Historical Sources of Science-as-Social-Practice: Michael Polanyi's Berlin

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IN THE LAST decades of the 20th century, studies in the history and sociology of science undermined distinctions between scientific content and social context, scientific ideas and social relations of science, and internal and external histories of science. Many histories of science focus on material or social practices in local

*Department of History, Oregon State University, Corvallis, OR 97331-5104 USA; nyem@onid.orst.edu. It is a privilege to contribute an essay in a volume honoring Russell McCormmach, whose scholarship has profoundly influenced my own and so many of my colleagues in our historical understanding and research interests. Among other debts, I am grateful for the experience of working with Russell in the publication of my first major article, on Gustave LeBon's "black light," in *HSPS, 4* (1972). The present essay makes considerable use of the Michael Polanyi Papers, which are held at the Special Collections of the Regenstein Library at the University of Chicago. I am grateful for permission to have consulted these papers and ones at the Archives of the Max-Planck-Gesellschaft in Berlin-Dahlem. Research for this project was supported by the National Science Foundation grant no. SBR-9321305 and by the Thomas Hart and Mary Jones Horning Endowment at Oregon State University. I am grateful to have been a Visiting Scholar at the Max-Planck-Institut-für-Wissenschaftsgeschichte in Berlin for brief periods during 1999, 2002, and 2004, and a Senior Fellow at the Dibner Institute for the History of Science and Technology during 2000/1. This paper is based on a lecture that I gave as the History of Science Society Distinguished Lecture in Vancouver, British Columbia on 4 Nov 2000. I thank Robert A. Nye, Ruth L. Sime, and Diana Kormos Buchwald for comments on an earlier version of this paper, as well as Marjorie Grene, Arne Hessenbruch, and Lewis Pyenson for some later insights.

Bibliographies of Michael Polanyi's publications may be found in E.P. Wigner and R.A. Hodgkin, "Michael Polanyi, 12 March 1891–22 February 1976," Royal Society of London, *Biographical memoirs of fellows, 23* (1977), 413–448; Marjorie Grene, ed., *The logic of personal knowledge, essays presented to Michael Polanyi on his seventieth birthday 11th March 1961* (London, 1961); and Harry Prosch, *Michael Polanyi: A critical exposition* (Albany, 1986). I thank Martin X. Moleski, S.J. for making available to me before its publication the biographical manuscript that William Scott was unable to complete before his death in 1999, revised as William T. Scott and Martin X. Moleski, S.J., *Michael Polanyi: Scientist and philosopher* (Oxford, 2005).

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places in the making of scientific knowledge.[1] In this approach, the key to the nature of science as a body or system of knowledge lies not in individual genius or unique discovery, but in how networks of scientists routinely do science—literally, in their traditions of "savoir-faire."[2] Thus, the subject of study often is the analysis of ordinary scientists going about their everyday work just as much as stories of the great figures of Galileo, Darwin, or Einstein.[3]

In an essay of 1994, David Hollinger roots a trend toward discussions of scientific communities and scientific practices in the anti-Soviet and anti-centralization concerns of intellectuals during the 1940s and 1950s.[4] In the United States, these intellectuals included the scientists Vannevar Bush, James Bryant Conant, and Warren Weaver, and the sociologists Robert Merton and Edward Shils. For them, the self-governing autonomy of the scientific community, traced by Merton to the Royal Society of London and 17th-century England, was crucial historically for the advancement of scientific knowledge.

These postwar intellectuals also argued that the scientific community itself can serve as an exemplary model of organization for a free and democratic society. For these men, the structure, rules, and values of science outweigh in significance the contributions of individual men and women or a textbook account of scientific

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The following abbreviations are used: MPP, Michael Polanyi Papers; MPG, Archiv zur Geschichte der Max-Planck-Gesellschaft, Berlin; *Polanyiana, Polanyiana: The periodical of the Michael Polanyi Liberal Philosophical Association; ZP, Zeitschrift für Physik.*

1. For examples of studies of science as practice, Andrew Pickering, ed. *Science as practice and culture* (Chicago, 1992) and Jed Z. Buchwald, ed. *Scientific practice: Theories and stories of doing physics* (Chicago, 1995). Also, Stephen Turner, *The social theory of practice: Tradition, tacit knowledge and presupposition* (Chicago, 1994). For an excellent summary and critique, see Terry Shinn and Pascal Ragouet, *Controverses sur la science: Pour une sociologie transversaliste de l'activité* (Paris, 2005).

2. Frederic Grinnell, *The scientific attitude* (2nd edn., New York, 1992) [1st edn., 1987], 2. Also see H. Otto Sibum, "Les gestes de la mesure: Joule, les pratiques de la brasserie et la science," *Annales histoire, sciences sociales* (Jul–Oct 1998), *53,* nos. 4–5, 745–774, esp. 745; and Sibum, "Reworking the mechanical value of heat: Instruments of precision and gestures of accuracy in early Victorian England," *Studies in history and philosophy of science, 26* (1995), 73–106.

3. Steven Shapin and Arnold Thackray, "Prosopography as a research tool in history of science: The British scientific community, 1700–1900," *History of science, 12* (1974), 1–28. Lewis Pyenson, "'Who the guys were': Prosopography in the history of science," *History of science, 15* (1977), 155–188.

4. David Hollinger, "Science as a weapon in *Kulturkämpfe* in the United States during and after World War II," 155–174, and "The defense of democracy and Robert K. Merton's formulation of the scientific ethos," 80–96 in *Science, Jews, and secular culture: Studies in mid-twentieth century American intellectual history* (Princeton, 1996). Steve Fuller, *Thomas Kuhn: A philosophical history for our times* (Chicago, 2000) and Steve Fuller, *Kuhn vs. Popper: The struggle for the soul of science* (New York, 2004). More broadly, Greta Jones, *Science, politics and the cold war* (London, 1988) and Frances Stonor Saunders, *The cultural cold war: The CIA and the world of arts and letters* (New York, 1999).

method in explaining the successes of science. Their views, according to recent historiography of science, were incorporated into the work of Thomas Kuhn, whose *Structure of scientific revolutions* (1962), with its emphasis upon normal science and dominant paradigms, fostered new approaches for studying the history of science through the lenses of institutions, disciplines, communities, and practices.[5]

There is considerably more to be said, however, by way of explanation of a transition in emphasis within the history of science from scientific method to scientific practice. This shift can be seated with equal plausibility in scientists' own changing perceptions of the nature of their science in the *beginning* decades of the 20th century. One scientist who experienced this transformation of perception was the Polish bacteriologist Ludwig Fleck, whose *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (1935) was rediscovered by Kuhn and other intellectuals in the 1950s.[6] This essay concentrates on the physical chemist Michael Polanyi (1891–1976), who independently was arguing by the early 1940s that it is the *socially organized* community of scientists, not isolated men and women of genius, that constitutes the essence of scientific inquiry.

In essays and books, including *Science, faith and society* (1946) and *Personal knowledge: Towards a post-critical philosophy* (1958), Polanyi offered a detailed analysis of science as social practice and of the centrality of systems of authority, apprenticeship, and tacit knowledge in the disciplinary structure of the scientific community.[7] Polanyi joined philosophers, like Karl Popper and sociologists like Merton, in popularizing the view that the explanation for the historical success of science lies not in empiricism, positivism, and logical method of explanation, but in the internal organization of science and its tightly codified practices. As with Popper and Merton, some of Polanyi's motivations in his writings on the nature of science were political. However, unlike Popper, Merton, and most other philosophers, sociologists, and historians of science in the mid-20th century, Polanyi's views also were rooted in a distinguished scientific career that extended well over 25 years.

The German milieu that formed Polanyi's interpretation of science is the subject of Christa Jungnickel and Russell McCormmach's groundbreaking study

5. Hollinger (ref. 4), 112–114, 168–172; Jan Golinski, *Making natural knowledge: Constructivism and the history of science* (Cambridge, 1998), 5, 13–27.
6. Ludwig Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatachen: Einführung in die lehre von Denkstil und Denkkollectiv* (Basel, 1935); Thaddeus J. Trenn and Robert K. Merton, eds., *Genesis and development of a scientific fact*, trans. Fred Bradley and Thaddeus J. Trenn, with foreword by Thomas S. Kuhn (Chicago, 1979). See Robert Cohen and Thomas Schnelle, eds., *Cognition and fact: Materials on Ludwig Fleck* (Dordrecht, 1986). Thomas Kuhn first read Fleck's work in 1949 or 1950. It is not clear that Polanyi read Fleck until the 1950s, if then, when Fleck seems to have sent him an autographed copy of his book.
7. Michael Polanyi, *Science, faith and society* (Oxford, 1946); *Personal knowledge: Towards a post-critical philosophy* (Chicago, 1962); also see *The tacit dimension* (Garden City, 1966). For references to Polanyi in the literature of science studies, see Charles Thorpe, "Science against modernism: The relevance of the social theory of Michael Polanyi," *British journal of sociology*, 52 (2001), 19–35, on 19–20.

of the development of theoretical physics from Ohm to Einstein in their *Intellectual mastery of nature*.[8] Jungnickel and McCormmach focus on the working lives of German physicists in educational and cultural institutions, detailing how scientists' work was embedded in laboratories, lecture halls, bureaucratic offices, journal publications, and collegial groups. Jungnickel and McCormmach provide evidence and interpretations of many individual careers in specific cities throughout Germany. For Polanyi, the crucial city was Berlin and the pivotal institution was the Kaiser-Wilhelm institutes in suburban Dahlem.

1. THE SCIENTIST

In contrast to Polanyi's philosophy of science of the 1940s and 1950s, scientists, scientist-historians, and scientist-philosophers of the 19th century did not emphasize the practical and craft dimensions of their work. Physical scientists writing about chemistry and physics in the 19th century were doing so as they sought to establish and expand their disciplinary specialties within the teaching programs of the universities. These physical scientists walked a tightrope in their efforts to persuade university colleagues and administrators of the humanistic and philosophical value of studies of the natural world while they demanded very material instruments and equipment for their research and for their students' instruction. In 1840, the philosophical faculty at Berlin rejected Justus Liebig's request to teach chemistry in the Philosophical Faculty as well as in the Medical Faculty. They argued that a science that involves laboratory instruction has no place in the Philosophical Faculty. Chemistry, said the Berlin professors, is not a science of causes, and it is not a theoretical science like mathematical physics.[9]

In countering such objections, physicists and chemists came to insist on the moral as well as the philosophical value of their work for training students and scholars in self-discipline and analytical thinking. Experimental science was said to have humanistic value fully comparable to the study of mathematics or classical languages because of the role of experiment in teaching a rigorous method of thinking.[10] Thus, most scientist-philosophers of science of the 19th and early

8. Christa Jungnickel and Russell McCormmach, *Intellectual mastery of nature: Theoretical physics from Ohm to Einstein,* vol. 1, *The torch of mathematics 1800–1870,* vol. 2, *The now mighty theoretical physics 1870–1925* (Chicago, 1986).
9. See Steven Turner, "Justus Liebig versus Prussian chemistry: Reflections on early institute-building in Germany," *HSPS, 13* (1982), 129–162. Also, Christoph Meinel, "Theory or practice? The eighteenth-century debate on the scientific status of chemistry," *Ambix, 30* (1983), 121–132.
10. See Fritz Ringer, *Education and society in modern Europe* (Bloomington, 1978); George Weisz, "The academic elite and the movement to reform higher education 1850–1885" (Ph.D. dissertation, SUNY at Stony Brook, 1976); Harvey Becher, "William Whewell and Cambridge mathematics," *HSPS, 11* (1980), 1–48; Gerrylynn K. Robert, "The liberally-educated chemist: Chemistry in the Cambridge natural sciences tripos, 1851–1914," *HSPS, 11* (1980), 157–183; Andrew Warwick, "Exercising the student body: Mathematics and

FIG 1 Michael Polanyi during an Institute excursion in June 1931. Permission of Archiv zur Geschichte der Max Planck-Gesellschaft, Berlin-Dahlem.

20th centuries, including William Whewell, Ernst Mach, Heinrich Hertz, Pierre Duhem, and Henri Poincaré, described the intellectual methods and theories of the sciences rather than practical skills and routines.[11]

The transformation of the text-based universities, which had been founded in the late medieval period, into the science-based universities of the late 19th and

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athleticism in Victorian Cambridge," in Christopher Lawrence and Steven Shapin, eds. *Science incarnate: Historical embodiments of natural knowledge* (Chicago, 1998), 288–326, esp. 293–295; Lewis Pyenson, *Neohumanism and the persistence of pure mathematics in Wilhelmian Germany,* American Philosophical Society, *Memoirs, 150* (Philadelphia, 1983).

11. William Whewell, *The philosophy of the inductive sciences, founded upon their history* (2nd edn., London, 1847); Ernst Mach, *The science of mechanics: A critical and historical account of its development*, trans. Thomas J. McCormmach (4th edn., Chicago, 1919); Heinrich Hertz, *Principles of mechanics*, trans. D.E. Jones and F.T. Walley (London, 1899); Pierre Duhem, *The aim and structure of physical theory*, trans. Philip P. Wiener (Princeton, 1954); Henri Poincaré, *The foundations of science*, trans. George B. Halsted (New York, 1913).

20th centuries took place first in Germany. Everyone recognized this. In England, in 1865, Matthew Arnold counseled university reformers to learn from the Germans: "The French university has no liberty, and the English universities have no science; the Germany universities have both."[12] In France, Adolphe Wurtz simultaneously argued the need to emulate and imitate German science with its advantages of government subsidies, regional industrial support, and fully equipped, modern laboratory facilities in association with universities.[13] The German model was heralded as the model for all of modern science.

During most of the 18th and early 19th centuries, physicists and chemists had used their own private funds to set up laboratories that they used for teaching and research. Increasingly, universities accepted funds from wealthy patrons and local industrial groups for the endowment or initial funding of laboratories and research institutes, transforming the appearance and structures of universities by the early 20th century, at the same time that student enrollments and the numbers of faculty positions expanded dramatically. Science was becoming a larger and larger enterprise. Much has been written about these developments in the history and sociology of science and education.[14] In the field of chemistry, for example, Germany by 1900 was far ahead of other countries in the numbers of chemists educated in the universities and in university-level *Technische Hochschulen* and other institutions; in the numbers of papers they produced; and in the paid positions they held in industrial companies.[15] From 1840 to World War I, nearly 800 British and American students earned doctoral degrees in chemistry in German universities. In 1905 at Berlin, Emil Fischer's research group included some 24

12. Matthew Arnold, *A French Eton. Higher schools and universities in France. Higher schools and universities in Germany,* vol. 12, *Works* (London, 1904), 249–256.
13. See Alan J. Rocke, *Nationalizing science: Adolphe Wurtz and the battle for French chemistry* (Cambridge, 2000).
14. For physics in general, see Paul Forman, J.L. Heilbron, and Spencer Weart, *Physics 'circa' 1900: Personnel, funding, and productivity of the academic establishments,* vol. 5, *HSPS* (1975), 1–185; for France, Terry Shinn, "The French Science Faculty system 1808–1914: Institutional change and research potential," *HSPS, 10* (1979), 271–332; Robert Fox and George Weisz, eds., *The organization of science and technology in France, 1808–1914* (Cambridge, 1980); and Mary Jo Nye, *Science in the provinces: scientific communities and provincial leadership in France, 1860–1930* (Berkeley, 1986); for Germany, Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie* (Berlin, 1970); and Peter Borscheid, *Naturwissenschaft, Staat und Industrie in Baden (1848–1914)* (Stuttgart, 1976); Kathryn M. Olesko, *Physics as a calling: Discipline and practice in the Königsberg seminar for physics* (Ithaca, 1991); for Great Britain, Michael Sanderson, *The universities and British industry, 1850–1970* (London, 1972), Colin Divall, "Education for design and production: Professional organization, employers, and the study of chemical engineering in British universities, 1922–1976," *Technology and culture, 35* (1994), 258–288.
15. These included BASF (Badische und Soda-Fabrik), Höchst, Bayer, and AGFA (Aktiengesellschaft für Anilinfabrikation).

to 30 assistants, advanced students, and guests. At his death in 1919, Fischer's collected works included 600 experimental articles, of which 120 were published under the sole authorship of his Ph.D. students.[16] Research now was rarely the work of a single scientist working in isolation.

In what became a famous lecture on "Science as a vocation," given at the University of Munich in 1918, Max Weber decried what he saw as the waning of asceticism and devotion in German scientific life, meaning both the human and natural sciences; self-interested professionalism, rather than the pure pursuit of learning, was a fellow traveler of the new *Wissenschaft* in modern Germany.[17]

> Academic life is a mad hazard. If the young scholar asks for my advice with regard to *habilitation,* the responsibility of encouraging him can hardly be borne. If he is a Jew, of course one says "lasciate ogni speranza" [abandon all hope]. But one must ask every other man: Do you in all conscience believe that you can stand seeing mediocrity after mediocrity, year after year, climb beyond you, without becoming embittered and without coming to grief? Naturally, one always receives the answer: "Of course, I live only for my 'calling.'" Yet, I have found that only a few men could endure this situation without coming to grief.

Weber's public characterization of a tension between an older and idealized tranquil tradition of noble calling and a modern trend toward frenzied professional life was reiterated by Svante Arrhenius in a private letter to Jacobus van't Hoff following the suicide of the physicist Paul Drude in 1906. Drude had fallen victim to the Berliners' zeal, wrote Arrhenius: to "this notion that scientists necessarily have to be the most noble [people] in the world," an attitude which was straining people beyond their capacities. "This crazy system cannot continue forever," concluded Arrhenius.[18] The fictional physicist Victor Jakob in McCormmach's novel *Nights thoughts of a classical physicist* similarly broods over Drude's fate—his youthful exuberance, his accommodating reception of the new quantum theory, the editorship of the *Annalen der Physik*, and his fateful move from the quiet of Giessen to the grueling demands of the university and institute of physics in Berlin:[19]

> What courage it must have taken to walk through the entrance of the Berlin institute the day he became its master!. . . . He had to administer the institute

16. See Mary Jo Nye, *Before big science: The pursuit of modern chemistry and physics 1800–1940* (Cambridge, 1999), 2–5, 139.
17. Max Weber, "Science as a vocation," in H.H. Gerth and C. Wright Mills, trans. and ed., *From Max Weber: Essays in sociology* (London, 1948), 129–156, on 134.
18. Quoted in Diana Kormos Barkan, *Walther Nernst and the transition to modern physical science* (Cambridge, 1999), 226.
19. Russell McCormmach, *Night thoughts of a classical physicist* (New York, 1983), quote on 113, also see 99, 100–101, 95. John Heilbron's portrait of the responsibilities of Max Planck in Berlin is similarly daunting. See John Heilbron, *The dilemmas of an upright man: Max Planck as spokesman for German science* (Berkeley, 1986).

> and the instrument collections, deliver experimental physics lectures to huge audiences, conduct laboratory courses for beginners, pharmacists, and advanced students, direct the colloquium, and examine students from all over the university. All of this was only his official responsibility. . . . Besides that, he had his editing and his work for the Germany Physical Society and, at the end, the Prussian Academy of Sciences. And he was permanently on call to answer all questions from ministries and faculties about physics and physicists and questions from all sides about optics. Staggering as all of this responsibility was, he had an even greater one: to do first-class research in a time of rapid advances in physics.

In the end, Drude was defeated by the demands of the new modern science: "A sinister force had defeated Drude, broken his soul."[20]

Michael Polanyi took up an appointment at the Kaiser Wilhelm Institute for Fiber Chemistry in Berlin in 1920. Then 29 years old, he was Hungarian-born and of Jewish origin. In 1912, while completing medical studies in Budapest, Polanyi had taken time off to study physical chemistry at the Karlsruhe Technische Hochschule in 1912 with Georg Bredig and Kasimir Fajans. Polanyi arrived in Karlsruhe just as Fritz Haber left to become Professor of Physical Chemistry at the University of Berlin and Director of the new Kaiser Wilhelm Institute for Physical Chemistry. Bredig succeeded his old friend Fritz Haber in the Karlsruhe chair of physical chemistry.[21]

Polanyi entered the Austrian army as a military surgeon in 1914, but managed to defend a doctoral thesis at the University of Budapest in 1917. In the next few years in Budapest, while his brother Karl served as general secretary of the progressive Radical Party, Michael served in the Ministry of Health in a short-lived liberal republican government, resigning when the Communists came to power in Budapest under Béla Kun. Polanyi then worked as an assistant to Georg de Hevesy at the University of Budapest before the anti-Semitic and anti-liberal policies of Admiral Miklós Horthy's new government led to both Polanyi's and Hevesy's dismissals from the University. Polanyi decided to return to Karlsruhe.[22]

In preparation for leaving Hungary, Polanyi re-registered his earlier change of name from Pollacsek to Polanyi, and he chose Austrian rather than Hungarian citizenship. He received Catholic baptism, later writing his longtime acquaintance Karl Mannheim that he had become a "converted Christian on the lines of

20. McCormmach (ref. 19), 112.

21. Dietrich Stoltzenberg, *Fritz Haber; Chemist, Nobel laureate, German, Jew* (Philadelphia, 2004), 113.

22. Interview of Michael Polanyi with Thomas Kuhn, 15 Feb 1962 in Berkeley, Sources for the History of Quantum Physics, American Institute of Physics; and William Taussig Scott, "At the wheel of the world: The life and times of Michael Polanyi," *Tradition and discovery: The Polanyi Society Periodical, 25* (1998–1999), 10–25, on 17. On Polanyi's life, especially the early years, see Wigner and Hodgkin. And Gábor Palló, "Michael Polányi's early years in science," *Bulletin for the history of chemistry, 21* (1998), 39–43.

FIG 2 Fritz Haber's residence at the Kaiser Wilhelm Institute for Physical Chemistry and Electrochemistry. Photograph by Mary Jo Nye.

Tolstoy's confession of faith."[23] Finally, Polanyi arranged to maintain his income as a consultant at Izzo, the United Incandescent Lamp and Electric Corporation's research laboratory in Budapest, which was directed by his former Hungarian scientific mentor Ignác Pfeiffer.[24]

Polanyi owed his new assistantship in the Fiber Chemistry Institute to Bredig's recommendation and his own publication record of almost twenty papers.[25] Among

23. On the name change, Scott (ref. 22), 17. Polanyi received a formal certificate of baptism from Vienna, where the baptism was registered. Regarding the certificate, letter to Michael Polanyi (Misi) from Károly Pollacsek (Uncle Karl), Vienna, 7 Jan 1920, MPP: Box 1, folder 6. Comments to Mannheim, quoted in Endre J. Nagy, "After brotherhood's golden age: Karl and Michael Polanyi," in Kenneth McRobbie, ed., *Humanity, society and commitment: On Karl Polanyi* (Montreal, 1993), 81–112, on 87, from letter from Michael Polanyi to Karl Mannheim, in English, 14 Apr 1944 (also see MPP, 4:11).On Tolstoy, also see Max Weber (ref. 17), 143. For Tolstoy's commitment to the principles of the Sermon on the Mount, but not to the divinity of Christ, as well as his struggle for belief, see *A confession of faith and other religious writings*, trans. with introduction by Jane Kentish (London, 1987).

24. Pállo (ref. 22), 42. See also Gabor Pallo, "Polányi Mihály és a kriptonlámpa," *Fizikai Szemle* (1996), #9, 311–316.

25. Before his 1920 appointment, he had briefly served as an assistant to Warburg in the KWG's Institute for Biology in 1914. Michael Polanyi, "Curriculum vitae," composed June 1933, MPP, 2:12. Of great utility is John M. Cash, *Guide to the papers of Michael Polanyi* (Chicago, 1977). Also, Wigner and Hodgkin, 413–415. On his earliest employment at the KWI, letter from Michael Polanyi to Dr. Telschow at the Max-Planck Gesellschaft (Göttingen), 27 June 1952, MPG, PA Polanyi 4, L:1952 B:07. For a bibliography of Polanyi's publications, see references cited at the headnote.

these was work on Walther Nernst's heat theorem and Einstein's quantum theory of specific heats.[26] At Polanyi's request, Bredig sent an early draft of the paper on specific heat to Albert Einstein, who indicated a favorable response.[27] "Bang, I was created a scientist!" Polanyi later said.[28] Polanyi's doctoral thesis concerned the adsorption of gases. He assumed the operation of forces within the framework of classical thermodynamics;[29] not until his collaboration with Fritz London in 1930 did Polanyi obtain a firm explanation for the adsorption forces using the new quantum mechanics.[30] In the meantime, in the 1910s and 1920s Irving Langmuir at the General Electric Laboratory in New York State published experimental results on adsorption, rejecting Polanyi's thermodynamic approach, and using instead the chemical theory of the electron valence bond. To Polanyi's disappointment, Langmuir's interpretation of adsorption prevailed, as Langmuir received the 1932 Nobel Prize for his discoveries and investigations in surface chemistry.[31]

26. Michael Polanyi, "Eine neue thermodynamische Folgerung aus der Quantenhypothese," *Verhandlungen der deutschen physikalischen Gesellschaft, 15* (1913), 156–161, esp. 157; and "Neue thermodynamische Folgerungen aus der Quantenhypothese," *Zeitschrift für physikalische Chemie, 83* (1913), 339–369, discussed in William T. Scott, "Michael Polanyi's creativity in chemistry," 279–307 in Rutherford Aris, et al., eds., *Springs of scientific creativity* (Minneapolis, 1983), 282–283. See also Wigner and Hodgkin, 416.
27. Letters from Polanyi to Alfred Reis, 11 Dec 1912; Georg Bredig to Polanyi, 1 Feb 1913; Georg Bredig to Polanyi 12 Feb 1913 (MPP, 1:2). More than 30 years later Percy Bridgmann wrote Polanyi that he was surprised to have just learned of this work and thought it unfortunate that it had escaped general notice: letter from Percy Bridgman to Polanyi, 19 Dec 1946 (MPP, 5:2). 1946 was the year in which Bridgman, who had just received the Nobel Prize in Physics, gave a lecture at the Boston AAAS meeting titled "Scientists and Social Responsibility" reiterating his opposition to making science a servant of the state and arguing that the scientific community is a model for all democratic societies. See S.S. Schweber, *In the shadow of the bomb: Bethe, Oppenheimer and the moral responsibility of the scientist* (Princeton, 2000), 6.
28. Michael Polanyi, "Autobiographical notes, 1966" ms. for *Midcentury authors* (MPP: 38.8).
29. Michael Polanyi, "Adsorption von Gasen (Dämpfen) durch ein festes nichtpflüchtiges Adsorbens," *Verhandlungen der physikalischen Gesellschaft zu Berlin, 18* (1916), 55–80. See Scott (ref. 26), 283; Wigner and Hodgkin, 417.
30. Fritz London and Michael Polanyi, "Ueber de atomtheoretische Deutung der Adsorptionskräfte," *Die Naturwissenschaften, 18* (1930), 1099–1100.
31. Irving Langmuir, "The constitution and fundamental properties of solids and liquids. I. Solids," and II. "Liquids," *Journal of the American Chemical Society, 38* (1916), 2221–2295; *39* (1917), 1848–1906. On Langmuir, see George Gaines, "Irving Langmuir (1881–1957)," in Laylin K. James, ed., *Nobel laureates in chemistry, 1901–1993* (Washington, D.C., 1993), 205–210. On Polanyi and adsorption theory, see Mary Jo Nye, "At the boundaries: Michael Polanyi's work on surfaces and the solid state," in Carsten Reinhardt, ed., *Chemical sciences in the twentieth century* (Weinheim, 2001), 246–257; and "Michael Polanyi's theory of surface adsorption: How premature?" in Ernest B. Hook, ed., *Prematurity in scientific discovery* (Berkeley, 2002), 151–163.

Polanyi's interest in reaction velocities and reaction mechanisms led in the 1920s to his experimental method of "highly dilute flames" for studying the course of reaction of two gases. This work became the basis for groundbreaking studies with the American physical chemist Henry Eyring, who arrived in Berlin in 1929 as a National Research Council Fellow.[32] Using semi-empirical methods combining quantum mechanics and experimental data, Eyring and Polanyi constructed a potential-energy diagram for the reaction of a hydrogen atom with a hydrogen molecule. They co-authored a paper in 1931, thereby founding transition-state theory, a fundamental approach to chemical kinetics. This work cemented Polanyi's longterm reputation.[33]

The problems that most interested Polanyi were predominantly theoretical in nature. However, in joining the Fiber Chemistry group in 1920, he found himself in a research center established in 1919 to address problems of interest to industrial firms and the Prussian state.[34] Polanyi was no stranger to industry and engineering. His father had studied civil engineering at the Zurich Polytechnic Institute,[35] and, as we know, Polanyi was an industrial consultant.[36] Both Nernst and Haber, the two great luminaries of German physical chemistry whom Polanyi greatly admired,

32. Haber's interest in chemiluminescence in the early 1920s was one of the starting points for Polanyi's work with dilute flames. Dietrich Stoltzenberg, *Fritz Haber: Chemiker, Nobelpreisträger, Deutscher, Jude* (Weinheim, 1994), 475.
33. See Henry Eyring, "Physical chemistry: The past 100 years," *Chemical and engineering news, 54* (1976): 88–104, on 90–93; and Jeffry L.Ramsey, "Between the fundamental and the phenomenological: The challenge of 'semi-empirical' methods," *Philosophy of science, 64* (1997), 627–653. Also see Mary Jo Nye, "Working tools for theoretical chemistry: Polanyi, Eyring, and debates on the semi-empirical method," *Chemical heritage, 23* (Sep 2005), 25. Henry Eyring and Michael Polanyi, "On the calculation of the energy of activation," *Naturwissenschaften, 18* (1930), 914–915; Henry Eyring and Michael Polanyi, "Ueber einfache Gasreaktionen," *Zeitschrift für physikalische Chemie, B12* (1931), 279–311.
34. Eckart Henning and Marion Kazemi, *Max-Planck-Gesellschaft Berichte und Mitteilungen: Dahlem—Domain of science* (Munich, 1998), 52; and Herman [or Hermann, in earlier publications] F. Mark, *From small organic molecules to large: A century of progress* (Washington, D.C., 1993), 20.
35. Interview of Eugene P. Wigner with Thomas S. Kuhn, 21 Nov–4 Dec 1963, Session II, 3 Dec 1963, p. 5 in transcripts in Sources for History of Quantum Physics, AIP Niels Bohr Library.
36. His correspondence in the early 1920s includes frequent exchanges with industrial firms about the manufacture of hydrogen and nitrogen, the fabrication of lanolin, and the design of electrical lamps. Letter to Polanyi from Aktien Gesellchaft für Betriebsökonomie (signed Arthur), dated 22 Apr 1920, 18 May 1920 (MMP, 1:8). Letter to Polanyi from patent division of Badische-Anilin and Soda Fabrik (BASF), 20 Jul 1920; Letter to Polanyi from Philips Glühlampenfabriken Aktien Gesellschaft, Eindhoven, Holland, 30 Jul 1921 (MPP, 1:15). Letter to Polanyi from Vereinigte Glühlampen und Electricitäts-Actien Gesellschaft, Budapest, 15 May 1923 (MPP, 1:19).

enjoyed considerable contacts and income from light, electrical, gas, and chemical companies. Diana K. Barkan has shown how Nernst's career illustrates the closely integrated framework of physics, chemistry, and industry in modern Germany, just as Margit Szöllösi-Janze and Dietrich Stoltzenberg each have shown Haber's close ties with industrial and military projects. From their inception, the Physikalische Technische Reichsanstalt and the Kaiser-Wilhelm institutes relied on industrial funding and carried out researches of immediate or potential applications for private enterprise and the German government.[37]

Polanyi's first assignment in Berlin was to head a research group to develop x-ray diffraction studies of natural fibers and metals that were of interest to industry.[38] A paper proposing that cellulose might have the structure of a huge molecule, later called a macromolecule, was met with skepticism from organic and colloidal chemists, and Polanyi temporarily abandoned the problem.[39] More successfully, he began to see the possibility of exploiting the analogy between the diffraction patterns for cellulose fibers and metals under stress, thereby moving into the field that would become known as solid-state and materials science.

In 1923 he changed affiliation from the Fiber Chemistry Institute to Haber's Physical Chemistry Institute as director of a research group that included Hermann Mark and briefly, Eugene Wigner.[40] Polanyi and his co-workers constructed a

37. Barkan (ref. 18), esp. ix–x, 93–116. Also, Stoltzenberg (ref. 21) and Margit Szöllösi-Janze, *Fritz Haber 1868–1934* (Munich, 1998). Also, on Haber, Fritz Stern, "Together and apart: Fritz Haber and Albert Einstein," 59–164 in *Einstein's German world* (Princeton, 1999), esp. 80, 82, 85–87, 88, 108–111. David Cahan, *An institute for an empire: The Physikalisch-Technische Reichsanstalt 1871–1918* (Cambridge, 1989); Jeffrey Alan Johnson, *The Kaiser's chemists: Science and modernization in imperial Germany* (Chapel Hill, 1990); Carola Sachse and Mark Walker, eds., *Politics and science in wartime: Comparative international perspectives on the Kaiser Wilhelm Institute*, vol. 20, *Osiris* (Chicago, 2005).
38. Michael Polanyi, "My time with x-rays and crystals" (1962), 97–104 in Marjorie Grene, ed., *Knowing and being: Essays by Michael Polanyi* (Chicago, 1969), 97. On his collaboration with R.O. Herzog and Willi Jancke, see E.E. Hellner and P.P. Ewald, "Schools and regional development: Germany" in P.P. Ewald, ed., *Fifty years of x-ray diffraction* (Utrecht, 1962), 456–468, on 461–462. And, R.O. Herzog and W. Jancke, "Roentgenspektrographische Beobactungen an Zellulose," *ZP, 3* (1920), 196–198; R.O. Herzog, Willi Jancke, and M. Polanyi, "Roentgenspektrographische Beobactungen an Zellulose II," *ZP, 3* (1920), 343–348.
39. See Yasu Furukawa, "Hermann Staudinger and the emergence of the macromolecular concept," *Historia scientiarum* (1982), 1–18, on 7–9; also, Polanyi (ref. 38), 99. On Polanyi and x-ray crystallography, see Mary Jo Nye, "Laboratory practice and the physical chemistry of Michael Polanyi," in F. L. Holmes and Trevor Levere, ed., *Instruments and experimentation in the history of chemistry* (Cambridge, 2000), 367–400.
40. Scott (ref. 26), 288; Hermann Mark, "Recollections of Dahlem and Ludwigshafen," in Ewald (ref. 38), 603–607, on 603. Eugene Wigner, *The recollections of Eugene P. Wigner as told to Andrew Szanton* (New York, 1992), 78–80.

variety of machines, drawing out molten metals into single-crystal wires and studying stress-strain properties.[41] In 1932 Polanyi developed the concept of dislocation (Versetzung) for describing the strength of crystals, presenting his results at Abraham Joffe's institute in Leningrad.[42] These investigations, too, became part of the founding work of solid-state physics and materials science.

2. MILIEU

When *Manchester guardian* journalist J.G. Crowther visited Berlin in the spring of 1930 he asked Mark what had prompted his study of the chemistry and physics of fibers. Mark explained that the postwar shortage of raw materials in the textile industry had caused industrialists to seek the aid of scientists. This, Mark said, led to *fundamental* research on fibers.[43] Crowther also talked with Haber, who had directed a huge military weapons project at his Physical Chemistry Institute during the war. Some 150 scientific co-workers and a total of 2,000 employees worked in the Berlin suburb of Dahlem on the production of nitric acid for explosives and fertilizers, as well as the preparation of poison gases.[44] Describing the current relationship between German industrial firms and academic chemists, Haber told Crowther in 1930 that "there [is] no split between the scientific and commercial side," that in this development chemistry had led the way and was followed by metallurgy and other fields.[45]

The Prussian government provided the land on which Haber's institute was built and also Haber's salary and a portion of operating expenses. Leopold Koppel, owner of a Berlin bank, the Auer Gaslight Company (Auer Gasglühlicht Gesellschaft), and other enterprises, donated an endowment which included 700,000 marks for the initial building and equipment of the Institute for Physical Chemistry followed by a 10-year budget of 35,000 marks annually.[46] While the precise means of funding changed during the ensuing years of war and inflation, with Haber contributing his own patent royalties for a time,[47] funding was always a mix of state, private, and industrial monies, including money in support of military interests.[48] After the postwar devaluation of the mark, Koppel agreed to support Haber's Institute with 15,000 gold marks annually, but Haber also had to turn to both the national and the

41. See Scott (ref. 26), 291; Mark (ref. 34), 25; and Hellner and Ewald in Ewald (ref. 28), 462.
42. Scott (1983), 292. Michael Polanyi, "Ueber eine Art von Gitterstörung, die einem Kristal plastisch machen könnte," *ZP, 89* (1934), 660–664.
43. J.G. Crowther, *Fifty years with science* (London, 1970), 63.
44. Fritz Stern (ref. 37), 119; Henning and Kazemi (ref. 34), 74.
45. Crowther (ref. 43), 63–64.
46. Henning and Kazemi (ref. 34), 71–72.
47. Henning and Kazemi (ref. 34), 74.
48. See Stern (ref. 37), 133.

Prussian governments to help cover salaries and operating costs, and he succeeded in stabilizing funding for the institute around 1924.[49]

In 1922, while Polanyi was a researcher at the Fiber Chemistry Institute, its director, Reginald Herzog set up a *Studiengesellschaft* there. Herzog was a close friend of Haber's and had completed his Habilitationsshrift under Haber in Karlsruhe. The *Studiengesellschaft* was an in-house consulting group that contracted with industrial firms to conduct specific pieces of research. When Polanyi was offered a fulltime position in 1922 at Izzo in Budapest, where he had been serving as a consultant, he arranged instead to devote 30 percent of his time in the Fiber Institute to work for Izzo, providing tasks for assistants which the firm sent to Berlin. In return, Izzo was to pay 30 percent of his regular stipend.[50]

Haber's policy for the Physical Chemistry Institute was that 30 percent of all patent income generated from work done at his institute had to be returned to it, an agreement that Polanyi signed in 1923. He continued to work as a consultant and develop patent claims over the next few years.[51] In the late 1920s he set up with his Russian colleague Abraham Joffe a *Studiengesellschaft* that employed the Russian physical chemist Stefan Bogdandy to do work for Siemens and Halske and for AEG (Allgemeine Elektrische Gesellschaft).[52] Job offers in industry and in universities gave Polanyi an opportunity to improve his Institute contract and salary, which included a living allowance, a child subsidy, and a bonus in 1929.[53]

When the Great Depression hit in late 1929, the administration of the Kaiser-Wilhelm Society called upon the national and Prussian governments for continuing subsidies. They argued that German scientific development was essential to economic recovery, that falling subsidies would spell catastrophe, and that the subsidies had to come without curbs on the scientific freedom that was necessary for researchers. The depression hit in 1932/3 with a cut of 30 percent in state funding in comparison to 1930/1.[54] By this time, the ratio of national to Prussian funds

49. Stoltzenberg (ref. 21), 230–231.
50. See William Taussig Scott and Martin X. Moleski, S.J., *Michael Polanyi: Scientist and philosopher* (Oxford, 2005), 87.
51. Document of 27 Jul 1923, signed by Polanyi, Harnack, and Franz v. Mendelsohn. See MPG, Hauptabteiling. II. Repositur 1A. PA Polanyi. Folder for 1914–1932, Blattzahl 14. Scott and Moleski mention as his clients Siemens Electric Works and the Osram Lamap Work in Berlin, the Philips Lamp Works in Eindhoven, and the United Lamp Works in Budapest. Scott and Moleski (ref. 50), 103.
52. Letter from Fritz Haber to Generalverwaltung, KWG, 21 Nov 1928. MPG: Abt. I Rep. 000 1A #1165. Also see letter from Haber to Stefan Bogdandy, 16 Nov 1928, regarding the 30% policy. MPG: Abt. I. Rep. 000 1A #1165.
53. Document of 26 Oct 1929, following a 4 Jul 1929 letter from Haber to Polanyi regarding negotiations. MPG: Hauptabteilung. II. Repositur 1A.PA Polanyi, Michael. Folder for 1914–1932, Blattzahl 14. Scott and Moleski write of offers from the German University in Prague in 1928 and from the University of Szeged in May 1929. Polanyi also had an ongoing offer from Izzo. Scott and Moleski (ref. 50), 112–113.
54. Stoltzenberg (ref. 21), 231.

in the Society's budget was approximately four to one. A striking aspect of the funding of the Kaiser-Wilhelm institutes was their relative autonomy within the Society and their relative freedom from governmental control. The Society secured this last benefit on the ground that its institutes received some of their support from private donors and industries.

Brigitte Schroeder-Gudehus notes that that were no major limitiations on the autonomy of the Society (nor of the Notgemeinschaft der deutschen Wissenschaft, which dispensed funds to individual researchers) before 1933. Like Adolf von Harnack, who was President of the Kaiser-Wilhelm-Gesellschaft until 1930, Haber successfully argued that scientific research was essential to the national economy, public health, and social welfare, and that, since it did best when free, no strings should be attached to research funding.[55] In an article in the May 1930 issue of *Der deutsche Volkswirt (The German economist),* Polanyi also insisted on the need for government support of science even when practical benefits might not be immediately obvious.[56]

There were rivalries, frustrations, and missed opportunities for Polanyi and his colleagues in Dahlem. Like most scientists, Polanyi travelled a good deal, not just in Europe, but to the U.S., the USSR, and the UK. However, he never seems to have spotted a professional position that he preferred despite continual worries about his personal income in Berlin.[57]

In January 1933 Polanyi ended 10 months of discussions by declining the generous offer of a chair and a laboratory in physical chemistry at the University of Manchester.[58] He made this decision to stay in Berlin in early 1933 although Haber encouraged him to accept the lucrative Manchester position, given the Institute's increasing financial problems and the uncertainty of the future in Germany for those of Jewish descent.[59] Polanyi turned Manchester down, only to be forced to resign from the Institute three months later, in April 1933, in consequence of the Nazi policy of "cleansing" the civil service.

55. Brigitte Schroeder-Gudehus, "The argument for the self-government and public support of science in Weimar Germany," *Minerva, 10* (1972), 537–570, esp. 564–567. And Stoltzenberg (ref. 21), 231.

56. Michael Polanyi, "Förderung der Wissenschaft," *Der deutsche Volkewirt* (23 May 1930), 1149–1151.

57. According to Scott and Moleski (ref. 50), he turned down an offer from Harvard in 1929. Polanyi's notes from his 1929 trip to the U.S. are thoroughly negative about scientific reseach opportunities in American universities, where he saw too much emphasis on applied research and on teaching.

58. Carbon copy of letter from Polanyi to Arthur Lapworth, 13 Jan 1933 (MPP, 44:4). It was on 30 Jan 1933 that Adolf Hitler was appointed Chancellor of Germany. Carbon copy of letter from Polanyi to Arthur Lapworth, 13 Jan 1933 and carbon copy of letter from Polanyi to F.G. Donnan, 17 Jan 1933 (MPP, 2:11).

59. Letter from Fritz Haber to Polanyi, 27 June 1932, in response to a note from Polanyi of 26 June 1932 (MPP, 2:9).

FIG 3 Research staff of the Kaiser Wilhelm Institute for Physical Chemistry and Electrochemistry, with some of their family members, in Spring 1933. Michael Polanyi and Fritz Haber are seated in the second row, 2nd and 3rd from the right side of the photograph. Permission of Archiv zur Geschichte der Max-Planck-Gesellschaft, Berlin-Dahlem.

In her biography of Lise Meitner, Ruth L. Sime explained why Meitner, an Austrian Jew, put off leaving Berlin and her position at the Kaiser-Wilhelm Institute for Chemistry as long as she could, until 1938.[60] Polanyi's reasons were similar to Meitner's, namely the exceptional atmosphere and opportunity for research in the Kaiser-Wilhelm institutes. The colloquium of Haber's Physical Chemistry Institute was open to all and considered subject matter ranging broadly over the overlapping boundaries of physics, chemistry, physical chemistry, and biology.[61] *Mitarbeiter* from the many research groups in Berlin-Dahlem met regularly for dining, colloquia, and socializing at the Society's luxurious Harnack House, which opened in 1929. Polanyi joined Hahn and others for gymnastic exercises and tennis matches, as well as for the train ride to Nernst's internationally famous physics colloquium in Berlin.[62] Polanyi himself ran a Sunday evening dining and discussion group at

60. Ruth Lewin Sime, *Lise Meitner: A life in physics* (Berkeley, 1996), esp. 139–154.
61. Stoltzenberg (ref. 32), 442.
62. In *Night thoughts of a classical physicist,* Victor Jakob remembers Drude: "Trim in your gymnastic suit with its dark sash, you looked like someone who got more than his share of satisfaction from life. When you took flight on the parallel bar or the wooden horse, you gave the impression that you could handle any problem with ease and confidence" (ref. 19), 110–111.

Harnack House focused on philosophical and economic matters. These members included Eugene Wigner, Fritz London, Leo Szilard, and John von Neumann. Crowther reported that it was like nothing he ever had seen in England.[63]

Crowther enthusiastically praised Dahlem. [He did not praise it without qualification, as noted further in the text.] "The first principle of the KWG," he wrote in an article for the *Manchester guardian*, is "to search for the newest developments in science and encourage them, and to employ the best managerial ability for the administrative side of scientific organization and [to] relieve geniuses of all possible distractions."[64] The dream seemed to have been realized for establishing a "German Oxford" in the quiet suburb of Berlin for scientists who would be independent of "clique and wealth."[65] Unlike Oxford, the scientists generally were quite free of students and exams, although they often had appointments at the University or Technische Hochschule. (Polanyi was horrified by the teaching loads at American universities when he visited the U.S. in 1929.)[66] Asked in 1928 by the editor of a Hungarian newspaper about his experiences in Berlin, Polanyi replied:[67]

> [Here in Berlin] the professors grab with great enthusiasm the hands of students who are thought to be gifted. They are like art collectors whose main passion is to discover talent. . . . They educated me, they placed me where I can do my utmost. They provide me with everything and do not ask for anything. They trust that the man who is aware of the joy of science, will not leave it for the rest of his life.

Polanyi's longtime friend Eugene Wigner later said of him, "I doubt he was ever again as happy as he had been in Berlin."[68]

In many respects, his idealized perception of his Berlin institute *was* the "republic of science" of which Polanyi would write in the abstract in 1962 in a famous article in Edward Shils's journal *Minerva*. On the occasion of Haber's 60th birthday celebration in 1928, Polanyi offered a tribute with themes identical to the essay of 1962. Polanyi told the audience that there have been innovators and traditionalists throughout the history of science, revolutionaries and conservatives, the *Zerstörer* and the *Erhalter*. Einstein, Planck, and Rutherford are among the revolutionaries, he said. In contrast, Fritz Haber is an *Erhalter* who believes in the substantial correctness of the scientific picture. He is a leader who recognizes that

63. Crowther (ref. 43), 66. Polanyi discussed the meetings in his interview with Thomas Kuhn (ref. 22), 10–11. Also see letter from Polanyi to Frau Dr. Toni Stolper, 25 Jan 1930 (MPP, 2:6) and handwritten note, undated (MPP, 43:2).
64. Crowther (ref. 43), 65.
65. Stern (ref. 37), 108–109; Henning and Kazemi (ref. 34), 7–12.
66. Polanyi also thought that faculty on American campuses, particularly the land-grant public university of Minnesota, were expected to spend too much time on research that had practical applications. Scott and Moleski (ref. 50), 115. See "Notes from a trip to America 1929" (MPP, 44:3).
67. M. Polányi, *Pesti Futár, 21* (1928), 37–38, quoted in Gabor Pallo (ref. 22), 39.
68. Wigner (ref. 40), 156–157.

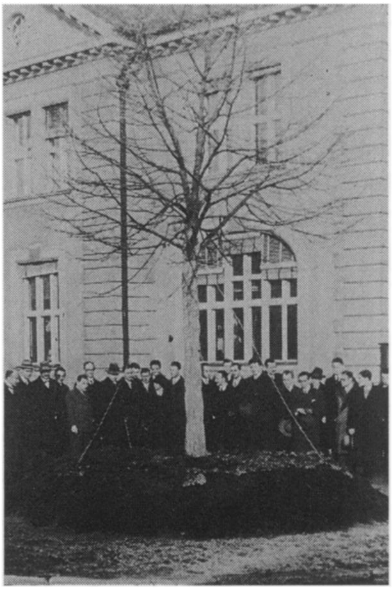

FIG 4 Planting the Haber Linden in celebration of his 60th birthday on December 2, 1928. Max Planck and Michael Polanyi, 6th and 7th from the left side of the photograph; Lise Meitner near the center. Permission of Archiv zur Geschichte der Max-Planck-Gesellschaft, Berlin-Dahlem.

science can no longer be considered just pure enlightenment, but that its pursuit and stewardship require decisive actions. Scientists must control the life of science as a family of scholars, knowing how to rule themselves in relation to the state and the economy.[69] In a vision of the ideal aims and structure of the Kaiser-Wilhelm

69. Michael Polanyi, "Geheimrat Fritz Haber Vollendet am 9 Dezember sein 60. Lebensjahr," *Metallwirtschaft, 7*, #49 (7 Dec 1928), 1316–1317. Polanyi later reflected on his difficult leave-taking from Haber in 1933 and his wish for Haber to take an even stronger stand for his Jewish colleagues than just resigning himself, which was the action Haber took at the same time that he was ordered to request Polanyi's resignation. If Polanyi knew of Haber's May 1931 letter to Hermann Dietrich (Minister of Finance) advising the government to "adopt a dictatorship and a planned economy as its own program," thereby temporarily abandoning the rule of the market economy, Polanyi surely disagreed. On Haber's letter, see Stern (ref. 37), 150.

Institute for Physical Chemistry, Polanyi described a republic of science with Haber as its chief legislator.[70]

Crowther had a similar point of view but with a more critical perspective. Crowther was a leftist-leaning journalist who would become a partisan of what came to be called Bernalism and the scientists-for-social-responsibility movement in England. There was something about Berlin, he wrote in 1930, that was "a little frightening." "I was left with the impression that the brilliant scientific efflorescence . . . had an intellectual life of its own, above that of industry and the people, in spite of the integration of the scientific research with industry." "This division of the high intellectual life from the brutal rumblings underneath was one of the most striking features of the Weimar Republic."[71] For Crowther, scientists and other intellectuals had an obligation to direct their efforts to the public good, but in Berlin they felt free from this responsibility. In Crowther's view the scientist's vaunted freedom from society and politics was an irresponsible and dangerous flaw in German scientific life, while for Polanyi, this alleged autonomy was a strength to be maintained.

3. THE PHILOSOPHER

By the fall of 1933, Polanyi had moved to Manchester. After a few years of actively directing research, Polanyi began to spend more time on other matters. By the end of his stay in Manchester in 1937, the American chemist Melvin Calvin found it difficult to interest Polanyi in chemical subjects.[72] Returning in 1935 from a visit to Moscow, where he had engaged Nikolai Bukharin, head of science policy in the Soviet Union, in heated discussion about the future of "pure science" under five-year plans,[73] Polanyi had begun putting together a film on the free market titled "The working of money."[74] The Rockefeller Foundation, which had supported

70. Michael Polanyi, "The republic of science," *Minerva, 1* (Oct 1962), 54–73. On Francis Bacon and the "Republic of science," see William Whewell (ref. 11). In analyzing the ideas of Bacon, Whewell writes: "It may be universally true, that Knowledge is Power; but we have to do with it not as Power, but as Knowledge. It is the formation of Science, not of Art, with which we are here concerned" (p. 246).
71. Crowther (ref. 43), 65, 66.
72. Melvin Calvin, "Memories of Michael Polanyi in Manchester," *Tradition and discovery, 18*, #2 (1991–1992), 40–42.
73. Michael Polanyi, *The tacit dimension* (ref. 7), 3–4.
74. The film consisted of diagrams illustrating economic processes and debuted in a soundless version with subtitles in Nov 1937. Typescript, "Memorandum on economic films," 6 pp. (MPP, 3:6) and carbon copy of letter from Michael Polanyi to Charles Vale in London, 4 Sep 1937 (MPP, 3:9). It was distributed by Gaumont British Instructional films and it was shown to viewers who included members of the Manchester Statistical Society, the technical employees of Imperial Chemical Industries in Northwich, the Hope Street Church Social Study Group in Liverpool, Walter Lippmann's colloquium in Paris, and Professors Friedrich von Hayek and Richard von Mises.

work in Berlin and Manchester, financed the improvement and distribution of the economics film.[75]

Open-mindedly, the Marxist physicist J.D. Bernal arranged a viewing of the film at a meeting of the left-wing Association of Scientific Workers in London, but he was not persuaded to Polanyi's Keynsian notions of private enterprise and government control of the money supply.[76] Nor was Michael Polanyi's brother, the socialist economic historian Karl Polanyi, who was living in London and with whom Polanyi had argued these matters since boyhood. Also unpersuaded was Polanyi's close friend in Manchester, the physicist and socialist Patrick Blackett.[77] After receiving a copy of Polanyi's *Contempt of freedom* (1940), a collection of essays against central planning, the Soviet Union, and Bernalism, Blackett wrote its author that he found in the work a hostile attitude not just to the Soviet Union, but to progressive politics generally.[78]

For Polanyi, opposition to the policies of the Soviet Union had firm roots in personal observations during visits there, as well as in long-held political convictions against the ideologies of communist parties.[79] On a visit to the Soviet Union in 1935, Polanyi saw his niece Eva Striker, recently married to Alex Weissberg, who worked at the Physical Technical Institute in Kharkov. She was arrested shortly afterwards on charges that she had surreptitiously inserted swastikas into the flowered pattern on ceramic teacups. Shortly before she was freed and expelled from Russia, her husband was arrested, only to be released to Nazi troops and to more horrors in Poland following the Stalin-Hitler pact. Arthur Koestler, who first met Eva Striker while they both were in the Budapest kindergarten run by Polanyi's eldest sister, later said that Eva's story was the inspiration for *Darkness at noon*.[80]

75. Letter from the Film Centre to Polanyi, 9 May 1938 (MPP, 3:11), followed by Letter from Michael Polanyi to Mr. Sale, 28 Apr 1939, reporting that he had received funds from the Rockefeller Foundation in order to release the film for general instructional purposes; also Letter from John Jewkes of the Economics Research Section of the University of Manchester to Robert Letort, Rockefeller Foundation in Paris, 18 May 1939, regarding support for continuing experimental work on the production of diagrammatic films illustrative of economic processes (MPP, 3:15).
76. Letter to Polanyi from Association of Scientific Workers, 24 Aug 1938; and letter to Polanyi from J.D. Bernal, 10 Sep 1938 (MPP, 3:12).
77. On Karl Polanyi, see Lee Congdon, "Between brothers: Karl and Michael Polanyi on fascism and communism," *Tradition and discovery, 24* (1997–1998), 7–27; Endre J. Nagy (ref. 23). On Blackett and Polanyi, see Mary Jo Nye, "Scientific practice and politics: A preliminary look at Blackett and Polanyi in Manchester" *Polanyiana, 5* (1996), 21–35.
78. Letter from Patrick Blackett to Michael Polanyi, 3 Nov 1941 (MPP, 4:7).
79. On Polany's sympathies with the Russian people, although not with Russian communism, see the typescript "Review of scientists in Russia: Prof. E. Ashby" (MPP, 31:4). Polanyi's mother was originally Lithuanian.
80. On this, Judit Szapor, "Laura Polanyi 1882–1957: Narratives of a life," *Polanyiana, 6,* #2 (1997), 43–54, on 47–49 and note 14,49; David Cesarani, *Arthur Koestler: The homeless mind* (New York, 1998), 13, 78, 83; Alex Weissberg, *Conspiracy of silence*, trans. Edward Fitzgerald, pref. Arthur Koestler (London, 1952), viii–xii, 13.

By the early 1940s, Koestler and Polanyi, both now in England, were firmly on the same side in the struggle against the ideology and policies of the Soviet government.[81] Polanyi located his decision to write about the nature of science in the Lysenko affair. Nikolai Vavilov's public defense of Mendelian genetics against the anti-Mendelian Trofim Lysenko in 1939 had been based on the authority of Western science. The defense failed and Mendelian genetics largely disappeared from Soviet science from the mid-1930s until the mid-1950s as a result of the imposition of Marxist-Leninist ideology. For Polanyi, the episode made urgent the definition and defense of the Western conception of science to which Vavilov had appealed.[82] What distinguishes true science and scientists from their imitators and opponents?

The traditional path of empiricism, inductivism, and logical positivism pointed to one possible answer. Karl Popper, the Viennese-born philosopher who came to England in 1945, embarked upon a different path, however. He argued against the sufficiency of empiricism, and he advocated the use of skepticism and falsifiability, rather than prediction and confirmation, as a basis for demarcating true science from pseudo-sciences.[83] Polanyi, too, broke with purely empiricist and positivist accounts of science, but went down a different track from Popper's. In so doing, Polanyi drew directly upon his career experiences in Berlin: what Kuhn would call the practice of "normal science," and what Fleck had described as the "moderne wissenschaftliche Denk-Kollectiv" or "Thought-collective."[84]

Like Fleck and Kuhn, Polanyi denied that scientists are, or should be, swayed by anomalies and the failure of predicted results. Popper was wrong to make falsification the hallmark of scientific theory. Writing in 1946, Polanyi reflected:[85]

81. The subtitle of his collection of essays, *Science, faith and society* (ref. 7). Note, too, that this turn connected with a renewal of religious convictions in the late 1930s after what he described to Mannheim as a 10-year period, the Berlin period, in which his religious interests had weakened. Letter, Michael Polanyi to Karl Mannheim, 14 Apr 1944 (MPP, 4:11).
82. Michael Polanyi, *Science, faith and society* (ref. 7) (2nd edn., with new introduction, Oxford, 1964), 31.
83. On Polanyi and positivism, see Gerald Holton, "Michael Polanyi and the history of science," in Kostas Gavroglu et al., eds., *Physics, philosophy and the scientific community* (Dordrecht, 1995) (*Boston studies in the philosophy of science, 163*), 205–233. On Popper, Malachi Haim Hacohen, *Karl Popper-The formative years, 1902–1945: Politics and philosophy in interwar Vienna* (Cambridge, 2000); and Michelle-Irène Brudny, *Karl Popper: Un philosophe heureux* (Paris, 2002).
84. Ian Hacking remarked of Charles Sanders Peirce that Peirce "is perhaps the only philosopher of modern time who was quite a good experimenter" and that "the only great philosopher familiar with measurement was C.S. Peirce." Ian Hacking, *Representing and intervening: Introductory topics in the philosophy of natural science* (Cambridge, 1983), 60, 240.
85. Polanyi (ref. 82), 31.

> In my laboratory I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties.

In response to the prevailing *cliché* that scientists do, or *should*, seek bold breakthroughs, Polanyi cautioned that the quest for novelty and recognition is not an easy one. "The scientist's surmises and hunches," he wrote: "involve high stakes, as hazardous as their prospects are fascinating. The time and money, the prestige and self-confidence gambled away in disappointing guesses will soon exhaust a scientist's courage and standing."[86]

The scientist is not mechanical and objective in his work; he is not "indifferent to the outcome of his surmises." Nor does a scientist leave his thinking cap outside the door when he dons a lab coat. In contrast to Popper's rule of falsification, Polanyi's scientist "risks defeat but never *seeks* it."[87] The scientist is rooted in beliefs about a real world, but also in a craving for success. This is Weber and Haber's world of practice, not Popper or Comte's world of ideas.

Polanyi did not think of himself as revolutionary. He wrote in 1962:[88]

> The example of great scientists [like Einstein] is the light which guides all workers in science, but we must guard against being blinded by it. There has been too much talk about the flash of discovery and this has tended to obscure the fact that discoveries, however great, can only give effect to some intrinsic potentiality of the intellectual situation in which scientists find themselves. It is easier to see this for the kind of work that I have done than it is for major discoveries.

Polanyi recognized that he had failed to win over physical chemists to his theory of adsorption and to interest theoretical physicists in his work on the solid state in the 1920s.[89] He recognized that scientific communities judge results on the basis of their intrinsic interest and plausibility, as well as their originality,

86. Ibid., 76.
87. On detachment and skepticism, see the classic positivist text Claude Bernard, *Introduction to the study of experimental medicine*, trans. Henry Copley Green (New York, 1957; orig. 1865).
88. Polanyi (ref. 38), 97.
89. Carbon copy of letter to Wichard von Moellendorff [W is correct], 27 Mar 1929 (MPP, 2:5). As Hermann Mark wrote: "the results of our studies failed to impress the leading members of the scientific community in the Kaiser Wilhelm Institute, including Max von Laue, Fritz Haber, O. Hahn, Lise Meitner, James Franck, K.F. Bonhoeffer, and others who were preoccupied with radioactivity, atomic and molecular quantum phenomena, and catalysis." Mark (ref. 34), 29.

within current frameworks or dogmas. "There must be at all times a predominantly accepted scientific view of the nature of things, in the light of which research is jointly conducted by members of the community of scientists."[90] Polanyi called the predominant consensual view "dogma," while Kuhn, who read some of Polanyi's work in the late 1940s and the 1950s, called the view a "paradigm."[91]

McCormmach dramatically illustrates Polanyi's conception of scientific dogma and its consequences in *Night thoughts of a classical physicst.* One consequence is that most of the work of most physicists is ordinary,[92] "No one is dishonored if he does not become an eminent discoverer, the professor [McCormach's protagonist, Victor Jakob] consoled himself. Inner gifts set apart certain physicists."[93] The professor's Geheimrath rebukes him on one occasion: "The trouble with you, Jakob, is that you have no convictions. Maybe you didn't make an error, but a discovery. You should have insisted on it. No wonder you have had so little success."[94] In order to make a mark in science, one's work must fit into a consensus about its significance to the community. Heinrich Hertz advised a friend that, the only way for him to advance in his career was to work on problems that correspond to the current direction of physics.[95]

The fictional Jakob remembers that in his 40s he first became aware that he no longer felt as happy about his work as he once had been.[96] Polanyi's writings about his own work in science often strike a similar melancholy or rueful note. McCormmach remarks that Richard Willstätter in his autobiography recalls a loss of freshness and happiness in his scientific work.[97] Wilhelm Ostwald reflects in his biographies of other scientists on the demands of the goddess of science and the inevitable sadness in the life of some devoted scientists.[98] Like Polanyi, the

90. Polanyi, "My time with x-rays and crystals" (ref. 38), 91–92.
91. On Kuhn, J.L. Heilbron, "Thomas Samuel Kuhn. 18 July 1922–17 June 1996," *Isis, 89* (1998), 505–515, on 505. On Kuhn's familiarity with Polanyi's writings, see, for example, letter from Thomas S. Kuhn to William H. Poteat, 28 Feb 1967, cited in Scott and Moleski (ref. 50), 246; and "A discussion with Thomas S. Kuhn," with Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi, in James Conant and John Haugeland, eds., *The road since structure: Thomas S. Kuhn* (Chicago, 2000), 255–323, on 296. I discuss Kuhn's familiarity with Polanyi and his work in a paper "Science and politics in the philosophy of science: Popper, Kuhn, and Polanyi" in Moritz Epple and Claus Zittel, eds., *Science as cultural practice/science in an age of extremes* (Berlin, in press).
92. McCormmach (ref. 20), 20–31, 117; *J.C. Poggendorffs biographisch-literarisches Handworterbuch zur Geschichte der exacten Wissenschaften* (8 vols., Leipzig, 1863–1985).
93. McCormmach (ref. 19), 27.
94. Ibid., 89.
95. Ibid., 182.
96. Ibid., 108.
97. Cited in ibid., 198.
98. Cited in ibid., 198, from an obituary for Gustav Wiedemann (1899) and from Ostwald's biographies of Drude and Ludwig Boltzmann in Ostwald's *Grosse Männer* (5th edn., Leipzig, 1919).

fictional Jakob of 1918 feared, too, that demands for scientific applications and inventions were undermining the traditions of pure research valued within the scientific community and that the national government's drive toward centralization of research menaced freedom and autonomy in scientific work.[99]

According to Polanyi, the roots of scientific *knowledge* lie in scientific *practice*. This practice is not only an *intellectual* tradition, but a *social* tradition based in a system of authority and apprenticeship, which imposes discipline but values dissent. Both tradition and innovation, though not novelty alone, characterize true science. Science is defined and constituted not by lone individuals but by a community of peers who share their experience and authority with one another. Philosophers of science and scientists themselves rarely talked about science in these terms before the mid-20th century.

Since it is social in essence, the scientific way of life cannot be mechanically replicated in strange places by reading formulaic instructions on scientific method and reasoning. "The rules of research cannot usefully be codified at all. Like the rules of all other higher arts, they are embodied in practice alone."[100] The scientific life builds upon craft skills and tacit understandings that often cannot be explained or transmitted verbally or logically. They must be learned in place, in the laboratory, the seminar, and the study. It is apprenticeship in the regimented discipline of the scientific community that serves as a demarcation between science and non-science. What counts in science is learned from within, not imposed from without. Insistence upon community, tradition, dogma, apprenticeship, and tacit knowledge is Polanyi's method for refuting the political claims of governments for ideologically based scientific theories.[101]

Polanyi worked out these basic ideas in essays published in the 1940s. His target was not only science policy in the Soviet Union or Nazi Germany, but the ambitions in Great Britain for postwar central planning and funding of science.[102] Although private, industrial, or government funding of scientific research was a common and indeed a necessary condition for scientific progress, in Polanyi's view, neither these sponsors nor any other agency should plan or control the scientist's everyday work. Scientific applications must evolve naturally from the patron's confidence in the fruits of internal scientific endeavor rather than from externally determined targets, schedules, and deadlines. Polanyi's fears that postwar British science would fall prey to planning proved ungrounded. His writings had

99. McCormmach (ref. 19), 117–188.
100. Polanyi (ref. 85), 33.
101. See the essays "Science and reality," "Authority and conscience," and "Dedication or servitude" (ref. 85). Also, Michael Polanyi, "The autonomy of science" [Feb 1943], *Memoirs and proceedings of the Manchester Literary and Philosophical Society, 85* (1941–1943), 19–38; and Polanyi (ref. 70), 54–56.
102. See William McGucken, *Scientists, society and the state: The social relations of science movement in Great Britain, 1931–1947* (Columbus, Ohio, 1984).

considerably more impact in the United States, where the structure of scientific organization in the late 1940s resembled that of Weimar Germany. Scientists and science policy-makers in the United States, for example, Bush, Conant, and Weaver, admired Polanyi's views, as did the engineer Harvey Brooks, who served on the President's Science Advisory Committee in the Eisenhower, Kennedy, and Johnson administrations. As Brooks wrote, a social contract emerged in the postwar United States between politics and science, in which science was to be supported largely through grants and contracts to institutions—universities, industries, and government agencies—leaving internal control of policy, personnel, and the method and scope of research largely to the scientists themselves.[103] This was Polanyi's vision of how the modern state should support science.

Polanyi's insights were less successful with philosophers of science than with policy-makers. Philosophers had difficulty making coherent Polanyi's attempt to define what he called *personal* knowledge rooted in the scientist's faith that science can establish "contact with a hidden reality."[104] For Polanyi, science is not reducible to social relations; scientists are constrained by what is really there in the world. Polanyi related this reality to a God-like presence, and therefore, to something like spiritual faith, unlike Kuhn and Popper, and rather like Einstein and Planck.[105] Nonetheless, Polanyi, like Kuhn after him, clearly raised the problem for many people of how objective scientific knowledge can result from the intensely personal and fallible endeavor of creative scientific inquiry.

In describing his own career, Polanyi undervalued his scientific work, but he accurately portrayed the aspirations, misgivings, and frustrations of many of his fellow scientists. In Germany, earlier and more thoroughly than in other countries, the scientists who led in their disciplines had to negotiate compromises and accommodations in their everyday work between the demands of their discipline, the university, the state, and industry, on the one hand, and their personal curiosities, passions, preoccupations, and ambitions, on the other. The professional scientific structures in France, Britain, the United States, and other countries all resembled those in pre-Nazi Germany to different degrees by the 1930s. Michael Polanyi is an unusual figure for the authenticity of personal experience and personal witness that he brought to philosophy of science, but he is representative of a generation of scientists who struggled to maintain faith in rationality, collegiality, and progress during difficult times.

103. See David H. Guston, *Between politics and science: Assuring the integrity and productivity of research* (Cambridge, 2000); and a review by Norman Metzger, "Reassessing science's social contract," *Chemical and engineering news* (29 May 2000), 71–73.
104. For example, May Brodbeck, review of *Personal knowledge* in *American sociological review, 25* (Aug 1960), 583; Michael Oakeshott, "The human craft," review of *Personal knowledge* in *Encounter, 11* (1958), 77–80; William Earle, review of *Personal knowledge* in *Science, 129* (27 Mar 1959), 831–832. Clippings in MPP, 47:2.
105. For example, see Heilbron (ref. 19), 138–140, 146, 147, 184–185, 191.

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Historical sources of science-as-social-practice: Michael Polanyi's Berlin

ABSTRACT

Historians and sociologists of science often identify the efflorescence of social studies of science with the work of postwar American intellectuals such as Robert K. Merton and Thomas S. Kuhn. They often also refer to the views of Michael Polanyi (1891–1976) on the roles of tacit knowledge, apprenticeship, social tradition, and intellectual dogmas (or what Kuhn popularized as "paradigms") in the construction of scientific knowledge. The roots of Polanyi's views on the social nature of science and his insistence on the need for scientists' autonomy in managing their own affairs lie specifically in his career experiences as a physical chemist from 1920 to 1933 in the Kaiser-Wilhelm-Gesellschaft Institutes in Berlin-Dahlem. Polanyi worked in an institution in which scientific research was supported by an array of state, industrial, and philanthropic funds, but in which he and his colleagues enjoyed substantial autonomy in their everyday research. His own successes and failures in the fields of physical chemistry, x-ray crystallography, and solid-state chemistry led him to reflect upon the everyday practices of normal science and to stress the role of the ordinary rather than the revolutionary scientist in the production of scientific knowledge. Polanyi's views lend insight into the character of German science and the research institutes in Berlin-Dahlem in the late 19th and early 20th century.

KEY WORDS: scientific practice, scientific community, German science, Michael Polanyi, Thomas Kuhn, Kaiser-Wilhelm-Institute for Physical Chemistry