observation (surely meant now in a cognitive sense much wider than vision) and what is salient to creative problem solvers in frontier contexts. (Once again, Hanson beat Kuhn to this point.) However, I think it is misleading, certainly more trouble than it is worth, to retain the term 'logic of discovery' for the complex mixture of causal and inferential pattern-matching mechanisms involved here. In my view the terms 'logical' and even 'rational' often get in the way of attempts to outline a naturalistic epistemology of any kind and certainly a frontier epistemology. That said, I am firmly with Hanson and Lund in believing that frontier epistemology is an important and badly neglected topic.

Russ Hanson's life ended unexpectedly in 1967, in the crash of his Bearcat airplane into a hillside on the way to give a lecture at Cornell. It would have been interesting to see how his views continued to develop in the philosophical turmoil of the late 1960s and 1970s and as the new cognitive sciences matured. Nonetheless, brief as they were, Hanson's life and philosophical trajectory were scintillating. We should be grateful to Matthew Lund for providing a valuable account of both, one that fills a major gap in our understanding of the history of philosophy of science.

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Mary Jo Nye. *Michael Polanyi and His Generation: Origins of the Social Construction of Science*. Chicago: University of Chicago Press, 2011. Pp. xxi+405. $45.00 (cloth).

Mary Jo Nye's new book incorporates many of her previously published articles on Michael Polanyi's activities in chemistry, rewritten in a social constructivist framework. Although there are several publications on Polanyi's work and life in existence to which Nye is indebted (Scott and Moleski, Gulick, Mullins, Pallo, Knepper, Nagy, Jha), she presents a new thematic approach and shows the network of his peers.

The book consists of an introduction, eight chapters, and an epilogue. The chapters are paired thematically: on social, political, and cultural conditions; on research and development of science and Polanyi's disappointments and triumphs in chemistry in a social context; and on Nye's main thesis about what she believes to be the economic foundation of Polanyi's thought in the sociology and politics of science. The final chapters argue for the political foundations of Polanyi's philosophy of science and that of his contemporaries

and interpret Polanyi's philosophy of personal knowledge with a social emphasis. The epilogue gives an overview of science studies and an assessment of Polanyi's legacy.

There is an extensive bibliography, and the footnotes are richly packed, some with no page citations and some errors. The index is not helpful enough for scholarly work. Here the editors needed to do a better job.

Social and political contexts and the social dynamics of laboratory life are described in detail throughout. There is a brief biography: Polanyi was born in Budapest in 1891 in a well-off acculturated Jewish family, trained as a doctor, participated in progressive intellectual groups, then studied chemistry in Germany, received a doctorate, and worked in the Kaiser Wilhelm Institute in Berlin until 1933 where he was a leading member of an expatriate group of intellectuals. Nye describes the role of Haber and Planck as directors and mentors and the destructive changes with Hitler's rise to power. The research group in Berlin became Polanyi's idealized scientific community of explorers, aware of the preconditions for science's flourishing, and the basis of Polanyi's sociologically inflected philosophy of science (83). His career as a member of the twice-exiled Hungarian phenomenon was crucial for the development of what Nye calls his social epistemology of science (3).

Nye narrates Polanyi's disappointments and missed opportunities in Berlin—his experiments in surface chemistry results (adsorption of gases by solids) and in x-ray diffraction on crystal structure (showing large molecular weights of organic compounds). Polanyi drew sociological and philosophical conclusions from these occurrences about authority and recognition in the scientific community (109).

Polanyi's work in physical chemistry, started in Berlin and continued in Manchester, met with recognition. Nye describes experiments and the resulting papers on chemical reaction velocities and kinetics and the various hypotheses considered. An important original contribution in collaboration with Eyring was the "semi-empirical method" combining visual and verbal approaches, a top-down and bottom-up method (127).

One of the supporting arguments of Nye's thesis regarding Polanyi's sociology of science is the influence of his brother Karl, an economist with Soviet sympathies, who used the guild concept of institutional life (146). Michael was critical of Soviet planned economics. He developed an economic model of money circulation and simplified and explained Keynes's economic theories (155).

In the *Logic of Liberty*, he introduced the concept of spontaneous order by mutual adjustment in the context of coordination of scientists and aspects of freedom (177). He contrasted it to a pyramidal structure of corporate order

and central planning. It is Nye's claim that Polanyi's interest in economics formed a bridge to his sociologically inflected philosophy of science, as evidenced by his essay "The Republic of Science" (179), which notes resemblances of the scientific community to a national body—cooperation of members, choosing and pursuing problems—and shows that a single authority would eliminate such coordination. The functioning of scientific and economic spheres are polycentric problems; therefore, planned research and economy are ineffective. In Nye's reading, Polanyi's interest in economic theory and this analogy is a foundation of his philosophy of science (176). Mirowski and Thorpe, to whom Nye refers and relies on this score, have a poor understanding of Polanyi's philosophy.

The 1930s debate between the British Left influenced by Bolshevik dogma and the classical Liberals advocating freedom of science is given in great detail—the debate was on the role of the state and the social duty of scientists. Bernalists advocated science serving social needs guided by the state, while classical Liberals (Polanyi, Hayek) advocated for a universal notion of science, freedom of pure science and thought. Nye finds similarities beyond crystallography between Polanyi and Bernal in arguing for the social turn in history and philosophy of science (184). The Left is presented in detail, and Polanyi's *Science, Faith and Society* is shown as a reaction to Bernalism (210). Nye uses highly charged (negative) language when describing Polanyi and Hayek's response to the Left's efforts to dominate academic and political life; similar negative language is not used for the opposition.

Nye comments that Polanyi was less influential among philosophers of science than were Popper and Kuhn. Polanyi's *Personal Knowledge* was published in 1958. Kuhn's concept of paradigm in the 1962 *Structure of Scientific Revolutions* was very successful with social scientists (223). It is not mentioned that the 1959 preface of Popper's *The Logic of Scientific Discovery* (English translation) pointedly states it is saving science from (Polanyi's) "a-critical and illogical obscurantist faith in the expert's skill"—the one method of philosophy is Popper's critical, rational one. This tag has been used in long-running debates in philosophy of science by Popperians. Nye notes that both Polanyi and Kuhn thought they described science as it is actually done (252). (But Popper's philosophy legislates how science should be done!) Polanyi's frequent use of analogies and neologisms that are crucial for understanding his philosophy of science in *Personal Knowledge* are uninterpreted. Nye's synopsis of this book highlights Polanyi's intention, as she reads it, to show science as a subset of knowledge, which prevails through personal persuasion, conversion, and commitment (261). She oversimplifies Polanyi's discussion of the phenomenon of discovery and sees a difficulty in Polanyi's

thought, which he could not resolve: the tension between autonomy of pure science and the socially constructed interpretive framework on which it operates, between individuality and effective change (262–63), not recognizing that for Polanyi these were integrated facts of scientific life.

The epilogue describes the Edinburgh School of the sociology of scientific knowledge that adapted Polanyi's texts, blending them with Kuhn and Bernal. The aim of this "strong program in the sociology of science" was to construct an empirically informed view of the social nature of scientific knowledge (296), an empirical program of relativism ("the third wave"). Opposing this is Bourdieu, holding to the truth value of science (299). Nye reiterates that Polanyi's concern with a new epistemology of science grew out of the history and politics of his times and that his political and economic writings became the bases for his philosophy of science. Now he is enlisted for an end, the social construction of science, which he would not entirely condone (305).

It must be said here that Polanyi would not recognize himself in this portrait and that historical determinism and socially constructed reality, as a theoretical framework, would have been anathema to him. As he wrote to Mannheim, he considered historic determinism a restriction of freedom of ideas. He rejected all social analysis of the development of ideas and history that make social conditions anything more than opportunities for the development of thought. He also thought that there *is* a reality "out there" to be discovered. His philosophy of science is fallibilist. His theory of truth is a continuum from objective to tacit (verification for the natural sciences, to various grades of validation for the social sciences and humanities); therefore, science is not a cultural act in the same sense as the humanities or crafts are. His new epistemology of science, knowledge grounded in the tacit, was an answer to positivism, showing the process of scientific creativity. He considered *this* epistemology, not social epistemology, as his legacy. His metaphysics may be called organicism with emergent levels of integration its main principle. His description of the scientific community meant to show its conventional procedures. His writings on the freedom of science were part of his duty as an intellectual, declaring his ethical values grounded in principles of classical liberalism. Nye does not say clearly what norms guided Polanyi in his research, what counted as evidence when he evaluated findings, nor does she trace his implicit reasoning. The reductionist social constructivist framework that holds social aspects causal and minimizes the cognitive distorts the account of his many-faceted thinking about science. Nye's approach illustrates Polanyi's thesis that one sees through one's framework. In spite of interpreting Polanyi, as he would not have done, the book is worth reading

for descriptions of Polanyi's experiments, the mention of his many contemporaries in context, and especially for references to Nye's more readable articles.

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Jeroen van Dongen. *Einstein's Unification*. Cambridge: Cambridge University Press, 2010. Pp. x+213. £55.00 (cloth).

During the first 40 years of his life, Einstein established himself as the preeminent physicist of his time. By 1919, his general theory of relativity, which offered a view of space, time, and gravity that was radically different from that of Newtonian physics, had been confirmed in spectacular fashion by Eddington's eclipse observations. Einstein had also made important contributions to radiation theory and atomic theory, which helped to spark the quantum revolution. When he won the Nobel Prize in 1921, Einstein stood atop the world of physics, a colossus whose only true peer was Isaac Newton.

In the remaining 35 years of his life, however, as his celebrity among the general public grew ever greater, Einstein's standing within the physics community declined dramatically. He gradually drifted into obscurity, becoming so isolated that, by the end of his career, he was openly ridiculed by the younger generation of physicists. From an operational standpoint, the reason for this is clear: he had rejected the productive research programs of the day revolving around the new quantum theory and instead had focused on a line of research that did not produce any successful results and looked increasingly antiquated as time went by. The obvious question is this: Why did Einstein, the most successful physicist since Newton, ignore so many important empirical discoveries and theoretical advances, instead pursuing a line of investigation that very few others thought worthwhile?

This is the question that Jeroen van Dongen proposes to answer in *Einstein's Unification*, and in this he follows a long line of distinguished Einstein scholars, including Abraham Pais, John Stachel, Don Howard, John Earman, and John Norton. Van Dongen, an assistant professor at the Institute for History and Foundations of Science at Utrecht University, has served as an editor for the Collected Papers of Einstein project, and this has provided him intimate knowledge of Einstein's writings and correspondence. He uses this special access to great effect, peppering the manuscript with