

A Response to Theodore L. Brown and Richard Henry Schmitt

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ABSTRACT Key Words: Michael Polanyi, economic theory, scientific autonomy, tacit knowledge, paradigms, realism, Thomas S. Kuhn, Stephen Toulmin.

This essay responds to reviews of Michael Polanyi and His Generation: Origins of the Social Construction of Science by Theodore L. Brown and Richard Henry Schmitt. Special attention is given to Polanyi's economic views, to the conception of tacit knowledge (in Polanyi's philosophy of science and in Thomas S. Kuhn's description of paradigms), and to Polanyi's commitment to scientific realism.

It is a pleasure to respond to Theodore L. Brown's and Richard Henry Schmitt's essay reviews, which offer the reader informative and thoughtful analyses of my new book. Their reviews are occasionally overlapping in content, but complementary in their different perspectives. Brown writes as a chemist and academic administrator whose chemical researches, like Polanyi's, partly have focused on reaction kinetics and mechanisms. Brown's interdisciplinary interests have led him, also like Polanyi, into the study of the history, philosophy, and sociology of science, most recently reflected in his book *Imperfect Oracle: The Epistemic and Moral Authority of Science in Society*.¹ Schmitt's principal research lies in the philosophy of science, with an emphasis on Ludwig Wittgenstein, and a longstanding interest in the social and cognitive sciences. As Schmitt writes in his review, he had personal acquaintance at the University of Chicago with some of the characters in my book, notably Michael Polanyi, Edward Shils, and Stephen Toulmin, and as Schmitt writes elsewhere, he shares with Polanyi a Hungarian ancestry.²

Brown offers the reader a finely detailed and accurate summary of the main themes and content of my book, including an overview of Michael Polanyi's life and career. I especially appreciate Brown's explicit recognition of the links that I make between Polanyi's day-to-day career experiences in chemistry and his gradual development of a philosophy of science with emphasis on community, tradition, and authority in science. In particular, as Brown writes, Polanyi developed a strong sense of science as a closed community while he was in Berlin, and this feeling contributed, along with his political and economic experiences and reflections, to Polanyi's conviction of the necessity for scientific freedom and autonomy within the scientific community. Within such a community, in Polanyi's view, scientists exercise among themselves an interlocking communal and expert authority which determines directions for research, the value of results, and recognition of scientific achievement. Brown himself employs the themes of authority and autonomy in his book *Imperfect Oracle*, where, he notes, as in his essay review, that many scientists today share Polanyi's views on these matters, despite the larger and more complex milieu today for scientific research than in the 1930s.

Brown wonders if I have given too much attention to Polanyi's economic interests, but writes that perhaps the emphasis is justified, given that economic theory is a leitmotif of Polanyi's well-known 1962 essay "The Republic of Science." Here is a point of intersection with one of Schmitt's comments. In my view, Polanyi's substantial time spent in economic work, including his essays, book and educational films, was one of his major preoccupations and not a casual interest. This economic work structured the gradual transition between his life in chemistry and his life in the philosophy of science. In this regard, Schmitt notes in his review the importance to Polanyi's thinking of debates in the 1920s and 1930s about the implementation of centralized, "planned" economies (including heated debates with J. D. Bernal). Polanyi opposed centralized

planning for national economies, and he sounded an alarm to scientists of the dangers of central planning and mandates for socially useful science not only in the Soviet Union and Germany, but potentially in Great Britain and the United States.

Schmitt is correct in reminding the reader that Polanyi's opposition to planning did not advocate a primitive "laissez-faire" approach. Rather, as I describe in my book (e.g., pp. 146, 161, 177, 203), Michael Polanyi marshaled arguments for a modified capitalist system, warning against unfair disparities in income distribution, complete separation of government from the market, and failure of mutual communal responsibility among employers, workers, and governmental agencies. Partly influenced by reading John Maynard Keynes, and partly spurred by disagreements in economic theory with his brother Karl, Michael Polanyi advocated the role of economic experts to advise government on measures to regulate the money supply in a free market, including the use of deficit spending when needed, but not including public works projects. Friedrich von Hayek and Polanyi drew upon each other's ideas, and each adopted the (liberal) view that mutual adjustments occur among freely acting individuals in economic systems and in systems of knowledge (e.g., science) with the result that productivity and order emerge spontaneously.

Schmitt writes that Hayek and Polanyi shared recognition of the permanence of "ignorance surrounding our experiential knowledge." Interestingly, by way of contrast, Brown points out an occasion when Polanyi "seems to have forgotten his commitment to the notion that our understanding of nature is always contingent," noting Polanyi's remark to Erika Cremer that his theory of surface adsorption was "absolutely right" and Irving Langmuir's was wrong. This remark on Polanyi's part highlights a theme in my book that addresses, I think, Schmitt's question about what I may mean by "paradox" in the adoption or adaptation of Polanyi's work by leaders in the social construction of science (or sociology of scientific knowledge, SSK) in the 1970s and 1980s. The theme has to do with scientific realism and scientific truth, a theme discussed at length by Brown in his books *Imperfect Oracle* and *Making Truth: Metaphor in Science* (2003).³ First, however, before returning to this theme, I want to set that stage with some comments about paradigms, sociology, and psychology, as brought up in Schmitt's review.

Thomas S. Kuhn's *The Structure of Scientific Revolutions* (1962) popularized the term "paradigm," which had been used a few years previously, with reference to patterns or ways of seeing the world, by philosophers including N. R. Hanson (1958), Stephen Toulmin (1961), and Polanyi himself (1958). Polanyi's usage occurs in *Personal Knowledge* in his dismissal of Laplacian mechanistic determinism as "the paradigm of a conception of science pursuing the ideal of absolute detachment."⁴ Kuhn wrote that he used "paradigm" in two different senses: (1) the sociological sense of the "constellations of beliefs, values, techniques, and so on shared by the members of a given community;" and (2) the philosophical sense, which "is the deeper of the two," of that element in (1) that has to do with exemplars, which do not employ rules and logic, but rather tacit knowledge and "intuitions" that are "tested and shared possessions of the members of a successful group which the novice acquires through training."⁵ These two uses both are found in *Personal Knowledge*, and Kuhn in his 1962 book explicitly mentions his debt to Polanyi's explanation of tacit knowledge.⁶

Tacit knowledge became recognized as one of Polanyi's most novel contributions to the philosophical understanding of science. In Stephen Toulmin's *Return to Reason*, a reference for which I am grateful to Schmitt, Toulmin praises Polanyi's explanation of tacit knowledge for helping "to dissolve away the intellectualist accounts of the Philosophy of Science that were in fashion in the earlier part of the twentieth century," adding that tacit knowledge harmonizes with the later views of Ludwig Wittgenstein in showing "how scientific terms

and statements acquire their meaning from association with particular constellations of human situations and actions.”⁷

In *Making Truth*, Brown writes that in his demonstration of the “embodied nature of our conceptual frameworks,” Polanyi has a good deal in common with developmental psychologists and cognitive scientists. Schmitt writes in his review that he sees Polanyi’s primary contribution not as one to sociology, but to psychology, in Polanyi’s explanations of discovery, creativity and theory-choice. The link to psychology, however, was precisely one of the major points of criticism for Polanyi (and Kuhn) from philosophers of logic.⁸ And of course, this problem is a hard one. How can we account for discovery? How do individuals come to be creative? What is the role of the individual and of the community in effecting scientific change?

As I discuss in my book, Polanyi explains discovery as an operation that is not strictly logical because there is a “logical gap” between the current dominant interpretive framework and a new way of seeing things. A phenomenon that has been puzzling or worrying is resolved by crossing a logical gap in a kind of conversion, but this revelation is based in long-practiced skills and in judgment acquired through apprenticeship in a community of connoisseurs.⁹ The individual scientist’s peers may or may not adopt a new explanation depending, as Polanyi puts it, on their judgment of its accuracy, relevance, and intrinsic interest.¹⁰ Thus, the psychological dimension of scientific innovation, whether on the individual or the group level, is intimately linked with the sociological dimension of the standards and traditions of the scientific community as a whole.

Schmitt mentions toward the end of his review that the knower-centered part of Polanyi’s work is the least assimilated part of the legacy adopted in SSK. I think, however, that we need to distinguish two psychological senses of Polanyi’s “knower”: (1) the skilled connoisseur who produces “embodied knowledge” and (2) the passionate explorer who seeks “contact with a hidden reality.” The knower in the first meaning has been an object of considerable interest in SSK, but not the knower in the second sense.

The problems of realism and truth are crucial to understanding differences among Polanyi, the younger Kuhn, and the next generation of social constructionists. As I mention in my book (255), Martin X. Moleski discovered Polanyi’s annotation “Truth!!!!!!!!!! This *really* needs analysis” next to Kuhn’s statement in *Structure* that “we may . . . have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.” Kuhn further suggested that “If we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish to know, a number of vexing problems may vanish in the process.” For Kuhn, there was no one big mind-independent world to be discovered as truth, although the varieties of knowledge acquired in different specialties were by no means arbitrary and science remained progressive in nature.¹¹ This view largely carried the day in the second half of the twentieth century among scientists and philosophers, as well as sociologists. Brown writes in *Making Truth*, for example, that “We have no grounds for believing that there exist objective, mind-independent truths awaiting discovery. Rather, statements we regard as truths about the world are the product of human reasoning.”¹²

Like many philosophers of his generation (notably Toulmin) and the next generation (for example, David Hull and Ronald Giere), Kuhn found congenial a naturalist and generally Darwinian evolutionary model of scientific innovation and scientific change. As I write in my book, Polanyi wrote of evolution as a process of emergence leading to greater complexity and achievement both in knowledge systems and in biological species (265-266, 278-279). His notion of evolution was less Darwinian and more progressive and teleological

than most contemporary views, “guided by the urge to make contact with a reality, which is . . . waiting to be apprehended.”¹³ Polanyi compared his frame of mind in this conviction to the worship of God.¹⁴

Polanyi had a commitment to realism and confidence in the power of natural science to know the world—an attitude that also informed the sociological writings of his generational peers J. D. Bernal, Karl Mannheim, and Robert K. Merton. Polanyi’s belief protected him against worries that his writings could be used to undermine the authority and autonomy of science. His remark to Erika Cremer that his adsorption theory was absolutely right captures Polanyi’s conviction that correct scientific theories reveal the reality of things, not what is effective or convenient or fashionable to think. In my book, I suggest that there is irony in the later severing of his social epistemology of science from its original metaphysical realist premises, with the result that some of his most novel insights into the way that science and scientists work have indeed been used to undermine Polanyi’s campaign for the autonomy and authority of the scientific community.

Endnotes

¹Theodore L. Brown, *Imperfect Oracle: The Epistemic and Moral Authority of Science in Society* (University Park, PA: Pennsylvania State University Press, 2009).

²As mentioned in Richard Henry Schmitt, review of John E. Gedo’s *The Evolution of Psychoanalysis: Contemporary Theory and Practice* (1999) in *Tradition and Discovery*, 27:3 (2000-2001): 41-42.

³Theodore L. Brown, *Making Truth: Metaphor in Science* (Urbana and Chicago: University of Illinois Press, 2003).

⁴Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958), pp. 139-140.

⁵Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., enlarged (Chicago: University of Chicago Press, 1970), pp. 175, 187, 191.

⁶Kuhn, *Structure of Scientific Revolutions*, p. 44, n. 1.

⁷Stephen Toulmin, *Return to Reason* (Cambridge, MA: Harvard University Press, 2003), p. 183.

⁸Brown, *Making Truth*, pp. 8-9.

⁹Polanyi, *Personal Knowledge*, p. 263.

¹⁰Polanyi, *Personal Knowledge*, pp. 135-136. As I note on p. 246 of my book, Polanyi used the terms validity, profundity and intrinsic interest in *Science, Faith and Society* (Chicago: University of Chicago Press, 1964, originally 1946), and plausibility, scientific value, and originality in “The Republic of Science: Its Political and Economic Theory,” *Minerva*, 1: 54-74.

¹¹Kuhn, *The Structure of Scientific Revolutions*, pp. 170, 171; and (1969 Postscript) 206.

¹²Brown, *Making Truth*, p. 12.

¹³Polanyi, *Science, Faith and Society*, p. 35; also, e.g., *Personal Knowledge*, p. 106.

¹⁴Polanyi, *Personal Knowledge*, p. 405.