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EDITORIAL

Michael Polanyi—pupils and crossroads—on the 125th anniversary of his birth

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Abstract Michael Polanyi (1891–1976) was a Hungarian-born British physician turned physical chemist turned philosopher. His milestone epistemological treatise *Personal Knowledge* followed his substantial discoveries in adsorption studies, X-ray crystallography, materials science, and the mechanism of chemical reactions. Michael Polanyi was one of the last polymaths and his teachings impacted the world views of other outstanding contributors to twentieth century science and culture.

Keywords Michael Polanyi · Adsorption · Reaction mechanisms · X-ray crystallography · Epistemology · Eugene P. Wigner · Melvin Calvin · John C. Polanyi

Two roads diverged in a wood, and—
I took the one less traveled by

Robert Frost, "The Road Not Taken"

This Editorial is loosely based on an invited contribution to the symposium marking the 125th birthday of Michael Polanyi, organized by the Fritz Haber Institute, at the Technical University in Berlin on October 5, 2016.

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Introduction

Michael Polanyi (1891–1976, Fig. 1) was born into an upper-middle-class Jewish family in Budapest during an era of unprecedented progress in Hungary, which was then part of the dualistic Austro-Hungarian Monarchy. He attended the secular Model High School (Minta Gimnázium, Fig. 2) in downtown Budapest, one of the city's many excellent high schools. The Model High School, over the years, graduated such future luminaries as the American aerodynamicist Theodore von Kármán, the British economists Baron Thomas Balogh and Baron Nicholas Kaldor, Polanyi's economist historian brother Karl Polanyi, the British physicist Nicholas Kurti, the molecular and nuclear physicist Edward Teller, and the American Abel laureate mathematician Peter Lax.

At the time, the high school, called gimnázium, was an important venue for the intellectual development of young boys. Girls were not yet supposed to attend such a school; rather, they went to schools that more directly prepared them for their future tasks in family life. One of Michael Polanyi's siblings (Fig. 3), Laura Polanyi, was exceptional; she attended another famous high school, the Lutheran Gimnázium, as a private student with a special permission.

Beginnings

The discoveries, the writings, and the pupils are the true legacy of a scientist. In this account I focus on how some of Polanyi's former pupils remembered him, in particular Eugene P. Wigner and Melvin Calvin. The noted physicist and historian of science, Abraham Pais, opined that Polanyi "decisively marked Wigner's thinking, not just about physics, but also about philosophy and politics." [1]

Fig. 1 Michael Polanyi in 1937 in Manchester (courtesy of John C. Polanyi)

Fig. 2 The Model (Minta, now Trefort) Gimnázium in 2014 (photo by the author)

Wigner was referring to Polanyi when he stated that "Man's capacity to think is his most outstanding attribute." [2].

I met Michael Polanyi only briefly (see, below), but that brief meeting gave me an impression how fortunate those were that could spend longer periods of time with him. I have been fascinated not only with Polanyi's science but also with his life. He was hesitant in moving from Germany to England, because he found it difficult to accept that the Nazi madness could take over such a cultured land as Germany. Wigner commented on Michael Polanyi's emigration from Germany, "He moved to Manchester, England, in 1933, when Hitler came to power, for a reason *very similar* to that which had originally prompted him to leave Hungary." ([2], p 154) Here, I added emphasis to "very similar," because there are some that do not consider the departure of Wigner, Polanyi, and others from Hungary in the early 1920s, at the time of the anti-Semitic Horthy regime, to be forced emigration. Wigner knew better.

Fig. 3 The Polanyi siblings: standing from *left* to *right*: Sofie, Adolf, Laura, and Karl; sitting, from *left* to *right*: Paul and Michael (courtesy of László Füstöss)

Michael Polanyi (Figs. 4, 5) graduated from the Model High School in 1908 and obtained his MD degree from the Budapest University in 1913. He served as a physician in

Fig. 4 Michael Polanyi in 1915 in uniform (courtesy of John C. Polanyi)

Fig. 5 Michael Polanyi and Magda Kemeny on their honeymoon in 1921 (courtesy of John C. Polanyi)

the Austro-Hungarian Army in World War I. He had started his scientific research before having completed his medical degree. His professors sent his results in thermodynamics to Albert Einstein who liked Polanyi's paper a great deal. Polanyi received his Ph.D. degree in physical chemistry in Budapest, based on his 1917 dissertation entitled "Gázok (gőzök) adsorptiója, szilárd nem illanó adsorbensen" ["Adsorption of gases (and vapors) on non-volatile solid adsorbent"].

Polanyi had important appointments both under the democratic revolution in 1918 and under the communist dictatorship in 1919 in Budapest, but his activities were of purely professional rather than of political nature. Theodore von Kármán occupied an even higher position in the revolutionary governments than Polanyi. Von Kármán, Polanyi and their colleagues saw to it that the best people were appointed at the universities. When the extreme right counter-revolution took over, and the autocratic and anti-Semitic Horthy regime came to power, those appointees became unemployable for the entire quarter-century of the Horthy era. These were tragic consequences of von Kármán's and Polanyi's most benevolent activities. Polanyi understood that in the Horthy regime, a young ambitious scientist, especially if Jewish, had no future in Hungary. This is also why von Kármán, Polanyi, and many others, such as, for example, George de Hevesy, John von Neumann, Leo Szilard, Edward Teller, Eugene P. Wigner, Dennis Gabor, felt compelled to leave.

As forced as Polanyi's departure from Hungary was, it upset him when some time in the 1920s he was accused of denying being Hungarian. I am quoting here, in full, his answer in 1929 to this accusation [3]:

> In 1904, when I was 13, I lost my father. Since then I have supported myself from stipends and my earnings. In the model high school, where I went, my teachers were taking care of me, got stipends and tutoring engagements for me. From the second semester of the university, I have been engaged in Ferenc Tangl's laboratory, who did not cease taking care of me. I graduated in 1913 as Doctor of Medicine. Due to the concern of Ignác Pfeifer, the next year I got to the Technical University of Karlsruhe to study chemistry, as a companion of a rich boy. I was then 22.
>
> In Germany the professors grab the students' hands, if he is supposed to be gifted. They are like art collectors whose obsession is discovering talent. They educated me and gave me a position where I could address myself to my abilities. They gave me everything and demanded nothing of me. They trust that who gets to know the joy of scientific work, will never leave it as long as he lives.
>
> Why am I telling you this? Because, looking back, its meaning is exactly what Ady had written about a hundred times, a long time ago, when only a few gray clouds hinted at the upcoming night. Looking back, I see the depth from which I was rescued by helping hands, the lucky one out of many. Looking back, I see other Michael Polanyis bogged half-way down and disappearing, I see them in my good friends, who stayed behind, I see them in unknown poor boys, by the dozen, like me and worthier, cast out of the university, thrown to the ground in front of the barbed wires of *numerus clausus* and other restrictions – onto a hip of invalids.
>
> Yes, a few words by Ady suffice: On the heap of invalids – In the Gare l'Est – Am I not Hungarian!? – This is what connects me with you, my comrades at home, Endre Ady's spirit. The hope that Ady's nation has not pushed itself away from the West forever, that there will be another Széchenyi and Kazinczy, that there will be new Ferenc Tangls and Ignác Pfeifers at the universities – open doors, helping hands.
>
> The professors will be looking for talent among the poor, honoring the new manifestation of the spirit for which they have lived. Everybody will be ashamed if his betters are in a lower position than himself, and

won't rest until he lifts them into among his colleagues. There will though be unfortunate Official Authorities, but they won't be able to bar the way of the true spirit. I believe what we have here in Germany as the natural foundation of our lives, won't stay a utopia back home forever [4].

Polanyi (Figs. 6, 7) makes references to the Hungarian poet Endre Ady (1877–1919). Ady published "Am I Not Hungarian?" in 1907 (*Budapesti Napló*). Its original title was "Who Is Hungarian?" It was Ady's response to his accusers who waged a concerted attack on him against his new lyrics. The attack against Polanyi was not dissimilar to the one against Ady, containing accusations of treason and cosmopolitanism, un-Hungarian behavior. There is unison between Ady's poem and Polanyi's response to the question of the editor of the *Pesti Futár*.

I met Michael Polanyi in 1969 in Austin, Texas, at a luncheon in the plush private club, the "Forty Acres," attended by three of us. Polanyi was the guest of honor, the chairman of the Physics Department, Harold P. Hanson, was our host, and I was the third participant. At that time Polanyi was a famous physical chemist for me, and I was not familiar with his works in social sciences, such as his seminal book, *Personal Knowledge* [5]. Polanyi was gentle and unpretentious. Our conversation covered a broad range of topics, from the Turkish and Russian/Slavic words in the Hungarian language to history and philosophy. We also talked about the difficulties of keeping up with the exploding scientific literature. The aura of our conversation remained more in my memory than the actual topics and I am still under its impression. The quiet and simple way of

Fig. 7 Michael Polanyi and his research group in Berlin-Dahlem in August 1933, immediately before his move to Manchester. On the back of the photo, there is a dedication of the photo by Polanyi to Andreas Szabo (first from the *right*, *first row*) (courtesy of Éva Gábor)

Fig. 6 Associates of the Kaiser Wilhelm Institute for Physical Chemistry and Electrochemistry in 1931 (courtesy of Éva Gábor). Fritz Haber is second from the *left* in the *upper row* sitting and Michael Polanyi is second from the *right* in the *same row* standing

communicating firm and reliable knowledge has been imprinted in my mind [6].

Polanyi was an excellent pedagogue who recognized the needs of young men (getting higher education at the time was almost exclusively men's business) who turned to him for advice. The future noted low-temperature Oxford physicist Nicholas Kurti (Miklós Kürti, 1908–1998) had also studied at the Minta Gimnázium. Then, he attended the Sorbonne in Paris and in 1928, he moved to Berlin to study for his doctorate in physics. This is how in 1994 Kurti described what happened and I am quoting Kurti liberally in order to convey the atmosphere in which he found himself following Polanyi's advice [7]:

> … I had a letter of introduction to Michael Polányi who was at that time in Berlin. Polányi suggested to me to do one year of postgraduate work and then to do a doctorate. The field I chose was low-temperature physics and Professor Franz Simon was my supervisor. He was one of the founders of low-temperature physics in Germany. Those three years, between 1928 and 1931, in Berlin were the most fantastic. As a city to live in, Berlin did not appeal to me. What I missed most was the Quartier Latin of Paris where I used to live. Walking up and down the Boulevard Saint Michel was the best recreation I could ever have. Berlin was different. Compared with Paris, it was a soulless city. It was all right though because I just wanted to work hard. Still I managed to do a few good things. For example, a few weeks after the premiere of the *Dreigroschen Opera* by Bertold Brecht, I went to see it four times.
>
> The most important thing though was the *Physik Kolloquia*, organized by Max von Laue in the Physics Department. These were not colloquia in the present sense of the word. They were more like the American journal clubs, just one two-hour session every Wednesday. A few people simply reported on recent publications from the literature. It was characteristic that in 1929 or 1930, Max von Laue could have an overview of the whole physics literature by looking at the *Proceedings of the Royal Society, Physical Review*, and *Physikalische Zeitschrift*.
>
> If you went regularly to this colloquium, you could know what was going on in physics. Then you could keep up with everything. Laue would ask the audience about papers as he was looking for volunteers to review them for next time. It was regarded as the thing for graduate students to volunteer. Just think of it, you were reporting about a recent paper by a famous physicist and there was the audience, in the front row, Planck, Schrödinger, von Laue, Gustav Hertz, Haber, Nernst, about 6 or 7 Nobel Laureates or future Nobel Laureates. Behind them were Wigner, Szilárd, and others.
>
> It was a very interesting experience. It was also wonderful to see that every now and then the great men could also make some silly mistakes. I remember when once Schrödinger suddenly stood up in the middle of a discussion of the spectra of triatomic molecules and suggested that the calculations could be simplified if you assumed that the three atoms are in the same plane. There was a silence, followed by laughter.

Wigner: "What a mentor Michael Polanyi was!" [8]

Wigner (Fig. 8), with Andrew Szanton's assistance, produced a gentle autobiography in which Wigner narrated his encounters with Polanyi. It is interesting to notice that Wigner spotted Polanyi's early interest in philosophy ([8], pp 76–79):

> …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

Fig. 8 Eugene P. Wigner and the author in 1969 in front of the old Physics Department of the University of Texas at Austin (by unknown photographer)

as many fields as Polanyi. After László Rátz of the Lutheran Gimnázium, 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.

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.

Further down, Wigner mentioned their joint work ([8], pp 76–79):

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 of 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 [Max] Born and [James] 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 worked out a variation of his original idea and published it, but it never made the impact it might have if Wigner had secured his priority in tackling the problem. This is quite a story and it is always a delicate question when the mentored overtakes the mentor even if it is in a single research idea. Both Polanyi and Wigner came out of this story impeccably though.

Wigner did his research for his Diploma work (Master's degree-equivalent) with Herman F. Mark, but opted to do something different for his doctoral work. He decided to investigate the rates of chemical reactions and he signed up for being Polanyi's doctoral student ([8], pp 80–81):

> Polanyi advised my doctoral dissertation at the hochschule. … 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").

Once Wigner completed his studies in Berlin, he returned to Budapest in 1925 and started working in the tannery directed by his father. He may have not been an enthusiast for tannery work, but he was conscientious in everything he did. He learned whatever there was to learn about the processes involving leather and even visited other tanneries to learn more about the processes he was using. Even decades later, he was proud of his knowledge of the chemistry of leather treatment. Yet he missed physics and subscribed to the *Zeitschrift für Physik* to keep up with the developments in his favorite subject. A year had barely passed when he received an invitation to return to Berlin to work for the crystallographer Karl Weissenberg at the Kaiser Wilhelm Institute (today, we would call this a postdoctoral position). The invitation was the work of Michael Polanyi, who knew that Wigner was destined not for tannery work but for creative science.

Wigner adored Polanyi (Fig. 9), "Michael Polanyi was really the miraculous one [teacher]. Polanyi loved to ask the fundamental question: 'Where does science begin?' He listened to the thoughts of others on this question, but he also had his own well-crafted answer [see below]. … 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." ([8], pp 80–81)

When Wigner's Nobel Prize came and he had to give the traditional two-minute speech at the Nobel Banquet, he returned to what he had learned from Polanyi about where science begins: "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." [9]

Wigner's interactions with Polanyi did not end when both had left Germany and Wigner spent a few precious months with Polanyi in the mid-1930s in Manchester. In his memoirs, Wigner gratefully remembered that Polanyi was still capable of praising Wigner even when Polanyi's faculties were diminishing during Polanyi's terminal illness. One wonders how much Polanyi's example influenced Wigner in Wigner's later years when he was increasingly turning to discuss philosophical questions.

Melvin Calvin about Polanyi's "curious mind"

The American Melvin Calvin (1911–1997, Fig. 10) received the Nobel Prize in Chemistry in 1961 "for his research on the carbon dioxide assimilation in plants."

Fig. 9 Eugene P. Wigner (on the *right*) with Michael Polanyi and his son, John C. Polanyi in 1934 in Manchester (courtesy of John C. Polanyi)

Calvin spent 2 years with Polanyi as postdoctoral fellow for which Polanyi (Figs. 11, 12, 13) used a grant from the Rockefeller Foundation. Calvin referred to his time with Polanyi in his Nobel lecture in the following way: "Our own interest in the basic process of solar 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. It was there I first became conscious of the remarkable properties of coordinated metal compounds, particularly metalloporphyns as represented by heme and chlorophyll." [10]

Calvin narrated in detail about these studies in a recorded conversation with Clarence Larson, former Commissioner of the US Atomic Energy Commission. Larson and his wife, Jane Larson, in their retirement recorded conversations with famous scientists and technologists. Melvin Calvin was one of them and their recording took place in 1984 [11]:

> 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 D_2 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, starting with 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 D_2 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

Fig. 10 Melvin Calvin in 1962 at Berkeley by Berkeley LRL Graphic Arts (courtesy of Marilyn Taylor and Heinz Frei)

Fig. 11 Michael Polanyi (*middle*) and Alwyn G. Evans (*right*) in 1940 in Manchester (courtesy of John C. Polanyi)

Fig. 12 Faculty and chemistry honors students in 1936 in Manchester (courtesy of Éva Gábor). Michael Polanyi is fourth from the *left*, *first row*

with water, was an iron-porphyrin-bearing 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 were 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 H_2 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 volume 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 tetrapyrrols 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.

Fig. 13 Faculty and chemistry honors students in 1947 in Manchester (courtesy of Éva Gábor). Michael Polanyi is fourth from the *right*, *first row* (note the threefold increase in the ratio of female students as compared with 1936)

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 iron phthalocyanide. That was the beginning of a new dyestuff, which turned out to be very stable, and became one of the most important organic pigments for a period of 20 or 30 years. It is known as a tetraazaporphyrin. The bridges between the four pyrrol 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 metalphthalocyanines with copper and zinc.

Michael Polanyi was willing to participate in the war efforts in Great Britain. At about the outbreak of World War II, he made inquiries of whether he could participate in the war efforts doing applied research, but was given a negative response. However, his teachings found their way, through Wigner, into the Manhattan Project. As soon as nuclear fission was discovered, the imagination of physicists captured the possibility of the atomic bomb. One of them was John A. Wheeler who helped Niels Bohr in working out the theory of fission, and in this, Wheeler enlisted Wigner's assistance. This is how Wheeler recalled this period in the early 2000s [12]:

> We had to understand this new nuclear phenomenon, fission. It was obvious that the nucleus of such a heavy element as uranium must undergo a considerable deformation before it splits. For that it needs energy. When the uranium is bombarded by neutrons, the neutron can provide this energy; we say that the nucleus is excited. This excitation then could initiate a vibration in the nucleus that could deform it. Our Hungarian friend, Eugene Wigner helped us out. He ate some oyster downtown Princeton and got sick and was in the hospital on the campus. I went to see him at the hospital to get some help. The questions that Bohr and I were dealing with were like a chemical reaction. Uranium breaking up is like carbon monoxide breaking up into carbon and oxygen. I remembered that he [Wigner] had worked in that field with Michael Polanyi. And he helped us and, eventually, getting also ideas from discussions with other colleagues, such as Placzek and Rosenfeld, Bohr and I saw how fission works. Bohr left Princeton in April of that year and during the following months I wrote the paper and we submitted it to *Physical Review* in June. It came out in the September 1, 1939, issue; by strange coincidence the same day when Germany invaded Poland.

John C. Polanyi: learning directly and indirectly

Considering that having a father of the stature of a Michael Polanyi may not only provide a great advantage, but may also be a great burden, John C. has handled it with grace. I am quoting here a few excerpts of our recorded conversation in 1995 (Fig. 14) at the University of Toronto [13]:

> *Let's speak about your teachers. Was your father your teacher?* (Figs. 15, 16)

> JCP: Formally he was my teacher for one year. I entered Manchester University in 1946 when I was 17. He lectured to me in the first year. That was the last year he lectured in science. Then he transferred to philosophy. He also taught me a great deal in conversations despite my many absences away from home, first in boarding school and then for three years as an evacuee in Canada.

Fig. 14 John C. Polanyi in 1995 at the University of Toronto (photo by the author)

Fig. 15 John C. Polanyi and Michael Polanyi (courtesy of John C. Polanyi)

> Most of what he taught me about physical chemistry I learned at one remove from him. I was a student for six years in the Department that he had shaped in Manchester. My professor Meredith Evans was one of his favorite students and my Ph.D. supervisor Ernest Warhurst was another student of his. What I learned from his students gave me a sense of scientific values — where the field was going, what were the important questions to tackle, and, to a degree, how to tackle them. Without those things I would have been lost. But it happens that I didn't get them

Fig. 16 John C. Polanyi and Michael Polanyi in Oxford (courtesy of John C. Polanyi)

directly from him, but from people who owed a lot to him.

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

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

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

JCP: 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.

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

JCP: 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.

Wigner and R. A. Hodgkin penned Michael Polanyi's obituary in the *Biographical Memoirs of Fellows of the Royal Society*. It relates to the above when they noted that "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." [14].

Researcher and pedagogue

In 1995, I talked with Dudley R. Herschbach about Michael Polanyi, among other topics [15]. Herschbach, John C. Polanyi and Yuan T. Lee [16] jointly received the Nobel Prize in Chemistry in 1986 "for their contributions concerning the dynamics of chemical elementary processes."

Michael Polanyi was an early influence on Dudley Herschbach. He cherished the memory of all his five meetings with Polanyi. The first time they met was in 1962 when Michael Polanyi came to Berkeley to give some lectures. Polanyi visited Herschbach's laboratory and Polanyi was telling him stories about his son John. Polanyi was surprised that John became a scientist because, he said, John in his teenage years used to bitterly criticize his father, saying that he was writing papers, all the time, that were not connected with the real world.

At the time of Michael Polanyi's visit to Berkeley, in 1962, he had already switched to philosophy. Herschbach had read some of Polanyi's books, among them *Personal Knowledge*. Herschbach thought that Polanyi's books helped making people aware of what scientists really do. Scientists get excited about their ideas and they want to see them work. Yet they have the discipline, and they must have the discipline because the scientific community as a whole insists on it, to test their ideas. These ideas do not always pass the test and the scientists have to give them up or modify their ideas. In contrast to John C. Polanyi, who came from an exceptional family of intellectual giants, Herschbach came from a family where he was the first scientist, possibly even the first university graduate. It hurt but he was not handicapped by it.

Considering John's and Dudley's backgrounds, the third co-recipient of the 1986 Nobel award, Yuan T. Lee, considered his in the middle: "Mine was somewhere in between. My father and mother were school teachers." [17] Lee met Michael Polanyi in 1968 when Lee started his career at the University of Chicago and they both were attending a conference in Toronto.

The pedagogue Michael Polanyi influenced many more outstanding scientists than those few Nobel laureates mentioned above so far. Wigner and Hodgkin's obituary quoted W. Mansfield Cooper, Vice-Chancellor of Manchester University that "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." Wigner and Hodgkin attributed this "to Polanyi's systematic coverage of detail, through handouts and guided reading, which he combined with profound exposés of major problematic themes in lectures."([14], p 424)

One of Polanyi's disciples, Erich Schmid, who later served as president of the Austrian Academy of Sciences, had this to say about Polanyi's pedagogical qualities: "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." ([14], p 420)

Ilya Prigogine (1917–2002) received the Nobel Prize in Chemistry in 1977 "for his contributions to non-equilibrium thermodynamics, particularly the theory of dissipative structures." In 1998, he remembered Michael Polanyi with the following words: "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." [18]

George Porter (Lord Porter, 1918–2002), shared the Nobel Prize in Chemistry in 1967 jointly with Manfred Eigen and R.G.W. Norrish "for their studies of extremely fast chemical reactions, effected by disturbing the equilibrium by means of very short pulses of energy." Porter considered himself a scientific grandson of Polanyi's [19]:

> 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 Athenaeum Club after a Faraday Society meeting. By this time, he had become a social scientist.

The Loneliness of the Discoverer

Making a discovery implies that the discoverer, at least for some time, will be alone as he or she knows something that nobody else does. This loneliness may be a heavy burden and it may last a short or a long while [20]. Making premature discoveries certainly prolongs this loneliness. Michael Polanyi must have experienced this loneliness on more than one occasion. In his book, *Paradoxes of Progress*, the late molecular biologist Gunther Stent used the story of Polanyi's discovery in adsorption to illuminate some points about premature discoveries along other examples, such as Gregor Mendel's discoveries related to genetics and Oswald T. Avery's discovery that DNA is the substance of heredity [21]:

> 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.

Pioneering in X-ray crystallography

X-ray crystallography has been a success story in science for over a hundred years. The technique has kept renewing itself and although for many tasks more powerful approaches have emerged, X-ray crystallography has kept its position. Polanyi would welcome and enjoy the development of the past few decades whereas crystallography has greatly expanded its scope under the name of generalized crystallography [22]. Polanyi placed the discovery of X-ray crystallography into an intriguing context in his *Personal Knowledge* ([5], p 277):

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

Robert Olby, the renowned chronicler of the story of the double-helix discovery, has pointed out Polanyi's merits in the X-ray diffraction investigation of fibers. Polanyi was rather ignorant about X-ray crystallography when he joined Fritz Haber's Institute for Physical Chemistry and Electrochemistry in Berlin, but soon enough he was already working on and solving fundamental problems in this field [23].

Incidentally, the Kaiser Wilhelm Society early on realized the importance of fiber science and established a research institute for fiber chemistry (Faserstoffchemie) and Polanyi continued his research there for a while. He had ideal conditions for his work. In his words, his studies were assisted "…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…" [24]

Herbert Morawetz in Herman F. Mark's obituary referred to Polanyi's achievements [25]:

> 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.

Polanyi's discoveries gain special importance in the light of the state of the related chemistry at the time. In the 1920s, it was still debated whether biological macromolecules existed or the relevant systems consisted of colloidal components. Many held the view that macromolecules did not exist and that molecules could not be larger than the elementary cell in the crystals. Polanyi was willing to brave the hostile reactions to his views that came as conclusion from his X-ray crystallography studies. It was a case in point what happened when he gave an account at institute director Fritz Haber's seminar. This is how Polanyi communicated the event with enviable self-irony ([23], p 30 and [24], p 631):

> 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.

To Conclude

Michael Polanyi (Figs. 17, 18, 19) did not continue his studies in crystallography after a while and from his perspective at the time, they may have not seemed sufficiently promising. In the 1920s, crystallography was immersing itself further deep in the study of fully crystalline systems. The study of less ordered structures appeared esoteric and when the British J. Desmond Bernal and William Astbury divided the field between themselves, Bernal thought that by choosing the crystalline ones he had the best of it. The development of science proved him wrong. Bernal confined the investigation of nucleic acids to their crystalline components, the nucleosides. His Norwegian associate, Sven Furberg determined the structure of cytidine, which was important but served only as one of several components from which Crick and Watson constructed the double-helix structure of DNA. Bernal later wrote, "A strategic mistake may be as bad as a factual error," [26] referring to his gentleman's agreement with Astbury. Had Bernal not honored this agreement, the story of the discovery of the double helix might have turned out differently.

Bernal's words, "A strategic mistake may be as bad as a factual error," reverberate in my ears when I think about Polanyi's exceptional achievements in science. I cannot help wondering whether Bernal's self-critical observation might not have been applicable to some of Polanyi's decisions in taking turns and choosing directions when his road in science appeared bifurcating, or multi-furcating, in front of him. He, who was so good in giving advice to others, might have found himself short of good advice himself.

In some ways, although Michael Polanyi never received a Nobel Prize, he appeared in full force—alas, only symbolically—in Stockholm twice over the years. In 1963, Wigner remembered him as his mentor and quoted him in his precious two-minute speech about what science really is (see above). In 1986, Michael Polanyi appeared there

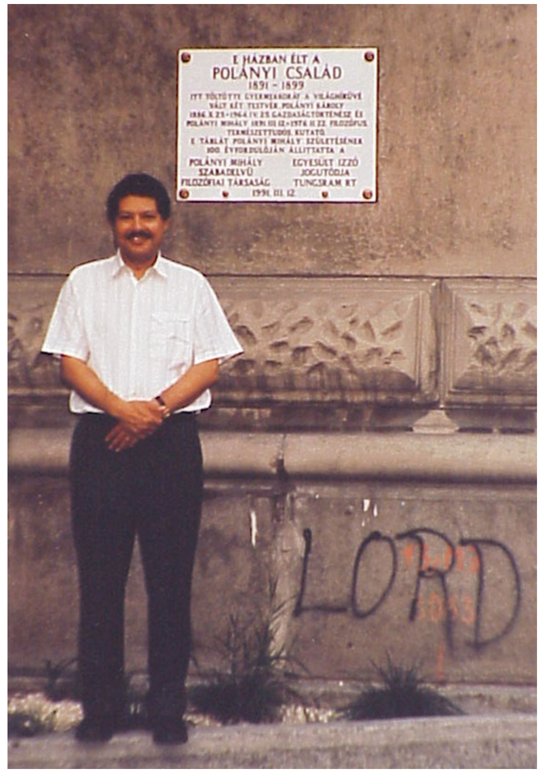

Fig. 18 Memorial plaque honoring the Polanyi Family and especially Karl Polanyi and Michael Polanyi at 2 Andrássy Avenue in Budapest in the 1980s with the Egyptian-American chemistry Nobel laureate (1999) Ahmed Zewail. Courtesy of Ahmed Zewail

Fig. 17 Michael Polanyi addressing a meeting on cultural freedom in 1956. On the *right*, the French philosopher Raymond Aron (1905–1983). Courtesy of John C. Polanyi

Fig. 19 The latest plaque (2012 photo by the author). Anti-Semitic vandals repeatedly destroyed the Polanyi memorial plaque

through his son, John C. Polanyi, and through the science of the three awardees in chemistry that could be considered a continuation of his own work. John C. Polanyi's evaluation of his father's works is engagingly realistic, yet gentle.

According to John, Michael Polanyi learned medicine and became a professional in it, but did not care for it. He stayed an "amateur" in everything else, where he became successful, such as chemistry, physics, economics, philosophy, and even a few other areas. He never had a mentor in any of these fields and he was the sole author of his first 15 papers, with only one exception. As Polanyi's career was at the very beginning, the mathematician George Pólya remarked: "Michael walks alone; he will need a strong voice to make himself heard." [27]

Further, according to John, Michael Polanyi stayed an outsider and chose the topics of his inquiry with great freedom. He started doing research in thermodynamics and in adsorption, and when his premature discoveries did not gain acceptance, he moved on. He was successful in crystallography as far as he went. Finally, he arrived at the ultimate puzzle in chemistry of what makes molecules stable and what makes and how do chemical reactions happen? He succeeded in providing an insight that did not merely prove correct, but turned out also excitingly attractive. As an irony of Polanyi's fate, his most fruitful period of scientific creativity was the years of his forced transition from Nazi Germany to democratic Britain: 32 papers appeared indicating both Berlin *and* Manchester as the venues of his work.

Role model?

We cannot recommend to anyone to follow Michael Polanyi's footsteps, because one would need too large shoes to fit for doing so. But he has served and will be serving as inspiration in doing research in science; in maintaining interest in more than in one culture; and in watching out for our fellow human beings. We have no doubt that Polanyi's thoughts expressed in his *Personal Knowledge* and elsewhere will be remembered "long after his contributions to science will have joined the melting pot of anonymity." [28]

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