Michael Polanyi and the Social Studies of Science:

Comments on Mary Jo Nye's *Michael Polanyi and His Generation*

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This review offers comments on the book, Michael Polanyi and His Generation: Origins of the Social Construction of Science *by Mary Jo Nye.*

Mary Jo Nye's book about Michael Polanyi and his fellow pioneers in social studies of science is a joy to read. Her perspectives on the development of this field of inquiry, and her assessment of Polanyi's role are presented through an unusual organizational structure. While her book is not intended to be primarily biographical, her account of Polanyi's career, with its twists and turns, triumphs and defeats, is filled with many fascinating insights into his life and work. Nye begins by relating her own early introduction to the social aspects of science as a member of the 1960s graduate school generation studying the history of science. It was at about this time that study of the social aspects of science was ceded as having an importance in its own right, apart from science's intellectual history. Nye has aimed to show that the origins of new social conceptions of science can be traced to the scientific culture and political events of Europe in the 1930s. Michael Polanyi's story is an excellent vehicle for conveying just how those conceptions arose and evolved over time into a full-blown field of intellectual endeavor. Nye's organization of her materials is thematic rather than simply chronological, so that one finds the same temporal ground being trod more than once. For example, Polanyi's life in Weimar Berlin is the subject of Chapter 2, but his research accomplishments and challenges during the same period are covered in Chapter 3, and his contemporaneous interests in political and economic issues are treated in Chapter 5. This can at times be a bit challenging for the reader, but the end result is satisfying.

Michael Polanyi was born in 1891 in Budapest into a family living comfortably in an elegant apartment situated in a fashionable part of the city. At that time, the tenor of life in Budapest was liberal, and Jewish families such as Polanyi's enjoyed wide acceptance. However, his family's comfortable life came unraveled in stages, as his father was forced to declare bankruptcy and then died from pneumonia when Michael was 14. Michael enrolled as a medical student and received his medical degree in 1913. He served during World War I assigned to a medical hospital, but was ill for much of his service time. While recuperating, much of the time in Budapest, he was able to complete a doctoral thesis in physical chemistry. He also participated in discussion groups that dealt with ethical and religious issues, which, of course, weighed heavily on those concerned with the fate of society in light of the carnage that raged around them. His elder brother Karl was also active in these discussions.

There was great political turmoil in Hungary in the aftermath of the war. In 1919, a Christian, nationalistic regime took power, and a wave of anti-Semitism swept the country. To escape some aspects of the laws restricting the freedoms of Jews, many converted to Christian faiths. Michael Polanyi was baptized in the Roman Catholic Church, while others of his close friends chose Lutheran or Calvinist denominations.

It was clear that Hungary was not to be a hospitable place for Jews; Polanyi and most of his close friends with interests in science, including Eugene Wigner, Leo Szilard, Edward Teller and John Neumann, all destined for fame as scientists or mathematicians, undertook what Nye terms their first exile, mainly to institutions in Weimar Germany. Nye describes the struggles of the "refugee" generation in Chapter One, detailing how they had to deal with many varieties of exclusion and alienation over their careers. In the next chapter, she then doubles back, as it were, to describe in detail Polanyi's experiences as a young scientist in Weimar Germany.

In September, 1920, Polanyi began a thirteen-year period of intense scientific activity as a member of the staff of the Kaiser Wilhelm Gesselschaft (KWG), in the Berlin suburb of Dahlem. The KWG was headed by Fritz Haber, who, despite being Jewish, was something of a scientific hero in Germany because of his contributions to the war effort in the Great War. The KWG exemplified a new way of organizing and carrying out scientific work in the universities and basic research institutions. Scientists were free to work on problems of inherent interest without regard for the immediate applications of their work to the solutions of practical problems. The guiding ethos of the new German university had been spelled out earlier by Wilhelm von Humboldt, who argued for an idea of *Wissenschaft* that embraced a disinterested, passionate and free pursuit of knowledge. It permeated the scientific community of Weimar Berlin, in which were gathered a stellar array of scientists, including Max Planck, Walter Nernst, Fritz Haber and—most notably—Albert Einstein. While participating to some extent in the frenetic cultural scene in Berlin, natural scientists for the most part withdrew from the political debates that raged around them. The sense of science as a closed community was strong, and participation in it was a heady and challenging experience for a young scientist such as Polanyi. The Wednesday afternoon colloquium at the University's Physical Institute was regularly attended by several scientific stars who were already Nobel Laureates or soon would be. Polanyi later recalled these Wednesday colloquia as "the most glorious intellectual memory of my life." As Nye says, "The freedom of research that he had experienced in a tightly networked community of world-class colleagues within the tree-lined precincts of Dahlem became an induplicable but idealized memory that formed the foundation for his later writings on the nature of scientific life and scientific achievement"(p.83).

It was during this period that Polanyi learned just how competitive and challenging scientific research could be. One of his primary research themes dealt with adsorption of gases on surfaces; it had been the subject of his doctoral thesis, completed in Budapest. His aim was to develop a general theory of adsorption of gases on surfaces, based on classical thermodynamic concepts. He made good progress with a model that involved multi-layer adsorption, but there arose competition from Irving Langmuir, an American who had taken his doctoral degree in Berlin with Nernst. Langmuir, employed at General Electric Research Laboratory in Schenectady, New York, rejected the commonly held assumption that adsorption was a multi-layer process, and proposed a radically new theory that the adsorption happens on a single layer. The competition went on for several years, and Polanyi modified his model over time, in response not only to new experimental evidence but also to Langmuir's competing model. In the end, Langmuir's approach won the day. Langmuir received the Nobel prize in chemistry in 1932 "for his discoveries and investigations in surface chemistry." In his Nobel lecture he made no mention of Polanyi. For many years afterward, Polanyi reflected on the disappointment of what he felt was a rejection of his work. He remarked to Erika Cremer, "Whose fate is better, mine or Langmuir's? My theory is absolutely right but not accepted. Langmuir's theory is wrong but he is very famous . . . Langmuir is better off!" (97). In this interesting quotation, Polanyi seems to have forgotten his commitment to the notion that our understanding of nature is always contingent, that "absolutely right" and "wrong" are not appropriate terms in which to describe alternative models when each comports well with at least some aspects of the observation domain.

X-ray diffraction studies of fibers formed a second major area of research interest. Polanyi's work attracted great interest for the light it might shed on whether materials such as cellulose were composed of low molecular weight molecules stacked in regular arrays or of long, high molecular weight molecules. In 1921, he reported that the x-ray evidence was consistent with either interpretation. He was greeted with protests from the organic chemists that only the low molecular weight hypothesis could be correct; the idea of long-chain molecules had not yet taken hold. Polanyi deferred to his organic chemist colleagues on the matter. Hermann Staudinger at the Zurich Polytechnic Institute almost alone argued for the high molecular weight interpretation that eventually proved to be correct; in 1953 he received the Nobel prize for his work. Polanyi later expressed the view that he had missed an opportunity, by failing to see the importance of the problem.

Nye concludes, quite correctly I believe, that these and other experiences during Polanyi's time in Berlin formed the basis of his arguments about the ways in which scientific authority is constructed and recognition conferred in science. In later writings and interviews, Polanyi used these two and other examples from his own work to characterize the sometimes ruthless manner in which models other than the one that becomes dominant are frequently consigned to the scrap heap, even though they may contain valuable insights.

The rise of Adolf Hitler and the National Socialist Party made Germany an inhospitable place for Jews, and even Jewish scientists of the highest rank were not excluded. Haber, for example, was eventually driven from Germany, a broken man. Despite his angst at the prospect of leaving Dahlem, events forced Polanyi's hand. He accepted an appointment at Manchester University in England, and arrived there in 1933. In Chapter 4, Nye describes this phase of Polanyi's scientific life. The move was not easy, but he in due course embarked on studies of chemical reactions which he had already begun in Germany. The work on reaction mechanisms, and development of the theory of the "transition state," which Polanyi began in Germany and continued at Manchester, form the body of work for which he is most widely recognized as a scientist. Collaborative studies with Henry Eyring, who had come to study in Berlin with Polanyi as a postdoctoral student and continued his work at Berkeley and later at Princeton, and theoretical work by Eugene Wigner, his old friend from Budapest, shaped the direction of research in modern chemical dynamics. Nevertheless, from the time of their first publication in 1931, Eyring and Polanyi's work met with considerable criticism. The chemical world was not yet quite ready for the applications of new theoretical understandings to traditional chemical topics such as organic reactions. Despite initial skepticism, their theoretical model steadily gained acceptance, and Polanyi's stock rose accordingly. Why then did several visitors and collaborators in that period report that Polanyi seemed to be increasingly preoccupied with matters outside chemistry?

In Chapter 5, Nye's account of Polanyi's career takes a sharp turn away from science and into economics. She relates that his early interest in economics was a natural outgrowth of his education in Budapest, and likely due also to the influence of his older brother Karl, who became a widely recognized pioneer in economic history. In his first publication in economics, a short essay written in 1930 for *Der Deutche Volkswirt*, Michael argued for government support of long-range, fundamental science. He also organized a Sunday evening dinner and discussion group that included his close scientific friends as well as economists. At Manchester, prompted in part by travels to Russia he wrote a long paper critical of Soviet economics that eventually was published in 1935 in *The Manchester School of Economics and Social Studies.* Nye goes on to relate Polanyi's increasing preoccupations with economic issues, arguing that they significantly influenced his sociological view of science. Nye's account of this aspect of Polanyi's career strikes me as rather lengthy, but her emphasis on his economics interests may be justified, given that economic theory is a leitmotif of "The Republic of Science", 1962, one of his best-known publications, and a presence in much else that he wrote.

Nye goes on to argue in later chapters that Polanyi's economic and political views were closely coupled to his evolving ideas of science's place in modern society.

As the 1930s wore on, Polanyi became increasingly involved in political concerns. He was alarmed by what he saw as an increasing interest in socialist planning in England. His personal history had imbued him with a strong antipathy toward heavy-handed government of whatever description. He rose to argue for an independent "pure" science not predicated on social and economic needs, with autonomy for individual scientists in choosing research agendas. During the 1930s, there was considerable discussion in England around the question of whether there was a useful distinction to be made between pure and applied science, and, if there were, what that might mean for how scientists should regard their social responsibilities. J.D. Bernal's sympathetic portrayal of Soviet science in his 1939 *Social Function of Science* infuriated Polanyi. Indeed, it may have catalyzed a shift already in progress of Polanyi's major interests away from the practice of scientific research to study of the epistemic and social aspects of science. In 1941, he became one of the founding members of the Society for Freedom in Science. He later identified the launch of the SFS as the "entrance of my career as a philosopher . . . this was the turning point of my life. At any rate the last of its turning points" (p. 207-8). The transition was complete in 1948 when Polanyi exchanged his chemistry professorship for a chair of "social studies" at Manchester.

In the later chapters of her book, Nye masterfully describes the growth of interest in the political and social implications of science, beginning in the 1930s. Chapters 7 and 8 and the Epilogue form a superb summary and synthesis of the work of those who contributed importantly to the evolution during this period of fully formed descriptions of the social nature of science. The work of these scholars involved crossing indistinct boundaries between the history, philosophy, economics and sociology of science. Polanyi was a prominent voice among many, including J. D. Bernal, Karl Popper, Robert Merton, Thomas Kuhn, Ludwik Fleck, Julian Huxley and Patrick Blackett: practicing scientists, philosophers, sociologists and historians. Nye shows how differences in political and economic outlooks, to say nothing of the usual competitions for recognition between scholars, served at times to block acknowledgements of commonalities, particularly between Bernal and Polanyi.

Scientific authority is a prevailing theme in Polanyi's views of science's place in society. He learned in the course of his own scientific career that the internal authority of science is powerful in determining what new results are accepted and thus become what he refers to as "scientific opinion", the settled judgment of the community. He had faith that, left on its own, the scientific community is capable of autonomously arriving at self-governed, optimal resolutions of contested issues. It is just this faith that allowed him to argue for scientific authority in matters of scientific import affecting the larger society. But his insistence on science's special claims to authority and autonomy left an impression of science as a sharply bounded, self-contained community. His views were criticized in the years following the publication of "The Republic of Science" in 1962 and *The Tacit Dimension* in 1966. They were rejected even more forcefully by later waves of social constructivists. Notwithstanding, as I wrote in *Imperfect Oracle: The Epistemic and Moral Authority of Science*, Polanyi's vision of science was, and still is, shared by many in the world of science. It does not fit comfortably in many respects with what we observe today of a vastly larger and more complex enterprise. Yet, he had lived the life of a practicing scientist, and knew how science works. It is still the case that every active scientist pursuing basic research is expected to adhere to norms and ideals that Polanyi, Merton and Kuhn identified as core values; that competition in research is often very keen and priority the coin of the realm. However, it was left to others who followed Polanyi and his contemporaries to delve more deeply

into the question of the fiduciary responsibility of scientists, individually and collectively, to apply scientific expertise in addressing societal needs.

One comes away from Nye's account of Polanyi's career with the understanding that his life was shaped at times by urgent necessities; that he was resourceful, ambitious and—most importantly—intellectually restless. He moved from a dedicated and highly successful pursuit of science, in arguably the most imposing scientific environment of his time, into economics and politics, and then finally, by his reckoning, into philosophy. Nye has made an excellent contribution to our understanding not only of Michael Polanyi's career, but also of the historical development of the sociology of science, the field of inquiry that he helped to define.

Works Cited

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Submissions for Publication

Articles, meeting notices and notes likely to be of interest to persons interested in the thought of Michael Polanyi are welcomed. Review suggestions and book reviews should be sent to Walter Gulick (see addresses listed below). Manuscripts, notices and notes should be sent to Phil Mullins. Manuscripts should be double-spaced type with notes at the end; writers are encouraged to employ simple citations within the text when possible. MLA or APA style is preferred. Because the journal serves English writers across the world, we do not require anybody's "standard English." Abbreviate frequently cited book titles, particularly books by Polanyi (e.g., *Personal Knowledge* becomes PK). Shorter articles (10-15 pages) are preferred, although longer manuscripts (20-24 pages) will be considered. Consistency and clear writing are expected. Manuscripts normally will be sent out for blind review. Authors are expected to provide an electronic copy as an e-mail attachment.

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