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An Example of the Good Life

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Michael Polanyi and His Generation: Origins of the Social Construction of Science by Mary Jo Nye
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Michael Polanyi lives on in the footnotes. If you want to invoke the idea of 'tacit knowledge', Polanyi is your reference of choice. You'll probably cite his major book *Personal Knowledge* (1958), maybe the earlier *Science, Faith and Society* (1946), maybe the later *The Tacit Dimension* (1966). 'We know more than we can tell' was Polanyi's dictum. We know how to ride a bicycle, but we can't write down how to do it, at least not in a way that allows non-cyclists to read our instructions, get on their bikes and ride off. We can reliably pick out a familiar face in a crowd, but we can't say just what it is about the face that we recognise. And, crucially, since Polanyi is now known mainly as a philosopher of science, a scientist can't adequately describe how to do a bit of science through any version of formalised 'Scientific Method'. Whether the craft is cooking, carpentry or chemistry, the apprentice learns by watching and doing. Where knowledge and skill are concerned, it's not all talk.

Citing Polanyi in these connections is itself a sort of craft convention for historians and sociologists who want to say something about the nature of scientific practice. They do it to indicate that there is a history to appreciations of the informal, perhaps unformalisable, dimensions of science, supposedly the most rationally specifiable practice that we have. Yet the citations don't index the extent to which the texts are actually read. There isn't a lot of current interest in who Polanyi was and how he came to hold the views he did. Mary Jo Nye's excellent and richly researched book aims to tell us and, along the way, uncovers a genealogy for the notion of tacit knowledge that situates it in the force fields shaping much 20th-century thinking about politics and economics as well as science. Two biographical strands run through the book: first, before Polanyi was a philosopher, he was a physical chemist, abandoning the laboratory when he became convinced that telling the world about science was more important for him than doing science; second, he was an émigré Hungarian intellectual whose thinking was forged in the crucible of Central Europe

between the wars.

In wartime Los Alamos, there was a conversation piece known as the Fermi Paradox, posed by the Italian physicist Enrico Fermi. Given the high overall probability that intelligent life existed elsewhere in the universe, why hadn't the extraterrestrials made contact? 'They are among us,' Leó Szilárd replied, 'but they call themselves Hungarians.' The story was told by the Hungarians themselves and it went like this: the Men from Mars were a restless sort and, in search of new worlds to colonise, they long ago came to Earth, landing on the banks of the Danube. They had effectively concealed their true identity, but there were several signs that could give away their Martian origins. One was their wanderlust: they loved to travel and they readily upped sticks; second was their language, which had no known earthly relation; and third was their supernatural intelligence – they knew things, and could think in a way, that no other people did. One could add a corollary: though they often had a profound understanding of the whole spectrum of mere earthly culture, they seemed to understand it, as it were, from the outside. When one of the Martians, the mathematician John von Neumann, was appointed to the Princeton Institute for Advanced Study at the age of 29, a story went around that he was 'a demigod but had made a thorough, detailed study of human beings and could imitate them perfectly'. In Britain and America, the Martian-English accent was much loved and, sometimes, much played up by its speakers, adding both to its charm and its otherworldly weirdness.

Among the Hungarian scientists and intellectuals who came of age around the First World War, many grew up in the same Budapest neighbourhood. They went to a small number of elite schools: the progressive Minta gymnasium alone produced Szilárd and his fellow physicists Edward Teller (who rejoiced in the initials E.T.) and Nicholas Kurti, the engineer Theodore von Kármán, and the economists Nicholas Kaldor and Thomas Balogh. They were overwhelmingly Jewish or from a Jewish background. Almost all were non-observant, some converted to Christianity, but all were quite Jewish enough to be eligible for the gas chamber under the Nazis. Some were politically socialist or philosophically *marxisant*; some were violently opposed to anything to do with Communism and the Soviet Union; yet their intellectual lives were framed by the cultural rips between totalitarian and liberal society, between free enterprise and central planning, between (as the Austrian Karl Popper put it) the open society and what were taken to be its enemies.

Many Hungarian intellectuals of that generation passed through double exile. After the 1914-18 war, and the break-up of the Austro-Hungarian Empire, their geographically challenged homeland experienced, first, the brief Red Terror of the Hungarian Soviet Republic headed by the Bolshevik Béla Kun, followed immediately by the longer-lasting White Terror of Admiral Miklós Horthy's government. Kun had a Jewish background; the commissariat was heavily Jewish (or formerly Jewish); and, even though well-off Jews had

suffered under the Kun regime, the White reaction sometimes referred to the displaced Soviet Republic as the 'Jewish Republic' and presided over the 'Magyarisation' of Hungarian institutions. This included modern Europe's first numerus clausus law, radically reducing the proportion of Jews in Hungarian cultural institutions – Jews so many of whom had thrown over their religious identity and enthusiastically Magyarised themselves. Fleeing the White Terror to Austria or Germany, the Martians embraced Weimar's cosmopolitanism and liberalism, comfortably at home in Vienna or Berlin.

Polanyi was an archetypal Martian. His family name was Pollacsek, which his father – a railway engineer and businessman – had Magyarised to Polanyi. His older brother was Karl Polanyi, the economist, journalist and author of the anti-capitalist tract *The Great Transformation* (1944). Michael's early Hungarian friends included von Neumann, the physicist Eugene Wigner, the sociologist Karl Mannheim and the novelist Arthur Koestler. Michael attended the Minta, qualified as a physician, served as a military doctor during the war, and, having had himself baptised, married a Catholic. (Polanyi's maternal grandfather had been the chief rabbi of Vilna, so that was a rapid turnaround, though in no way exceptional in his circle.) Polanyi left Budapest during the anti-semitic purges of the Horthy regime, and in 1920, having turned to physical chemistry, was appointed to a position at the great Kaiser Wilhelm Institute for fibre chemistry established in the Berlin suburb of Dahlem.

Berlin was wonderful. Despite Weimar economic instability, it was what Budapest had once been, only much, much better. There, Polanyi was surrounded by what was possibly the modern world's most cosmopolitan, intelligent, liberal and open society. There were weekly physics colloquia at the University of Berlin, intellectual *Spaziergänge* in the Tiergarten, intellectual coffee at the Romanische Café in the Kurfürstendamm, intellectual sailing on the Wannsee. Regular attendees at the Wednesday colloquium included Einstein, Max Planck, Walther Nernst, Max von Laue, Gustav Hertz and Polanyi's boss, Fritz Haber – all Nobel Prize winners.[*] Polanyi later called these colloquia and related discussions 'the most glorious intellectual memory of my life'.

In the 1920s and early 1930s, science flourished in Berlin as nowhere else, and Polanyi reflected on the conditions of that flourishing. The Kaiser Wilhelm Institutes (KWG) were lavishly supported by a unique blend of resources from the Reich, the Prussian *Land*, German high-tech industry, private foundations and philanthropists – many of them Jewish, eager to augment the power of the German state which underpinned this cosmopolitan society. At the KWG, Polanyi could get on with his research, free from the demands of routine teaching, free from requirements for immediately useful results and, for some time, free from expectations of political conformity. Science and commercial concerns were closely aligned, but you could persuade yourself that this was more an opportunity than a constraint on free inquiry.

The rules of the game for the KWG chemistry institutes allowed its scientists – the way Nye puts it is telling – to 'focus on basic research in response to problems posed by industry without concern for immediately applicable results'. Polanyi emerged as an able academic entrepreneur. He had a regular income as a consultant with a Hungarian electrical company; he joined an in-house consulting group at the KWG which worked with a range of industrial firms, including the giant Siemens and AEG; he had a stream of patent income; he got funds for scientific equipment from the Rockefeller Foundation in the US; and he skilfully played the offer-from-another-university game to increase his already generous salary and benefits.

His own research was going well, if not precisely as he would have wished. Two of his major research lines were the study of the dynamics of chemical reactions and the adsorption of gases on solid surfaces. There were critics: some said that the methods he used were unreliable and unintelligible; some suggested that certain of his results were spurious. The criticism wasn't seriously damaging to Polanyi's reputation, but Nye plausibly suggests that it was just significant enough to ensure that he never found the judgment of the scientific community transparent or self-evident.

The idyll did not last. Some aspects of Polanyi's vision of Berlin were naive and others were fragile. By 1930, some observers saw quite clearly what was wrong and what might soon happen. The delusion was that science was then the totally free and spontaneous production of a cosmopolitan elite and that the political powers recognised the concrete value of absolutely unconstrained research. The visiting *Manchester Guardian* journalist J.G. Crowther met Polanyi and put his finger on the problem: 'I was left with the impression that the brilliant scientific efflorescence in the period of the Weimar Republic had an intellectual life of its own, above that of industry and the people, in spite of the integration of much of the scientific research with industry. The cosmopolitan character of Berlin science emphasised this division.' Many scientists permitted themselves to believe that their freedom was more complete and durable than it really was. The resources, the equipment and the free time were facts, but they depended on a social contract that few of the scientists cared carefully to inspect. Three years after Crowther's visit, and with the *Reichstagsbrand* ashes still warm, Szilárd called on Polanyi and told him that he thought the Nazis were responsible for the fire and that seriously bad things were about to happen. Polanyi wouldn't hear of it, thinking 'that civilised Germans would not stand for anything really rough happening ... This insanity will pass.' Szilárd had already packed his bags and now took the train to Vienna. By the autumn, Polanyi – finally convinced there was no future even for baptised, war veteran Jews in German science – departed for a chair in physical chemistry at Manchester, where he worked in the laboratory for 15 more years before the university accepted his wandering interests and created a chair for him in what they called 'social studies'.

Polanyi had always been a polymath. As early as 1943, contemplating the big book that would bring it all together, he told one of his sisters that he was going to write a 'magnum opus to deal with science and everything else in the world'. One chunk of 'everything else in the world' was political economy. Specifically, he wanted to think about the nature of science and the nature of the economic order, about how the two were connected, and about why it was practically and morally important to get everyone to understand the nature of the scientific enterprise. For Polanyi, this was never merely an academic exercise; or, better, it was a way of ensuring that the existential conditions for academic exercises were appreciated and secured.

The Martians were political animals – scientists included – and Polanyi was theorising about political economy long before he gave up doing science. As a young man, he argued heatedly with his brother Karl about whether the economic order was, or ought to be, independent of politics: Karl thought not, believing indeed that free trade and a market economy had historically depended on political planning and force; Michael thought, with few exceptions, that political meddling with a self-organising economy was wrong and destructive. (Nye notes that Michael Polanyi, Hayek and von Mises were all using the notion of 'spontaneous order' at that time, and while it has been claimed that Polanyi's scientifically derived concept had priority, the usage was common in 19th-century European liberal thought.) Some form of liberal society was Michael Polanyi's goal – basically, he wanted to top and tail Hayek's Austrian economics by giving the state permission occasionally to intervene to do something about mass unemployment or inequality – but the contemporary threats to liberal order were not equivalent. Polanyi referred to National Socialism as 'accidental outbursts of Fascist beastliness', while Soviet Communism was 'a single coherent process, one vast general upheaval'. The Nazis were thuggish brutes, and Fascism was a spasm of no world-historical significance, but the Communists were far more dangerous, for they represented a mode of modernity. Marxism, he wrote in 1940, 'is a more intelligent and more complete philosophy of oppression than is either Italian or German Fascism'.

The Nazis had wanted only to exterminate him, but with the Communists it was personal. Polanyi visited Russia four times before 1935 and in 1936 published a report on economic realities several years into the Second Five-Year Plan. Industrial production of consumer goods hadn't changed since the time of the Revolution; agricultural production and per capita food consumption were down; workers' housing was worse than in Engels's Manchester; and the death rate was higher than anywhere else in Europe. The Soviet state wasn't working, and the reason for its failure was centralised planning, 'a corollary of Communism'. As disastrous as planning had been for the economy, it was even worse for science. In 1935, Polanyi met Bukharin, a member of the Comintern, leading Marxist theoretician and editor of *Izvestia*, and was appalled to hear his views about the nature of

science and the role of the scientist: 'The distinction between pure and applied science made in capitalist countries was [in Bukharin's opinion] due only to the inner conflict of a type of society which deprived scientists of the consciousness of their social functions, thus creating in them the illusion of pure science. Accordingly ... the distinction between pure and applied science was inapplicable in the USSR.' Bukharin insisted that Soviet scientists were free to undertake any line of research they wanted, but genuine freedom meant they would 'inevitably be led to lines of research which would benefit the current Five-Year Plan'. Polanyi later insisted it was this encounter – not anything to do with the Nazis – that first compelled him to engage with 'questions of philosophy'.

It was one thing to hear this sort of thing in Moscow and another to encounter it in Manchester. Arriving in Britain in the autumn of 1933, he was disturbed to find the National Government seriously considering economic planning measures. Worse, the British scientific community was far to the left of the intellectual classes in general. Scientific Red Eminences were articulate, respected and energetic in getting their ideas before the public – Joseph Needham, J.B.S. Haldane, Julian Huxley, Lancelot Hogben, Hyman Levy and Polanyi's Manchester colleague and political sparring partner, the physicist P.M.S. Blackett (the subject of one of Nye's previous books). In 1939, the publication of *The Social Function of Science* by the charismatic crystallographer (and one-time Party member) J.D. Bernal lit Polanyi's fuse. Bernal's book was both a celebration of Soviet-style scientific planning and a historical reflection on the nature of science. Polanyi instantly recognised Bukharin's agenda in British 'Bernalism', not least because the epiphany for Bernal had been a major conference on the history of science, held in London in 1931, at which the Soviet delegation was headed by Bukharin. In particular, Bernal had been struck by a paper given by the physicist Boris Hessen, asserting that Newton's science responded powerfully to the economic needs of 17th-century society – proof that social responsiveness was in the nature of science and that it was a Good Thing for both society and science.

The urgent priority for Bernal, and for many left-wing British scientists, was the fight against both Fascism and mass unemployment, to which science could contribute provided that state resources unleashed its potential and state planning harnessed its powers. Following Bukharin, Bernal wrote that freedom was 'the understanding of necessity'. Scientists had a moral as well as a professional duty to produce the technologies needed in the fight; this was no time for self-indulgent and snobbish purity. Polanyi's response was immediate: there could be no responsibility higher than the scientist's duty to the integrity of inquiry. If you wanted to settle the proper political arrangements for the control of science, you must first understand its true nature. Polanyi reckoned that science did not have a political purpose, but that the philosophy of science definitely did. Polanyi's 1939 review of *The Social Function of Science* argued that the Bernalists had bought the wrong

philosophy of science.

Bernal mistakenly conflated science with technology: science is a coherent intellectual organism; practical knowledge is an assemblage of bits and pieces. Scientific research, in its essential nature, is spontaneous, self-directing and self-organising, driven on only by its 'internal necessities'; technology is, in its essential nature, responsive to its social environment. True, pure science inevitably possesses the potential for useful application, but the conditions for science and technology to realise their respective goals are different. You can direct technology and it responds if it can; if you try to plan science, you will destroy the thing whose energies you mean to release. It does not matter in the end whether society values science for the 'intrinsic appeal' of truth-finding or for the ultimate practical application of its findings: in either case, society must provide the resources and leave scientists to pursue their own agendas. Polanyi did not give an inch on the issue. Bernal talked of the freedom of necessity, while Polanyi asserted the necessity of freedom. Scientific autonomy had historically been a substantial fact and it had proved its rightness: 'The mature scientist chooses his subject at his own discretion and pursues it day by day in the same discretionary manner. He draws his own conclusions and stakes such claims as he thinks right. At no point of his research work is he subject to any specific instructions from any superior authority.' Bernal thought it absurd that 'any economic system would pay a scientist just to search for truth'; Polanyi thought it absurd that scientists should be paid for any other reason. The natural and obligatory state of science for Polanyi was the way it had all supposedly been on the banks of the Wannsee.

The Bernalists' philosophical error was their belief that science was wholly rational and that it was effectively governed by some version of formal scientific method – though they were typically coy about which of the many versions this was. What was it about the nature of science and its practice, Polanyi asked, that made it intractable as an object of planning? This was the project in which his critique of scientific rationalism developed. 'Complete objectivity as usually attributed to the exact sciences is a delusion and a false ideal,' Polanyi said. Scientists must believe before they can know; scientific knowing is a 'passionate pouring of oneself into untried forms of existence'. 'The ultimate justification of my scientific convictions lies always in myself. At some point I can only answer: "For I believe so."' Gesturing at Plato's *Meno*, Polanyi said that to see a scientific problem was to have a prior intimation of what its final solution must be, 'a tacit foreknowledge of yet undiscovered things'.

When scientists make a discovery, representing a new way of reasoning, they cannot use formal argument to persuade others; they must 'induce a conversion' and must even, rules being insufficient, 'attack the opponent's person'. Whatever counts as scientific method is always insufficient to guide scientific conduct. The impotence of rules to fix their own implementati0n is attributed to Wittgenstein's *Philosophical Investigations* (1953), but,

seven years before that, Polanyi was insisting that rules could be said to guide scientific decisions only when supplemented by 'personal judgments', and that exceptions to rules might be taken not as refutations but as prompts to elucidating the rules' 'deeper meaning'. The notion of 'connoisseurship' hasn't often been attached to scientific judgment, but Polanyi repeatedly did just that: 'Connoisseurship, like skill, can be communicated only by example, not by precept. To become an expert wine-taster, to acquire a knowledge of innumerable different blends of tea or to be trained as a medical diagnostician, you must go through a long course of experience under the guidance of a master.' And so too to become a scientist.

Science is a vast fiduciary system. Scientists know what they do by finding trustworthy sources and then trusting them. It is also what Polanyi called a polycentric system, in which autonomous and only loosely co-ordinated groups of specialists – mildly sceptical and mainly trusting – periodically keep an eye out for what is going on next door. The coherence and integrity of the body of scientific knowledge arise through these processes of mutual adjustment. Finally, the bases of scientific judgment cannot be completely articulated because the 'tacit dimension' is ineliminable. It is not a fly in the formal ointment; it is what makes science science. You would understand that, Polanyi suggested, if you knew what it was to be 'confronted with the anxious dilemma of a live scientific issue'. The further away you are from the quotidian life of scientific practice, the more you tend to be infatuated with myths of method.

Nye convincingly argues that the major purpose of Polanyi's anti-rationalist philosophy of science was political and, specifically, that it was meant to counter Communist visions of hierarchical control. The political machinery of Communist planning proceeded through rational and formal method and it presumed rational and formal method in the object of planning. Conceptions of effective method had been devised to celebrate science, but in the middle of the 20th century they were having the unintended consequence of making people think they could command and control scientific inquiry in whatever direction they thought society needed. But you cannot plan and co-ordinate practices that are in their nature self-organising and whose most basic judgments are not formally specifiable. It was not just that a proper understanding of the nature of science was necessary to defend it; a proper understanding of science could contribute to the defence of liberal society as a whole: 'The world needs science today above all as an example of the good life. Spread out over the planet scientists form even today, though submerged by disaster, the body of a great and good society.' The fabric of science was political.

At just the same time that other scientists, philosophers and sociologists were celebrating the unique formalism and impersonality of science, Polanyi was insisting that it is personal, passionate and prejudiced – and that this was crucial to its capacity to find truth. The contest between Polanyi and Bernal is evidence that there is no determinate

relationship between any particular story about 'what science is' and the value placed on science. Rationalism has traditionally performed the celebratory role, but, depending on historical circumstances, anti-rationalism can do just as well.

What came of Polanyi's ideas and projects? The theoretical bits have had a mixed fate. Some American theologians have recruited his work to justify Christian belief through its apparent parity with science – all that Augustinian language of 'believing before knowing' and the 'passionate pouring of oneself into existence'. But while he tried intermittently to be a good Christian, Polanyi never succeeded in passionately pouring himself into church pews on a regular basis or into Christian doctrines and forms. While opposing Zionism, he found himself addressing the Manchester faithful in 1942 as 'we Jews'.

Polanyi's most substantial academic impact was, as Nye shows, on the sociological study of science, but even here the legacy is ambiguous. For many years, interest centred not on understanding and extending Polanyi's views but wondering how much Thomas Kuhn was indebted to him and whether his contributions had been adequately acknowledged in Kuhn's vastly influential *The Structure of Scientific Revolutions* (1962). (Kuhn said, in effect, 'not very much'; others thought 'quite a lot'; and, while academic manners were at stake, that's about all.) By the mid-1970s, the notion of tacit knowledge had been incorporated into close historical and sociological studies of the processes by which scientific knowledge is made and transmitted, work that shared Polanyi's scepticism about both formal method and scientific impersonality. This 'sociology of scientific knowledge' was, however, little concerned with anything else in Polanyi's work and not at all concerned with using his ideas to defend or celebrate science. Polanyi died in 1976, but by that time the enfolding of science in political, military and commercial institutions had proceeded so far that it was hard any longer to recognise the autonomy that Polanyi was so concerned to justify. The circumstances of science had changed and the point and purposes of stories about the nature of science were changing too.

When Polanyi gave up science, it was, he said, in favour of 'philosophy'. But the philosophers, with few exceptions, never admitted him to their club. G.J. Warnock dismissed Polanyi by insinuating his *foreignness*: Polanyi, he was quoted as saying, 'was not a philosopher at all. He was only a *philosophe*.' The bibliography wasn't right; the idiom was alien; the sentiments were embarrassing. All that 'passionate pouring' didn't go down well in Anglo-American philosophy, and the anti-rationalism offended philosophers who still took as their task not the description of science but its justification. Isaiah Berlin exaggerated Polanyi's stature as a scientist when he evidently said to Raymond Aron: 'These Hungarians are strange ... Here is a great scientist giving up the Nobel to write mediocre works of philosophy.' Nye describes *Personal Knowledge* as 'a dense and difficult book'. She is being charitable; for many readers, it has proved an impossible book, destined to be ransacked rather than read.

There was no reason for philosophers to take Polanyi seriously, or, indeed, to take him at all. I assembled six of the best-known modern Anglo-American introductions to the philosophy of science and couldn't find a single mention of Polanyi in any of them and only one glancing (and unattributed) reference to the idea of tacit knowledge. A notable philosophical exception is Michael Oakeshott's celebrated essay 'Rationalism in Politics' (1947), a conservative critique of the rationalist vision in modern politics, a vision which supposed that there was a 'rule book' for everything and that everything was thought to be encompassed in the 'book'. Disliking Bernal's views even more than Polanyi did, and making a philosophical assault on planning, Oakeshott rightly saw Polanyi as an ally. To get the point of Polanyi's philosophy, or even to see it as philosophy, it helped to share his political sensibilities.

Polanyi's project was practical as well as programmatic: he meant to influence state policy. So far as the institution of science and its social relations are concerned, his analysis has proved spectacularly wrong. In the early 1940s, he was the leading ideologue of the Society for Freedom in Science, formed specifically to combat 'Communist tendencies in science', but he did not see that the most substantial future threats to whatever counted as the self-direction of science would not come from the left. Despite his familiarity with the influence of BASF, Siemens and AEG; despite his own consulting work for ICI in Manchester; and despite his familiarity with the military mobilisation of science by the German and British states, Polanyi did not apparently envisage a near future in which it was not socialist policy but capitalist interests that would come to determine what sorts of research could be supported and where science could go. He did not foresee the role of scientists themselves in helping to run high-tech, entrepreneurial corporations or, again, the role of governments spouting free-market ideology while attempting rigidly to control scientific agendas. Bernal thought the state would never pay scientists just to search for truth; Polanyi thought the state had done just that and, properly educated, must continue to do so. Only in that analysis has the left triumphed.

[*] Steven Shapin wrote about Fritz Haber in the *LRB* of 26 January 2006.

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