

Michael Polanyi, the Scientist

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If he were ever boastful, Michael Polanyi would have described himself as a scientist of the second rank. The first rank, in his terminology, constituted the pillars on which the edifice of science rested. It was the plentiful existence of this first tier in Michael's generation that made it possible for one so casually educated in science as he, to make the contribution that he did.

Remarkably, in every subject in which he had success – chemistry, physics, economics and philosophy – Michael was self-educated. He performed indifferently only in the endeavour for which he was trained – medicine. Science he learned at first in the intervals of his pre-World War I medical studies, and later when invalided out of the Austro-Hungarian army during the war years.

He did not learn entirely on his own since in 1913–1914 he had a year in Karlsruhe (Germany) as a student of chemistry. Nonetheless of his first fifteen scientific papers fourteen bear only his own name, and none were co-authored with a mentor. The papers in question cover the period 1910 to 1917, through the ages 19–26.

There is a lesson to be learnt from this solitary activity. Michael, in his intellectual endeavours, cultivated the position of an outsider. He worked for years as an amateur, in the spare time from his profession of medicine. Later he would be an amateur economist, an amateur on patent law (on which subject he published, and testified before a Committee of the House of Lords in the UK) and, most conspicuously, an amateur philosopher.

What did all this amateurism mean? Many things. A gentlemanly disdain for the professional. A belief in the special ability of the outsider to see beyond cant and convention. Perhaps a romantic admiration for the lonely cow-boy.

Michael was 18 when George Pólya, the great-mathematician-to-be (then in his 20's) made the memorable remark, "Michael walks alone, he will need a strong voice to make himself heard". At the age of 75 Michael lamented: "my voice has not yet carried far". All his life, however, he was convinced that it would do so. The fact that I am asked to write this memoir over a century later, suggests that he may have been right.

His science is still of interest, in part because of the philosophical writing it engendered. I propose (as I already have) to reflect on his science as an indication of his thinking.

He was bold in choosing to be an outsider. He was equally bold in his choice of topics. He was anxious to make use of the freedom that his amateur status gave him. His family (first rich, then poor following the failure of his father's railway in the 1890's) cultivated young Hungarian painters, poets, novelists and scholars. "I grew up in this circle", Michael wrote, "taking it for granted that I could do great things".

This is how it came about that in his twenties he was attempting to extend the reach of one of the great philosophical generalizations of the physics of his day, the Third Law of Thermodynamics. This 'law' embodies a prohibition, namely the unattainability of the absolute zero of temperature.

Michael's early attempt to extend that prohibition was ultimately a failure. Nonetheless, his boldness caught the eye of the newly-crowned monarch of modern science, Albert Einstein. Einstein corresponded at length with the totally unknown army officer Michael Polanyi on this topic, and the related one of 'osmosis' (the tendency of liquids to pass through membranes toward dissolved material). This correspondence, with its pages of writing and its neatly sketched diagrams in Einstein's hand, survives.

Invalidated out of the army, Michael spent the later war years (1916–1917) developing his theory of 'adsorption'; the attachment of gases to solids. Once more the theory was based on thermodynamics. This meant that it was statistical in nature, relevant only to molecules *en masse*. It was, however, an insightful theory, of importance to this day. But

because it failed to take account of contemporary thinking it was destined to be rejected for decades. This is the price of amateurism.

The failure of this theory, initially, to gain acceptance could easily have been the end of my father's scientific career, had he not then taken a step in the direction of becoming a professional. Following a year in Count Karolyi's Ministry of Health in post-war Budapest, the young MD utilised his Karlsruhe connections to get a position as a physical chemist in Berlin, then the global capital for science.

Working in government research laboratories there, he found his *métier*. It was not in the statistical world of thermodynamics, but in the emerging domain of molecular science. Here was something he could visualize. Molecules, however diffuse the new 'quantum mechanics' made them, were, as he felt, tangible. If you put your faith in their reality, they would reward you by giving new insights. Believing in them passionately, he was well-rewarded.

The topic his first Institute offered him was the study of natural fibres. Within months of arrival he succeeded in interpreting the X-ray diffraction pattern of cellulose. This was the first analysis of the molecular structure of a natural material by X-ray diffraction. Additionally it inaugurated the rotating crystal method that proved central, for many years, to the field of structure determination.

From the shapes of molecules he was led to their mode of stacking in solids, and to the explanation of the weakness of large-scale materials as being due to molecular 'dislocations', that is to say to imperfect molecular stacking. He was now moving confidently to and fro between the macroscopic world of materials and the microscopic one of the molecule.

It was time to turn to the central question of chemistry; why are some chemicals stable and others not? Differently stated, why do chemical reactions occur? The rates of the chemical reactions must be linked at the molecular level to the rates of molecular collisions, to the strengths of chemical bonds, and to the collision energies required to sever those bonds.

It was his good fortune that the new mechanics required to describe molecular motions – quantum mechanics – was being developed nearby. The equations of quantum mechanics were, however, famously insoluble. With Henry Eyring, an American visitor to Berlin, he used experimental evidence ingeniously to calibrate the equations so that they

could be solved. The outlines of the hills over which the molecules would have to travel in progressing from reagents to products then emerged from the mists in which they had been hidden.

Incredibly, the years of greatest political turmoil, 1933–1939, which marked his registration from the Kaiser Wilhelm Institute in the face of Nazi legislation and the transfer of his laboratory equipment to Manchester, were his most productive (32 papers published, dated Berlin and Manchester).

On arrival in Manchester Michael collaborated with a young colleague, later his successor as professor, Meredith Evans, to apply thermodynamics to the calculation of the rates of progress across the barriers, recently revealed. Unknown to them, Henry Eyring was engaged in a similar intellectual odyssey in Princeton. The so-called ‘absolute theory of reaction rates’ was being born on both sides of the Atlantic.

Henry Eyring threw himself into the elaboration of this theory. Michael Polanyi viewed it with mild distaste, wishing he had not been forced back into the arms of thermodynamics and hence statistics. Surely, he reasoned, the details of the molecular collision could tell the whole story. Reaction would occur if molecular collisions took place in the correct geometry and with sufficient energy.

Like all-right thinking people, he preferred the clean lines of causality to the haze of probability. This despite the fact that in his ‘Personal Knowledge’ he was to insist on the need for the viewer to project himself onto the viewed. In his science he strove nonetheless for the ideal of untainted, as we all do.

In fact, Michael’s culminating scientific insight was mechanical rather than statistical. It had a difficult birth, beginning with the perplexing observation in 1929 that certain chemical reagents reacted more often than they encountered one another. This “reaction at more than every collision”, as he described it, turned out to have a simple and elegant explanation.

Michael described it to Dudley Herschbach in an interval between lectures on philosophy in Berkeley, around 1960. The central concept was ‘harpooning’. Dudley Herschbach, they realized, had demonstrated it most vividly. The element of the concept had, however, been present in Michael’s publications with Richard Ogg and Meredith Evans, a quarter of a century earlier.

'Harpooning' occurs when a charge-carrying 'whaler' molecule comes within range of a charge-attracting 'whale'. The electron leaps the gap between the molecules, giving rise to a residual positive charge on the whaler and a negative one on the whale. Since plus and minus attract over large separations, the whaler pulls in the whale. A 'reaction' has occurred binding one molecule to another, while the two remain at a large separation. The hurled electron is the harpoon.

Though Michael may not have suspected it, variants of molecular harpooning are highly important in nature. I can attest, however, that he sensed this to be a powerful insight into the the mechanincs of chemical reaction, since he tried, gently but insisently, to persuade me to make it the subject of my research. Like most of the young I only heard his voice much later. I am, it would seem, among a number who continue to hear it.