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MICHAEL POLANYI

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1. Early years

Michael Polanyi was born in Budapest, Hungary, in 1891. He used to recall how his father, a civil engineer and entrepreneur involved in the planning and development of railways, would return from his travels in Germany and the West with tales of tunnels and washouts and with new scientific and educational ideas. But, as a result of some unfortunate mishap, he lost all his fortune in 1899 when Michael was eight years old. This placed a severe financial strain on the family which was greatly aggravated in 1905 when Michael's father died. Michael or Mishi, as his early friends called him throughout life, then earned some of the money needed in the family by tutoring other high school students. The widowed mother, despite straitened means, was able to remain the centre of a social and intellectual circle of which many of the young poets, painters and scholars of that period in Hungary were members. Thus it was that his mother's persistent interest in social problems, in poetry and art, came to have a great influence on Michael's emotional development. He had two brothers and two sisters, older than him, who were all, in their ways, distinguished.

Paul Ignotus (1961) describes the atmosphere of Budapest's intelligentsia in those days as 'a pattern of ... protest mingled with artistic and scientific curiosity'. This was a soil in which many original minds were developing—J. von Neumann and L. Szilard were perhaps equally famous. Michael Polanyi was a founder of the Galilei Circle, of which his brother, Karl, was president. Even then Michael was noted for his quiet authority and, in contrast to his brother, for scepticism about socialist ideologies. 'In a flock of black sheep', writes Ignotus, 'he shocked many by seeming almost white. And while others excelled in extolling science, he excelled in practising it. ... From this background he inherited the limitless liberality of mind, the simultaneity of personal and technical interests and the ability to coordinate them in behaviour as well as in philosophy. What made him differ most from those around him was his reverence.' Perhaps there was another quality which Polanyi acquired from those early years—the capacity to reflect on the workings of his own mind and body, to make sense of his actions and of what befell him.

Polanyi graduated from high school in 1909 and entered the University of Budapest as a student of medicine. From about this time his thoughts turned increasingly towards research and he wrote a paper of medical interest the next year, at the age of 19. This was followed by two papers on similar subjects. Meanwhile, however, his interest in other fields had been developing. During the summer of 1912 he visited Professor Bredig at the Technische Hochschule in Karlsruhe (Germany). Here he was encouraged to set down and publish his ideas on the third law of thermodynamics. The pull of pure science gradually prevailed and, soon after qualifying in medicine in 1913, he entered the Karlsruhe Institute as a student of chemistry under his favourite teacher, Bredig. Meanwhile the latter, not feeling competent to judge the value of Polanyi's paper, sent it on to Einstein who seems to have been considerably impressed by it. The article containing this idea was actually published before Polanyi really entered the Karlsruhe Technische Hochschule. It brought him into personal contact with Einstein who corresponded with him on the third law and other topics in a most friendly and amicable way throughout, and after, World War I.

When war broke out Polanyi had to return to Austria–Hungary. He served in the army as a medical officer but his interest in physical chemistry continued to manifest itself vividly during this period and he wrote several papers. Three of these were on the derivation of the third law of thermodynamics (Nernst's theorem) but, more importantly, the other four dealt with the adsorption of gases by solids. Actually, he had already written a paper on this subject in Karlsruhe, before becoming a medical officer. The last of the four papers became his doctoral dissertation submitted to the chemistry department of the University of Budapest. The nature of his work at this time, his theory of adsorption, together with his other scientific contributions, will be discussed in the second part of this memoir.

World War I ended with defeat of the central powers and led to the break-up of Austria–Hungary. Polanyi remained in Budapest until he had taken his doctoral degree in chemistry. However, the confused political situation and the dictatorial nature of the new régime induced him to return to Karlsruhe in 1919. It was here that he became interested in the problem of the rate of chemical reactions. Here too he met his future wife, Magda Kemeny, also a chemist, whom he married in 1921 in Berlin.

Polanyi's move to Berlin, to the Kaiser Wilhelm Institut für Faserstoffchemie (fibre chemistry), took place late in 1920 and it brought about a fundamental change in the subject and also in the mode of his work. His interest shifted, at least temporarily, to the X-ray analysis of fibrous structures, in particular of cellulose, but also of metals and to new methods of X-ray analysis in general. This then led him to the study of the structure and properties of crystals, principally those of metals. The mode of his work was also greatly altered partly as a result of the more permanent association with a definite institution, partly because at the Institute he made many friends with whom he found pleasure in collaborating. Until his move to Berlin, most of his papers had a single author, but his first paper on fibrous structure was by three authors, one of whom, R. O. Herzog, was the director of the Institute.

Collaboration with students and colleagues, and close personal relations with them, remained a permanent feature of Polanyi's later work in the physical sciences. Well over half of his papers on physics and chemistry were published jointly with collaborators. He had, altogether, almost sixty collaborators and his contact with most of them was neither short nor restricted to a single article. These discussions with collaborators ranged over much wider fields than the problem in physical chemistry which they hoped to solve. They included the complexities of human relations and of political and economic theory. Polanyi and his collaborators, or at least most of them, formed a closely knit society with almost a family atmosphere. Here he was gaining that knowledge of working in a trusting but critical team—experience of what he later called the 'conviviality' of intellectual work—which was to provide an essential element in his subsequent thought about the process of discovery. Because of the breadth of the interests developed in this group few of Polanyi's scientific collaborators were surprised when, later on, his prime interest changed again to economic and social problems and then to those of human understanding and the general problem of the nature of scientific endeavour.

After three years at the Institute of Fibre Chemistry, and close collaboration with colleagues there, Polanyi was invited by F. Haber, the director of the Institut für Physikalische Chemie und Electrochemie to head one of the departments of his Institute. Ever since his invention of the process for the synthesis of ammonia Haber had been highly respected, not only by his fellow scientists, but also in government circles. He directed his Institute somewhat from a distance; one rarely saw him and he rarely attended the scientific conferences. Hence the departmental heads had to make most of their decisions by themselves. This was very fortunate as far as Polanyi was concerned as it allowed him to maintain for several years a great variety of contacts with former colleagues at the Institute of Fibre Chemistry and to keep up his interests in crystallography and crystal structure. He collaborated on several papers on this subject as late as 1925. The focus of his attention, however, was moving increasingly to his earlier favourite subject, that of the rate of chemical reactions. This brought him into contact with a new set of collaborators, some theoreticians, others experimentalists. His work and ideas on rates of reactions, at that time very much a centre of interest in physical chemistry, brought him much pleasure and also a great deal of outward recognition. His 'habilitation' at the University of Berlin to the status of *Privatdozent*, in 1923, was principally in recognition of his work on the structure and properties of crystals. However, his professorial title, received in 1926, and his appointment to life membership of the Kaiser Wilhelm Society (now the Max-Planck-Gesellschaft) were due to his work on reaction kinetics and to his stimulating influence on a great number of collaborators. The essence of the advances made by this work will also be described below.

The next event in Polanyi's scientific career was sad but important. The Nazi party had considerable influence in Germany even before Hitler became

Chancellor in January 1933. This influence led to the dismissal of several Jewish scientists. Polanyi was greatly perturbed by this and in November 1932 he invited about ten leading scientists, including Planck and Schrödinger, to a private meeting and suggested a joint protest against those dismissals. Though the participants at the meeting, including Planck and Schrödinger, were in agreement with Polanyi, very little came from this initiative. As a result, Polanyi resigned from his life membership of the Kaiser Wilhelm Society and soon gave up his position at the Institute. Incidentally, his resignation was soon followed by that of Schrödinger. Polanyi's widow recalls that, after a dinner at their house, Schrödinger pointed to his table cloth, saying: 'This white tablecloth is beautiful but were I to throw a bottle of ink over it, what would it be like ? To my mind, that is what Hitler has done to Germany.'

It was not easy for Polanyi to give up his position at the Institute of Physical and Electro-chemistry for it inevitably meant leaving many friends and collaborators behind. It was after some hesitation that he accepted the Chair of Physical Chemistry at the University of Manchester. He started work there in the autumn of 1933.

2. Scientific research

Polanyi's first scientific paper, 'The chemistry of the hydrocephalic liquid' (1), appeared in 1910, when he was barely 19 years old. However, even though he published two other articles on related subjects, his attention soon turned to problems of chemical physics, at first toward thermodynamics but especially to the third law (8, 9, 10, 11).

According to this law, also called Nernst's theorem, the entropy of all bodies tends toward zero with decreasing temperature. The reason for this is that the probability of the excitation of a vibration with the frequency ν is $e^{-h\nu/kT}$ and tends to zero as the absolute temperature $T \to 0$. Most other ways of excitation require even more than the excitation energy $h\nu$ of the vibrations so that the probability of their excitation decreases even faster with decreasing T. Hence, the properties of materials approach, with decreasing temperature, the property of the state of lowest-energy, the state with 0 entropy. The most interesting point that Polanyi made in connection with the third law is that an increase of the pressure, leading to a decrease of the volume, has an effect similar to that of decreasing temperature because it increases the frequencies of the vibrations. It is remarkable that this point is not mentioned in books on thermodynamics, the reason being probably that even the highest pressures that can be produced in our laboratories have less effect on the entropy than a very modest decrease of the temperature. Einstein's reaction to Polanyi's observation showed, nevertheless, that this observation is interesting and worth knowing. The subsequent correspondence with Einstein is also interesting. Now, after 60 years, it is easy to see that Einstein's point of view corresponds to the practical, realizable situation, Polanyi's to an idealized one.

However, the subject of the third law of thermodynamics occupies only an early and very small part of Polanyi's contributions to physical chemistry.

These were widely spread, yet centred, principally, on three subjects: adsorption of gases on solids, X-ray structure analysis of the properties of solids, and the rate of chemical reactions.

In order to explain the adsorption of gases by solid surfaces, Polanyi introduced in 1914, and in more detail in 1916, the most natural assumption: that there is an attractive force between the solid surface and the atoms or molecules of the gas. However, at that time the nature and magnitude of the attractive force could not be derived from basic principles. It could be described by postulating a relation between the magnitude of the adsorption potential ε and the volume φ in which this potential is present, i.e. a function $\varepsilon = f(\varphi)$ which connects the two quantities. Conceptually it would be, of course, more natural to consider φ, that is the volume in which the adsorptive potential is ε, to be a function of the latter but the result of the calculation can be expressed more easily by considering the potential ε to be the function of the magnitude of the volume in which it prevails. Polanyi assumed further that the potential is independent of the temperature of the adsorbing wall and that the pressure exerted by the adsorbed material on its neighbourhood is the same as that which the material would exert, at that density and temperature, in the free (i.e. not adsorbed) condition. Naturally, the force acting on the outer adsorbed layers puts the layers closer to the surface of the adsorber under pressure.

If one knows the pressure of the gas as function of its density and temperature, the function $f(\varphi)$ determines the total adsorbed quantity. The connection between $f(\varphi)$ and this quantity—which depends, naturally, on the density of the gas surrounding the adsorber as well as on the temperature of the system—was derived by Polanyi, actually in a somewhat artificial way. He showed that just as $f(\varphi)$ determines the pressure dependence of the adsorbed quantity, conversely the pressure dependence of this permits one to obtain $f(\varphi)$. This can then be determined from the observed pressure dependence of the adsorbed quantity at one temperature. Then, knowing $f(\varphi)$, the pressure dependence of the adsorbed quantity at all other temperatures can be calculated and compared with the experimental data. This is what was done by Polanyi in his 1916 article, using the data of Titoff for the adsorption of CO_2 by charcoal at three different temperatures and the agreement with his theory was quite satisfactory.

In spite of this, Polanyi's theory of adsorption was rejected by most of the scientific community at the time of its proposal. The idea of a simple attraction between adsorber and gas was foreign to most scientists of those days. Only two types of force acting on atoms were recognized at that time: electric and valence forces. The valence forces are saturated by a single adsorbed layer and the electric forces depend on the charge. Neither of these two forces would attract several layers of gas and it was believed that only a single layer of molecules or atoms can be adsorbed to the adsorber's surface. This belief was so strongly held that even Polanyi was affected by it. The writer of these lines (E. P. W.) remembers that when he pointed to some experimental data strongly supporting the multilayer character of adsorption, Polanyi was quite taken aback. It is true also that the phenomenon of adsorption is very complex for there are several types of adsorption forces. However, the idea of a simple potential, as proposed by Polanyi, found a theoretical justification in the Wang–London theory (1927 and 1930) of dispersion forces and a joint publication of London & Polanyi (115), also in 1930, brought this out very clearly. It is very interesting, in this connection, to read Polanyi's reflections in 1963 on the history of his theory (326). He points out that too much adherence to, and belief in, the prevailing theory may, under certain conditions, hamper the acceptance of a novel but correct idea. He shows no resentment toward those who criticized his theory even though he subsequently remarked that 'professionally, I survived . . . only by the skin of my teeth'. He came to see that the kind of criticism which his theory underwent was an unavoidable consequence of the belief in the correctness of the prevailing theory—a belief necessary for the continuous development of science. Perhaps it should also be mentioned that even before 1930 Polanyi realized that there are different kinds of forces which can be responsible for the adsorption and that some of these forces are not in accordance with the picture he developed. But the dispersion forces do satisfy his assumptions to a very good approximation.

Polanyi's interest in X-ray crystallography started with his association with the Kaiser Wilhelm Institute for Fibre Chemistry in 1920. He found there several enthusiastic colleagues interested in the subject and also facilities for experimental work, though the latter were on a modest scale. As was mentioned above, the former circumstance deeply influenced the mode of his scientific work and the second one also had a profound influence. He ceased to be a pure theoretician and started to devise, and also participate in, experimental explorations. Not many of our colleagues today equal his interest and participation in both theoretical and experimental work and such a joint interest in both modes of research was not common in those days either.

Before Polanyi joined the Institute of Fibre Chemistry, X-ray studies had been undertaken there by R. O. Herzog, the Director of the Institute, jointly with W. Jancke, and they showed that cellulose has a kind of crystalline structure. This was also noted by P. Scherrer. If the incident X-ray is perpendicular to the direction of the fibre, the reflected beams have definite directions and these directions remain the same even if the fibre is rotated about its axis. If the incident beam was parallel to the direction of the fibre, the reflected beams covered all directions of cones, they darkened, instead of points, whole circles of a photographic plate perpendicular to the beam. The pattern they produced had the appearance of the Debye–Scherrer diagram. All this not only showed that the fibre, as a whole, had a rotational symmetry about its axis, it also made it possible to draw far-reaching conclusions concerning its structure. To the understanding of this, Polanyi contributed very significantly.

An X-ray beam travelling originally in the incident direction i will be scattered by all atoms which it reaches. The waves scattered by the different atoms will interfere, however. If the scattering atoms form a crystal, the interference will be destructive in all directions except those for which the difference of the lengths of the paths of two beams, scattered at two points removed from each other by an identity period of the crystal, is an integer multiple of the wavelength of the X-ray. However, if, for a direction *e*, this is true for three primitive identity periods, it will be true for all identity periods. All this was, of course, well known since von Laue's discovery of the X-ray analysis of crystal structure. It is clear also that, since a crystal has three primitive identity periods, there are three equations connecting the direction *i* of the incident beam with that of the scattered beam, *e*. Since *e* can be characterized by two numbers, the three equations have for a definite *i* in general no solution—an ideal crystal, for most incident beam directions *i*, does not give a scattered beam. If, on the other hand, *i* extends over all directions, as is the case for a mixture of microcrystals with all possible orientations, the directions of the scattered beams form a one-dimensional manifold. Because of the symmetry of the system with respect to rotations about the direction of the incident beam, the directions *e* form circular cones, *i* being the axis of these. This is the Debye–Scherrer diagram. If the incident beams' directions with respect to the crystal form a one-dimensional manifold, as is the case if one of the crystal directions includes a definite angle with a given direction, there will be as many equations as unknowns and the directions *e* of the scattered waves will form a discrete manifold. Such a discrete manifold of directions, that is of two parametric quantities, contains much more information than the manifold of the opening angles of the Debye–Scherrer cones—the opening angles provide a set of numbers, the aforementioned directions a set of *pairs* of numbers. In fact, the Debye–Scherrer diagram can be obtained from the faser or 'rotating crystal diagram' by drawing a circle through every point of the faser diagram, the centre of the circle being on the incoming beam. Conversely, the position of the points of the faser diagram cannot be obtained knowing only the circles on which they are situated.

These points, the advantages of rotating crystal diagrams, given naturally for fibres by their structure, and obtainable for a crystal by rotating it about a crystallographic direction, were probably known to others. In fact, they were implicit in an article of Scherrer, quoted also by Polanyi. They were, however, most clearly articulated and best utilized by the members of the Institute of Fibre Chemistry: Polanyi and his collaborators, including M. Ettisch, R. O. Herzog, W. Jancke, H. Mark, E. Schmid and K. Weissenberg. Their attention was drawn to the value of such diagrams by the early work on the X-ray analysis of cellulose fibres, carried out at the Institute. They did determine, almost completely, the atomic structure of these cellulose fibres and their application of the method of rotating crystals greatly advanced the analysis of the structure of many crystals.

The analysis of the structures of crystals was far from being the only subject to which Polanyi contributed as a result of his association with the Institute. He and his collaborators grew crystals, principally metallic ones, investigated their properties under stress and found many interesting phenomena. One of these was the increased plasticity of rocksalt, if submerged in water. This was first noted by Ewald & Polanyi (59). In fact, the problem which attracted Polanyi's interest most, and most lastingly, was the question of the shear and the rupture strengths of crystals. This is less than a hundredth of what a simple theory would suggest—the theory according to which rupture, for instance, would occur when the external force applied exceeds the maximal force with which the two crystallographic planes, which are to be separated by force by the rupture, can attract each other. According to Polanyi's theory—and similar theories were proposed also by others—the much earlier rupture is caused by imperfections in the lattices around which the stress lines concentrate so that a wedge of increasing depth is formed. The situation is similar with respect to shearing—a phenomenon even more closely investigated at the Institute of Fibre Chemistry. According to Polanyi, one had to assume that there is a lattice imperfection around every thousandth atom, and that the number of these imperfections cannot be decreased by increasing the temperature.

In order to investigate further the behaviour of crystals and aggregates of crystals under increasing stress Polanyi developed and used, in collaboration with H. Mark and B. Rosebaud, a special piece of apparatus. This is often called the Dehungsapparat of Polanyi and has subsequently proved extremely useful. Polanyi maintained a considerable interest in the problems which he had encountered at the Institute of Fibre Chemistry even after his move to the Institute of Physical Chemistry. He published articles on them as late as 1930 and 1931. His work on the physics of metals, and other solid state materials, was most aptly praised by one of his early collaborators, later President of the Austrian Academy of Sciences, E. Schmid, in an article published in the *Logic of personal knowledge* (Ignotus *et al.* 1961)—a book written in honour of Polanyi. The last sentence of this article reads: 'Just as he was for his collaborators the paradigm of the scientist constantly seeking for fundamental explanation, so, along with his charming wife, he also taught them to bear with good humour, or even to overlook altogether, the difficulties and limitations of the time'. These limitations were indeed serious in the early '20s, the time of his association with the Institute of Fibre Chemistry.

On becoming departmental head at Haber's Institute of Physical and Electrochemistry in 1923, Polanyi soon reverted to his earlier interest in the rate of chemical reactions. His first paper on the subject was actually published in 1920, even before his entering the Institute of Fibre Chemistry (20, 24). As at this latter institute, he found also in Haber's a circle of interested and devoted collaborators, many of whom, including H. Beutler, S. V. Bogdandy, H. Eyring and one of the present writers (E. P. W.), maintained their interest in the subject for a long time. Here, too, Polanyi was active in both theoretical and experimental approaches.

The experimental work consisted principally in the measurement of reaction rates—an enormous number of them. Polanyi's original idea was to form atomic or molecular beams of the substances the reaction between which is to be studied. These beams were to be arranged in such a way that they intersected so that the number of reactions taking place would directly give the reaction cross section—a quantity from which the rate could easily be calculated. However, the experimental methods at that time were not sufficiently well developed for the technique to be applicable to his work and so the method used was different. Polanyi always hoped to be able to use the colliding beam technique which gives more information about the process of the reaction and which now is certainly practicable.

In the method originally used, in collaboration with Beutler and von Bogdandy, the two reacting gases entered a glass tube—about 1 m long and 3 cm in diameter—at its two ends at very low pressure. As they met, they reacted near the middle of the tube and the length of the reaction was established. Some of the reactions were accompanied by chemiluminiscence and some produced solid reaction products which were then deposited on the wall. From the length of the reaction zone one could estimate, by means of the theory of diffusion and the knowledge of the gas kinetic cross section, how many scattering collisions the incoming atoms suffered before reacting, i.e. the ratio of scattering to reaction cross sections (76). This method underwent several modifications and was used in one form or another for the measurement of about a hundred reaction cross sections, most of them between an alkali and a compound containing a halogen, including the halogen molecule itself.

These measurements were widely recognized to be of fundamental importance; the rates for the reactions measured were spread by a factor of nearly a million. The methods of measurement were also adopted by other scientists and proved to be very useful.

Polanyi recognized very early the need for an adequate theory of chemical reaction rates. He saw, even in his Karlsruhe days in 1920, that the available rate theories could not be truly valid because the comparison of the forward and back-reaction rates did not give the equilibrium postulated by the laws of thermodynamics. In a subsequent article he questioned the possibility of establishing a reaction rate theory compatible with the then known laws of physics.

In fact, the first theory of the rates of association and dissociation reactions, written in collaboration with E. P. Wigner, introduced assumptions which appeared drastic at that time but which subsequently proved to be correct (70). It postulated that the excited states of the molecule obtained by the association have a finite energy spread and that this energy spread $\Delta\varepsilon$ is related to the average life time $\Delta\tau$ of that molecule by the relation $\Delta\varepsilon\Delta\tau = h$, that is Planck's constant. It further postulated that the molecule with angular momentum $nh/2\pi$ could be formed from the colliding particles if their relative angular momentum is between $(n-1)h/2\pi$ and $nh/2\pi$, i.e. that either the law of the conservation of the angular momentum is invalid, or that all angular momenta are integer multiples of $h/2\pi$. As we now know, the latter assumption corresponds to quantum mechanical theory more closely, and also that the $(n-1)h/2\pi$ to $nh/2\pi$ should be replaced by $nh/2\pi$ and $(n+1)h/2\pi$—zero angular momentum systems were not believed to exist in 1925, when this article was written. It is characteristic of Polanyi's modesty that it required considerable persuasion to induce him to have his name associated with the article. It may also be worth observing that the reaction rates postulated in this article are valid not only for chemical but also for nuclear association reactions; they gave the impetus for establishing the formula for the cross section of the absorption of neutrons by nuclei.

The article just summarized gave the correct ratio for the rates of association and dissociation reactions so as to give the connection postulated by thermodynamics for the ratio of the densities of associated and dissociated molecules in thermal equilibrium. The ratio of the two reaction rates is independent of the widths of the energy levels—both the association and the dissociation rates are proportional thereto. However, in order to obtain a numerical value for the reaction rates, at least one of the two rates, and hence the $\Delta\epsilon$, has to be calculated numerically. This was done, by the same authors, using the picture that dissociation occurs if a sufficient number of the various vibrational modes of the molecule happen to stretch the bond to be broken at the same time to a sufficient extent. This picture is also generally accepted now and has been widely used also for specific cases.

The two articles just reviewed dealt with association–dissociation reactions. For reactions of the more common type $A + BC \rightarrow AB + C$ Polanyi adopted the theory of London (also called the Born–Oppenheimer approximation) in which the nuclei of atoms move essentially according to the laws of classical mechanics under the potential given by quantum mechanics for the given configuration of the nuclei. He made, in collaboration with H. Eyring (114, 118), the first reasonably accurate determination of such energy surfaces for reactions

$$H + H_2 \rightarrow H_2 + H,\ H + HBr \rightarrow H_2 + Br,\ H + HBr \rightarrow HBr + Br.$$

The calculation, which gave the activation energy for these reactions, was not entirely theoretical, it also used some experimental data. The surfaces obtained were used later, also by Polanyi and Eyring, and on the basis of the transition state method developed in his laboratory by H. Pelzer and E. P. Wigner, not only to calculate the activation energy but also the reaction rate, completely.

The preceding discussion gives only the basis of Polanyi's extended work on the rates of chemical reactions. His interest in the subject started in 1920 and lasted until he became a professor of social science at Manchester in 1948. His last article on the subject, 'Mechanism of chemical reactions', was published in 1949 (217). His work on the subject of chemical reaction rates can well compete in significance with that of any of his contemporaries.

3. Manchester University

The Polanyi family adapted quickly to their new environment when they arrived at Manchester in 1933. Michael's command of English was soon perfect and almost all his papers, after 1933, were in English. His two young sons found their feet without difficulty. There was George (1922–75), who became an economist, and John (1929–) who followed his father's footsteps into the field of physical chemistry.

The picture one gets of Michael as a parent is of a father powerfully influencing the young towards truth and towards being enterprising wherever they were, always with an emphasis on thoroughness. Holidays in Cumberland, or in Britanny or—this time—by a lake in Yugoslavia: Michael has just swum right across the lake with George and now it is John's turn—practising, filling his lungs, really floating in the shallows. But inevitably Michael was often away, writing in his study or in the summer house or off to America or India; meanwhile Magda and the boys were exploring along the shore, or in town or getting ready for their return to Manchester Grammar School, all vigorously coming to terms with a new social and intellectual world.

Professor John Polanyi, F.R.S., comments in a letter on the 'warm and lasting welcome' that the family received in Manchester and on 'the vigorous social-cum-intellectual life which enveloped them, not only in a milieu of university professors of many disciplines, but also of remarkable doctors, lawyers, factory and mill owners, people connected with the art galleries, the Northern Service of the B.B.C. . . . the *Manchester Guardian* and the odd (really quite odd) aristocrat —a remarkable village community drawn together by a liveliness of mind through which they triumphed over the . . . nastiness of their environment'.

John Polanyi also fills out the picture of his father's scientific work during those Manchester years:

> 'Perhaps the two major conceptual innovations that came out of Polanyi's laboratory in the Manchester years were the derivation (and rationalization) of a parallelism between reaction heat and reaction rate in related families of chemical reactions, and the development of the "transition state theory" of chemical reactions. Both of these concepts are stimulating fruitful thought and discussion over forty years later and are likely to continue to do so for years to come. The first stemmed from collaboration with an American visitor to Polanyi's laboratory in Manchester, Richard Ogg, who was there in 1934. It was later extended and clarified in collaboration with M. G. Evans (167, 168, 190).
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> 'The transition state theory took as its starting point Pelzer & Wigner's paper (1932) in which the properties of an "activated complex" or "transition state" were calculated for the first time (using Eyring & Polanyi's then newly published potential-energy surface (118)). In 1935 Evans & Polanyi succeeded in generalizing Pelzer & Wigner's approach; an undertaking that was paralleled in America by Eyring in the same year, partly in collaboration with Wynne-Jones (169).
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> 'It will not have escaped the English reader that each of the main actors in this second drama—Polanyi with "transition state theory" and Eyring with "activated-complex theory"—had an *éminence grise* from Wales. Michael Polanyi's collaboration with Meredith Evans was protracted, fruitful and marked by a loving respect on both sides for the other's special qualities of mind. It was a source of abiding pleasure to both when the Chair of Physical Chemistry, vacated by Michael, was filled by Meredith (who at

that date held the corresponding chair at Leeds). Meredith became once again a frequent visitor to the Polanyi household where he followed Michael's move toward philosophy with wonder. He died tragically of cancer in his forties and the Evans–Polanyi school of chemistry, which might have been flourishing in Britain today, became in large part a memory.'

The stream of Polanyi's writing, both his chemical papers and his new work on economic and political subjects, on patent law and on philosophy, did not result in any withdrawal from university affairs. One of the strengths of the Manchester Chemistry faculty at that time lay in the quality of the supporting cast, especially in the Reader, Colin Campbell, who made it possible for a brilliant sequence of stars to succeed each other. They could rapidly establish themselves, shed their light and move on: Heilbronn, Hirst, Todd and E. R. H. Jones followed each other at intervals of a few years, but there was continuity and stability despite the stress of change. Such stimulus and shared confidence were congenial to Polanyi in many ways. As was expected he built up a flourishing research school and his own teaching of undergraduates was highly successful. Sir William Mansfield Cooper (1976), who later became vice-chancellor, writes: 'There is no doubt that the good student got much from him, but the remarkable thing is that the poor ones were happily carried along.' This was due to Polanyi's systematic coverage of detail, through handouts and guided reading, which he combined with profound exposés of major problematic themes in lectures.

In the three years 1933–36 he published 37 papers on reaction kinetics, 29 of these jointly with collaborators. This, however, was the climax of his productivity in physical chemistry research. The approach of the outbreak of war with Nazi Germany, which he clearly foresaw, put a brake on his research. When the first rumours of possible work on the atomic bomb reached him he was sceptical of its feasibility and this probably diverted him from being drawn into one exciting stream of new research. As the war continued he published less on the physical sciences and more on political and philosophical subjects.

In 1939 he turned, not for the first time, to economic problems. He led a small team which produced a pioneer educational film on economics and unemployment (223). In 1944 he published an original paper on the need for reform in patent law advocating more favourable provision for the encouragement of inventiveness (236). 'Running through all his writings in economics', writes another former colleague, Professor John Jewkes (1976), 'there seems to be one central strand: how best to reconcile the safeguarding of individual liberty with the controls upon the individual inseparable from a complex and organized society, or, as Polanyi succinctly put it, the relation between spontaneous and social order. His analysis was pointing not only to the inhumanities of totalitarianism but also to the muddle and drift which so often result from overconfident planning in freer societies.' There was an interesting contrast in Manchester in those days between P. M. S. Blackett, the still wholehearted apostle of planning, and the quiet, speculative Polanyi who questioned the popular trend. Mansfield Cooper (1976) writes: 'Blackett the great theoretical planner had a genius for improvisation and getting things done. Polanyi the exponent of freedom and the critic of planning was most meticulous and cautious in action. ... With such polarity of views clashes ... were inevitable but there was no feuding ... always reason and something near affection and certainly respect supervening.' Perhaps it was inevitable that Polanyi's widening interests should produce reactions of suspicion and perplexity from some colleagues but the attitude of most of these, certainly on the scientific side, was one of respectful interest and occasional perplexity but never of antagonism. During the nineteen-forties it was becoming increasingly evident that he would like to concentrate all his energies on the philosophical foundations common to these diverse interests and, if possible, to have a chair from which to forward such enquiry. The vice-chancellor, Sir John Stopford, was keen on the idea but it raised many problems, not least with other philosophers who understandably wondered if Polanyi realized just how different and how rigorous would be the judgements he would face once he ceased to be gentleman player and became a 'pro.' Sir William Mansfield Cooper explains how the problem was solved:

> 'The vice-chancellor [Stopford] knew that the chance of establishing an additional chair of philosophy at that time was nil and that a dozen other chairs would rank in priority. He was aware of much American interest in Polanyi and was determined to keep him in Manchester. He was not deterred by suspicions that Polanyi could never be more, academically speaking, than an amateur philosopher. He was, in my judgement, bound to penetrate this argument. Was not Polanyi, in the strict academic sense, an amateur in everything except his early skills in medicine ? And could anyone quarrel with the result ? Had one not merely to know Polanyi in an unprejudiced way to realize that here one was dealing with Erasmian man, with the protean scholar ? So Stopford, without any University authority, transferred Polanyi from Chemistry to a non-existent new chair and carried the University with him. The price was doubtless the name of the chair; but "social studies" did not really do violence to the general movement of Polanyi's interests (1976).'

So in 1948 Polanyi assumed his personal chair in social studies. A circumstance which speeded up this changeover was that Polanyi had been invited to give the Gifford Lectures and he needed a period of thought and extensive study for their preparation. The lectures—eighteen in all—were delivered in Aberdeen in 1951–52 and they were subsequently developed into Polanyi's major philosophical work, *Personal knowledge* (301).

Polanyi's concern, which had been growing for more than twenty years, was, in its negative form, a fear for the dehumanizing of science and, indeed, of all knowledge. The extreme manifestation of this was in the totalitarian states of Europe. Communism, however, appeared to him as the most significant symptom of a much more pervasive ill. 'Marxism', he wrote in 1940, 'is a more intelligent and more complete philosophy of oppression than is either Italian or German

Fascism (226)'. It seemed to Polanyi that the intellectual resistance being offered by most liberals was both shallow and divided. So it was to the positive task of articulating a new world view, adequate to the crisis facing all advanced societies, that he turned his mind. Some scientific friends found the perspective that he was beginning to describe both profound and inspiring; others regretted his shift of interest and saw it as a loss to science and a darkening of reason. Polanyi himself, however, was quite clear about the importance of his change of tack and subsequently referred to 1946 as being the year in which 'I found my true vocation'—as a philosopher.

4. From chemist to philosopher

Posterity will judge just how important Polanyi's writings *about* science and society were. His stature as a scientist was already firmly established and to conjecture about what physical chemistry lost after 1946 is not fruitful. From the relatively close perspective of the nineteen-seventies, however, it is interesting to look at the nature of the deliberate turnabout which he made. In attempting this it is necessary to touch on some of Polanyi's main ideas about the nature of science and technology, about the processes of discovery and how these are rooted in shared traditions, for underlying all his theoretical analysis was a profound respect for, and understanding of, skilled action.

Polanyi's first published writing on subjects outside his own scientific field was his pamphlet on the planned economic development of the U.S.S.R. (219). He recounts how shocked he was when, on a visit to Moscow in 1936, he heard Nicolai Bukharin arguing that science under socialism must cease to be carried out for its own sake and must be firmly harnessed to the needs of the Five Year Plan (332).* In the late nineteen-thirties similar utilitarian arguments were getting strong support in Britain from J. D. Bernal, Lancelot Hogben and others (Bernal 1938). Then in 1939 J. R. Baker wrote 'A counter-blast to Bernalism' in *The New Statesman* and this led to his friendship with Polanyi who had himself produced a similar critique of Soviet science, subsequently published as *The contempt of freedom* (226).

Partly as a result of this encounter Baker sent a letter to 49 British scientists suggesting the formation of a society for the defence of free scientific enquiry. From this grew the Society for Freedom in Science which lasted until 1961 and in which Polanyi played an active part. There is an unpublished letter, written by him in reply to Baker's circular, which foreshadows many of the lines along which he subsequently worked. 'We cannot', he asserts, 'defend the freedom of science unless we attack . . . collectivism.' Then he continues:

> 'If the community acting through the power of the state is to be the sole judge of what is bad for men living in society, then it has to claim also supremacy over what is to be considered true and what untrue. Science

* This encounter and Polanyi's subsequent philosophical reflections on machines (see p. 434) forms the starting point of Joseph Weizenbaum's *Computer power and human reason* (1975).

cannot be free in a state formed as sovereign master of the community's fate, but only under a state pledged to the guardianship of law, custom, and of our social heritage in general, to the further advancement of which—on the lines of the universal ideas underlying it—the community is dedicated.

'I have recently read Rauschnigg's book *Hitler speaks* and was impressed by the fact that Hitler and Himmler use exactly the same terms about the necessity of subordinating science to collective aims as the Bolsheviks.

'Our critics have—I think—rightly maintained that the contrast between despotism and democracy is superficial if we define democracy as the right of the people to determine their own destiny. They point out how little democratic operations, by elections and votes, can change the established order of such things, how dependent such operations are on the intellectual and moral traditions in which the community is steeped. We ought not to attempt to deny this, but rather to proclaim . . . that democracy is the form of public life by which a community, dedicated to certain universal ideas . . . cultivates these ideas and develops its institutions under their guidance. The adventure of scientific research, undertaken regardless of the possibilities to which it may lead, is only *one* of the ideas to the service of which our civilization is pledged; and it cannot retain its claim on society by defending its title in isolation from the other ideas similarly endangered by the absolute state.

'Events have discredited a purely defensive liberalism. Peaceful states, reluctant to move unless invaded, have met with ignominious destruction. The planet has been challenged to unite against tyranny or else to accept obliteration under its dominion. We scientists in fact represent one of the principal ideas for which this planetary struggle is being waged. We cannot rid us of such responsibility. Our customary detachment, our established rights for its protection, have been gained through the fierce struggle of centuries, and many martyrs have suffered for them. In the previous period this detachment had to be cultivated against minor incursions and also against the temptation offered to scientists by sensationalism or the abuse of their standing on political lines. That was appropriate to the times; but today it is the detachment of the scientists which blinds them to the danger of science. The cultivation of detachment in the face of an advancing foe is a certain way to enslavement.'

Democracy, a form of life by which a community, dedicated to certain ideas, lives; the adventure, risk and responsibility of science, which cannot maintain its claims in isolation from other ideals and commitments similarly endangered; these were ideas which animated Polanyi's radical, yet liberal, beliefs about science and society. But there is a further, more controversial idea at the end of his letter which, because it was then, and to some extent still is, so diametrically opposed to much conventional thinking about science, forced Polanyi to dig deeply round it, if he was not just to bury and forget it: 'the detachment of scientists . . . blinds them to the danger of science'. If detachment had to be questioned and given a lower place than commitment to certain values, then there was certainly some rough philosophical ground ahead (301, 308).

Polanyi was well aware, at an early stage, how much turned on the problem of objectivity and it was this realization which led him to give *Personal knowledge* the sub-title *Towards a post-critical philosophy*. By this he meant a philosophy of science which would bridge the gap in western thinking between 'the knower' and 'what is known'. This division, crystallized by Descartes in the *Discourse on method*, gave central place to the method of systematic doubt and it is the centrality of scepticism which Polanyi questions. It seemed to him that in the physical sciences many naïve ideas as to what science *is* had already been overthrown by Einstein and his followers. Polanyi, who had been in close touch with many of these, knew that values and commitments, far more positive than any systematic doubt, underlay their achievements. He was not at that time directly influenced by the existentialists though, like them, he attempted to reverse Descartes's *cogito* and to regard a person's whole being, all his experience, as the ground from which rational, articulate thinking grows. Polanyi does not deny the value of clear ideas, of precision, of scepticism and of objectivity; what he claims is that commitment to the discovery of truth by a scientist, working in a community which shares that commitment, is a prior condition for doing science, just as a commitment to justice by a judge, within a judicial community, is a prior condition for doing justice.* And further, Polanyi claims, such essentially value-laden commitments are not always explicit but are rooted inarticulately in the experience of individuals and in the traditions of communities. It is from such rootedness that the effective authority of science itself, or of any living tradition, derives its strength.

> 'Throughout the formative centuries of modern science, the rejection of authority was its battle cry; it was sounded by Bacon, by Descartes and collectively by the founders of the Royal Society. . . . These great men were clearly saying something that was profoundly true and important, but . . . they aimed at adversaries who have since been defeated. The more widely the republic of science extends over the globe . . . the more clearly emerges the need for a strong and effective authority to reign over this republic' (343, 347).

Polanyi makes it clear that it is the undogmatic authority of science itself which needs to be understood and cherished and only if it is, can the encroachment of powerful unscientific ideologies and pressures be checked. One of the central themes of *Personal knowledge* is that shared values are a necessary condition for shared understanding:

> 'Science . . . can no longer hope to survive on an island of positive facts, around which the rest of man's intellectual heritage sinks to the status of subjective emotionalism. It must claim that certain emotions are right; and

* Polanyi discusses, in interesting detail, problems relating to truth claims in *Personal knowledge*, pp. 255–256.

if it can make good such a claim, it will not only save itself but sustain by its example the whole system of cultural life of which it forms a part' (301, p. 134).*

How then was the cultural life of the free world to be sustained and given clearer awareness of the dangers which threatened? During the nineteen-forties Polanyi had been active in an informal group of intellectuals, calling itself The Moot, which met mainly at St Julian's, Horsham. Here he came in touch with Karl Mannheim, Walter Moberley, J. H. Oldham and others. These meetings helped him to formulate his metaphysical and theological ideas. Then in 1952 Polanyi's old friend Arthur Koestler and an acquaintance from his days in pre-war Berlin, Alexander Weissberg-Cybulski, who had both been involved in starting the Congress of Cultural Freedom in West Berlin invited him to help in setting up an international conference of scientists opposed to all forms of totalitarian control. He responded enthusiastically and in 1953 the Hamburg Conference on Science and Freedom was held. Later Polanyi became a member of the Congress of Cultural Freedom's Executive Committee and remained on it until the Congress was reorganized in 1968. Polanyi was not a party political man but all his experience and his thinking during this period was confirming his belief that neutrality, in important and political matters, is illusory and eventually leads to nihilism or servitude. So in the fifties and sixties he gave unstinting support to the Congress. Many of his essays on science and culture during that period appeared in the pages of *Encounter*. In 1955 his son, George, edited the Hamburg essays in *Science and freedom* to which Michael wrote the preface (Josselson, 1977).

In thinking about planning, first in relation to science, then to economics and politics, Polanyi had been developing his ideas about 'spontaneous organization'.† He meant by this the process through which an assembly of individuals (cells, say, or humans) can do more, and indeed that an individual, as part of such a group, can *be* more, than one in isolation. This idea with its implication of formal, hierarchical relationships of units, was not new. Polanyi recognized in the work of H. Driesch, interpreted by Paul Weiss, C. H. Waddington and others, vivid examples of how a morphogenetic field shapes the development of living tissue, with the outcome determined at two levels, by genetic information *and* by context. Here was a vivid model for Polanyi's thinking about all spontaneous organisation, 'a primordial form of originality' he called it, to be manifested far more richly at higher evolutionary and cultural levels.

* It is in regard to clear ideas and methodological doubt that Polanyi and Popper differ most markedly. They are agreed that science only thrives in an open, questioning society, that the knowledge which emerges should be checkable, refutable, communicable. But Polanyi is more interested in the *approaches* to clear knowledge than is Popper. He doubts whether researchers normally *wish* for refutability in their theories; for it is truth they desire and refutation they must risk.

† Polanyi makes it clear, when he first uses this term in *The logic of liberty* (276), that he is referring to the natural order of complex relationships and not to any vitalistic process. By the time he came to write *Personal knowledge* he avoided using this phrase, probably because of its associations with 'spontaneous generation'.

During the decade which followed World War II Polanyi was also becoming increasingly interested in the processes by which skill is gradually acquired. Taking clues from the Gestalt psychologists he came to regard perception, and indeed all knowing, 'as an active comprehension of the things known, an action that requires skill'. So he became particularly interested in those high level skills which are characteristic of the traditions of craftsmanship and connoisseurship and in how such accomplishments are shared and extended. It was from bringing together these two ideas—the hierarchical, organismal nature of thought and its sharedness—that Polanyi developed his concept of *tacit knowledge*—that we always know more than we can communicate explicitly and that the quality of this tacit knowledge varies greatly between individuals and groups and, further, that tacit knowledge, to some extent, guides our thought and practice even when we are groping. Polanyi was criticized for the alleged vagueness of this idea. However, as it was central to all his later thinking, it is appropriate to explain the concept and to touch on some of the problems which it raises.*

5. A PHILOSOPHY OF DISCOVERY

In the first chapter of *Personal knowledge* Polanyi discussed the history of Einstein's special theory of relativity as an example of a major scientific discovery which appears to have been made 'for the wrong reasons'. Despite a slender empirical basis and even despite occasional contrary evidence, it was the 'inherent rational excellence' of the theory (301), 'the grandeur, the boldness and the directness of the thoughts involved' (Born 1924), which made people take the theory seriously and which still sustains the attempt to widen its empirical foundations. Polanyi was by no means the first to notice this reversal in the conventional view of how evidence relates to theory, but he grasped and doggedly followed up the central epistemological questions which this reversal implied: what is the nature of the knowledge that we have when we are in the early stages of making a discovery? And parallel to this, but more profound, is another question: in what ways does the hidden reality we are seeking affect our search for an understanding of it?

Personal knowledge is mainly an exploration of the first question but towards the end of the book and in Polanyi's later writings the second is followed up. He starts his main investigation by considering skills in general and he substantiates the general proposition 'that . . . a skilful performance is achieved by

* Polanyi was not alone in taking an interest in the penumbra which surrounds clear knowledge. He indicates an indebtedness to William James, A. N. Whitehead and others. The phenomenologist M. Merleau-Ponty comes near to Polanyi, but from a very different starting point (see his *Phenomenology of perception*, 1962). There is no evidence that either he or Polanyi read each other's work at an early stage. Professor Marjorie Grene (1977) comments on this: 'The context within which Polanyi has developed his view is so different from the milieu of Merleau-Ponty's thought that the two works may certainly count as two major ways of philosophising to a similar outcome.' In his later writings Polanyi prefers the term tacit *knowing*, as the participle stresses the dynamic *process* of getting, having and sharing knowledge.

the observance of a set of rules which are not known as such to the person following them' with a wide range of examples in perceptual and motor skills. Polanyi then turns to simple tool use and he stresses that not only are some aspects of knowledge below the threshold of consciousness but that awareness of *what* we are doing and the doing of it are, at the moment of action, incompatible. Two kinds of awareness must therefore be recognized.

> 'When we use a hammer to drive in a nail we attend to both nail and hammer, *but in a different way*. We *watch* the effect of our strokes on the nail and try to wield the hammer so as to hit the nail most effectively. When we bring down the hammer we do not feel that its handle has struck our palm but that its head has struck the nail. Yet in a sense we are certainly alert to the feelings in our palm . . . but these are not watched in themselves; we watch something else while keeping intensely aware of them. I have a *subsidiary awareness* of the feelings in the palm of my hand which is merged into my *focal* awareness of my driving in the nail.
>
> 'We may think of the hammer replaced by a probe, used for exploring . . . a hidden cavity. Think how a blind man feels his way by the use of a stick, which involves transposing the shocks transmitted to his hand and to the muscles holding the stick into an awareness of the things touched by the point of the stick. We have here the transition from 'knowing *how*' to 'knowing *what*'. . . .
>
> 'Subsidiary awareness and focal awareness are mutually exclusive. If a pianist shifts his attention from the piece he is playing to the observation of what he is doing with his fingers while playing it, he gets confused and will have to stop' (301, p. 56).

This passage illustrates Polanyi's method: he uses his own and other people's carefully observed experience of perception of skilled action, he analyses it psychologically and then develops a novel concept—the subsidiary/focal distinction—and uses this to loosen a philosophical knot. Such mixing of psychology, scientific and technical knowledge with epistemology did not endear him to contemporary philosophers who were at the time much concerned with conceptual and linguistic purity.

Here too we may notice Polanyi's interest in the probe as representing a heuristic tool and therefore as a possible paradigm for theory in general. This was a model which Niels Bohr and Polanyi both favoured and, because it makes clear the two sides of Polanyi's essentially monistic thought, we shall return to it.

First, however, what range of meaning does Polanyi attach to tacit knowledge? In terms of any skill—physical or mental—there is in the mind of the practical craftsman or of the theoretical thinker a realm of potentially explicit knowledge which he can consciously monitor and from which he can articulate some of the rules which guide his action. Some of this tacit domain is open to introspection, but for any skill or art there is always a large part which is inexplicit and, at a given level of experience, inexplicable. Polanyi uses the term tacit knowledge to cover the whole mass of psycho-motor experience beyond immediate consciousness on which we draw and whose elements we integrate in the performance of a task. 'All knowledge', he writes, 'is *either tacit* or *rooted in tacit knowledge*. A *wholly* explicit knowledge is unthinkable' (343, p. 144). But the threshold is moveable: when we are unselfconsciously engaged in action all relevant powers are integrated and are temporarily immune to reflective thought. When, however, we pause and monitor our action and even more if we discuss it, as we would if we were teaching a skill, then we make a conscious effort to lower the threshold so that *some* of the operational principles which were tacit may become explicit as communicable rules and maxims.

Polanyi stresses that the tacit domain is much more than a reservoir of incoherent material. He sees it, in terms nearer to Piaget than to Freud, as a patterned hierarchy, from the upper levels of which we abstract all articulate discourse. But because such unspecified knowledge is already organized it has a pervasive influence, for it provides an experienced craftsman, artist or scientist with an initial orientation to his task, or with hunches, or with a sense of probabilities, long before he is ready to make his work public. Polanyi acknowledges Whewell, Poincaré and others who had already approached these heuristic and creative problems on similar lines.

> 'Whewell's description of discovery in mathematical physics (he had Kepler's discovery of elliptical orbits in mind) has shown us a typical act of tacit integration at work. Discovery comes in stages, and at the beginning the scientist has but a vague intimation of its prospects. Yet these anticipations which alert his solitary mind . . . contain a deepened sense of the nature of things and an awareness of the facts that might serve as clues to the suspected coherence of nature' (301, p. 143) (the reference is to W. Whewell, *Philosophy of discovery* p. 254).

Here we see the main elements of Polanyi's picture of the process of scientific discovery: vague but powerful intimations grounded in tacit knowing; empirical clues to be integrated in a theoretical model; which model *may* match the suspected coherence in nature; but which may not, for in every part of the process there is the risk of error.

Both Polanyi and Bohr used the analogy of a blind man and his stick when discussing the way in which a scientific theory can be used to probe what is ambiguous or as yet unknown. 'When the stick is held loosely,' writes Bohr (1934), 'it appears to the sense of touch as an object. When, however, it is held firmly, we lose the sensation that it is a foreign body and the impression of touch becomes immediately localized at the point where the stick is touching the body under investigation.' Here we have Bohr using Polanyi's concept of subsidiary and focal awareness. Both have appreciated that there is an element of complementarity in the knowledge a person derives when using a probe or, for that matter, any measuring instrument or conceptual model; each came to this complementarity concept along different paths. Over both was the shadow of Einstein who had shown the need to relativize our view of both theory and of measuring instruments. This convergence of thought as well as the significant differences between Bohr's and Polanyi's view of the nature of such heuristic probing is fully discussed in a recent paper by Professor Torrance. He makes it clear that Polanyi's 'personal knowledge' was very far from mere subjectivism.

> 'Since the intrinsic structure of reality is consistent and universal and is independent of our knowing it, scientific commitment to the investigation of the world through submission to its compelling claims on our mind carries with it what Polanyi calls "universal intent" (301, p. 65). But because the statements formulated under the authority of reality fall under its judgement and are relativised by it they have no absolute or final status in themselves and must not be allowed to usurp the legislative authority of reality for themselves. That is why all dogmatism is excluded'. (Torrance 1974).

What then is the nature of the arbitration process which may force scientists to scrap one theoretical probe and to look for another ? Polanyi argues that the crucial judgement is not given by the established authority of science, though this may be important, or of the state, though its pressures may be hard to escape, nor is it the neatness of the fit of the new hypothesis into the coherent matrix of world-wide 'objective knowledge', nor again can it be seen as a *post hoc* verdict after a revolution in basic concepts (Thomas Kuhn's paradigm shifts) though such do happen. Polanyi goes beyond these and stresses that it is reality itself which judges the probing act or, rather, it is that newly emerging aspect of reality on which 'the probe' has touched. As with mechanical constructions, however, because the probe is made from materials or concepts belonging to a simpler, lower level, its meaning is given, its validity is established, in terms of a higher level.* So Polanyi's epistemology and indeed his wider philosophy as it embraces society and culture was strongly opposed to any reductionist world view which would represent mind essentially in terms of a machine or chemistry essentially in terms of physics.

6. The Oxford years

In 1958 Polanyi moved to Merton College, Oxford, as Senior Research Fellow. During the 15 years that followed he travelled extensively and each year published papers on a wide range of subjects—scientific, political and aesthetic. In 1962 he gave the Terry Lectures at Yale and these were published as *The tacit dimension* (332), the most succinct statement of his theory of knowledge. *Knowing and being* (347) is a further, more varied collection, containing papers

* Polanyi draws support for these ideas from Gödel's theorem limiting the formalization of closed logical systems. The theorem, he writes, 'offers a model of conceptual innovation in the deductive sciences which illustrates in principle the inexhaustibility of mathematical heuristics and also the personal and irreversible character of the acts of discovery which continue to draw upon these possibilities. . . . We never know altogether what our axioms mean. . . . But this uncertainty can be eliminated for a particular deductive system by shifting it into a wider system of axioms within which we may be able to prove the consistency of the original system' (301, p. 259).

on the Hungarian Revolution and a retrospect on the wayward development of his own theory of adsorption. At the end he presents two strongly anti-reductionist papers and in one, 'Life's irreducible structure', he develops, from his knowledge of physics, general ideas about boundary conditions.

Polanyi argues that it is only when we recognize the boundary conditions which circumscribe and define a mechanism or a process that we can properly understand it. 'The machine as a whole', he writes, 'works under the control of two distinct principles. The higher one is the principle of the machine's design, and this harnesses the lower one, which consists in the physical and chemical processes on which the machine relies.' Polanyi develops this analysis of levels to consider the machine-like and code-like systems operating in living organisms and discusses similar hierarchical structures in art and language: 'a vocabulary sets boundary conditions on the utterances of the spoken voice; a grammar harnesses words to form sentences' (347). He goes on, here and elsewhere, to show that *meaning* can be understood as the process of integrating such disparate elements in an act of speech or of perception: 'the subsidiary clues are not of intrinsic interest in the transaction. It is the object of focal attention that possesses intrinsic interest. It is what is at the end of the stick that engages the blind man's attention, not the feelings in the palm of his hand. It is the meaning of a communication in words, that engages our . . . interest, not the words as such' (356). Polanyi carries this integrative approach further and, following F. S. Rothschild, regards the mind as the meaning of the body. 'Meaning as making'—though Polanyi was never fully aware of the fact he had an ally here in the Wittgenstein of the *Philosophical investigations*.* As a philosopher Polanyi was undoubtedly isolated and by pressing his enquiries as energetically as he did he unwittingly exposed himself to murmurs of criticism and suspicion.

Though Polanyi was pleased by the reception which many of his ideas received in the United States, in Oxford there was sometimes a disappointed note in his conversation. If one visited him at home or walked with him round Merton he would always listen, comment or criticize with great kindness and he would speak of his own arguments and intellectual struggles with tranquil humour; nevertheless there was a sense of frustration too. Why was it that people were not taking more notice of his ideas? And, especially in Oxford, why were the traditional pillars of positivism and of linguistic philosophy not showing more signs of bending or yielding when he and his continental allies—Merleau-Ponty for example—had done them the honour of exposing some of their weaknesses?

The question itself is complex; the answer more so. Part of it lies in Polanyi himself. Having been a highly successful scientist he had turned, not merely to philosophy, but to mixing this with science and aesthetics and even with occasional prophetic homilies. Some of the most powerful passages in *Personal knowledge* have the ring of St Augustine or of Cardinal Newman. Successful scientists may have to wait ten or twenty years for their accolade; prophets

* C. B. Daly discusses both this similarity of view and Polanyi's failure to understand fully what the later Wittgenstein was saying. See 'Polanyi and Wittgenstein' in Longford T. A. & Poteat W. H. (1968), pp. 136–168.

generally have to wait a good deal longer. Further, because Polanyi was such an exceptionally gentle man he felt very acutely the arrows of criticism or misunderstanding when they came and, though he was always ready for controversy, this provided him with a few of the fierce joys which reward more belligerent knights.

The manner in which Polanyi formulated his ideas was not, in any case, impregnable. He did not, for example, define the concept of tacit knowing with sufficient precision for some critics. His ready movement in discussion from a consideration of perceptual to cognitive processes and from these to broad philosophical and ethical generalizations was beset with dangers of which he was inadequately aware. In addition to philosophical opposition there was a groundswell of criticism from scientists—those such as Jacques Monod whom Polanyi criticized and who in turn suspected in him an element of vitalism and mystification, of smuggling in God and teleology by the back door. Polanyi is in fact extremely careful about such issues and the strength of *Personal knowledge* lies partly in the fact that he draws on a vast field of evidence and example and creates an open but non-dualistic world view; but it was also very decidedly non-mechanistic and this is what attracted some scientific opposition.

It must also be admitted that Oxford was the intellectual home of many of the philosophical positions which Polanyi had been criticizing. In the nineteen-sixties the student population had begun to ask for more substantial philosophical food than the logic of A. J. Ayer or the conceptual clarity of Gilbert Ryle could offer; but the response to this need was slow to develop. When Polanyi arrived on the scene he had already articulated a radically new conceptual scheme in which many familiar philosophical problems would lose their hold and new ones would appear. Here was this quiet polymath, 'unqualified' in philosophy, asserting that the dichotomies of fact and value, thought and action must be closed and showing how this could be done with a bewildering range of metaphor, scientific example and rhetorical flourish. He questioned the centrality of scepticism and of objectivity in science and reinstated commitment and even, therefore, certain kinds of faith as prior conditions for effective action and understanding. Through it all came a calm confidence which must have disconcerted critics who shared neither his breadth nor his conviction.

Polanyi was clear about the personal dimension in his own thinking. In *Personal knowledge* he almost invites attack when he admits that 'the principal purpose of this book is to achieve a frame of mind in which I may firmly hold what I believe to be true, even though I know it might conceivably be false' (301, p. 214). He knew that a theory, especially a novel one, is more than something to debate, to check and to share with others; it is what we explore *with*—an extension of ourselves and therefore neither totally impersonal nor infallible. In every exploration there is risk, of error or folly, of misunderstanding or neglect and Polanyi took the risk, for he also knew that 'the freedom of the subjective person to do as he pleases is overruled by the freedom of the responsible person to do as he must' (310, p. 309). Even so he did sometimes feel disappointed in Oxford.

Michael Polanyi's last years were clouded with some loss of memory but he retained his great sweetness of disposition to the end, valued and cherished by those who loved him. Raymond Aron (1968) characterizes Polanyi as 'the man of reconciliation'. For him a contradiction or a conflict is there to be probed, not patched. 'The science of Polanyi', Aron writes, 'leads without a break to faith.' 'Without a break' is the essence of his reconciling thought, for though its pattern is elaborate, woven with many colours, contrasts and even, occasionally, knots, it is seamless. No privileged domains or other-worldly dimensions are invoked in Polanyi's world view. Yet he gives a place for value in, and implies unbounded hope for, all that lives.

7. Honorary degrees, special lectures and fellowships

Hon. D.Sc: Princeton 1946; Leeds 1947; Manchester 1966; Cambridge 1969. Hon. LL.D.: Aberdeen 1959; Notre Dame 1915; Wesleyan 1965; Toronto 1967; elected Life Member of the Kaiser Wilhelm Gesellschaft, Berlin, which after World War II was renamed Max-Planck-Gesellschaft 1929. Foreign Member of the Society Science, Letters and Arts, Naples, 1933. Ridell Lecturer, University of London, 1945. Lloyd Roberts Lecturer, University of Manchester, 1946. Made Foreign Life Member of the Max-Planck-Gesellschaft, 1949. Alexander White Visiting Professor at the University of Chicago, 1950. Gifford Lecturer, University of Aberdeen, 1951–52. Visiting Professor, University of Chicago, 1954. Lindsay Lecturer, First Lindsay Memorial Lecture, Keele University, 1958. Appointed Senior Research Fellow, Merton College, Oxford, 1959. Eddington Lecturer, Cambridge University, 1960. Gunning Lecturer, University of Edinburgh, 1960. J. C. Bose Lecturer, Calcutta, 1960. Distinguished Research Fellow, University of Virginia, 1961. McEnnerny Lecturer, Berkeley, California, 1961. Foreign Honorary Member of the American Academy of Arts and Science, 1962. Terry Lecturer, Yale, 1962. Member of the International Academy of Philosophy of Science, 1962. Fellow of the Center for Advanced Studies on Behavioral Science, Stanford University, 1962–63. James Duke Visiting Professor at Duke University, North Carolina, 1964. Senior Fellow at the Center for Advanced Studies, Wesleyan University, 1965. Visiting Professor, University of Chicago, 1967. Nuffield Gold Medal, Royal Society of Medicine, 1970. Visiting Professor, Austin University, Texas, 1971.

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Harré, Sir Alan Hodgkin, O.M., K.B.E., F.R.S., Mr M. Josselson, Mr Arthur Koestler, Mr J. Lucas, Sir William Mansfield Cooper, Professor H. Mark, Professor H. O'Neil, Dr G. Price, Professor H. Prosch, Professor E. Shils, Lord Todd, P.R.S., The Rev. Professor T. Torrance, M.B.E., D.D.

The photograph is by W. Bird.

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II. A selected list of Michael Polanyi's social and philosophical writings

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