The Personal and the Paradigmatic

Richard Henry Schmitt

ABSTRACT Key Words: Personal knowing, paradigms, sociology of scientific knowledge (SSK), Michael Polanyi, Mary Jo Nye
Mary Jo Nye's book, Michael Polanyi and His Generation*, argues that the social conception of science originated in the scientific culture and political debates of the 1930s. As a historian, Nye covers the major themes of Polanyi's work, from his science research to his interest in economics and the political organization of science, concluding with the book* Personal Knowledge *and its reception. She reflects on the paradoxical legacy of his work (largely opposed to planning) in contemporary programs of social constructivism. Perhaps the paradox can be resolved by attending to adapting, learning persons as illustrated by Polanyi himself.*

It is significant in my view that Mary Jo Nye learned of the work of Michael Polanyi from a footnote in Thomas Kuhn's 1962 *The Structure of Scientific Revolutions*, the book that introduced the terms "paradigm" and "paradigm shift" into the discussion of scientific theories. Those terms are indeed appropriate to patterns laid down in science textbooks, the materials addressed in Kuhn's study. Such paradigms necessarily undergo "revolutions" from time to time, even though the origins of the new paradigm have no such necessity. Kuhn himself admitted that "the existence of a paradigm need not even imply that any full set of rules exists" so that "much of a scientist's success depends on 'tacit knowledge,' i.e., upon knowledge acquired through practice and that cannot be articulated explicitly,"[1] citing Polanyi's *Personal Knowledge* (1958). This citation led the historian in Nye to Polanyi's work, and to Karl Popper and his "misnamed" *Logic of Scientific Discovery* (1959), and subsequently to other scientists and academics of Polanyi's generation.

For Nye, trained in the late 1960s, the basic research paradigm is the social epistemology of science. Her argument in this study of Polanyi and his generation is that "the origins of this new social conception of science lie in a historical period considerably earlier than the 1960s," namely the scientific culture and political events of Europe in the 1930s, and that "Polanyi was among the leading protagonists" in cultural debates that continued into the 1950s (p. xv).[2] Nevertheless, Nye recognizes that the outcome has been paradoxical, since Polanyi's arguments for the autonomy of scientific governance have been appropriated by her contemporaries to argue for more purely sociological explanations of scientific knowledge.

Let me say from the start that Nye works primarily as a historian, without a dogmatic agenda. She provides the details of Polanyi's career and affiliations in such a way that it is possible to see beyond any single, comprehensive paradigm, such as the strong program for the sociology of scientific knowledge (known by the acronym SSK). And, so, she gives us a lot to think about, especially when we consider the concept of personal knowing.

Origins of the Social Construction of Science

This is not a biography of Polanyi; rather it considers the historical origins of a certain view of science, loosely, a kind of social constructivism. Eight chapters and the epilogue are organized thematically; these are roughly chronological though not strictly so, following the course of Polanyi's personal and intellectual development. Polanyi undoubtedly saw the environment of his postgraduate scientific work, first at Karlsruhe

and then in Berlin, as the model for organizing science. Yet, in his scientific work and subsequently in his other interests and concerns, Polanyi's life was in many ways polycentric and dynamic, using different techniques and styles and adapting old concepts to new uses, as he moved from Budapest to Karlsruhe to Berlin, then to Manchester and Oxford, and as he shifted his research focus within physical chemistry, and then his primary audience and affiliations from laboratory research to economics and the organization of the scientific enterprise, and finally to philosophical reflections on his personal experience. One can view these as changes in colleagues and opponents against a shifting institutional backdrop; and one can also see personal evolution that led Polanyi to hold the theories and positions he did.

The first four chapters cover Polanyi's scientific career, taking us from Budapest to Manchester. The first chapter covers Polanyi's upbringing, but also more broadly the period after the first war and the resulting émigré status of many educated Hungarians. The second follows Polanyi's early scientific career in Germany, first at Karlsruhe, where the *Technische Hochschule* had strong ties to the main German scientific establishment in Berlin, and then to the center itself, where Polanyi worked from 1920 to 1933, ultimately at Fritz Haber's Institute for Physical Chemistry and Electrochemistry. Here we also learn about broader connections to German science and industry existing in Weimar Berlin and how it was all dismantled under the National Socialists. The next chapter covers Polanyi's work on adsorption and using X-ray diffraction to study the structure of fibers and large molecules; the reception of his work helped to form Polanyi's views of authority within science. The fourth chapter covers Polanyi's work on chemical dynamics at Berlin and then at Manchester, along with Polanyi's awakening (perhaps re-awakening) interest in social dynamics and in the relationship between empirical results and theory as experienced in laboratory science.

By the end of the 1930s, Polanyi had largely turned to concerns other than physical chemistry. The second half of Nye's book covers these aspects of Polanyi's work: economics and planning, the organization of science and its functions, politics and philosophy, and finally the book *Personal Knowledge*. They are, as the subtitle of the final chapter puts it, about "argument, audiences, and sociological engagement." In each, there are colleagues and issues, with opponents who have to be taken seriously. In the debates about economics and planning, there are Keynes, Hayek, and Polanyi's brother Karl. Applying these questions to science, the sixth chapter concerns questions of pure and applied science, the sad fate of genetic research in the Soviet Union, and engagement with the left-leaning J.D. Bernal about the function of science. The seventh chapter discusses what Nye calls "the political dimensions of post-empiricist history and philosophy of science": Karl Popper's opposition to historicism and the "enemies of the open society," Thomas Kuhn's goal of general scientific education in a democracy, and Polanyi's efforts to describe an autonomous scientific tradition and its authority. The eighth chapter concerns Polanyi's philosophical efforts during the 1950s and early 1960s: context, responses, alternative efforts and views, specifically to *Personal Knowledge*. Finally, Nye concludes with an epilogue. Here she summarizes the current paradigm for the sociology of science, calling it a paradoxical legacy of Polanyi and the 1930s generation, given Polanyi's distrust of science having a social agenda.

Paradigms, Disputes, and Interpretations

In a book covering as many topics and people as this one, any reviewer is likely to find matters that might be seen in a different light. Let me mention three things that I noticed.

First and foremost, I wonder: Is it possible that a focus on scientific paradigms puts too much emphasis on theory? Nye writes (p. 43), "Most scientists who reflected on the philosophy of science in the nineteenth and

twentieth centuries . . . described the essence of scientific knowledge as knowledge of ideas and theory. They did not emphasize practical skills and laboratory routines." She suggests that this applies equally to England, France, Austria, and Germany. In a way, an emphasis on ideas would be guaranteed whenever you reflect on the philosophy of science. Yet Jed Buchwald—reconstructing devices used by Heinrich Hertz at Karlsruhe in the 1880s—found a marked difference between his work and that of British researchers. Having learned Helmholtz's way of doing physics at Berlin, though it was never explicitly articulated, Hertz was interested in making and investigating new effects; his lab results often looked like failed experiments until he provided a new theory. By contrast, the British community investigating similar problems worked at their desks rather than in the laboratory, solving problems on paper.[3] Of course, Nye gives us plenty of evidence about Polanyi's research work, showing his concern for practice: building laboratories, learning new techniques (such as X-ray diffraction), balancing time at bench and desk, the "semi-empirical" method, *ad hoc* accommodations, etc. (pp. 65, 71, 105, 124, 129, 141-44). This broadens our view of research methods; it is not simply that observations come first or that theories do. Rather they co-exist in our experience in ways that remain largely unconscious.

Second, the themes of planning and economic values apply both to questions of the economy—whether and to what extent and by whom it should be planned—and to the nature of the "republic of science". In many ways, the planning debate of the 1920s and 1930s has been overtaken by change, not only the collapse of many of the centralized, "planned" economies but huge quantitative changes that produced qualitative differences: multinational firms that are larger and more powerful than most historical empires, vastly improved techniques in information gathering and distribution, along with the unraveling of old regulatory disciplines and understandings. Two things stand out for me in the positions taken by Polanyi and Friedrich Hayek during the debates of the 1940s and 1950s. First, neither of them proposed, or even considered, anything like real "laissez-faire," despite the occasional appearance of that term as a contrast with "planning." Nye is not entirely clear on this; see, for example, her introduction of that term (p. 212) while discussing Edward Shils's report on the Hamburg conference in 1953.[4] In his report, the "methodological issues that raced like a hare across the field" were between idealists and positivists, not between planning and laissez-faire. And, despite the caricature we now get of Hayek, I believe that even he always assumed that free markets would need to be well-regulated.[5] A second and related point, which is easy to forget: the position shared by Polanyi and Hayek recognized the nature, better: the permanence, of ignorance surrounding our experiential knowledge.[6] This is critical. There is no sense of value, no permanent truths, that can be stated in the abstract, *ab initio*; instead there are a series of choices made *ad hoc*, largely unconsciously and without exact knowledge, somewhat provisional decisions, about which projects to pursue, which anomalous observations to ignore, which researchers to encourage and fund, etc. Hayek's *The Sensory Order* makes an even more fundamental argument about cognitive psychology: experience itself must be mediated by previous experience, rather than by some kind of innate, inherited pattern—the argument is against certain views of Ernst Mach, also held by the Vienna Circle.[7] Neurological structures are reinforced by repeated use, and so the sensory order is self-organizing, dynamic, and plastic, rather than routinized, static, and predictable.

In passing, let me note that Nye also contrasts Polanyi with the early Ludwig Wittgenstein (p. 263). Though his *Tractatus* probably did not directly influence Polanyi, it likely did influence Wittgenstein's cousin, Friedrich Hayek. Nye accepts a conventional but mistaken association between Wittgenstein and the logical positivists, those in Schlick's circle. In fact Wittgenstein's argument was with Bertrand Russell (and by extension with the Vienna Circle) about the boundary between expression in logical analysis and consideration about values and empirical facts. So, I do not see his view as inconsistent with "tacit knowledge"; instead it addresses a different problem, the limitations of logical atomism.[8]

Finally, in these and other discussions, the similarity between Polanyi and his opponent – at least in retrospect – is emphasized. We seem to have a rear-view mirror in which objects appear closer to one another, and oppositions are viewed within the same paradigm, *Zeitgeist*, or thought-style. On p. 178 we learn about views that Michael shared with his brother Karl—which certainly did exist—but shouldn't we also work out what in Michael's experience put them in opposition? On p. 195, Polanyi sounds like historians of ideas "writing in a semi-positivist mode on the internal logic of science." In the discussion at pp. 218-22, we see how "Marxism and Liberalism meet at the social turn," i.e., what assumptions were shared between Polanyi and J. D. Bernal. At p. 279, "Polanyi sounds almost like Jean-Baptiste Lamarck but without the mechanism of the inheritance of acquired characteristics." Elsewhere, "[Karl] Mannheim in 1929 sounds much like the later Polanyi or Kuhn . . . insofar as observers . . . eradicate as error whatever deviates from their unanimity" (p. 281). Again, we are forced to consider what caused Polanyi to formulate and hold his own position, sometimes accepting a shared framework but in opposition to others who shared it. And we need to consider his negative personal experiences as well as positive ones: for example, the unimaginably dark collapse of the idyll in Weimar Berlin, or the unjust imprisonment of his niece Eva Striker in Lubianka, over which Michael and Karl disagreed. Nye herself gives us enough of these details for us to ponder this.

Paradoxes often arise because a premise misleads us from the start. In the barber's paradox, the initial premise states that "The barber shaves only those men in town who do not shave themselves." This cannot be true unless the barber is not a man, does not live in town, wears a beard, or does not have facial hair. Of course it is even more likely that the word "only" is the root of the problem, because the person asserting the claim has simply forgotten to consider the barber himself. While the problem is not fully apparent at the start, we are asked to hold a problematic, or even contradictory, proposition. In the end, we see the paradox, and we recognize that something must be missing from the premise. Is that the explanation of Polanyi's paradoxical legacy to the social constructivists?

Knowing and Adapting Persons

Nye mentions (p. 271) the criticism of *Personal Knowledge* by Gerald Holton: he found it "maddening in spots;" that it might best be considered an example of confessional literature.[9] Actually that is a very shrewd observation. Polanyi's reflections are personal, about the nature of his experience. He presents a theory of knowledge centered on the knower. His "confessions" reveal something that he tacitly experienced that was overlooked in epistemologies limited to certifiably positive facts or explicit formal theories or textbook paradigms. When you see Polanyi's reflections in the tradition of Montaigne, they do not seem so eccentric.

Belatedly, let me make a personal disclosure. I was introduced to Michael Polanyi in quite a different way—though I must add that this was purely accidental and reflects no cleverness, indeed no planning, on my part. I wandered in like Candide, but without the benefit of a panglossian philosophy. As a teenager, I worked a few weeks one summer in the physical chemistry laboratory of Edward Bair; at the time he was developing new spectroscopic procedures and equipment to study explosive chemical reactions, looking at transition states and free radicals in the decomposition of hydrazine. As an undergraduate at the University of Chicago, I spent some time listening to pragmatic philosophers like Richard McKeon and studying social sciences with German émigrés like Gerhard Meyer, a member of the first Frankfurt School. I had the luck to hear Michael Polanyi lecture on several occasions. As a part-time graduate student, I worked with Edward Shils and wrote a dissertation with Stephen Toulmin. In other words, "social constructivism" is not directly part of my scientific heritage. On the contrary, Toulmin's "interest" in Kuhn's views (mentioned by Nye, p. 267)

was mostly negative. He used to tell me that, when Kuhn's *Structure of Scientific Revolutions* was published, it sucked the air from the room, though he also said that at the 1965 conference they were able to get Kuhn to soften many of his more extreme positions. Toulmin taught me to take a wider look around, to consider the adaptability of concepts to new circumstances. I should add that, along with Toulmin's 1959 review of *Personal Knowledge*, in which he criticized Polanyi for being outside the mainstream of philosophical discussion, one needs to consider his much more positive view in *Return to Reason* (2001).[10]

A further connection to Polanyi occurred in an unexpected way, through conversations with John Gedo, another émigré Hungarian but of a younger generation than Polanyi, displaced as a teenager. At the time, Gedo was a Chicago-trained psychoanalyst and scholar about artists and creativity. He told me that his generation of psychoanalysts felt restricted by the theoretical framework they inherited, but that Polanyi's work validated the use of their clinical experience when it was at odds with theory, breaking the logjam, and leading to new theoretical frameworks. Gedo documented this debt in his *Evolution of Psychoanalysis* (1999).[11]

As a result of this personal history, I see Polanyi from a different perspective. It seems to me that his primary contribution is not to the sociology of scientific knowledge, but rather to cognitive psychology, to explaining scientific discovery and creativity, to understanding the "husbandry" of variation in scientific research and the personal engagement of the scientist making choices. I do not claim that this is the only possible perspective, or even a better one than Nye's. In some ways the legacy of Polanyi's work to cognitive theory is now such an accepted part of the background that no one much bothers to cite Polanyi when they mention tacit, non-verbal, unconscious mental operations. Here Polanyi's legacy is not at all paradoxical; for many it is fully assimilated and increasingly anonymous.

Still, no one can deny that Polanyi did work on the sociology of science, or that science is affected by society and by its social organization. Again, I think Polanyi focused on the reality of a scientist's personal experience, as he had experienced it. I suspect that I'm seeing this through Shils's eyes, but in sociological terms that reality occurs in the primary group and an individual's affiliations and orientation to that group and its traditions. Perhaps if the program for the sociology of scientific knowledge can come to terms with the knower-centered part of Polanyi's work, his legacy to them may not seem so paradoxical. Given that nothing important can be ignored by those with an open mind, they will find there is something else that must be happening to explain the evolution, change, and replacement in those otherwise all-determining social constructions. In her history of Polanyi's career, by presenting the shifting themes of his work and the choices he made, Mary Jo Nye gives us all the materials anyone needs. Just look at where the paradigms, as fixed patterns, do not explain what happened.

On Tacit Knowing

Let me add a brief note about tacit knowing. These reflections go beyond the review of Nye's book, though in part they grow out of thinking about what Ted Brown so clearly says about Polanyi's career.

Tacit knowing is not itself a full-blown methodology, and certainly not a formal theory. Polanyi was well aware of the extent of human ignorance, of the difficulty of producing reliable observations, and of the challenge in sorting the significant from the insignificant. His ideas arose in opposition both to idealism and to positivism. He knew that circumstances change, that we need to adapt. So, he gave a description of what we resort to when there are no certain facts, when old patterns and values no longer apply, when new circumstances arise, when we approach a question with a fresh goal in mind. Mostly, we are not in a position

to plan on achieving a certain result. So we do something that Charles Darwin described as "unconscious selection"[12]—looking for what works, seeking what produces a reliable result and what manifests our values and realizes our goals, whether we have articulated them or not. Tacit knowing has no single paradigm, and that is its virtue.

Endnotes

[1]Thomas S. Kuhn, *The Structure of Scientific Revolutions,* (Chicago: University of Chicago Press, 1962), p. 44 and footnote.

[2]Page references without any other notation are to Mary Jo Nye, *Michael Polanyi and his Generation: origins of the social construction of science* (Chicago: University of Chicago Press, 2011).

[3]Jed Z. Buchwald, *The Creation of Scientific Effects: Heinrich Hertz and electric waves* (Chicago: University of Chicago Press, 1994), pp. 321-29.

[4]Edward Shils, "The Scientific Community: thoughts after Hamburg," reprinted in his *The Intellectuals and the Powers, and Other Essays* (Chicago: University of Chicago Press, 1972), pp. 204-212.

[5]I am basing my observation on the essays in Hayek's *Individualism and the Economic Order* (Chicago: University of Chicago Press, 1948), in which a regulated marketplace is an ever-present and fundamental assumption. Recently bloggers have discovered that Hayek even allowed for national insurance plans in his *The Road to Serfdom* (1944), chap. 9.

[6]For example, see Hayek's response to Polanyi's "Pure and Applied Science and Their Appropriate Forms of Organization," in *Science and Freedom: the proceedings of a conference ... held in Hamburg on July 23rd-26th, 1953* (London: Congress for Cultural Freedom, 1955), pp. 53-54.

[7]Friedrich A. Hayek, *The Sensory Order, an inquiry into the foundations of theoretical psychology* (Chicago: University of Chicago Press, 1952), pp. 175-76.

[8]Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge & Kegan Paul, 1922). See Wittgenstein's preface, which states his aim, and Bertrand Russell's introduction, which discusses the difficulties this makes for Russell's own theory.

[9]Gerald Holton, "Michael Polanyi and the history of science," *Tradition and Discovery* 19 (1), 1992-93, p. 24

[10]Stephen Toulmin, *Return to Reason* (Cambridge, MA: Harvard University Press, 2001), especially chap. 11 & 12.

[11]John E. Gedo, *The Evolution of Psychoanalysis: contemporary theory and practice* (New York: Other Press, 1999); Polanyi's collection, *Scientific Thought and Social Reality*, is the first of twenty-five books that are discussed.

[12]Charles Darwin, in "Variation under Domestication" (chap. 1 in *The Origins of Species*), discusses methods used by breeders and cultivators to produce new and strikingly different domestic stocks by the slow accumulative action of unconscious selection, i.e. "without a distinct object in view" and largely without any explanation of variation or inheritance.

Electronic Discussion List

The Polanyi Society supports an electronic discussion group that explores implications of the thought of Michael Polanyi. Anyone interested can join. To join yourself, go to the following address: http://groups.yahoo.com/group/polanyi_list/join. If you have difficulty, send an e-mail to Doug Masini (Douglas.Masini@armstrong.edu) and someone will see that you are added to the list.