including the necessity for “pure” science and scientific autonomy, the craft nature of scientific practice, the roles of “schools of research” and “apprenticeship,” the tension in science between innovation and tradition, the authority structure of science, and the existence of specifically scientific norms and values. His study of Gestalt psychology led him to consider dynamic elements of perception and to develop a terminology of “subsidiary awareness” and “focal awareness,” in order to distinguish the observer’s subsidiary awareness of the elements or particulars of an observation from his focus on the wholeness or closed form (“Gestalt”) of a thing.

“Tacit” knowledge was an important preoccupation in *Personal Knowledge*, with the claim that there are two kinds of knowledge: explicit, articulated, and formal knowledge on the one hand, and tacit, unarticulated, and non-formalized knowledge on the other hand. Polanyi argued that the first cannot be achieved without the second, just as “focal” knowing relies on “subsidiary” awareness. The rule-bound knowing of empiricism and logic is linked to objectivity, and the tacit knowing of know-how, intuition, and passion is linked to subjectivity. For Polanyi, personal knowledge is the unification of the objective and subjective aspects of all knowing, including, prominently, scientific knowing.<sup>15</sup>

There was a larger program in *Personal Knowledge*, however, than just the philosophy of science. Polanyi aimed to undermine what he believed to be the false ideal of “objectivity” in post-Enlightenment scientific and “critical” thinking, a rationalist outlook, he believed, that prevents unification of the biological and physical sciences and, more broadly, the natural sciences and social sciences.<sup>16</sup> He identified the ideal of objectivity with the wrongful elimination of realism from the philosophy of science, and he reiterated forcefully and at length in *Personal Knowledge* his earlier statements of faith in scientific discovery as an effort to make contact with reality in something like a “prayerful search for God.”<sup>17</sup> Calling the reader’s attention to the atrocities committed by totalitarian regimes during the 1930s and 1940s, he condemned “objectivity” for its moral blindness and indifference to human freedom. In so writing, Polanyi distanced his philosophy from mainstream philosophy of science, despite the value of many of his insights and observations.<sup>18</sup>

The immediate reaction to *Personal Knowledge* among professional philosophers of science was largely negative, in contrast to its more favorable reception among interested scientists, theologians, and religiously-concerned intellectuals. I find it ironic, given the later incorporation of some of Polanyi’s themes into the sociology of science, that one of his principal targets in *Personal Knowledge* was sociology in general and Karl Mannheim’s sociology of knowledge in particular. Polanyi had known Mannheim since the meetings

of the “Sunday Circle” at Béla Balázs’s home in Budapest during the First World War. Mannheim’s stated aim in *Ideology and Utopia* was to overcome relativism in order, like Polanyi, to demonstrate the stability of scientific knowledge (although Mannheim’s focus was social science, not natural science).<sup>19</sup> Mannheim saw his sociological epistemology as one of relationism, not relativism or relativization, drawing upon recent work in art history on style. Just as art may be definitely dated according to its style, so each form of knowledge, or what Mannheim called a “thought-model,” is possible only under certain historical conditions, and a scientific community’s thought-model binds it together to see things in a similar way. This was an insight that we can find as well in Polanyi’s interpretive frameworks, in Ludwick Fleck thought-styles and thought-collectives, and in the younger Thomas Kuhn’s paradigms.<sup>20</sup>

In Mannheim’s view, sociology of knowledge should be able to translate the results of one thought-model into another and to discover a common denominator for different perspectives.<sup>21</sup> Like Polanyi, Mannheim argued against objectivism and against the kind of demand made by Popper for the scientist’s psychological detachment from his results. In Mannheim’s view, “the problem lies not in trying to hide these perspectives or in apologizing for them, but in inquiring into the question of how, granted these perspectives, knowledge and objectivity are still possible.”<sup>22</sup>

Again, I suggest, as in the case of Bernal, politics separated Polanyi and Mannheim more than epistemology united them. They met together in London and talked, but Polanyi objected to Mannheim’s views on social planning and to the implication in Mannheim’s sociology of knowledge that “thought is not merely conditioned, but determined by a social or technical situation. I cannot tell you how strongly I reject such a view.”<sup>23</sup> Nor was Polanyi alone in his unease with Mannheim’s sociology of knowledge. Like Polanyi, Robert K. Merton detected Marxist presuppositions in Mannheim’s sociology. More significantly, Merton self-consciously oriented his sociology of science away from sociology of knowledge on the grounds that sociology of science required a stronger empirical base of knowledge about the social processes internal to the scientific community before seeking to understand extra-theoretical influences on science.<sup>24</sup>

Around the time of Polanyi’s death, the mainstream in history and philosophy of science diverged into separate channels with a wider and wider path turning away from the history of ideas, logical empiricism and critical rationalism toward the sociology of scientific institutions, the social history of science, and the sociology of scientific knowledge. I argue that Thomas Kuhn was a transitional figure in three generations of scholars in the social study of science. The first generation included Polanyi, Bernal, Mannheim, Fleck, and Merton, all born during the period 1890 through 1910. The second transitional generation prominently featured Kuhn, but also included John Ziman, Jerome Ravetz, and David Edge, all in Great Britain. A third generation, born in the 1940s, enrolled Harry Collins, Steven Shapin, and Bruno Latour, among others. Members of the second two generations acknowledge some debt or inspiration to the first generation, and they routinely cite the importance of Polanyi’s notions of tacit knowledge, apprenticeship, and the social nature of science. Kuhn’s *Structure* book brought renewed attention to Polanyi, as well as to Fleck, whose 1935 book on the conditioning of the development of scientific facts by thought-styles and thought-collectives began to attract wide notice in its 1979 English translation.<sup>25</sup>

The science studies scholars in the 1970s, in contrast to Polanyi or to Kuhn, were unrelenting in a methodology of sociological history and sociology of knowledge that largely abandoned psychology. In contrast, Kuhn, like Polanyi, had enlisted psychology into a sociological account of the way science works. Both Kuhn and Polanyi employed Gestalt theory, and both explored the notion of a psychological conversion experience

when one paradigm or framework is abandoned for another one. Kuhn insisted on radical discontinuity and even incommensurability between old and new paradigms, whereas Polanyi—like Fleck independently of him—described scientific change as a continuous process of transformations.<sup>26</sup> Kuhn dismissed Polanyi's emphasis on the individual scientist's innate act of faith or belief, saying that he found a kind of mysticism in Polanyi's emphasis on faith that made him uneasy. Kuhn also criticized what he called Polanyi's failure to work out fully the sociological implications of Polanyi's own ideas.<sup>27</sup> In this respect, the next generation followed Kuhn's sociologically-oriented program further than did Kuhn himself.

What most clearly distinguishes Polanyi and his generation from their intellectual children and grandchildren is that Polanyi's generation—Mannheim included—felt a deep reverence for natural science and mathematics. They shared a conviction of the transcendence and universalism of scientific thinking—an aesthetics of a vision beyond ourselves. This was a conviction found equally in the tenets of the logical empiricism of the Vienna Circle and of Popper's philosophy of critical rationalism. It is a view of science rooted in that generation's common culture of the 1930s, as I have tried to demonstrate in detail in my book.

In contrast, Kuhn had no fears for the life of science or for public confidence in science during the expanding scientific enterprise in the United States and Western Europe in the 1950s. Kuhn was committed to science, if not to scientific truth with a capital "T." He had a pragmatic attitude that scientific knowledge is successful problem solving, and that science is very good at what it does. In response, however, to a younger generation's interpretations of the *Structure of Scientific Revolutions* with which he disagreed, Kuhn fought hard against any epistemological relativism that would devalue the scientific enterprise as a whole. His problem, like Polanyi's, was to explain the almost sacred mystery of the overall stability and reliability of science.

The next generation after Kuhn was considerably more skeptical about Truth and about science in general. Science studies scholars took as their task the destabilization of standard scientific knowledge and the disunification of science—what Shapin has called a "lowering of the tone" and what the Cambridge historian of science Simon Schaffer has called the "loss of the obvious."<sup>28</sup> No longer concerned with Fascism or Stalinism, and for the most part abandoning debates about capitalism and Marxism, these American and British scholars who turned from studies in science to studies of science did so within a politics of criticism of a scientific power-elite that they observed working closely with the military-industrial sectors of the national state during the post-Cuban missile crisis Cold War and the Vietnam War. The expansion of Big Science across the national landscape turned many former science enthusiasts against what the science journalist Daniel Greenberg called the "immaculate conception of science," using Polanyi's *Logic of Liberty* as its exemplar.<sup>29</sup>

Scholars in science studies pursued a program of examining and unmasking the mechanisms by which scientists gain credibility within and outside their disciplinary communities in local times and places. They were largely suspicious of the claims of the scientific elite for the privileged status and civic virtue of scientific knowledge. Whereas Polanyi had insisted upon the role of the scientist's belief and commitment—the eminently personal—in daily scientific practice, science studies scholars turned Polanyi's notion upside down and often argued to the public that what is said to be scientific knowledge is only belief.

Clearly, the insights of the 1970s generation were used for different ends than intended by Polanyi and his contemporaries. The aim of Polanyi's generation was to strengthen, not diminish, public valuation of science, scientific credibility, and universal knowledge. They all would have disagreed to the last breath with Harry Collins's provocatively radical statement in the early 1980s that science is only social relations, and that

the natural world plays a “small or non-existent role in the construction of scientific knowledge.”<sup>30</sup> Of course, postmodernism—and history more generally—teaches us that authors cannot control the uses of their texts in different times and places. In a recent turn, as Bruno Latour has complained, the writings of Harry Collins and Latour have been enlisted by deniers of biological evolution and of climate change, who say that these scientific theories are only one closed scientific community’s belief. In this instance, Latour disagrees with his avowed acolytes.<sup>31</sup> Polanyi, too, would have disagreed, as did Kuhn, with some of the later uses made of his work, but this is the price that must be paid for offering powerful ideas and insights to an audience far removed from their cultural origins.

## Endnotes

<sup>1</sup>“Personal Knowledge at Fifty,” Conference, Loyola University, Chicago, 13-16 June 2008; “Reconsidering Polanyi,” Conference, Budapest, June 26-28, 2008.

<sup>2</sup>Tamàs Demeter, “Introduction,” *Studies in East European Thought*, 64 (2012): 1-4.

<sup>3</sup>On the “Hungarian phenomenon,” see Gábor Palló, “Scientists’ First Step of Emigration: From the Hungarian Periphery to the Centre,” *Periodica Polytechnica*, 34 (1990): 319-323 on 319, 320 and Gábor Palló, “Hungarians’ Second Step of Emigration: Toward the New Centers,” *Periodica Polytechnica*, 35 (1991): 78-86, on p. 85. Further, Tibor Frank, *Double Exile: Migrations of Jewish-Hungarian Professionals through Germany to the United States, 1919-1945* (London: Peter Lang, 2009); Lee Congdon, *Exile and Social Thought: Hungarian Intellectuals in Germany and Austria, 1919-1933* (Princeton: Princeton University Press, 1991); Laszlo Somlyody and Nora Somlyody, eds. *Hungarian Arts and Sciences 1848-2000* (New York: Columbia University Press, 2003). For quotation, Karl Mannheim, *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, trans. Louis Wirth and Edward Shils (New York: Harcourt, Brace, and World, 1936), pp. 154-155.

<sup>4</sup>Quoted in Kati Marton, *The Great Escape: Nine Jews Who Fled Hitler and Changed the World* (New York: Simon and Schuster, 2006), p. 11. On the Hungarian scientists and patterns of creativity, see Gábor Palló, “Scientific Creativity in Hungarian Context,” *Hungarian Studies*, 19 (2005):215-231. Also, Laura Fermi, *Illustrious Immigrants: The Intellectual Migration from Europe 1930-1941* (Chicago: University of Chicago Press, 1968), pp. 53-59.

<sup>5</sup>Michael Polanyi wrote that the ideal community of scientists resembles a body politic that works according to the economic principles of independent initiatives coordinated “as by an ‘invisible hand’ in the joint endeavor of discovery. Michael Polanyi, “The Republic of Science: Its Political and Economic Theory,” *Minerva*, 1 (1962): 54-74; quotation from pp. 54-55. For a minimal definition of Austrian Liberalism, see Deborah R. Coen, *Vienna in the Age of Uncertainty: Science, Liberalism and Private Life* (Chicago: University of Chicago Press, 2007), p. 10. For Polanyi’s early uses of the terms “dynamic order” and “spontaneously arising orders,” see his 1941 essay “The Growth of Thought in Society,” in *Economica* 8 (1941): 428-456, esp. 431-432, 435. Hayek had extended the notion of spontaneous order from economics (a 1933 lecture) to general knowledge in a 1936 lecture, published as Friedrich von Hayek, “Economics and Knowledge,” *Economica*, new series, 4 (1934): 33-54. . Note that the U.S. Justice Oliver Wendell Holmes in 1919 wrote that the best test of truth is the power of thought to get itself accepted in the competition of the market. In Massimo Pigliucci, *Nonsense on Stilts: How to Tell Science from Bunk* (Chicago: University of Chicago Press, 2010), p. 112.

<sup>6</sup>J. D. Bernal, *Social Function of Science* (Cambridge, Mass.: MIT Press, 1967), p. 11.

<sup>7</sup>Bernal, *Social Function of Science*, especially pp. 310, 317. While lobbying for a central governmental

agency in support of science, he also wrote that the organization and financial support of science must be largely controlled by the efforts of scientists themselves, not by bureaucrats. Bernal, *Social Function of Science*, pp. 65, 310, 321-323.

<sup>8</sup>See Gary Werskey, *The Invisible College: A Collective Biography of British Scientists and Socialists of the 1930s* (London: Allen Lane, 1978), p. 189, on Bernal, *Social Function of Science*, p. 416. See Robert K. Merton, "A Note on Science and Democracy," *Journal of Legal and Political Sociology*, 1 (1942): 115-126, reprinted as "The Normative Structure of Science" in Merton, *Social Theory and Social Structure: Toward the Codification of Theory and Research* (Glencoe, Ill.: Free Press, 1949), pp. 307-316. A recent analysis of Merton's norms is found in Stephen Turner, "Merton's 'Norms' in Political and Intellectual Context," *Journal of Classical Sociology*, 7 (2007): 161-178. Merton changed his own characterization of the scientific community's communism to "communalism" in order to make clear the non-Marxist meaning of the common ownership of scientific information, discoveries, and theories.

<sup>9</sup>Karl Popper, *The Poverty of Historicism* (London: Routledge, 2004), p. 140, p. 3.

<sup>10</sup>Popper, *Poverty of Historicism*, pp. 43, 58, 62-63, 69. Karl Mannheim's *Mensch und Gesellschaft im Zeitalter der Umbaus* (Leiden: Awsijthoff, 1935) appeared in English in 1940 as *Man and Society in an Age of Reconstruction* published in London by Kegan Paul Publishers.

<sup>11</sup>Popper, *Poverty of Historicism*, pp. 143-144. On Popper as making the social turn, see Ian Jarvie, *The Republic of Science: The Emergence of Popper's Social View of Science* (Amsterdam: Rodopi, 2001).

<sup>12</sup>Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), pp. 22-26, quotation on p. 24.

<sup>13</sup>Mary Jo Nye, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011), p. 269. See Struan Jacobs and Phil Mullins, "Relations between Karl Popper and Michael Polanyi," *Studies in the History and Philosophy of Science*, 42 (2011): 426-435; and Struan Jacobs and Phil Mullins, "Michael Polanyi and Karl Popper: The Fraying of a Long-Standing Acquaintance," *Tradition and Discovery*, 38: 2 (2011-2012): 61-93.

<sup>14</sup>On the Gifford Lectures see <http://www.giffordlectures.org/aberdeen.asp> (downloaded 26 June 2012).

<sup>15</sup>Useful guides to *Personal Knowledge* are Polanyi's *The Study of Man* (Chicago: University of Chicago Press, 1959); Marjorie Grene, "Personal Knowledge," *Encounter*, 11:4 (1958): 67-68; and Stefania Ruzsits Jha, *Reconsidering Michael Polanyi's Philosophy* (Pittsburgh: University of Pittsburgh Press, 2002).

<sup>16</sup>On the post-Kantian and early nineteenth-century origins of modern notions of objectivity and subjectivity, see Peter Galison and Lorraine Daston, *Objectivity* (New York: Zone Books, 2007), pp. 30-31.

<sup>17</sup>On the prayerful search for God, Michael Polanyi, *Science, Faith and Society* (Chicago: University of Chicago Press, 1964), pp. 34-35.

<sup>18</sup>In a notice of the 2009 re-issue of *The Tacit Dimension* (Chicago: University of Chicago Press, 2009), which is a 1966 book based on Polanyi's 1962 Terry Lectures at Yale, Steven French voiced mystification at Polanyi's "cosmic panorama" and references to a fateful conflict between the "moral skepticism of science and the moral demands of modern man." French notes how far distant Polanyi's work lies from mainstream philosophy. Steven French, notice of *The Tacit Dimension* in *Metascience*, 19 (2010): 157-158, quoting from *The Tacit Dimension*, p. 57.

<sup>19</sup>Karl Mannheim, *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, trans. Louis Wirth and Edward Shils (New York: Harcourt, Brace and World, 1936) includes *Ideologie und Utopie* (Bonn: F. Cohen, 1929), pp. 264, 5.

<sup>20</sup>See Tamàs Demeter, "Weltanschauung as a priori: Sociology of Knowledge from a 'Romantic' Stance," *Studies in East European Thought*, 64 (2012): 39-52; Michael Hagner, "Perception, Knowledge and

Freedom in the Age of Extremes: On the Historical Epistemology of Ludwik Fleck and Michael Polanyi,” *Studies in East European Thought*, 64 (2012): 107-120.

<sup>21</sup>Mannheim, *Ideology and Utopia*, pp. 271, 300-301.

<sup>22</sup>Mannheim, *Ideology and Utopia*, p. 296.

<sup>23</sup>Eva Gábor, ed., with the assistance of Dézsö Banki and R. T. Allen, *Selected Correspondence (1911-1946) of Karl Mannheim, Scientist, Philosopher, and Sociologist* (Lewiston: The Edwin Mellen Press, 2003), p. 314; also William T. Scott and Martin X. Moleski, S.J., *Michael Polanyi: Scientist and Philosopher* (Oxford: Oxford University Press, 2005), pp. 194-195.

<sup>24</sup>Nye, *Michael Polanyi and His Generation*, pp. 286-287.

<sup>25</sup>See Ludwik Fleck, *Genesis and Development of a Scientific Fact*, trans. Fred Bradley and Thaddeus J. Trenn, eds. Thaddeus J. Trenn and Robert K. Merton (Chicago: University of Chicago Press, 1979). The original German edition was published in Basel in 1935 by Benno Schwabe Publishers.

<sup>26</sup>On this point, see Nicola Mössner, “Thought Styles and Paradigms—A Comparative Study of Ludwik Fleck and Thomas S. Kuhn,” *Studies in the History and Philosophy of Science*, 42 (2010): 362-371.

<sup>27</sup>See Aristide Baltas et al., “A Discussion with Thomas S. Kuhn,” in *The Road Since Structure*, eds. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000), pp. 255-323; on mysticism or extrasensory perception, see p. 296; and Thomas S. Kuhn, “The Function of Dogma in Scientific Research,” in *Scientific Change: Historical Studies in the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*, ed. A. C. Crombie, pp.347-369, on pp. 394-395 on sociology.

<sup>28</sup> See Steven Shapin, “Lowering the Tone in the History of Science: A Noble Calling,” in Shapin, *Never Pure: Historical Studies of Science as if It Were Made by People with Bodies, Situated in Space, Time, and Society, and Struggling for Credibility and Authority* (Baltimore: MD: The Johns Hopkins University Press, 2010), pp. 1-14; and Simon Schaffer, in John Pickstone et al., “What is the History of Science?” *History Today*, 35 (May 1985): 46-53, on p. 49.

<sup>29</sup>For the popularization of the critical notion of the scientific priesthood, see Ralph E. Lapp, *The New Priesthood: The Scientific Elite and the Uses of Power* (New York: Harper and Row, 1965). Also Daniel S. Greenberg, *The Politics of Pure Science*, new edition (Chicago: University of Chicago Press, 1999; orig. 1967), p. 5.

<sup>30</sup>Harry Collins, “Stages in the Empirical Programme of Relativism,” *Social Studies of Science*, 11 (1981): 3-10, on p. 3.

<sup>31</sup>Bruno Latour, “Why Has Critique Run Out of Steam? From Matters of Fact to Matters of Concern,” *Critical Inquiry*, 39 (2004): 225-248, on pp. 227, 230.