

Polányi Mihálytól tanultak...¹



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Hölgyeim és Uraim!

Egy tudós hagyatéka elsősorban tudományos felfedezései és tanítványai. Polányi Mihály hagyatéka mindkét szempontból gazdag. A jelen bevezető a kémikus Polányi Mihályra emlékező kétnapos konferenciát nyitja meg. Ebben a bevezetőben Polányi Mihályra, a tanító mesterre emlékezem, elsősorban Wigner Jenőt (1902-1995) és Melvin Calvint (1911-1997) megidézve. Itt jegyzem meg, hogy a 20. századi fizika történetének nagy krónikása, Abraham Pais szerint,² Polányi "decisively marked Wigner's thinking, not just about physics, but also about philosophy and politics." Megemlítem néhány további jelentős kémikus és mások Polányival kapcsolatos gondolatát is. Azon kívül kitérek a kémikus Polányinak azokra a

¹ A BME Filozófia és Tudománytörténet Tanszék, a Polányi Mihály Szabadelvű Filozófiai Társaság és az MTA TKI Tudománytörténet Tudományelmélet Kutatócsoportja által szervezett „A kémikus Polányi Mihály” című konferenciája (Budapest, 2003. Szeptember 26-27) megnyitójára készített írás.

² A. Pais, *The Genius of Science: A Portrait Gallery*. Oxford University Press, 2000, p. 334.

kutatásaira, ha csak érintőlegesen is, amelyek manapság a DNS kettős csavar szerkezete felfedezésének félévszázados jubileumán különösen időszerűek.

Polányi Mihálytól a kémikusok több generációja is tanult. Valószínűleg született pedagógus volt, aki azt is jó érzékkel felismerte, hogy mire van a környezetében előforduló fiatal kutatónak szüksége. Kürti Miklós (Nicholas Kurti, 1908-1998) 1928-ban a párizsi Sorbonne-on befejezett tanulmányai után Berlinbe érkezett, hogy doktorátust szerezzen. Polányi azt tanácsolta neki, hogy ne kezdjen rögtön doktori munkához, hanem szerezzen előbb általános tájékozottságot. Kurti életrajzírója szerint³ ez lett pályájának meghatározó időszaka. Kurti a Berlieni Egyetemen Max Planckot, Erwin Schrödingert, Max von Laue-t és Wigner Jenőt hallgatta és látogatta a híres szemináriumokat is, amelyeken az előbbieken kívül Albert Einstein, Walther Nernst és más nagyságok és későbbi nagyságok is részt vettek.

Különösen szerencsések voltak azok, akik nemcsak futólag találkoztak Polányival, hanem mellette dolgozhattak. Jómagam összesen egy ebédnyi időt tölthettem vele 1969-ben,⁴ amikor a Texasi Egyetem Fizikai Intézetében voltam vendégkutató és Polányi a Fizikai Intézet igazgatójának vendége volt. A találkozás nagy hatással volt rám, és csak elképzelni tudom, hogy mit jelenthetett mások számára vele dolgozni hónapokig, esetleg évekig. Erre a kérdésre próbálok legalább részlegesen választ adni az alábbi összeállításban.

WIGNER JENŐT IDÉZVE

Wigner Jenőtől Polányi tanítómesteri kvalitásaira vonatkozóan visszaemlékezéseinek eredeti angol nyelvű szövegéből idézek⁵:

...there at the Kaiser Wilhelm Institute worked a man who decisively marked my life: Dr. Michael Polanyi. Few people in this century have done such fine work in as many fields as Polanyi. After László Rátz of the Lutheran gimnazium, Polanyi was my dearest teacher. And he taught me even more than Rátz could, because my mind was far more mature. After Rátz and my parents, Polanyi was my greatest influence as a young man.

³ J.H. Sanders, "Nicholas Kurti, C.B.E. 14 May 1908 - 24 November 1998." *Biographical Memoirs of Fellows of the Royal Society*, 2000, Vol. 46, pp. 300-315.

⁴ Hargittai I., Hargittai M., *Szimmetriák a felfedezésben*, Vince Kiadó, Budapest, 2003, p. 29.

⁵ E. P. Wigner, *The Recollections of Eugene P. Wigner as told to Andrew Szanton*. Plenum Press, New York and London, 1992, pp. 76-79.

The Germans have a tremendous word for fiber chemistry: "Faserstoffchemie." Michael Polanyi had his own laboratory in the Kaiser Wilhelm Institute for Faserstoffchemie. The Mauthner Brothers tannery in Budapest employed a fine chemical engineer named Paul Beer, who somehow knew Polanyi and gave me a strong letter of introduction to him.

So Dr. Polanyi asked me over to his home one evening. A chemist named Herman Mark also came that night. Mark was an energetic, chatty man from Vienna. He was only seven years my senior, but seemed much older.

Mark had fought in the Austrian ski troops during the First World War on both the Russian and Italian fronts and had escaped from an Italian prison camp disguised as an Englishman. He had quickly completed his education at the University of Vienna and taught at the University of Berlin before joining the Kaiser Wilhelm Institute as a research associate.

Polanyi and Mark had a fabulous discussion that evening, just two physical chemists discussing one topic after another. Mark smoked a few cigarettes. I sat by without opening my mouth, amazed at how much physical chemistry they knew. Topics at the farthest edge of my comprehension they discussed with the greatest fluency and ease. They spoke with graceful insightful wit, following each other perfectly. When Herman Mark finally rose to leave, my involuntary reaction betrayed my great disappointment. Mark put on a little half-smile, sat down again, and revived the conversation. My embarrassment at having kept Mark in the room soon faded in the face of their startling conversation. Listening with all of my limited intelligence, I knew that I was deeply happy.

That was my introduction to Dr. Mark and Dr. Polanyi. Soon I knew Polanyi closely. He told me to call him "Misi" (pronounced "Mee-she"), placed me in his laboratory, and asked me to contribute to meetings and colloquia.

About three other students worked for Polanyi. I studied theory: crystal symmetries and the theory of the rates of chemical reaction. I spent just a few hours in the lab and many more hours calculating figures in my room. I also learned a great deal about the life of Michael Polanyi.

Polanyi and I wrote a joint article in 1925, introducing assumptions that seemed drastic then; they later proved quite correct. We wrote another joint paper in 1928. What a pleasure it was to assist a man of such keen mind and deep insight.

Polanyi took an interest in all of his assistants, but I felt that he liked me especially. He freely advised me on various personal matters. In time his generous wife did too. Polanyi even loaned me a bit of money when I needed it.

But his finest gift was to encourage my work in physics, and this he did with all of his very great heart. In all my life, I have never known anyone who used encouragement as skillfully as Polanyi. He was truly an artist of praise. And this praise was vital to me because it was often missing at the great afternoon physics colloquia.

Because Polanyi was a decade my senior and held a far higher position, it was not quite proper for him to befriend me as he did. But Polanyi cared nothing for formal questions or age and status. That was part of his great sweetness. Polanyi was concerned instead that

young men should love science and labor to understand it. He was concerned that he could never fully share his love and the knowledge he had gathered.

Like me, Polanyi enjoyed asking questions outside the realm of basic science: Why is the world divided into separate nations? Why do all nations have governments? How should a man live his life in a world filled with evil? Polanyi even taught me some poetry. He made learning a great pleasure.

Dr. Polanyi and I did not always see eye to eye. Polanyi found quantum theory too mathematical for his liking. I was the only one in his lab deeply interested in it.

Once I made an observation to Polanyi about the impossibility of an association reaction. He heard my idea without grasping it. I felt sure that I was right and even that my idea had merit. But I was too modest to press it home.

Months later, Polanyi told me one day, "I am quite sorry. This point which you have always made on association reactions: I have just heard it in a paper of Born and Franck. I told them that you had the same idea, but they have already sent in the article, and nothing can be done." Polanyi paused a moment. "I am quite sorry," he said again, "I don't know why I failed to understand you."

Well, I think I know. Even a man as open-hearted as Polanyi does not easily accept the brash ideas of a modest and untried assistant. What I had told him was radically new, and however open-minded people may seem, very few are prepared to embrace radical ideas.

Wigner Polányi mellett készítette el doktori értekezését,⁶

Polanyi advised my doctoral dissertation at the hochschule. I chose a topic far from the crystallography of Weissenberg or Herman Mark: chemical reaction rates. I wondered: How do colliding atoms form molecules? We knew that hydrogen and oxygen make water in a container, but how soon? How much depends on pressure and how much on temperature? I pursued such questions with elements far more complex than hydrogen and oxygen.

Polanyi was a wonderful advisor. He understood chemical reaction rates both in theory and practice. He accepted my proposal that angular momentum is quantized and that the atoms collide in a proportion consistent with Planck's constant. This idea is now widely known, but then it was rather brash. And studying chemical reaction rates taught me much about nuclear reaction rates that would be useful in future years.

My thesis paper for the engineering doctorate was submitted, with Polanyi's name attached, in June 1925. We called it "Bildung und Zerfall von Molekülen" ("Formation and Decay of Molecules").

Wigner összehasonlítja Markot és Polányit és különösen érdekes ahogy Wigner leírja azt, amit Polányitól a tudományos módszerről tanult⁷:

So Herman Mark was a strong teacher, but Michael Polanyi was really the miraculous one. Polanyi loved to ask the fundamental question: "Where does science begin?" He

⁶ *ibid.*, pp. 76-79.

⁷ *ibid.*, p. 81.

listened to the thoughts of others on this question, but he also had his own well-crafted answer: "When a body of phenomena shows coherence and regularity."

Polanyi loved and honored the scientific method with great truth and devotion. He managed to keep all of science within his fond gaze and a great deal more besides. What a mentor Michael Polanyi was.

Amikor azután Wigner számára is eljött az az idő, hogy kiválassza saját kutatási területét a fizikában, úgy tűnt neki, hogy a kvantummechanika nélküle is megfelelően fejlődött. A maga részéről nem egyszerűen csak az egyik résztvevő akart lenni. Célja az volt, hogy valami eredetit alkosson. Így ír erről,⁸ "I realized how much I loved group theory. I had worked with Michael Polanyi on the symmetries of crystal, and these symmetries had suggested group theory." Wigner tehát döntött. Amikor Wigner is, majd Polányi is elhagyta Németországot, kapcsolatuk nem szakadt meg és Wigner melegen emlékszik vissza arra az időre, amikor "spent six pleasant months in Manchester, England at the invitation of Michael Polanyi."⁹

Polányi ösztönzése és útmutatása annyira fontos volt Wigner számára, hogy 1963 decemberében a Nobel-díj átvételét követő fogadáson, a hagyományosan kétperces beszédben Wigner, többek között, a következőket mondta¹⁰:

I do wish to mention the inspiration received from Polanyi. He taught me, among other things, that science begins when a body of phenomena is available which shows some coherence and regularities, that science consists in assimilating these regularities and in creating concepts which permit expressing these regularities in a natural way. He also taught me that it is this method of science rather than the concepts themselves (such as energy) which should be applied to other fields of learning.

MELVIN CALVINT IDÉZVE

Wigner Polányi doktoránsa volt Berlinben, míg Melvin Calvin, posztdokorként dolgozott mellette két évig Manchesterben. Calvin a kémiai Nobel-díjat 1961-ben kapta „a növények széndioxid asszimilációjának kutatásáért.” Nobel előadásában így utalt a Polányi mellett töltött idő meghatározó szerepére: "Our own interest in the basic process of solar

⁸ ibid., p. 145.

⁹ ibid., p. 158.

¹⁰ E. P. Wigner, *Symmetries and Reflections: Scientific Essays*. Indiana University Press, Bloomington, Indiana, 1963.

energy conversion by green plants... began some time in the years between 1935 and 1937, during my postdoctoral studies with Professor Michael Polanyi at Manchester.”

Calvin részletesen ír az említett tanulmányokról önéletrajzi kötetében,¹¹ ennek szentelve a “Postdoctoral Education, 1935-1937” című alfejezetet, amelyben Polányi fényképe is megjelenik:

In the course of writing my thesis, I became familiar with the work of Professor Michael Polanyi of the University of Manchester in England. He originally was in Berlin, but at the time I began to know his reputation he had moved to England. Polanyi had begun the study of the nature of chemical reactions and developed the whole theory of transition states in chemical reactions at Manchester. Therefore, with Polanyi in the chemistry department, Manchester seemed like a very interesting place to work. The research seemed to be the type with which I would be pleased to be further associated.

I wrote to Polanyi in the hope that he might be able to help me obtain postdoctoral support to work in his laboratory. He was able to do this, fortunately, through the auspices of a grant that he had from the Rockefeller Foundation. Thus, at the end of my thesis work in Minneapolis I undertook to spend a year at the University of Manchester, England, working with Professor Polanyi. It turned out that I was a successor to Richard Ogg, another American, who became a professor of chemistry at Stanford University after he left England. This postdoctoral period began in the fall of 1935, and it was a most fortunate thing for my whole interest in chemistry that followed.

When I first met Michael Polanyi in Manchester in 1935, he was well into his second career. He had come to Great Britain in flight from Hitler in the early 1930s and had begun some particularly sophisticated studies. He was to conclude these studies within a relatively short time to begin another career, that of a political scientist-economist. There is no doubt in my mind that the experience I had with Polanyi was instrumental in opening my eyes to the advantages of an interdisciplinary approach to science, as he had the type of mind that was curious about all things, even those not directly involved with his current work. I kept in close touch with Polanyi over the years until his death in 1976 at the age of 85. It is particularly fitting that his son John won the Nobel Prize in chemistry (with Yuan Lee of Berkeley) in 1986 for his elegant work in atom-molecule reaction mechanisms, thus continuing the unique family career in chemistry that had been started by his father.

I feel that my experience with Professor Polanyi had a profound effect on my subsequent career, as did the immediately following experience with Gilbert N. Lewis in Berkeley. During the 2 years in Manchester I became aware of the freedom of thought that allowed me to undertake work in any area of science that seemed appropriate to the questions I was faced with. Thus, I was not limited to physical chemistry, organic chemistry, biochemistry, or biology, but encompassed them all to some degree.

Professor Polanyi had been interested in reactions of atoms because this kind of reaction was most easily susceptible to theoretical treatment. Upon my arrival I began an outgrowth of that work. First of all, I should point out that Polanyi had already done his work on sodium flames that led him to measure the rates of reaction of sodium atoms with

¹¹ M. Calvin, *Following the Trail of Light: A Scientific Odyssey*, American Chemical Society, Washington, DC, 1992, p. 80.

alkyl halides and to formulate these reactions in terms of a three-center reaction. The sodium atom would approach the alkyl halide from either of two directions: from the halogen direction, pulling it off and leaving behind the carbonium ion, or from the other direction, in which the sodium atom would displace the halogen atom from the alkyl carbon. This latter turned out not to be the way in which the reaction went.

Thus began the whole idea of transition-state theory and the description of simple reactions in terms of a single reaction coordinate. In the case of the sodium atom and the alkyl halides, the coordinate was the line joining the carbon atom, the halogen atom, and the sodium atom. One could draw a potential curve showing the way in which the energy changes as a function of the sodium-halogen-carbon distances in various ways. It was this type of an idea that Polanyi wanted to apply to reactions of hydrogen atoms. The easiest way to get the hydrogen atoms at that time was to allow hydrogen to come in contact with platinum metal, in which case the hydrogen molecule was supposed to dissociate into hydrogen atoms on the surface of the platinum, and thus a platinum-hydrogen bond would exist for which the reactions could be calculated.

It seemed clear enough that the platinum-hydrogen bond, whatever it was, was not a simple nonpolar bond but had some polarity. Presumably, the platinum was positive and the hydrogen negative (or it could have been the other way around). I was set the task of determining some of these properties by measuring the effect of the potential placed on the platinum electrode on the rates of some of the hydrogen atom reactions that would take place at that platinum electrode. Some of these reactions were the H_2-D_2 reaction and the para-hydrogen conversion, which was not dissociation but rather a magnetic effect. Another was the D_2-H_2O exchange reaction. In any case, my work with Polanyi began by studying the effects of potential on a hydrogen reaction that took place at the platinum electrode.²

Polanyi had invented the method for measuring water density with an exquisite sensitivity. He had devised a micro method that was implemented by an extremely talented technician named Ralph Gilson. They made tiny glass floats (divers) that could contain in their shanks a few microliters of water and that had attached to the shanks a hollow glass bulb with a flat surface. The floating capacity of that bulb was determined by the pressure above it, which changed the volume of the bulb and thus its floating capacity. These bulbs were calibrated for different densities of water contained in the tiny chamber at the bottom of the float. We were able to measure the density of water on a few microliters to something like four or five decimal places, which was enough to determine the deuterium content to the accuracy that was required. The actual transfer of the water samples into the diver chambers was done by an especially trained young boy, who then transferred the divers to the water vessel under controlled pressure and measured the pressures at which it floated, neither rising nor sinking.

Most of our measurements at first depended on the change of deuterium content of the gas phase. Therefore another method, one not requiring combustion of the hydrogen to water, seemed appropriate. About that time Whytlaw-Gray in Leeds had devised a gas density balance that he was using to determine atomic weights (I think of the rare gases) very precisely. This seemed to Polanyi an appropriate method to be applied to the measurement of the hydrogen gas that we were using for the deuterium exchange reactions. The balance consisted of two glass bulbs mounted on the ends of a beam that, in turn, was supported on a bridge of quartz fibers. Here, again, the relative weight of each side was determined by the pressure and density of the gas in which we were interested, and proper calibration would allow us to determine the density of the gas to four or five

decimal places. Because we were using only hydrogen and deuterium, we were able to determine the total deuterium content of any gas mixture. Neither of these methods, however, could be used to determine the amount of HD in a mixture of gases of hydrogen and deuterium. Another method had to be devised.

Polanyi did this by recognizing that the thermal conductivity of a gas mixture would depend not only on the number of hydrogen atoms and the number of deuterium atoms, but on how they were combined. In other words, thermal conductivity would depend upon how much HD was present in the mixture of H_2 and D_2 equilibrated to HD, which presumably required the activation of the hydrogen down to the hydrogen atoms. The HD measurement was accomplished by measuring the thermal conductivity of the gas mixture after it was calibrated in terms of the amount of hydrogen, deuterium, and HD in the mixture. This was sufficiently accurate for the task we had in hand. This method was calibrated with platinum as the catalyst.

I can very vividly remember rambling conversations that took place in Polanyi's office in Manchester toward the end of my first year, when I had been working exclusively on platinum–hydrogen activation systems. He pointed out that most biological oxidation–reductions were mediated via a porphyrin molecule such as heme, which was present in most of the biological oxidation–reduction catalysts, as well as chlorophyll, which is the photocatalyst in plants. If biological oxidation was a dehydrogenation and reduction – that is, a hydrogenation reaction that took place on porphyrins – then it should be possible to do a study on porphyrin molecules similar to the one that we had been doing on platinum, by using molecular hydrogen as one of the reagents. Of course, the natural biological oxidation–reductions, having porphyrins as prosthetic groups, took place on proteins. Polanyi speculated that the proteins were simply wires for moving electrons around to match the protons that had ultimately to be moved, and that the reactions took place on the porphyrin molecules themselves. This was, at least to my mind, a remarkable feat of association. Thus, it became important for me to find porphyrin molecules that might be useful in studying hydrogen activation in the same way we had been studying it on platinum.

We wanted to study the H_2 – D_2 reaction that would produce HD in the gas phase on the surfaces of stable porphyrin-type molecules. This required a different method of measurement than the one that had been used for the exchange reactions $D_2(HD)H_2$. The exchange reaction, after all, could be measured by measuring the total amount of deuterium in the gas sample. This quantity could be determined by burning the gas (if it was hydrogen) or the organic compound (if it wasn't hydrogen) and measuring the density of the water so produced.

Our next task was to find a porphyrin redox catalyst model stable enough to be used in a gas-phase experiment at a variety of temperatures, including elevated temperatures. The ordinary porphyrins, even when most of the side groups were removed – that is, the naturally occurring porphyrins that could be obtained from hemoglobin or the catalase or any of the other porphyrin-containing oxido-reductases – were not stable enough for this purpose. About that time, Polanyi heard of the synthesis of a porphyrin analog in London by R. P. Linstead, one of the leading organic chemists of Great Britain. Linstead's discovery of the phthalocyanines, which really are tetraazaporphyrins, occurred as a result of an accident in the ICI factory that was making phthalonitrile. Apparently one of the glass-lined kettles, which were used to make phthalonitrile from phthalamide, cracked, and the whole reaction mixture turned a deep blue. Linstead, who was a consultant for ICI, was called in to help explain the chemical nature of the blue coloration. He was able to

determine that the structure of the blue pigment so formed was the tetraazaporphyrin (now called phthalocyanine) and that its formation was catalyzed by the presence of the iron on the phthalonitrile.

The phthalocyanine turned out to be an extremely stable compound, and I was sent down by Polanyi to Linstead's laboratory at Imperial College in London to learn how to make and purify phthalocyanine. I spent a relatively easy few days in the laboratory because the synthesis of phthalocyanine was a simple procedure. All that was required was the heating of the phthalonitrile in the presence of a good tetracoordinating metal, particularly copper. However, once the copper enters the center of the phthalocyanine ring (or, in fact, the phthalocyanine is created around the copper ion), it is impossible to remove the copper. Even dissolving the copper phthalocyanine in concentrated sulfuric acid and then pouring that onto ice gives back the copper phthalocyanine. So we had to use other metals. The metal of choice turned out to be zinc, which was a very good template for the formation of phthalocyanine and could be removed by simple acid treatment to give the free tetraazaporphyrin, into which any metal could then be introduced.

Having succeeded in preparing both metal-free and metal-containing phthalocyanines, we then undertook a study of the activation of molecular hydrogen in the gas phase by crystals of these materials. At first we thought we had indeed seen such an activation, but this later turned out to be a mistake, and the observation could never be repeated in a routine way. However, this kind of work was continued after I came to Berkeley as an instructor in 1937.

Before leaving this period, I would like to say a few words about the other students with whom I shared Polanyi's interest. The two principal ones were a pair of graduate students working on hydrogen activation problems in the laboratory adjacent to mine. One of them, E. C. Cockbain, worked for many years at the Malaysian Rubber Research Institute in London. The other, Dan Eley, became a professor of physical chemistry at the University of Nottingham. In fact, I recently (October 1986) saw him again when I gave the Holden Botany Lecture at Nottingham, as I did in 1958 when I received an honorary degree there. The group of students associated with Polanyi was a small one during the time I was in Manchester, and we each had our discussions with him separately. However, there was much exchange between the people in the basement laboratories at Manchester where we worked.

Korábban, 1984. július 16-án Clarence Larson videofelvételt készített Calvinnal folytatott beszélgetéséről Calvin szobájában Berkeley-ben, a Kaliforniai Egyetemen. Ebben a beszélgetésben Calvin részletesen szól a Polányival töltött időről, amit itt a felvétel alapján általunk készített leírásból idézünk.¹² A fenti és az alábbi leírásban sok az átfedés, ami természetes. Viszont érdekes is lehet az egybevetés. A fentebbi leírás egy nagy gonddal készített könyvben jelent meg. Az alábbi viszont 6-7 évvel korábban hangzott el egy beszélgetés során.

¹² Clarence Larson (1909-1999), kémikus PhD és egyetemi tanár, a Manhattan Projekt résztvevője, majd az Egyesült Államok atomenergia-biztosa Nixon és Ford elnöksége alatt, az 1980-as években híres tudósokkal vett fel beszélgetéseket. Halála után özvegye, Jane Larson az egyedülálló gyűjtemény eredetijét Hargittai Istvánnak és Magdolnának ajándékozta.

I got my Ph.D. in 1935 and it was a straight physical chemistry thesis. I studied mostly electron interactions with gaseous atoms. My thesis was to measure the energy of interaction of single halogen atoms with electrons. I did that in a vacuum tube all of which we had to build. It was a time when building things was common for a graduate student. I learned the relationships between the current/voltage curves in the vacuum tube and the charge to mass ratio of the carriers. It was from that current/voltage curve that I measured charge to mass ratios and from these ratios the number of halogen atoms that was in the vacuum tube. I could calculate the equilibrium constant between the atom, the electron, and the anion. That's how we determined the electron affinity of the halogens.

By that time, during the course of my graduate work I had to study the quantum mechanical theories of reaction mechanisms. The best statement about the quantum mechanical theory of reaction mechanisms that was extant at the time and was just being developed was that of Michael Polanyi.

Michael Polanyi had been studying reactions of sodium atoms with alkyl halides in a dilute gas. He also had undertaken a study of the reaction of the hydrogen atom with the hydrogen molecule. The way he made that measurement was to use H atoms and D₂ molecules and measured the formation of HD. He was measuring the simplest kinds of reactions, which were susceptible to first principles quantum mechanical calculations, and he succeeded in doing that and in developing what we now know as a transition state theory of reaction kinetics. His more famous pupil was Henry Eyring who preceded me in that work. By the time I got to Polanyi he had moved to Manchester and by that time the theory of transition state had been sorted out.

Polanyi asked me to study the mechanism of activation of molecular hydrogen on platinum, start with on polarized platinum. He had the idea that you could study the reaction of hydrogen atoms attached to polarized platinum with hydrogen molecules, which were not attached to platinum. That way you'd be able to affect the activation energy of the atom/molecule reaction, and that's what he put me on. I began to study the effects of polarization on platinum electrodes carrying hydrogen atoms on the rate of exchange between the hydrogen atom and the D₂ or HD molecule. This led to a more general question, which Polanyi now posed.

Before that though you should understand who Polanyi was. He was a refugee both from Hungary and Germany. He was a surgeon in World War I for the Hungarian Army. After the war was over he realized that his interests were in basic science. He went to Berlin and that's where his physical chemistry and his ideas about reaction mechanisms were born and developed, in Berlin-Dahlem. After Hitler came to power in Germany, Polanyi left. He went to England. I went there in 1935 and spent two years with him.

Polanyi's background had some biology in it, he was aware that there were enzymes in living systems that could deal with molecular hydrogen. He thought that those enzymes, and all had metals in them, would probably be important to understand how to activate hydrogen properly. At that time he believed that the active site of hydrogenase, the enzyme, which activates molecular hydrogen and allows it to exchange with water, was an iron containing enzyme. The reason, I think, he thought that way, and I have to say, I think because he never did tell me, was that most of these enzymes did things like that, such as oxidation and reduction enzymes, enzymes that catalyzed the addition or removal of electrons from substrates. If the enzyme activated molecular hydrogen so it will exchange with the protons of water, presumably the enzyme was oxidizing H₂ to get protons and

holding the electrons back somehow. When then the protons would exchange, they would then come back again as molecular hydrogen.

Polanyi had been studying these exchange reactions in various ways. He invented, for example, the micropicnometer to measure the density of water in order to measure the amount of deuterium in it. He would use a few tens of microliters of the water to measure its density. These micropicnometers were little floats. The picnometer would hold a hundred or fifty microliters of water and it was put in through a microcapillary. The top of that picnometer bore a little sphere, a bulb of five millimeters of diameter. That sphere was very thin glass and flat on one side. When the picnometer was dropped in water, it would float with the water containing part down and the bulb up. The size of that bulb depends on the pressure. He could measure the density of a hundred microliters of water to five or six or seven places that way. That was the kind of man he was. He invented it, designed it and had it built. We didn't have mass spectrometers in those days. So we were measuring water densities that way and measuring exchange rates that way.

Polanyi had the idea that the enzymes must have some peculiar properties, which are dependent upon the porphyrins because almost all redox systems in biology that he knew about, the hemin of red blood cells, the chlorophyll of the green plants, all were porphyrin type molecules with metal centers. The hemin had an iron center, chlorophyll had a magnesium center. He put me onto that after I had been there a year and a half. He supposed that there must be something very special about this tetrapyrrolic structure, which surrounds the metal and which makes it do funny things in biology. The biological tetrapyrrolics are very unstable compared to the kinds of things he was used to doing.

About that time, in 1934, R.P. Linstead, Professor of Organic Chemistry at Imperial College in London, had discovered phthalocyanine. He was a consultant for ICI. ICI was making phthalonitrile, which is ortho-dicyanobenzene in glass lined kettles. Phthalonitrile crystallizes in beautiful white crystals but on one occasion it turned into a blue mess. Linstead determined that the glass lining in one of the iron kettles had cracked and phthalonitrile had come in contact with the iron, and this had catalyzed the cyclization of the four phthalonitriles around an iron center. He had ironphthalocyanide. That was the beginning of a new dye stuff, which turned out to be very stable, and became one of the most important organic pigments for a period of twenty or thirty years. It is known as a tetraazaporphyrin. The bridges between the four pyrrole rings were nitrogen atoms instead of carbons that are the bridges in nature.

Polanyi told me to go down to London, find out how to make that stuff and bring it back. He gave me two weeks to do that. Polanyi then suggested to put different metals in the center and study their catalytic properties for activating hydrogen, like platinum. You could heat it up, cool it, do what you liked. I've spent a lot of time doing that and I enjoyed that very much. In so doing I became thoroughly aware of the importance of that particular type of structure, always involving the movement of electrons and protons. Of course, the chlorophyll in the green plants, although not the same, is a very close relative of porphyrin. That also involves photochemical oxidation/reduction. That's how I got started on that business. My last experiments with Polanyi were hydrogen activation on metal-phthalocyanines with copper and zinc.

JOHN POLANYIT IDÉZVE

Wigner és Calvin Polányira vonatkozó visszaemlékezéseit olvasva nem lehet kétségünk afelől, hogy Polányi nemcsak felfedezőnek, hanem mentornak is kiemelkedő volt. Van azonban még egy Nobel-díjas, aki szintén közvetlenül részesült Polányi Mihály huzamos közelségének jótéteményéből, és ez természetesen fia, John C. Polanyi (kémiai Nobel-díj, 1986). Itt egy 1995-ben a Torontói Egyetemen vele készült beszélgetés részletét idézem¹³:

IH: When you speak about transition-state spectroscopy, it seems to me to have a close relationship to Michael Polanyi.

JP: It does, of course, but I don't think that's the closest I got to his interests. He would have thought it far-fetched that one might get light to interact with this subpicosecond entity which is neither reagents nor products. Though it was not first done with lasers, it was the existence of lasers — of which of course, he never dreamed — that got people thinking about "seeing" the transition state.

I find myself now at the age of 66 engaged with great excitement in some novel experiments in which we are trying to look at transition states for sodium-atom reactions. It is this project that brings me eerily close to my father's interests of 1929 and subsequent years.

When I was being conceived (I was born in 1929), my father was establishing himself as the most perceptive interpreter of sodium-atom reactions, which he understood as being in a sense the simplest of all reactions. They are so simple that even a physicist can understand them. The sodium, which is easily ionised, comes up to a molecule with high electron affinity, and an electron jumps across. Then the positive sodium ion is drawn to the negative molecule. Because the electron hops a large distance, my father coined the term "harpooning" for this. It is also called this because the positively charged sodium hauls in its negative catch. This is a uniquely simple reaction. It is different from most reactions which are fascinating because they are *not* sequential events. Harpooning reactions can however be described as sequential. Step 1, reagent approaches; step 2, the harpoon jumps across; step 3, the alkali fisherman pulls in the catch. The end.

Today, in my lab, we are finding that it is possible to access the harpooning event, not by taking the reagents and bringing them together, but by forming a loose complex which is in the configuration of the transition state, that is to say, by starting in the middle of the reaction. That is what we are currently doing. And that is indeed a lineal descendent of my father's interests.

I am, however, only one of many who have seen the extraordinary possibilities offered by harpooning reactions. For example, Dudley Herschbach began his life as a dynamicist by studying that type of reaction. One should also add that my father himself was part of a continuous progression. What drew him to sodium reactions was that Fritz Haber had been studying an unexplained chemiluminescence from them. This was in Berlin and my

¹³ I. Hargittai, *Candid Science: More Conversations with Famous Chemists*. Imperial College Press, London, 2003, pp. 386-389.

father was in Haber's Institute as a young researcher. The history, as is usual in science, constitutes an unbroken chain.

IH: Was he the determining influence in the direction you took in science?

JP: He personally wasn't. But where I trained for six years was. If the question is whether he was the determining influence in my going into science, then, yes, but I should qualify that answer. At the time when I learned most from my father, in my late teenage years, his interests were even livelier in non-scientific fields than in scientific ones. He had another son, George, who went into the humanities, equally under his influence. I could just as easily have gone into economics or philosophy or theology and have ascribed it to my father's stimulus. He was, of course, delighted to see me go into science, just as he would have been delighted to see me go in many other directions.

Perhaps I am being disingenuous. I can only say that if he steered me towards science, I didn't notice.

IH: How did he make the transition from physical chemistry to philosophy? Were you a witness to this?

JP: We seem destined to discuss transition states. Yes, I witnessed this one directly. I got back to England right at the beginning of my fifteenth year, and until I was well into my twenties I saw a good deal of my father. That was the time, beginning in 1944, when he was making the transition. The fact that he made that transition isn't so surprising. There are a lot of scientists who have started to ruminate about how discoveries are made, how people learn anything, and the role of logic in this as compared with faith. And all this was of interest to him too.

What is striking, in my view, is the originality and impact that he had in his new field of epistemology, the theory of learning. He would have said confidently that what he did in that area was much more important than what he did in science.

I have a sense of wonder at all he did in science, and yet I believe he may easily have been right that his contribution to epistemology will turn out to be more lasting. The sales of his books and the interest in his ideas continue to be great. Eventually his name will, of course, be forgotten, but his philosophical ideas will live on as a significant contribution to the development of philosophical thought.

What is remarkable, then, is the quality of the contribution he made in his decades as a philosopher. Actually, his first book on a nonscientific theme was being conceived in the 1930s when he attacked the Russian economic system and at the same time confronted the leading British social scientists of his day, Sydney and Beatrice Webb, who'd published a learned volume explaining how the Soviet five-year-plan constituted a superb innovation and was bringing prosperity to the USSR. My father took this thesis apart in a series of essays, which became a book in 1940, that went far beyond economics and inquired why it was that British liberals, the so-called Fabians, were so careless of the freedoms that they enjoyed; the book was called *The Contempt of Freedom*. It was an influential book and a prescient one. It is forgotten today. His best known book is, instead, *Personal Knowledge*.

As with new scientific theories, my father's thinking was initially rejected by the professionals. He was not embraced by the philosophers of his day, who felt that he was an ignorant outsider. This lasted for a large part of his time in philosophy. The people who paid attention to his work were closer to theology. This was in part because the philosophy of the time was "linguistic analysis." That brand of philosophy, centered on the study of

the structure of language, passed. I don't know whether my father contributed at all to its passing. It is an interesting question. Whatever the case, there followed a school of philosophy far more friendly to his ideas.

IH: Do you share his interest in philosophy?

JP: Just as a human being; not as a philosopher. I have no qualifications or ambitions in that area. It was a very brave and extraordinary undertaking on his part to do something so ambitious in the realm of philosophy. Unless you read widely and deeply, you are vulnerable to attack. But he did read widely and could withstand a skeptical and at times hostile audience.

A Polányi Mihályról a Royal Society (London) *Biographical Memoirs* című sorozatában megjelent visszaemlékezés szerzői a fentiekkel összhangban ezt írják¹⁴: "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."

RÖVIDEBB IDÉZETEK

Az 1986-os kémiai Nobel-díjat hárman kapták megosztva, Dudley Herschbach, Yuan Lee, and John Polanyi, „az elemi kémiai folyamatokra vonatkozó kutatásaikért.” Polányi Mihály egy másik 1986-os kémiai Nobel-díjasra is hatással volt.¹⁵ Dudley Herschbach pontosan számon tartja mind az öt alkalmat, amikor Polányi Mihállyal találkozott. Első találkozásukra 1962-ben került sor, Berkeley-ben, ahol Herschbach önálló egyetemi pályafutását elkezdte, Polányi pedig előadást jött tartani. Polányi meglátogatta Herschbach laboratóriumát is és történeteket mesélt Herschbachnak fiáról, John C. Polanyiról. Herschbach megtudta például, hogy mennyire meglepődött Polányi Mihály azon, hogy fia természettudós lett. Ugyanis tizenéves korában keserű szavakkal bírálta apját azért, mert állandóan cikkírással foglalta el magát, ahelyett, hogy a valóságos világgal törődött volna. Természetesen Herschbach és Polányi első találkozására idején, Polányi már átváltott a filozófiára és Herschbach is olvasta Polányi *Personal Knowledge* című könyvét. John C. Polányit Herschbach már korábbról ismerte és tisztában volt azzal a hatalmas különbséggel,

¹⁴ E. P. Wigner, R.A. Hodgkin, "Michael Polanyi 12 March 1891 - 22 February 1976," *Biographical Memoirs of Fellows of the Royal Society*, p. 423. (A cikk magyar fordítása a *Polanyiana* 2002-es számában jelent meg.)

¹⁵ I. Hargittai, *Candid Science: More Conversations with Famous Chemists*. Imperial College Press, London, 2003, pp. 397.

ami családi háttérükben megnyilvánult. Herschbach családjának az első egyetemet végzett tagja volt.

Ilya Prigogine (1917-2002) 1977-es kémiai Nobel-díjas („a nem-egyensúlyi termodinamika, különösképpen a disszipatív szerkezetek elméletének kidolgozásában végzett kutatásaiért”) így emlékezett Polányi Mihályra 1998-ban¹⁶:

I admired him very much. He was interested in my early work in thermodynamics and invited me to Manchester when he was still Professor of Physical Chemistry. It was some time between 1945 and 1948. It was an exceptional period in Manchester. In addition to Polanyi, there was also Evans and Turing and others.

George Porter (Lord Porter, 1918-2002, kémiai Nobel-díj 1967, Manfred Eigennel és R.G.W. Norrish-sal együtt, „különlegesen gyors kémiai reakciók tanulmányozásáért, amelyeket nagyon rövid ideig tartó energiaközléssel mozdítottak ki egyensúlyi állapotukból”) a következőket mondta 1997-ben Polányi Mihállyal való találkozásáról¹⁷:

One of the early workers who advanced this concept [uncovering the mechanism of chemical reactions] originally, M.G. Evans, was one of my teachers at Leeds who greatly inspired me. He himself studied under Michael Polanyi at the University of Manchester. I met Michael Polanyi in my first year as an undergraduate, at the age of 17. I was given the daunting task, as the secretary of the student chemical society, of proposing a vote of thanks to Michael Polanyi for his lecture. I didn't really understand the lecture very well but I managed somehow to say what a marvelous lecture it was, and that even I could understand some of it. I met him many years later when his son, John took me along to dine with him at the Atheneum club after a Faraday Society meeting. By this time he had become a social scientist.

Egyébként Polányi közismerten kiváló pedagógus is volt, nemcsak kiváló mentor. A már említett megemlékezés szerzői a Manchesteri Egyetem Vice-Chancellor-jának, W. Mansfield Coopernek magánközlésként elhangzott szavait idézik ezzel kapcsolatban, “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.” A visszaemlékezés szerzői ezt annak tulajdonították, hogy Polányi a tananyagot teljes részletességében szisztematikusan tárgyalta, segédanyagokat osztott szét a hallgatók között, tanácsot adott nekik arra vonatkozóan, hogy mit olvassanak és mindezt azzal párosította, hogy előadásában kiemelte a téma minden olyan csomópontját, amelynek megértése nehézséget okozhatott.¹⁸

¹⁶ *ibid.*, pp. 431.

¹⁷ *ibid.*, p. 486.

¹⁸ E. P. Wigner, R.A. Hodgkin, “Michael Polanyi 12 March 1891 - 22 February 1976.,” *Biographical Memoirs of Fellows of the Royal Society*, p. 424.

Polányi egyik korábbi tanítványa, Erich Schmid (lásd alább), aki később az Osztrák Tudományos Akadémia elnöke lett a következő szavakkal foglalta össze Polányi tanítómesteri kvalitásait¹⁹: "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."

PRÓBATÉTELEK

Mindezekre az emberi tulajdonságok segítették Polányit a különböző megpróbáltatások elviselésében. A Polányi család röviddel Hitler uralomra jutása után hagyta el Németországot. Amikor Angliában a Manchesteri Egyetem kémia professzora lett, Polányi már világszerte elismert kutató volt. Ennek ellenére a londoni Királyi Társaság legidősebb tagja a sajtóban bírálta Polányi kinevezését, mert "How are we to make progress if we do not employ those we train?" – tette fel a kérdést, Polányira pedig úgy hivatkozott, mint "a physical chemist from somewhere in the Balkans."²⁰ A *Nature*-ben megjelent levelében tiltakozott "against the appointment ... of a gentleman who is not an Englishman nor in any way connected with us." Majd hozzátette, hogy "The introduction of a foreign outlook into Manchester is most undesirable..."²¹ Természetesen ez a példa nem csökkenti annak a segítségnek a jelentőségét, amelyet a menekült tudósok kaptak Nagy-Britanniában, ellenkezőleg, a nehézségekre emlékezve még jobban értékelhetjük a segítséget.²²

A kémikus Polányi Mihályt közvetlenebb szakmai megpróbáltatások is érték. Érdekes megvilágításban említi ezt a molekuláris biológus Gunther Stent²³ az idejüket megelőző tudományos felfedezésekkel kapcsolatban. Stent először részletesen tárgyalja Oswald Avery et al. felfedezését

¹⁹ *ibid.*, p. 420. Az itt szereplő idézet egy könyvfejezetből való, P. Ignotus et al. (szerkesztők), *The logic of personal knowledge: essays presented to Michael Polanyi on his seventieth birthday*. Routledge & Kegan Paul, London, 1961.

²⁰ H. E. Armstrong's letter to *The Times*, 7 May 1935, "Foreign Scientists in Britain. Professor Kapitza's Recall to Russia," reproduced in L. Badash, *Kapitza, Rutherford, and the Kremlin*. Yale University Press, New Haven and London, 1985, pp. 121-123.

²¹ H. E. Armstrong, "Physical Chemistry in the University of Manchester." *Nature* 1933, July 8, p. 67.

²² H. E. Armstrong állítólag notórius tiltakozó volt.

²³ G. S. Stent, *Paradoxes of Progress*. W.H. Freeman and Co., San Francisco, 1978, pp. 99-100.

1944-ben, amely szerint az öröklődés anyaga a DNS. Ezután Gregor Mendel genetikai felfedezésével foglalkozik, majd így folytatja:

Cases of delayed appreciation of a discovery exist also in the physical sciences. One example (as well as an explanation of its circumstances in terms of the concept to which I refer here as prematurity) has been provided by Michael Polanyi on the basis of his own experience. In the years 1914-1916 Polanyi published a theory of the adsorption of gases on solids which assumed that the force attracting a gas molecule to a solid surface depends only on the position of the molecule, and not on the presence of other molecules, in the force field. In spite of the fact that Polanyi was able to provide strong experimental evidence in favor of his theory, it was generally rejected. Not only was the theory rejected, it was also considered so ridiculous by the leading authorities of the time that Polanyi believes continued defense of his theory would have ended his professional career if he had not managed to publish work on more palatable ideas. The reason for the general rejection of Polanyi's adsorption theory was that at the very time he put it forward the role of electrical forces in the architecture of matter had just been discovered. Hence there seemed to be no doubt that the adsorption of gases must also involve an electrical attraction between the gas molecules and the solid surface. That point of view, however, was irreconcilable with Polanyi's basic assumption of the mutual independence of individual gas molecules in the adsorption process. It was only in the 1930s, after a new theory of cohesive molecular forces based on quantum-mechanical resonance rather than on electrostatic attraction had been developed, that it became conceivable that gas molecules could behave in the way Polanyi's experiments indicated they were actually behaving. Meanwhile Polanyi's theory had been consigned so authoritatively to the ashcan of crackpot ideas that it was rediscovered only in the 1950s.

A KETTŐS CSAVAR JUBILEUMA ÜRÜGYÉN

Jól ismert, hogy a kémikus Polányi Mihály egyik legfontosabb kutatási iránya a röntgenkrisztallográfia volt, amelynek jelentőségét a következőképpen értékelte. Különösen érdekes, hogy milyen kontextusba helyezi a röntgenkrisztallográfia felfedezését²⁴:

...The power to expand hitherto accepted beliefs far beyond the scope of hitherto explored implications is itself a pre-eminent force of change in science. It is this kind of force which sent Columbus in search of the Indies across the Atlantic. His genius lay in taking it literally and as a guide to practical action that the earth was round, which his contemporaries held vaguely and as a mere matter for speculation. The ideas which Newton elaborated in his *Principia* were also widely current in his time; his work did not shock any strong beliefs held by scientists, at any rate in his own country. But again, his genius was manifested in his power of casting these vaguely held beliefs into a concrete and binding form. One of the greatest and most surprising discoveries of our own age, that of the diffraction of X-rays by crystals (in 1912) was made by a mathematician, Max von Laue, by the sheer power of believing more concretely than anyone else in the accepted theory of

²⁴ M. Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy*. University of Chicago Press, 1958, p. 277.

crystals and X-rays. These advances were no less bold and hazardous than were the innovations of Copernicus, Planck or Einstein.

A kettős csavar szerkezet felfedezésének legelismertebb krónikása, Robert Olby,²⁵ hangsúlyozza Polányi érdemeit a szálak röntgendiffrakciós tanulmányozásában. Amikor Polányi csatlakozott a Kaiser Wilhelm Társaság Fizikai Kémiai és Elektrokémiai Intézetéhez, akkor még jószerivel a Bragg egyenletet sem ismerte, de néhány hét elteltével már alapvető jelentőségű problémákat oldott meg. Elismerésként, Polányit mindennel ellátták, ami csak a munkáját elősegíthette. Saját szavaival jellemezve²⁶ ellátták „...with every facility for experimental work, most precious of which were funds for employing assistants and financing research students. In this I was incredibly lucky. I was joined by Herman Mark, Erich Schmid, Karl Weissenberg, all three from Vienna, by Erwin von Gomperz and some others...” Herman Mark (1895-1992) volt Polányi egyik munkatársa, akinek a nekrológjában méltatója, Herbert Morawetz, így ír Polányi munkájáról²⁷:

Polanyi found that the X-ray diffraction from cellulose fibers indicated the presence of crystallites oriented in the direction of the fiber axis and that an analogous crystal orientation existed in metal wires. A full structure analysis of cellulose seemed beyond the experimental possibilities of the time, but Mark and Polanyi noted that the increase in the modulus of cellulose fibers on stretching seemed similar to the reinforcement of metal wires during cold-drawing. They embarked, therefore, on a detailed analysis of the changes accompanying the cold-drawing of a zinc wire.

Tudjuk, hogy a szálak röntgendiffrakciójának leírását csak az 1950-es évek elején adták meg a DNS szerkezetvizsgálatával párhuzamosan. Polányi eredményeit akkor tudjuk igazán értékelni, ha figyelembe vesszük, hogy az 1920-as években, és még azon túl is sokáig, a nagymolekulák létezését nem tekintették bizonyítottnak. Sokan inkább kolloidokkal próbálták értelmezni és leírni a nagymolekulákat tartalmazó rendszereket. Erre Olby is emlékeztet, amikor felidézi, hogy sokak számára abban az időben egyáltalán nem volt nyilvánvaló, hogy a molekulák jóval nagyobbak is lehetnek, mint a kristályok elemi cellája. Ellenkezőleg, sokak számára az volt nyilvánvaló, hogy egy molekula nem is lehet nagyobb, mint az elemi cella. Így érthető, hogy amikor Polányi az egyik alkalommal

²⁵ R. Olby, *The Path to the Double Helix: The Discovery of DNA*. Dover Publications, New York, 1994 (eredeti megjelenés, University of Washington Press, Seattle, 1974), pp. 28-30.

²⁶ M. Polanyi, "My Time with X-rays and Crystals," in P. Ewald, ed., *Fifty Years of X-ray Crystallography*. Utrecht, 1962, pp. 629-636.

²⁷ H. Morawetz, *Biographical Memoirs*, Vol. 68, National Academy of Sciences, Washington, DC, 1994, p. 197.

1920-ban beszámolt Fritz Haber intézetigazgató szemináriumán, kitört a botrány. Erről Polányi így számol be, irigylésre méltó önkritikával:

The assertion that the elementary cell of cellulose contained only four hexoses appeared scandalous, the more so, since I said that it was compatible both with an infinitely large molecular weight or an absurdly small one. I was gleefully witnessing the chemists at cross-purposes with a conceptual reform when I should have been better occupied in definitely establishing the chain structure as the only one compatible with the known chemical and physical properties of cellulose. I failed to see the importance of the problem.

Egyébként ezek a Haber szemináriumok pedagógiailag is különlegesek voltak és ebben is kitűnt Polányi Mihály. Így számol be erről a kiváló osztrák-amerikai biokémikus Erwin Chargaff²⁸:

...Fritz Haber had a marvelously Socratic skill of drawing the best out of speaker and audience. Many of the talks were way over my head. But how great was my relief when Haber got up at the end and declared: "I haven't understood a word." And then, turning to his paladins, "Herr Polanyi," or "Herr Weiss, could you perhaps explain to me what it was all about?" There followed a brilliant dialogue, or rather a polylogue, through which everything seemed to be clear, even to me...

Polányi Mihály tudományos hagyatékának feltárása nemes és tanulságos feladat. Látva Polányi Mihály nemzetközi hatását, mindent meg kell tennünk azért, hogy a hazai tudományosság, különösen pedig a tudományos pályára készülők és az ezen a pályán elindulók minél nagyobb mértékben részesülhessenek életműve értékeiből.

Köszönetnyilvánítás: Beck Mihály és Gábor Éva bátorítása segített az összeállítás elkészítésében.

²⁸ E. Chargaff, *Heraclitean Fire: Sketches from a Life before Nature*. The Rockefeller University Press, New York, 1978, p. 51.