

## Polanyi defanged

Social Studies of Science  
42(6) 945–953

© The Author(s) 2012

Reprints and permission: sagepub.

co.uk/journalsPermissions.nav

DOI: 10.1177/0306312712458479

sss.sagepub.com

**Stephen Turner**

Department of Philosophy, University of South Florida, Tampa, FL, USA



Mary Jo Nye, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011), 432 pp., £29.00/€38.99/\$45.00. ISBN 978-0-226-61063-4

### Keywords

James Bryant Conant, Karl Polanyi, Michael Polanyi, planning of science

Michael Polanyi is a significant figure in the history of science studies, not only for his account of the role of tacit knowledge in science, but also for his depiction of science as a community governed by tradition. Polanyi was a complex figure from a distinctive background, who was centrally located in 20th century science as a physical chemist, and later as a social, economic, and philosophical thinker. Reconstructing his context, his relation with his older brother Karl, his central European origins, and his understanding of the world of science he experienced, is a daunting task. It requires biographical understanding, but also a command of the history, social and political, of Austro-Hungary, of the science of the interwar years, of the institutional settings in which Polanyi worked (which included the Kaiser Wilhelm *Gesellschaft* Institute for Textile Science in Berlin and later the University of Manchester), and then much more—the history of economic thought in which Polanyi’s economic thinking needs to be placed, the history of the Left in science in the 1930s and 1940s, against which he reacted, and the development of his distinctive philosophical views, which went far beyond science. Mary Jo Nye manages to write a history that includes all of this, in considerable detail, and to be instructive and novel in all of these areas.

Mary Jo Nye’s book is ostensibly about the sources of social constructionism in science studies, which she finds in nascent form in Michael Polanyi, as well as in his affinities with his brother Karl. This is a thin and problematic thesis. The more interesting theme of the book is this: it is a radical revision of the standard story of the origins of science studies, a story that was ‘standard’ at least for the generation of Robert Merton and Edward Shils, who were part of it and knew the principals. Polanyi’s defense of the

---

### Corresponding author:

Stephen Turner, Department of Philosophy, University of South Florida, 4202 East Fowler Avenue, Tampa, FL 33620, USA

Email: Turner@usf.edu

autonomy of science, against John Desmond Bernal and a host of other Marxist scientists and science journalists of the 1930s, was taken up, in an odd way, by Merton, who was a sympathizer of Bernal and friends, and briefly endorsed the common thesis of the Marxist scientists that only with Communism would science be fully utilized.

The complex ins and outs of these ideological alliances boil down to a standard story: Polanyi, along with James Bryant Conant, Thomas Kuhn's mentor, and the architects of post-war American science policy, such as Vannevar Bush, had a 'liberal' theory of science that justified its autonomy, while the Left, primarily in Britain in the thirties, wanted 'planning'.<sup>1</sup> Science studies largely comes out of this 'liberal' theory of science: even such apparently innocent concepts as the community of science are part of the argument for autonomy. The Left styled scientists as 'workers', disparaged the idea of pure science, and reduced science to technology: an extension and application of Marxist ideas about base and superstructure.

Polanyi enters this story in many ways, but one stands out: he was a major figure in the Congress for Cultural Freedom, the anti-Communist body of intellectuals that derived from the Committee on Cultural Freedom founded by John Dewey in the 1930s as an anti-fascist Left alternative to the harder-left versions of the science movement. It was later found to be partly funded by the CIA during the Cold War. Michael's brother Karl, the author of *The Great Transformation*, a classic attack on the human effects of capitalism first published in 1944 (2001 [1944]), and a less well-known defense of state run economies, *Dahomey and the Slave Trade* (1966), was on the other side. Although Karl was not a Communist, it is something of a parlor game among reviewers and letter writers to the *Times Literary Supplement* to point out the totalitarian and authoritarian passages in his texts, which are undeniably there, though phrased in the most innocuous ways.<sup>2</sup> People who fail to conform to the new order of the future envisaged by Karl are not sent to camps, as with Stalinism, they are put in a niche:

Compulsion should never be absolute; the 'objector' should be offered a niche into which he can retire, the choice of a 'second-best', that leaves him a life to live. Thus will be secured the right to non-conformity that is the hallmark of a free society. (Polanyi, 2001 [1944]: 255)

The great goal is integration through planning and regulation, which Karl wishes to interpret as an increase in freedom.

Nye has written a polemical book against this origin story of science studies in the guise of a contextualist account of Polanyi's thought. The book examines Polanyi's political formation in Hungary, his brother's political involvements in Hungary and his own marginal role in the Galilei circle, his German years at the Kaiser Wilhelm Institute, his science, his activities as an economist, his ideas about science as a community and tradition, as well as his role as warrior against Communism. There is less about his philosophy proper. Her claim is that Polanyi had more in common with his enemies than he realized, and was simply mistaken in his rejection of Bernalism and the Left view of science. The history, for Nye, was a history of misunderstanding, and the misunderstanding was on Polanyi's part.

Nye has been a sympathetic writer on Patrick Blackett (Nye, 2004), who was on the Left in the disputes about science, and in general has had a long-standing friendly interest in the Left of this era. The book, long and well-researched, rewards reading in part for this distinctive political preference. Blackett and Bernal were among the British counterparts of the French intellectuals whom Tony Judt famously exposed in *Past Imperfect* (2011 [1992]) for their intellectual dishonesty in the face of Stalinist crimes. Nye is far more tolerant, even of such things as the Lysenko affair and the willingness of Bernal to swallow this travesty of science—which led to the death in the camps of Lysenko's orthodox geneticist rivals—for the Party. The originator of the account of science that Bernal preached, the Russian Boris Hessen, was judicially murdered in 1936, though this was not known until recently. Nikolai Bukharin, who denounced the idea of pure science as a piece of bourgeois ideology, was as well, a result of the show trials of the 1930s. Everyone knew this. The trials were the subject of Arthur Koestler's *Darkness at Noon* (1940). It is a matter of taste where one draws the line on these subjects, but it is impossible to make sense of the era without acknowledging the intensity and confusion of these conflicts. Should intellectuals have rethought the Soviet experiment then—or later? Nye chooses to draw no line at all, and downplays the seriousness of the conflicts and divisions that these events produced among intellectuals. This perspective has advantages, but it also obscures.

### Polanyi in context

Nye describes the Jewish intellectual and professional class of Budapest during the period of Karl and Michael's upbringing and young adulthood, and the political activities, soon suppressed, that they engaged in during the 1920s and early 1930s. Michael Polanyi was a Jew of a particular kind: horrified by the medieval mentality of the Ghetto and less than enthusiastic about Zionism, which he regarded as retrograde. In the face of anti-Semitism, official and non-official, he formally converted, along with many others. But in his case, the conversion was helped by the vogue for a Tolstoyan idea of Christianity. As in Vienna, intellectual life in Budapest was organized in overlapping circles. Here Polanyi and his older brother Karl encountered Georg Lukács and Karl Mannheim. Michael's different political instincts, however, showed themselves early: when Bela Kun proclaimed a Red republic he resigned from his government post; his Leftist peers took up high office. When this government was suppressed and replaced by the Horthy dictatorship, he took an opportunity to work in Germany.

The formative years of his experience as a research scientist were spent in the Kaiser Wilhelm *Gesellschaft* Institute for Fibre Chemistry in Dahlem, which provided him with a model for the way science should be funded and run. Berlin was an exciting place for science during the Weimar Republic, and Polanyi was friendly with the brilliant Hungarians who were also there. This was a great place for observing high science at its best. Nye goes into great detail on Polanyi's science, and on the experiences he had in constructing new theories and methods, as well as the difficulties he had getting his ideas accepted.

His main concern involved the adsorption of gases by solids as affected by temperature and pressure. This involved Van Der Waals forces, 'weak attractive forces between

atoms or nonpolar molecules' (Nye, 2011: 88). Polanyi's idea was that there were layers of adsorbate and that the effect was analogous to compression, so 'that the first layer was under the greatest compression, the second layer under lesser compression, etc.' (p. 89). The simpler theory was American Irving Langmuir's, which worked from the idea that adsorption occurred only at the single molecular surface layer of a metal or oxide. This account won out at the time, gaining a Nobel Prize for Langmuir in 1932; much later, in the 1960s, some of Polanyi's ideas were vindicated.

This was a formative experience, and although Nye does not emphasize it, perhaps because it does not fit her thesis about the lack of difference with the scientific Left, this was a problem that the 'liberal' account of science was obsessed with. Writers such as James Bryant Conant (1951 [1947]: 98–99) were fascinated with the problem of ideas and facts born out of time, and rejected or ignored and misunderstood by dominant scientific opinion. This phenomenon is of course at the core of Kuhn's *Structure* (1996 [1962]) as well. An aspect of this problem that Polanyi also discussed at length in *Personal Knowledge* (1962 [1958]) was the phenomenon of intellectually closed systems, such as Freudianism and Marxism, which had the resources to explain anything without ever having to face revision. These concerns are what divided these thinkers from the Left, who in the era of Bernal both embraced dialectical materialism as genuine science and, by treating science as technology, pretended that 'science' was not a domain of contested ideas but rather a mechanical technique for solving problems that could easily and successfully be directed to the problems of social and economic life.

Nye acknowledges the biographical importance of Polanyi's struggle to get his ideas accepted, and the obstacles he faced in doing so, which resulted in part from his innovative employment of results from x-ray crystallography in chemical contexts, which audiences of chemists resisted, and the fact that Van Der Waals forces were not of much interest to chemists at the time. These are 'ideas born out of time' issues. But she suggests that Polanyi drew a different lesson from his scientific experiences in this period: an acceptance of the importance in science of tradition and authority, leading to a model of the 'typical scientist' who resembles Kuhn's practitioner of normal science. It is in this acknowledgment—as she quotes him, that 'there must be at all times a predominantly accepted scientific view of the nature of things, in the light of which science is jointly conducted by members of the community of science' (p. 111)—that she sees the seeds of a social account of science and thus social constructionism. It contradicts the popular image of science of the time as a process of replacing authority with discovery.

## Manchester and the turn to economics

The Weimar Republic ended in a complex fiscal crisis as part of the Great Depression, leading in 1933 to Hitler becoming Chancellor. Nye does not go into great detail about these events, but blames Heinrich Brüning, the Prime Minister, for failing to initiate a public works program to stave off the effects of the economic collapse. Hitler of course 'solved' the economic crisis in this way. But he did so by repudiating the debts imposed under the Versailles Treaty, which enabled him to solve the fiscal crisis and raise funds in the bond market, and incidentally to export the crisis to the countries to whom the debt was owed. Polanyi experienced all of this first hand in Berlin. His family background

and personal interests led him to economics. His brother Karl was a supporter of Soviet economic policies during the 1920s and 1930s. Polanyi himself visited the Soviet Union, and by the early 1930s had come to take an active interest in economic issues. His earliest interventions in economic periodicals, however, had to do with protecting government support for pure science, and preserving the autonomy of science.

Nye discusses the relation between the Austrian economics of Friedrich Hayek and Polanyi's developing views. Karl was an archenemy of Austrian economics and, like key thinkers in the Vienna Circle, he believed in planning. The issues were joined in the 'socialist calculation debate'. The basic issue in this debate was whether there was an alternative to the information on what people wanted and were willing to pay for other than the kind of information supplied by the actual actions people took in a market. Planning depended on the idea that planners could substitute for this source of information. Karl Popper and Michael Polanyi, as well as Hayek and the rest of the Austrian school, rejected this idea. Karl Polanyi was on the 'Red' side of this discussion, believing that democracy and capitalism were incompatible. After Michael moved to Manchester, Karl moved to London.

When Polanyi arrived at Manchester he befriended economists, and increasingly focused his thought on the great economic questions of the day: how to deal with the Great Depression, of course, and the efficacy of 'Planning', which was the solution urged by the Left and by Fascism. Nye's discussion of Polanyi's economics is interesting because it is written not as history of economic thought, but as history of science, with a stress on Polanyi's distinctive methods of visualization, characteristic also of his chemical work, and on public understanding. Polanyi used his laboratory resources to build a model out of glass tubes to illustrate the law of supply and demand, and another with conveyor belts and glass balls to illustrate the circulation of money. Another, with many wheels, was designed to show the effects of increasing the money supply, which was crucial to Polanyi's thinking about the depression. Methods of visualization were also used to educate the public. Eventually Polanyi made films, which were supported by the Rockefeller Foundation. Other visualizations appear in his book *Full Employment and Free Trade* (1948). Polanyi had a good visual sense, and these were pioneering efforts that had some success and an international audience.

The problem of the Great Depression was addressed by John Maynard Keynes, and Polanyi's thinking had a close but problematic relation to that of Keynes. Nye gives an accurate account of these differences: Polanyi was not a supporter of either borrowing or public works projects, but rather of monetary solutions that were neutral between needs. It was monetary expansion through the printing of money to finance deficit spending that enabled the recovery of the Soviet and German economies in the 1930s (pp. 165–6).

Polanyi first gained visibility as an economic observer, however, for his discussion of the actualities of Planning in the Soviet Union. His discussion is still a staple of economic analysis. Nye stresses the idea that 'plans' in fact changed all the time. Polanyi's point was somewhat different: for him planning was simply an organizational impossibility. The planners had to get their information someplace. Where they got it in fact was from the managers of the productive units—factories, for example. Where the managers got their information, among other sources, was from black market operations, which did indicate demand.

In some respects, notably his allegiance to the idea of spontaneous order and the invisible hand understood as relations of mutual adjustment (a key Polanyi concept), Polanyi was a 'liberal': Nye's Left-wing perspective gives her an advantage here. She is able to detect and analyze the many deviations from liberalism that actually appear in his economic writings. She stresses, for example, his suggestion that expert committees should play a role in such things as designing schemes for redistributive taxation. And she notes that he argues that the justification for private property must be found in the good of the community. She turns these points into an argument to the effect that Michael was in fact closer to his brother Karl, and that 'paradoxically, Polanyi's key conceptual formulation was rooted in some of the functionalist ideas of his brother Karl, while nonetheless tied to free market economic theory' (p. 178). There is something to this: Karl was insistent that state institutions were central to the creation of markets. Indeed, he devoted his (well-funded) later years to a discussion of the slave trade in Dahomey, which he used to invert the argument that markets setting prices were 'natural', to the claim that markets themselves were planned. In the case of Dahomey he claimed that markets, trade, prices, and money were institutionally distinct and controlled by the state, and that this system lasted for a century (Polanyi, 1966, xxiii–xxv).

The similarities are overshadowed by the diametrical differences. For Karl, markets were a derivative result of state action, and price-setting by markets was unnecessary; for Michael, markets were one type out of many of forms of spontaneous order where the orderly outcome was produced by mutual adjustment rather than central direction. For him planning in the sense imagined by the theorists of the Left and Right was 'administratively impossible—in the same sense in which it is impossible for a cat to swim the Atlantic' (1980 [1951]: 125–6). The two approaches did not entirely exclude one another, but they reflected fundamentally different outlooks. For Michael, spontaneous order and the freedom to pursue one's own ideals were goods. For Karl, as for Bernal, freedom was something that needed to be redefined. Both Bernal and Karl spent extended pages in doing so. Michael's image of the free society was one of overlapping domains of spontaneous order—a variant of the idea of liberalism as the preference for indirect means. Karl's preference was always for the direct means of state power. For him, the central fact of modern life was the failure of markets and of liberalism.

## **The republic of science**

The study of economics set the stage for the epochal struggle of Polanyi's career, against Bernal and the idea of Planning in science. Nye's strategy with this very rich material is to find places where Polanyi and his enemies agree or come close to agreement, and to emphasize those while dismissing as unimportant the places where they disagree. Her sympathies are on her sleeve throughout this discussion. Of the Lysenko affair, which was a test of whether one's values were with science or Stalinist terror, Bernal chose to side with Stalin, suggesting that Lysenkoism might have some scientific merit and saying nothing to criticize Stalin, and praising dialectical materialism.

Nye treats this as background to Polanyi's response to the 'planning of science', and regards his 'fury' at the Marxist trappings of Bernal's ideas about science as a motivator, but claims that it was misdirected, and that they agreed on more than they disagreed on.

... for all the radicalism of Bernal and the Bernalists, some of Bernal's core assumptions about science did not differ from most scientists and historians and philosophers of science writing in the thirties. For all of them, science was a positive good, and it was a good not only in the sense of human advancement but in its dedicated search for the truth. Bernal and Polanyi, just like Blackett and no less than Jeans and Eddington, subscribed to a broadly realist view of what they were doing in their work. (p. 219)<sup>3</sup>

Moreover, she claims, both Polanyi and Bernal 'fundamentally agreed' on 'the moral values and ethical conduct that result in scientific knowledge', because both had a social view of science. The famous passage from Bernal, which she quotes in support of this, says that 'science is communism. In science men have learned consciously to subordinate themselves to a common purpose *without losing their individuality*' (quoted on p. 220). This phrasing echoes the Saint-Simonian idea that the true scientist willingly recognizes and subordinates himself to the superior scientist, and also echoes the idea of the altruistic New Socialist Man: all squarely in a very specific tradition on the Left. Nye wants to see Polanyi's 'emphasis on the master-apprentice relation, science as a tradition and social practice, the competitiveness and personal devotion in scientific work, and the intimacies of independent actors in the community of science' (p. 220) as evincing the same 'social' orientation. Community, she thinks, means *Gemeinschaft*, and that this is the same as what Bernal had in mind, or close enough to dismiss the differences as superficial.

So how plausible is all this? By focusing on Karl, and not actually doing much to explain what Karl was committed to, Nye creates an impression that the brothers had more in common than they did, and that the actual contents of the beliefs of scientists on the Left were unimportant to this dispute. But what did people like Bernal believe? They rejected liberal politics, and indeed politics as such. The great theme of the 1930s was not just 'planning', but the idea that planning understood as a technical activity could replace politics, which merely prevented taking needed actions. This hostility to politics had deep roots in Marx and beyond, especially in the Saint-Simonian notion of the replacement of domination with 'the administration of things'. Science itself was understood as technology or a machine for generating correct results. The idea that science was analogous to liberalism, a domain of contestation for opinions within the framework of certain rules of discourse, an idea central to Popper whom Nye repeatedly invokes, was anathema to them, as was liberalism generally. Polanyi had great respect for the liberal parliamentary tradition. His version of this argument was that liberal politics and science were both strongly traditional activities. This was an argument about the framework of contestation, not a denial of contestation.

The term 'social' is the only link between the positions Nye tries to draw together. But 'social' does not mean the same for the people she wants to narrow the differences between. The differences become apparent when one realizes that, when Polanyi turns to the social aspects of science (see Moodey, 2012), he educates himself by reading the chapter on values in Parsons and Shils' (1951) *Toward a General Theory of Action*: Parsons was not Marx. Shils, who revered Polanyi, moved away from Parsons in the direction of a concept of liberalism that involved the tacit (Turner, 1999), tradition, and so forth, within a body of thought represented by Michael Oakeshott, and various central

European liberal refugees, including Hayek. Only by obliterating distinctions, such as the distinction between *Gemeinschaft* and a Communism populated by representatives of the New Socialist Man, can Nye's argument be made plausible.

Nye makes much of Polanyi's willingness to rely on experts when processes of mutual adjustment won't suffice. But here again, this argument works only by ignoring important nuances. Polanyi's idea of funding being controlled by leaders in science who were like Plato's Guardians, for example, was restricted: he didn't think they should decide the fate of projects, but only decide who was qualified for support. In any case, Polanyi's reliance on experts was limited to very specific needs, each with limits: Bernalism and the Leftism of Karl sought to replace politics by expert rule of a very extensive, indeed unlimited, kind.

Finally, what one misses in Nye's account is any sense of the passions of the 20th century. The Lysenko affair is the 'so-called Lysenko affair', and treated as a normal scientific debate. Everyone on the liberal side of Bernal is treated as giving 'savage critiques', a 'diatribe', 'ridiculing', and so forth. That there was an intellectual and moral basis for making sharp distinctions in a century in which ideologies were fed into a vast bloody machine of war, oppression, and prison camps seems to elude her. The fact that Boris Hessen was executed by a firing squad in 1936, or that Ludwik Fleck was sent to Auschwitz, should be reminder enough that the larger context of the discussion of science was one of ideologically motivated political brutality, pain, and death—and that the discussion itself, between those on different sides, was not a tea party spoiled by a minor misunderstanding.

## Notes

1. This standard story, which I learned from Shils, is reproduced with additions in Turner (2007).
2. For Karl's book on Dahomey, see the exchange collected on <http://polanyi.concordia.ca/comment/microbbie.html> and the extended comments at <http://www.rogersandall.com/amazing-dahomey/> (both sites accessed 10 July 2012).
3. This is misleading, for reasons that go to the heart of the conflict between the Left science of the 1930s and the emerging 'liberal' account of science. Writers on the Left in the 1930s valorized the empirical knowledge of the artisan and working-man, that is to say practical knowledge gained by trial and error. It was precisely this account of science and the scientific method that writers such as Conant, very explicitly (cf. Conant, 1951: 46–7), as well as Polanyi, especially in his account of discovery, sought to discredit.

## References

- Conant JB (1951 [1947]) *On Understanding Science: An Historical Approach*. New York: Mentor Books.
- Conant JB (1951) *Science and Common Sense*. New Haven, CT: Yale University Press.
- Judt T (2011 [1992]) *Past Imperfect: French Intellectuals, 1944–1956*. New York: New York University Press.
- Koestler A (1940) *Darkness at Noon*. New York: The Macmillan Company.
- Kuhn T (1996 [1962]) *The Structure of Scientific Revolutions*, 3rd edn. Chicago: The University of Chicago Press.

- Moodey D (2012) Polanyi, Shils, and the action frame of reference. Paper presented at the annual meeting of the Polanyi Society, Loyola University, Chicago (8–10 June).
- Nye, MJ (2004) *Blackett: Physics, War, and Politics in the Twentieth Century*. Cambridge, MA: Harvard University Press.
- Nye MJ (2011) *Michael Polanyi and His Generation: Origins of the Social Construction of Science*. Chicago: The University of Chicago Press.
- Parsons T and Shils E (1951) *Toward a General Theory of Action*. Cambridge, MA: Harvard University Press.
- Polanyi K (2001 [1944]) *The Great Transformation: The Political and Economic Origins of Our Time*. Boston, MA: Beacon Press.
- Polanyi K (1966) *Dahomey and the Slave Trade: An Analysis of an Archaic Economy*. Seattle: University of Washington Press.
- Polanyi M (1948) *Full Employment and Free Trade*. Berkeley: University of California Press.
- Polanyi M (1962 [1958]) *Personal Knowledge*. Chicago: The University of Chicago Press.
- Polanyi M (1980 [1951]) *Logic of Liberty: Reflections and Rejoinders*. Chicago: The University of Chicago Press.
- Turner S (1999) The significance of Shils. *Sociological Theory* 17: 125–145.
- Turner S (2007) The social study of science before Kuhn. In Hackett E J, Amsterdamska O, Lynch M and Wajcman J (eds) *The Handbook of Science and Technology Studies*, 3rd edn. Cambridge, MA: MIT Press, 33–62.

### Biographical note

Stephen Turner is Graduate Research Professor of Philosophy. He has written extensively on Polanyi, most recently in ‘Making the tacit explicit’, *Journal for the Theory of Social Behavior*, and on the history of science studies. He has also written on the politics of Central Europe in the interwar period, most recently in ‘Schmitt, *Telos*, the Collapse of the Weimar Constitution, and the Bad Conscience of the Left’, in *A Journal of No Illusions: Telos, Paul Piccone, and the Americanization of Critical Theory*, 2011. With Gerard Delanty he edited the *Routledge International Handbook of Contemporary Social and Political Theory*, 2011.