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HYLE Biographies

Michael Polanyi (1891-1976)

*by Mary Jo Nye**

One of the most novel philosophers of sciences in the 20th century was the physical chemist Michael Polanyi. His first group of philosophical essays appeared in 1946 under the title *Science, Faith and* Society (Oxford University Press, Oxford, 1946), followed by *Personal Knowledge: Towards a Post-Critical Philosophy* (University of Chicago Press, Chicago, 1958), *The Tacit Dimension* (Doubleday, New York, 1966), and other volumes. One of his notable philosophical interventions occurred at a conference in Oxford in 1961, where Thomas S. Kuhn summarized his thesis on paradigms, normal science, and scientific revolutions which would be published as *The Structure of Scientific Revolutions* (University of Chicago Press, Chicago, 1962). Polanyi followed up Kuhn's presentation with the comment that he had been trying to call attention to the very view that Kuhn now was arguing, which is a view undercutting philosophical preoccupation with logical positivism and falsification. Scientists, Polanyi argued, are not heretics driven by skepticism, but rather are steadfastly committed to established beliefs and dogmas within the scientific community. It is the social scientific community, not a rational scientific method, that is the determining condition of scientific knowledge.[1] By way of example, Polanyi published two essays dealing with chemistry and drawing from his own researches in x-ray crystallography, solid-state science, and adsorption of gases on surfaces.[2]

Born in Budapest, Polanyi completed a medical degree in 1913 and a Ph.D. in physical chemistry in 1917 at the University of Budapest. He had studied physical chemistry in Karlsruhe with Georg Bredig and Kasimir Fajans. After working briefly with Georg de Hevesy in Budapest, Polanyi returned to Karlsruhe, and then in 1920 he moved to Berlin to a position at the Kaiser Wilhelm Institute for Fiber Chemistry. In 1923 he became director of the chemical kinetics research group in Fritz Haber's Institute for Physical Chemistry and Electrochemistry, where the Fiber Institute was housed.

From the 1910s through the 1930s, Polanyi pursued several physical chemistry research programs, often simultaneously. One focus was the adsorption of gases on solid surfaces, which was the subject of his Ph.D. thesis. He assumed the operation of van der Waals-like forces within the framework of classical thermodynamics, but suffered criticism in the early 1920s for not using the new theory of the electron valence bond (and what came to be called 'chemisorption') as was done by Irving Langmuir. In 1930, in collaboration with Fritz London, Polanyi offered a new and generalized explanation for adsorption forces (as dispersion forces) using the new quantum mechanics.[3]

Another focus of his work was x-ray diffraction studies of natural fibers and metals, including cellulose, a substance for which Polanyi in 1921 proposed a long-chain structure of high molecular weight. This hypothesis met with strong opposition from chemists in Berlin who favored a theory of secondary valences holding aggregates of molecules together, in contrast to the theory of macromolecules which Hermann Staudinger was just beginning to develop in Zurich. Polanyi and his co-workers dropped their interpretation of the structure of cellulose (which was correct) and inaugurated a program, using the powder and rotating crystal methods for x-ray diffraction, of solid-state analysis for extended fibers and metals. In 1932 Polanyi presented the concept of dislocation, in describing the strength of crystals, at a meeting in Leningrad. Other research groups, including R.W. Pohl's in Göttingen, further developed the notion of dislocation and point defect, consisting of missing, misplaced or foreign atoms.[4]

Polanyi's best-known work was in the field of chemical kinetics and dynamics, beginning with work in 1919 in which he was one of those who argued for a radiation hypothesis to explain elementary gas reactions, such as the formation of hydrogen bromide from bromine and hydrogen molecules. The radiation hypothesis fell to the wayside, as chemists by the mid-1920s came to favor Max Bodenstein's theory of chain reactions and Cyril Hinshelwood's kinetic theory of collisions.[5] By this time Polanyi had begun to develop the experimental method of chemiluminescence and 'highly dilute flames' to study the rates of reaction of gases such as sodium vapor and chlorine gas. In 1929 he developed with Henry Eyring the procedure that came to be called the 'semi-empirical method' for predicting activation energies for chemical reactions. Taking off from a treatment by Fritz London of the coulombic and resonance-energy contributions to binding energy, Eyring and Polanyi's co-authored paper presented the first potential energy surface for chemical reactions, using a new language of the saddle-points or cols, valleys, barriers, and passes for understanding the energy transition from reactants to products. Independently, in 1935, Eyring published a paper on the 'absolute reaction rate' for chemical reactions, calculated from the probability of an 'activated complex' and the rate of decomposition of this activated state; Polanyi's 1935 paper proceeded similarly, using what he called the 'transition state method' for calculating reaction velocities. These 1935 papers, along with the joint 1931 paper and a 1932 paper by Eugene Wigner and H. Pelzer, came to be seen as the foundation papers of the modern field of chemical dynamics.[6]

By 1937, when the Faraday Society held a conference at Manchester on the subject of reaction kinetics, Polanyi was its host in Manchester, since he had been forced to leave Berlin in 1933 after new laws under the Nazi regime forbade those who were defined as Jews to remain in civil-service positions. By 1937, Polanyi was spending increasingly more time reading and thinking about economics, politics, and the nature of science, so that the American chemist Melvin Calvin expressed frustration during a stay in Manchester that it was hard to interest Polanyi in chemical subjects anymore.[7] Polanyi had long argued economic theories with his brother Karl Polanyi, who now was living in London. In addition, Michael Polanyi had longstanding concerns about economic and political developments in the Soviet Union, rooted both in professional visits there and in the personal experiences in the Soviet Union of members of his family.

Polanyi located his decision to write about science, rather than to continue to do science, in the failure of Mendelian geneticist Nikolai Vavilov to convince his Soviet accusers of the truth of Western science in comparison to the new Soviet plant-breeding theories of Trofim Lysenko.[8] After pursuing philosophical writings during the years of the Second World War, Polanyi in 1948 exchanged his chemistry professorship for a chair of 'social studies' at Manchester, retiring in 1959 to Merton College at Oxford as a senior research fellow. His son John Polanyi (b. 1929) would follow his father's chemical interests in dynamics of elementary chemical reactions, sharing the Nobel Prize for Chemistry in 1986 with Dudley Herschbach and Yuan T. Lee.

Michael Polanyi's philosophy of science argued that there is no scientific method that can be transmitted as a logical and rigorous method to be learned in textbooks (or philosophy books). Science is learned by the practice that is transmitted from master to apprentice, as in the guilds of medieval and early modern Europe. A crucial part of scientific knowledge that is learned is tacit in character, so that it cannot be spoken, but only demonstrated and imitated. The system of scientific knowledge is a social system of authority and apprenticeship, which imposes discipline and which values tradition, while teaching expert skills. In contrast to histories of science which emphasize the work of revolutionary heroes, most scientific work is accomplished within the framework of beliefs or dogmas that provide the problems and answers for ordinary scientific work. Experimental results that first may appear to cast doubt on the accepted theoretical framework are generally assumed to be the results of experimental errors, not indications that a theory is false:[9] "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." Rejecting the notion that scientists are objective in the sense of detachment from preconceived hypotheses in the face of experimental results, Polanyi characterized this false ideal as harmless only because, in fact, it is disregarded by scientists. Only those apprentices who have worked their way through the socially organized system of science have the expertise that qualifies them to exercise the authority of natural science. Thus, had Polanyi returned explicitly to the problem of Lysenkoist genetics, he would have eliminated Lysenko as an authority in science on the grounds that he had not learned the practice of science through a valid system of apprenticeship.

In Polanyi's references to his own researches in chemistry, in the articles of 1962 and 1963, he argued that his hypothesis of long-chained molecules of high molecular weight was rejected in 1921 because it fell outside the then-accepted estimates of molecular weights in the protein chemistry of Emil Fischer and in the colloidal chemistry of Kurt Hess and Reginald Herzog. Similarly, Polanyi argued that Irving Langmuir's theory of the monomolecular adsorption layer, founded in a theory of electron-pair bonding, overshadowed Polanyi's more classical theory because physicists and physical chemists were preoccupied with the novelties and promises of electron theory. What matters in a scientific community's attribution of scientific discovery and scientific originality, he suggested, is not simply experimental or logical plausibility, but intrinsic interest at the time within the scientific community ('Potential Theory', p. 1012).

Earlier analyses of the system of the scientific community are few. One, which Kuhn brought to broader attention, was the 1935 little-known work by Polish bacteriologist Ludwik Fleck on the 'moderne wissenschaftliche Denk-Kollektiv' or 'Thought-Collective', a work which makes many arguments similar to Polanyi's and Kuhn's later points.[10] Robert Merton's formulation of the working norms of the scientific community began appearing during the war years of the 1940s and received a powerful, independent statement by Polanyi in his essay 'The Republic of Science' (*Minerva*, **1** [October 1962], 54-73).[11] In the longer run, Polanyi's philosophy of scientific practice and tacit knowledge has influenced historians and sociologists of science, more than philosophers of science, perhaps because of the difficulty of following Polanyi's attempt to define scientific knowledge as *personal* knowledge while avoiding its characterization as *subjective* knowledge. For Polanyi, science remains objective, not in the detachment of the knower from the known, but in the power of science to establish contact with a hidden reality based in the skills and commitment of the knower (*e.g.*, *Personal Knowledge*, pp. 299-303, 311).

Notes

[1] 'Commentary by Michael Polanyi', in: A.C. Crombie (ed.), *Scientific Change*, Basic Books, New York, 1963, pp. 375-380 (375).

[2] 'My Time with X-Rays and Crystals', in: P.P. Ewald (ed.), *Fifty Years of X-Ray Diffraction*, Oosthoek, Utrecht, 1962, pp. 629-636; and 'The Potential Theory of Adsorption', *Science*, **141** (1963), 1010-3.

[3] See R.A. Hodgkin and Eugene P. Wigner: 'Michael Polanyi, 1891-1976', *Biographical Memoirs of Fellows of the Royal Society*, **23** (1977), 421-448; and Michael T. Scott: 'Michael Polanyi's Creativity in Chemistry', in: Rutherford Aris, *et al.* (eds.), *Springs of Scientific Creativity*, University of Minnesota Press, Minneapolis, 1983, pp. 279-307.

[4] See Mary Jo Nye: 'Laboratory Practice and the Physical Chemistry of Michael Polanyi', in: F.L. Holmes & T. Levere (eds.), *Instruments and Experimentation in the History of Chemistry*, MIT Press, Cambridge, 2000, pp. 367-400; and 'At the Boundaries: Michael Polanyi's Work on Surfaces and the Solid State', in: C. Reinhardt (ed.), *Chemical Sciences in the Twentieth Century*, Wiley-VCH, Weinheim, 2001, pp. 246-257.

[5] See Keith Laidler: *The World of Physical Chemistry*, Oxford University Press, Oxford, 1992, pp. 263-265.

[6] See the symposium 'Fifty Years of Chemical Dynamics', Berlin, 12-15 October 1981, in: *Berichte der Bunsen Gesellschaft*, **86** (1982), 348-464.

[7] Melvin Calvin: 'Memories of Michael Polanyi in Manchester', *Tradition and Discovery*, **18**, no. 2 (1991-2), 40-42.

[8] M. Polanyi: *Science, Faith and Society*, 2nd ed., University of Chicago Press, Chicago, 1964, p. 9.

[9] *Ibid*., p. 31.

[10] Ludwik Fleck: *Genesis and Development of a Scientific Fact*, ed. Thaddeus J. Trenn and Robert K. Merton, trans. Fred Bradley and Thaddeus J. Trenn, University of Chicago Press, Chicago, 1979.

[11] On Merton, see his *Sociology of Science: An Episodic Memoir*, S. Illinois Press, Carbondale, 1977, pp. 71-108.

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