A Forceful Life[1]

Thomas Hager, Force of Nature: The Life of Linus Pauling
Simon & Schuster 1995,

Istvan Hargittai

Thomas Hager first met Linus Pauling in 1984 at a presentation on vitamin C by the then 83-year-old scientist. Both men arrived early and found themselves alone in the seminar room. Pauling introduced himself and, wasting no time, proceeded to deliver an enthusiastic "minilecture on the chemical binding properties of tin." Hager's description of this encounter reminded me of my own meeting with Pauling, only a couple of years before Hager's, at the University of Oslo. Pauling lectured a packed auditorium about structural chemistry. He was deriving complicated expressions without using so much as a scrap of paper, marching back and forth in front of the long blackboard, which he covered with formulas. He kept his enthusiastic Norwegian audience in awe and only gradually did it dawn on me that the sophisticated derivations were superfluous to an understanding of the subject matter. During the luncheon after the talk, he stayed fresher and more alert than any of us.

Hager faced a daunting task in trying to document the energy and diversity that marked Pauling’s long scientific career (spanning almost 70 years until his death in 1994). To the public, Pauling is probably best known for his championing of the health benefits of vitamin C. In scientific circles, however, he is most renowned as the principal architect of structural chemistry, the fundamental science of the spatial arrangements of atoms in molecules and crystals and the interactions that bond substances.

This work, crowned by his book *The Nature of the Chemical Bond* (first published in 1939), earned him the 1954 Nobel Prize for Chemistry. Although Pauling possessed only a tiny fraction of what we know today about structural chemistry, his observations have withstood the test of time. His achievements have also demonstrated that a method of collecting information and arriving at a discovery may have as lasting an impact on the development of science as a discovery itself.

Hager, a science journalist, describes Pauling's science well—not a simple task, considering its breadth. He explains complicated concepts easily yet correctly and fixes ideas in the reader's mind with succinct descriptions. For example, the ability of hydrogen to bond simultaneously to two atoms instead of the usual one-a structural feature of vital importance to chemistry-becomes "hydrogen bigamy." Just as Hager uses human terms in explaining science, he also speaks about the "chemical bond" between Pauling and a fellow scientist.

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The author has exercised much restraint in condensing Pauling's exceptionally productive and inspiring life into a manageable book. Those aspects of the scientist's work that are left out of *Force of Nature* might suffice as life achievements for lesser researchers. For example, Pauling and his graduate student assistant, Lawrence O. Brockway (whose name is misspelled in the book) used gas-phase electron diffraction to determine the structure of volatile molecules during the early 1930s; they introduced a technique called Fourier transformation with which the distances between atoms can be determined directly. This approach is now applied daily in electron diffraction laboratories.

Hager also omits Pauling's role in the development of Corey-Pauling-Koltun (CPK) space-filling models, Tinkertoy-like objects whose relative sizes and connection points are based on those of actual atoms and molecules. They facilitate hands-on testing of proposed molecular structures and are still in widespread use. They were instrumental in launching the theory of host-guest chemistry, which earned Donald J. Cram a Nobel Prize in 1987, and have aided in many a chemist's education.

Force of Nature presents Pauling not only as a great scientist but also as an exceptional human being. Hager reaches back to Pauling's ancestors from Germany and Ireland, tracing his childhood in Oregon and his youthful travels to his happy and productive decades at the California Institute of Technology. (Hager also tells the love story of Pauling and Ava Helen Miller. We learn about their dogged pursuit of their goals; a pursuit they sometimes engaged in at the expense of their four children.)

Pauling played a major role in making Caltech a world center of scientific research. Yet the institute attempted to ease him out in the 1950s, when his leftist political activism began to embarrass its mostly conservative administration. His resistance to the actions of the House Un-American Activities Committee, the Federal Bureau of Investigation and the Passport Office of the State Department showed him to be a true champion of the spirit of American independent thinking. Even so, Pauling eventually felt compelled to choose between conspicuous political resistance and research opportunities, so he scaled down his political activism. There are lessons in this chapter of Pauling's story that remain important today.

Hager's book gives great emphasis to the other arena of Pauling's political work: his fight against nuclear weapons testing, for which he received the 1962 Nobel Peace Prize. In these activities he appears to have been somewhat one-sided, trying to pressure the U.S. (and Great Britain) more than the Soviet Union. Pauling explained that it was more natural for him to criticize his own country’s government than that of the U.S.S.R. He apparently was fooled by Soviet propaganda and did not see the Soviet Union for what it was.

It is ironic, then, that just as Pauling was facing political problems at home, he was declared a public enemy by the Soviet chemistry establishment. Some mediocre but influential professors considered his resonance theory to be ideological heresy and managed to terrorize the entire Soviet chemistry community into reviling it. Pauling thought Soviet chemists merely needed more time to appreciate his theory. In fact, generations of talented young Russians considered theoretical chemistry hazardous and continued to shy away from it long after the resonance theory had become a nonissue.

At times, Hager appears to succumb to the temptation to make his subject larger than life. Something of this bias may be seen when Hager details Pauling's unsuccessful attempts to help the son of a German crystallographer escape Nazi Germany. The relatively large weight given to this one episode—and the lack of similar ones—suggests that Pauling's aid to victims of German National Socialism was limited. This early stance is in pointed contrast to his work on behalf of Japanese-Americans interned in the U.S. during World War II and his later dedication to other causes involving the persecuted and oppressed.

Hager does not flinch, however, from recounting some of Pauling's personal and professional relationships that became very close, only to break apart, sometimes ending in lawsuits. Pauling's attitude toward the mathematician Dorothy Wrinch and her original (albeit probably erroneous) protein model, for instance, appears anything but magnanimous.

Another such story, which Hager does not mention, involves Pauling's unbending hostility toward quasicrystals after their discovery in 1984 by Dan Shechtman. Quasicrystals are regular but non-periodic structures that scientists once considered to be a physical impossibility; the evidence for their existence necessitated a change in the very definition of what a crystal is. Pauling never did believe in quasicrystals, and his immense influence may have hindered the broadening of crystallographic concepts. Despite the 627 pages of text and more than 50 pages of notes, it is inevitable that *Force of Nature* omits chunks of Pauling's life. Nevertheless, Pauling comes alive on the page—forceful, creative and unyielding. Hager has produced a book worthy of its subject.

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Chemistry Gallery

John Emsley, Molecules at an Exhibition: Portraits of Intriguing Materials in Everyday Life
Oxford University Press, 1998, 250 pp[1]

Istvan Hargittai

Marcel Berthelot once pointed out that chemistry resembles the arts. It is unique among the natural sciences in that it creates most of its objects by synthesis. The exhibits in this book include both natural and man-made substances, selected for being important, either useful or harmful, in our everyday life. Some, such as nitric oxide, have made a remarkable transition in our judgment. Eight galleries group the substances, from foodstuffs to vital components of our body, from illicit drugs to raw materials and energy sources, from agents that destroy us and our environment to those that give us pleasure.

Chemistry books, even those written for the general public, are often burdened with complex formulae. Fortunately, here John Emsley provides only a few familiar formulae, such as H_2O and CH_4. Knowing the formulae of the more complex molecules he describes would not make it easier to under- stand their function. A case in point is his description of the way we excrete unwanted nitrogenous material from the body by molybdenum-containing xanthine oxidase, a mammalian enzyme, producing uric acid. If uric acid is overproduced it accumulates in the form of sharp crystals in the joints, resulting in the painful illness of gout. The mechanism is understandable as described, and including the relevant formulae would not make it any easier to grasp the idea.

Most of us are largely unaware of the roles of many of the exhibited molecules in our everyday life. For example, phenyl ethylamine makes us feel good when we eat chocolate, sodium azide explodes on the impact of a car crash to save our lives, and thallium will poison us if the food of cows whose milk we drink had been exposed to thallium sulphate, which is used to kill rats.

Engaging stories are sprinkled through- out the galleries. Emsley tells us about the British discovery of penicillin and why British citizens had to pay US companies to use it. He explains that every time a young man thinks of sex — four times an hour, on average — his thoughts generate nitric oxide to help him fulfill his desires. And he provides a balanced assessment of the uses and dangers of DDT, one of the most worshipped and most feared molecules.

The descriptions are accurate without being pedantic and the captivating short stories didactic without appearing patronizing. But this is a chemistry book nevertheless, albeit an unusual one in that it provides a lot of natural and cultural history along with its chemistry. A broad audience, regard- less of whether it has a background in chemistry, will enjoy browsing and reading it. The composer Modest Mussorgsky's "Pictures at an Exhibition" is a classic; Emsley's own 'exhibition' will also be receiving many visitors for a long time to come.

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[1] Nature 1998, 393:641

Pierre Curie and his hands by the graphic artist Istvan Orosz (Budapest). Orosz stressed handedness, that is, chirality to symbolize dissymmetry. Orosz prepared this drawing, along with some others, at my Parents' request for their book, Istvan Hargittai and Magdolna Hargittai, *In Our Own Image: Personal Symmetry in Discovery* (New York: Kluwer/Plenum, 2000)

Eternal dissymmetry[a]

Magdolna Hargittai and Istvan Hargittai

The teachings of Louis Pasteur about chirality continue to instruct and inspire.[1]

Louis Pasteur was the first to suggest that molecules can be chiral. In his famous experiment 155 years ago, in 1848, he recrystallised a salt of tartaric acid and obtained two kinds of small crystals that were mirror images of each other. Pasteur prepared beautiful cardboard models, which have been preserved and are exhibited in the Pasteur Museum at the Pasteur Institute in Paris. Pasteur may have been motivated to make these models of large scale because Jean Baptiste Biot, the discoverer of optical activity, had very poor vision by the time of Pasteur's discovery.[2]

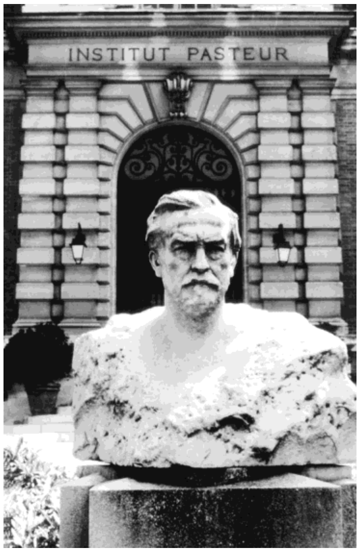

Figure 1 Louis Pasteur's bust in front of the Pasteur Institute, Paris (photograph by the authors, © Hargittai Photo).

Figure 2 Pasteur's models of enantiomeric crystals in the Museum of the Pasteur Institute, Paris (photograph by the authors, © Hargittai Photo).

As a moving moment in the history of science, it was left to Biot, rather old by then, to present Pasteur's findings to the French Academy of Sciences. The ever careful Biot first had Pasteur demonstrate his experiment to him in person. In Pasteur's description:[3]

'When [the solution] had furnished about 30 to 40 grams of crystals, he asked me to call at the Collége de France in order to collect them and isolate before him, by recognition of their crystallographic character, the right and the left crystals,

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[a] Mendeleev Communications 2003, 13:91–92. Reproduced with permission from Mendeleev Communications and Elsevier.

requesting me to state once more whether I really affirmed that the crystals, which I should place at his right, would deviate [polarized light] to the right, and the others to the left. This done, he told me that he would undertake the rest. He prepared the solutions with carefully measured quantities, and when ready to examine them in the polarizing apparatus, he once more invited me to come into his room. He first placed in the apparatus the more interesting solution, that which ought to deviate to the left. Without even making a measurement, he saw by the appearance of the tints of the two images, ordinary and extraordinary, in the analyzer, that there was a strong deviation to the left. Then, very visibly affected, the illustrious old man took me by the arm and said, 'My dear child, I have loved science so much throughout my life that this makes my heart throb.''

According to the Nobel laureate George Wald,[4] 'No other chemical characteristic is as distinctive of living organisms as is optical activity.' The roots of Pasteur's discovery reach farther back than Biot's work. According to J. D. Bernal,[5] Pasteur's discovery arose at a meeting place of hitherto distinct disciplines. They were crystallography, physics and chemistry. He also showed that the fruits of the discovery benefited new branches in these sciences.

Of course, not only material objects can have chirality or handedness. Bach's *The Art of the Fugue* is a beautiful example. Handedness is also an area of symmetry that is charged with philosophical implications. Immanuel Kant[6] wrote about the puzzle of the isometric left and right hands that cannot be made to coincide in space and called the nonsuperposable mirror images 'incongruente Gegenstücke' (incongruent counterparts). Then, of course, Lord Kelvin[7] gave a definition for chirality that has stood the test of time, 'I call any geometrical figure, or group of points, chiral, and say that it has chirality if its image in a plane of mirror, ideally realised, cannot be brought to coincide with itself.'

The early success of the chirality concept culminated in Pierre Curie's statement in an 1894 paper,[8] 'c'est la dissymétrie qui crée le phénomène' (dissymmetry creates the phenomenon). This most fundamental symmetry principle means that a phenomenon is expected to exist and can be observed only if certain elements are absent from the system. The forerunner of this principle was Franz Neumann's statement[9] in 1833 that 'the physical properties of crystals always conform to the symmetry of the crystal.'

Pierre Curie did not write much about symmetry and he did not live very long, but Marie Curie and the Russian crystallographer Aleksei Shubnikov did much to convey Pierre Curie's teachings on symmetry to a broader circle of scientists and thereby to help preserving the life of Louis Pasteur's teachings in this area of science.

Returning to Pasteur's story, it is important to stress that his discovery of molecular chirality did not happen out of nowhere. Pasteur himself stated that 'Dans les champs de l'observation, l'hasard ne favorise que les esprits préparés' (In the field of observation, chance only favors those minds that have been prepared). In fact, Pasteur's preparation for his discovery of molecular chirality was so perfect that to the famous biologist, Dubos,[10] 'it appeared as if fate had brought together many influences to prepare Pasteur for his scientific adventure.' He was a well-trained chemist with a definite idea about the importance of molecular structure, and he was also a crystallographer, who viewed the crystals as carriers of chemical information. The chemical and physical methods appeared in unison in Pasteur's mind.

It was not only that various areas of science came together in Pasteur's discovery, the discovery gave rise to new branches in science as well. The emergence of stereochemistry was one of the consequences of Pasteur's discovery although it came a quarter of a century later. The discovery also brought about the realization that, in living organisms, biologically important substances occur in one of the two possible versions. This also led to the great question, 'How did it all start? What was the way one of the two was chosen?' This question deeply bothered Pasteur and a century later Vladimir Prelog called this a question of 'molecular theology' in his Nobel lecture. Pasteur is buried in the chapel of the Pasteur Institute and the key phrases of his scientific activities inscribed on the chapel walls include *dissymétrie moléculaire*.

Pasteur considered the asymmetric nature of living matter as a fundamental characteristic. Experimentation with molecular asymmetry was always on his mind. Even as late as 1886, he discussed the two asparagines,[10] one of which is sweet while the other insipid. He suggested that the difference might be due to the difference in their actions on the two antipodes of the asymmetric constituents of the gustatory nerve. There are many conspicuous examples of different actions by enantiomeric isomers of various drugs. Suffice it to mention thalidomide, which was known as Contergan in Europe with which many tragedies were connected before it was withdrawn from the market. Since 1992, the U.S. FDA and the European Committee for Proprietary Medicinal Products have required manufacturers to research and characterise each enantiomorf of a potential drug.[11]

In 1960, there was a short note in *Nature*,[12] in which the British physiologist and geneticist John Haldane returned to Pasteur in the wake of the discovery of the violation of parity. The title was 'Pasteur and Cosmic Asymmetry,' and Haldane showed that the roots of Lee and Yang's[13] discovery were in Pasteur's notion 'The universe is dissymmetric.'[14] Parity was, of course, only the first example found to violate symmetry principles in the weak interaction. Next was the so-called CP symmetry violation (C stands for charge conjugation, meaning the change of a particle into an antiparticle, and P stands for parity. The combined CP symmetry means the change from a left-handed particle to a right-handed antiparticle, for example). The discovery of CP violation had conceptually profound implications concerning our ideas about the origin of the universe.[15] As T. D. Lee[16] has noted recently, 'The origin of these symmetry violations is still a mystery.' However, if we consider the combined CPT symmetry (here T stands for time reversal, which is a mirror symmetry with respect to time just as parity is a mirror symmetry with respect to space coordinates) that is not broken; CPT is a very solid symmetry.

The legacy of Louis Pasteur is rich in scientific achievements that have greatly contributed to the improvement of the quality of life. It is also rich in questions that are the best stimulants for scientific inquiry and they continue to help us in charting our labors in uncovering nature's secrets.

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Eyes on the prize[1]

J. Michael Bishop, How to Win the Nobel Prize: An Unexpected Life in Science
Harvard University Press 2003, 320 pp

Istvan Hargittai

If offered reincarnation, the Nobel laureate J. Michael Bishop would choose to come back as a musician (with exceptional talent, to be sure), because he thinks that one life-time as a scientist is enough. The son of a Lutheran minister, he grew up in rural Pennsylvania, and became enchanted with research during his last years at Harvard Medical School. He has been at the University of California, San Francisco since 1968, and worked for 15 years with his former postdoctoral associate, and ultimately fellow Nobel laureate, Harold Varmus.

Their work had its roots in the discovery of a cancer-causing virus in chickens by Peyton Rous in 1911, who was awarded the Nobel Prize fully 55 years later. Five individuals then went on to win Nobel Prizes for related work. David Baltimore, Renato Dulbecco and Howard Temin won in 1975 “for their discoveries concerning the interaction between tumour viruses and the genetic material of the cell”, and Varmus and Bishop became Nobel laureates in 1989 “for their discovery of the cellular origin of retroviral oncogenes”. Baltimore and Temin found the viral enzyme reverse transcriptase, which allows RNA to be copied into DNA, a reversal of the normal flow of genetic information. This discovery could have been Bishop’s had he been more daring. However, he learned his lesson and was fortunate enough to get another chance.

The discovery of oncogenes (cancer genes) raised the question of whether such genes might be present in the genetic composition of normal as well as cancerous cells. Locating them carried the promise of understanding human cancer at the genetic level. At first it was thought that oncogenes were viral genes, but Bishop and Varmus discovered that they were cellular genes that had been kidnapped by the virus. It took their team four years to identify them.

Bishop quotes a beautiful description of a moment of discovery by one of their post-docs, Dominique Stehelin: “The intensity of the emotion I experienced and the intellectual clarity induced by the situation at that moment were very special.” Furthermore, “I suspect that few have the privilege of enjoying such a moment when one is intensely and profoundly aware that a major step forward in Science has been made, and that one has contributed to it.” Alas, the quote was from an open letter to the Nobel Committee by Stehelin, who was not among the Nobel awardees. For every Nobel laureate there are others who might have also been included but were not, and every story about how to win the Nobel Prize may have its counterparts.

Bishop does not give a recipe for winning the prize, as any attempt to emulate a particular research career would be doomed to fail. However, throughout the book, he makes important points that budding scientists may find useful. For example, it is more useful to learn from one’s peers than from one’s teachers. Start a research career in a place where you feel genuinely needed, rather that choosing somewhere for its prestige. Being a pioneer in

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[1] Nature 2003, 423:921

research is fun, although it may bring more fame to be part of a team completing a discovery. Give a name to your discovery as soon as it is made. And finally, Bishop points out, good scientists should also market their ideas well.

Nobel laureates often seem to be standard-bearers for good causes, usually by signing petitions or making statements about issues with which they may not even be too familiar. Bishop's involvement in public causes has been different. He actively organized the participation of scientists in a non-partisan movement to increase legislative attention for science. Their high-level lobbying helped to achieve record support for research from taxpayer's money in the United States.

Bishop compiled his experience and ideas in this book for the general public. He also provides a crash course on the microbial world that is a gem of instruction without being condescending. And his copious use of art, including poetry, is a statement about the unity of the two cultures.

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EDITORIAL

Glenn T. Seaborg; discoveries; and the capital of knowledge

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Abstract Ten years from Glenn T. Seaborg's death we remember his achievements; his teaching about the importance of basic research is as timely as ever.

Keywords G. N. Lewis · Nuclear chemistry · Transuranium elements · Presidential advising · Twenty-first-century chemistry

> "… knowledge capital—a product of basic research—… might also allow us to compensate somewhat for declining physical capital and higher cost resources."
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> Glenn T. Seaborg [1]

Glenn T. Seaborg (1912–1999) with ion-exchanger column of actinide elements in 1950 (courtesy of Lawrence Berkeley National Laboratory)

A giant of chemistry departed a decade ago the importance of whose oeuvre extends much beyond anniversaries; yet 10 years from his passing away provides a nice opportunity to make a special remembrance of him. He was born in a little mining town Ishpeming in Northern Michigan 1912, where his father was a machinist, which Seaborg thought was as close to science as somebody could

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be in that environment. His entire family was Swedish and Swedish was the first language Seaborg learned to speak. In 1951, he started his Nobel address in his mother tongue. He shared the chemistry award with Edwin M. McMillan "for their discoveries in the chemistry of the transuranium elements. It was a long way from Ishpeming to Stockholm.

When Seaborg was 10 years old, the family moved to California, where he graduated from high school in Los Angeles in 1929. He became a student of the University of California at Los Angeles and received his bachelor's degree in Chemistry in 1934. For graduate studies, he moved to Berkeley and took his Ph.D. degree in Chemistry in 1937. He wrote his thesis about the inelastic scattering of neutrons. Following the receipt of his doctorate, Seaborg served as Gilbert N. Lewis's personal assistant at Berkeley for 2 years. Seaborg wrote warmly about this unique experience [2]. When he was asked to identify the greatest scientists he met during his long career, he named Lewis and Enrico Fermi.

Seaborg worked with an unusually large number of people on his many discoveries. He contributed to the discovery of 10 new elements and over a hundred new isotopes of elements. Much of his career was at the University of California at Berkeley where he became instructor of chemistry in 1939, assistant professor in 1941, and professor in 1945.

Edwin McMillan led a group, which discovered element 93 by making uranium capture a neutron and, following beta-emission (the ejection of an electron from the nucleus) the element of atomic number 93 was formed. They called it neptunium, Np, after the planet Neptune orbiting next, outwards, after Uranus. After McMillan's departure for other defense-related research, Seaborg and his colleagues took over the project. They detected the next transuranium element, formed by another beta-emission; it had atomic number 94. It was given the name plutonium, Pu, after Pluto, orbiting next outside Neptune, which at that time was considered to be a planet though today it no longer is. The nuclear reactions are depicted here in short-hand notation:

$^{92}\text{U-238} + \text{n} \rightarrow {}^{92}\text{U-239}$

$^{92}\text{U-239} \rightarrow {}^{93}\text{Np-239} + \beta$

$^{93}\text{Np-239} \rightarrow {}^{94}\text{Pu-239} + \beta$

In 1941, Seaborg, together with Emilio Segrè and Joseph W. Kennedy, showed that plutonium was fissionable and it became the fuel of the second atomic bomb exploded over Nagasaki in 1945. In 1942, Seaborg joined the Manhattan Project and became a group leader at the Metallurgical Laboratory at the University of Chicago. Here it was that the non-fissionable uranium-238 isotope was converted into plutonium-239. The procedure was further developed at the Clinton Engineer Work in Oak Ridge, Tennessee, and served as the basis for the breeder reactors at the Hanford Engineer Works in Washington.

During World War II, there were frenetic activities in the research of the properties of newly discovered transuranium elements. Manuscripts describing the results were duly compiled and submitted to journals, but were voluntarily withheld from publication until the end of the war. Thus, for example the pivotal paper "Properties of 94(239)" was received by The Physical Review on May 29, 1941, but appeared only in the combined numbers 7 and 8, Volume 70, in October 1946.

After World War II, Seaborg returned to Berkeley, but remained also part of national politics through his much appreciated advising from President Truman to President Reagan. He was a member of the General Advisory Committee (GAC) at the time of the great debate about the issue whether the United States should embark on an accelerated program of developing the hydrogen bomb.

The GAC was an advisory body consisting of important scientists, which augmented the Atomic Energy Commission created after the war for directing American policy in matters of nuclear energy. The GAC held long sessions at the end of October 1949 and the outcome of the GAC meeting concerning the development of the hydrogen bomb could not have been easily predicted. On the one hand, there was the Soviet menace whereas on the other hand, the hydrogen bomb, utilizing thermonuclear reaction of fusion of light nuclei was promised to be a thousand times more powerful than the atomic bombs. Gradually, however, the scale during the sessions was increasingly shifting toward opposing a crash program to develop the thermonuclear bomb. The only dissenting voice was Glenn T. Seaborg's, who was the only member absent from the meeting, but who had sent a letter to the chairman of the GAC.

There were two crucial sentences in Seaborg's letter that showed unambiguously his stand in the matter of the discussion. Both sentences were formulated with utmost care and one can almost sense the tormenting hesitation of their author: "Although I deplore the prospects of our country putting a tremendous effort into this [the thermonuclear bomb], I must confess that I have been unable to come to the conclusion that we should not." Then, a little later in the letter, "My present feeling could perhaps be best summarized by saying that I would have to hear some good arguments before I could take on sufficient courage to recommend not going toward such a program."

Concerning his dissent from the rest of the GAC members in his letter to Oppenheimer, Seaborg could have raised his objections upon his return, during November and

December, but he did not. At the time, Seaborg was a junior member of the GAC, who, eventually, would develop into a seasoned diplomat in addition to being a world-renowned scientist. Apparently he preferred to keep quiet for the duration of this debate. As is well known President Truman decided to have the hydrogen bomb developed. For a long time it was not known, but we know it today, that at the time of the American debate, the Soviet Union had already been deeply involved in developing its thermonuclear weapons.

Seaborg served as chairman of the U.S. Atomic Energy Commission for longer than anybody, between 1961 and 1971. During this decade he spent a lot of time in Washington, DC, whereas at other times he continued his research and educational activities at Berkeley. Considering Seaborg's principal role in the discovery of plutonium and in the determination of its properties, an embarrassing scene played out at a Senate hearing in 1970. It demonstrated the ignorance of the chairing senator when he asked Seaborg derisively, "What do you know about plutonium?" [3]. However, such episodes were rare and Seaborg enjoyed being involved in high politics for decades. Seaborg served 10 American presidents. He started keeping a journal at the age of 14, which was at the time of the Coolidge administration and published his documents and lessons from his encounters in 1998 [3].

Seaborg received many awards and distinctions, but none gave him as much joy as having an element named after him. In 1995, he was greatly disappointed when it seemed that this would not happen on account of his being alive and the appropriate organizations did not want to name an element after a living person. This followed a long story of sorting out the priorities in the discovery of element 106, because the discoverers have the right to propose a name for a new element.

The original discovery happened in 1974, and in 1993, the eight discoverers—members of the Lawrence Berkeley, including Seaborg, and the Lawrence Livermore Laboratories—were asked to suggest a name for the element. The votes diverged greatly; suggestions included Luis Alvarez, Frédéric Joliot, Isaac Newton, Thomas Edison, Leonardo da Vinci, Christopher Columbus, Ferdinand Magellan, Ulysses, George Washington, Peter Kapitza, Andrei Sakharov, and the country Finland. The group (without Seaborg) soon came together in a unified suggestion to name the element seaborgium after Seaborg. The final decision was made in Geneva on August 30, 1997 and seaborgium was adopted for element 106. Alas, Seaborg could not enjoy this new fame for long; 1 year later he suffered a stroke and died in half a year.

Of course, Seaborg's name is commemorated not only by element 106, but also by his many other discoveries. He gave the periodic table of the elements its final form in that he designated the actinides their proper place. The actinides with atomic numbers 89, 90, 91, etc., are characterized by their 5f electron shell being gradually filled. The series starts with actinium just as the lanthanides start with lanthanum. The actinides have similar chemical properties; absorption spectra in aqueous solution and crystals; crystallographic characteristics; magnetic susceptibilities; and spectroscopic data [4]. When Seaborg came to his new theory of the actinide series, he shared it with some colleagues before he published it. People warned him that publishing his theory might ruin his reputation. This sort of caveat is common when discoverers come to revolutionary ideas. Seaborg, however, felt very sure of the correctness of his theory; besides, he did not think he had yet gained much reputation yet to ruin. He published his theory and gave a new appearance to the periodic table of the elements [5].

Seaborg's fascination with the new elements was shared by others. An article in the magazine Discover in 1998 compiled an address of an imaginary letter to Seaborg using only element names in the following way:

seaborgium (addressee: Seaborg)
lawrencium (Lawrence Laboratory)
berkelium (Berkeley)
californium (California)
americum (United States)

Seaborg himself gave the answer to the question of why it was so important for him to have an element named after him. He said that even one thousand years from now it will still be seaborgium whereas by then what he did would probably have long before disappeared in oblivion. He would have gladly traded away his Nobel Prize for having the element 106 named after him, had such an exchange been possible. In the end all turned out to be all right. Incidentally, when Seaborg noted that sooner or later his works will disappear from collective memory, he did his best to slow down such a process. He himself edited the publication of his selected papers and furnished the collection with his commentaries. Characteristically, the volume was titled Modern Alchemy referring to his feat of turning even ordinary elements into gold, alas, the economy of the process was not viable [6].

Part of Seaborg's legacy is his teaching and this is why, in conclusion, we quote Seaborg's thoughts for the future, which he sent one of us shortly before he passed away [1]:

Glenn T. Seaborg and István Hargittai at the Springer-Verlag booth of the American Chemical Society spring 1995 meeting in Anaheim, California (by an unknown photographer)

Some Thoughts for the Future An important factor in the future, transcending the science of chemistry, will be the new public attitudes toward basic science and science in general—that is, the growing attitude toward ethical and human value considerations. The focus of this concern often is not on the question of whether the work is worth doing but instead on whether its potential harmful impact may outweigh any good it could do —that is, whether the research or project should be initiated at all. This attitude is affecting work on energy resources and technologies, biological research, aircraft development, and advances in the social sciences and education. This is going to have an increasing effect on the support and conduct of science, and I think most scientists are recognizing this.

As in the other cases of new influence, it is going to have its good and bad effects. Essentially, it is vital that science does serve the highest interest of society and contribute to the fulfillment of human values. And I believe that the science community for the most part is acting very responsibly and responsively in this direction. In many areas of research, such as genetic experimentation, atmospheric work, and the effects of chemicals on human health and the environment, it has taken the lead in placing human concerns above all.

But it should be realized that while there are certain values and ethical codes of a universal nature, there are also values that are more closely associated with the tastes, likes and dislikes, habits, and culturally induced beliefs of various individuals and groups attuned to certain so-called lifestyles. In a democratic society—and particularly one of growing advocacy and activism—there are bound to be many conflicts over these. And science and technology, with their increasing influence on life in general, certainly will be caught up in many of these. If this is the case, it may be essential that we find a way to establish some broad codes of conduct and values by which we can use science and technology to maximize human benefits within a framework of some type of consensus value scale. It seems to me that we must do this in order to avoid being paralyzed by a kind of case-by-case value judgment of all that we do. This does not mean that technology assessments and risk/benefit studies of individual concepts should not be conducted. Nor does it mean that science should not maintain a most profound sense of responsibility toward safeguarding society from possible errors on its part or misapplications of its work. It does mean, however, that we must find a way to avoid having a “tyranny of parochial interests” when it comes to the possibility of advancing the general good through scientific progress.

Perhaps I can summarize by suggesting that future directions of chemistry, and science and technology in general, may be influenced by two broad goals: more fully establishing the boundaries—physical, environmental and social—in which we can operate; and providing the knowledge capital that will allow us to operate within them. That knowledge capital—a product of basic research—upon which we have drawn so heavily in the recent past and which we must replenish with new ideas might also allow us to compensate somewhat for declining physical capital and higher cost resources.

Finally, a few general thoughts. Our success in chemistry, and science in general, over the past century, and especially the last few decades, has brought us to a high level of material affluence, but this success also has fostered many new problems for the world. It also has given many people the notion that science should move us toward a utopian, problemless, riskless society. But this is a false notion. We live and always will live in a dynamic situation, amid problems whose solutions will breed other kinds of problems, and in a society where the leaps of progress will be proportionate to the risks taken. Even within the bounds of a “steady-state society,” a “no-growth society,” or any other scheme of population-resource-energy equilibrium we might achieve, there always will be change and creative growth that will challenge the human intellect. There always will be dangers, risks, and increasing responsibilities that will drive us toward a new level of excellence in all we do or try to achieve. This is the process of human evolution at work, a process that started with man's ascendancy and will continue for some time.

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EDITORIAL

The Human Genome Project—A triumph (also) of structural chemistry: On Victor McElheny's new book, *Drawing the Map of Life*

István Hargittai

Abstract Structural chemistry greatly contributed to the feasibility of the Human Genome Project (HGP) by the discovery of the double helix structure of DNA. Victor McElheny's new book *Drawing the Map of Life* paints a panoramic picture of the story and the expected benefits of the HGP.

Keywords Double helix · DNA · Human Genome Project · James D. Watson · J. Craig Venter · Personalized medicine

The significance of the Human Genome Project (HGP) is difficult to overestimate and could be compared only to that of very few other grand projects such as harnessing nuclear energy, space exploration, interstate highway systems, transcontinental railroads, flood-control dike systems, and a few others. Its costs are lower but its long-range impact is greater than those of some of the others. For structural chemistry, the HGP is unique among these extraordinary projects because our domain of science has been part of the foundation of molecular biology through the discovery of the double helix structure of DNA.

Science journalist and biographer Victor McElheny has now published a book, *Drawing the Map of Life: Inside the Human Genome Project* (New York: Basic Books—A Merloyd Lawrence Book, 2010), which is worthy of close attention. The Sydney Brenner quote introducing it is surprising at first glance as it says that "progress in science depends on new techniques, new discoveries and new ideas, probably in that order." Intuitively one might assign preference to new ideas rather than to new techniques. However, closer scrutiny of various developments justifies Brenner's words. Thus, for example, one of the most crucial developments on the road to understanding the human genome—Frederick Sanger's discoveries of sequencing first proteins, then nucleic acids—clearly depended on new techniques in chromatography and elsewhere. Without them Sanger might have not even embarked on these tasks, but while working on his projects, Sanger himself became a great toolmaker.

Thus, at the start, McElheny justifiably focuses on the tools that eventually led to the HGP. These tools included enzymes, instruments, chemicals, and mathematical approaches, among them statistics. As molecular biology is in fact the conglomerate of all techniques used in modern biological research related to finding out about the molecular basis of life with genetics as a main focus, the bits and pieces communicated here come together as a rough history of this branch of science. And it is a highly personalized history: the discoveries and innovations are introduced along with some basic information about their principal protagonists.

At some point, there was a dilemma whether to wage a comprehensive attack in deciphering the human genome or continue to concentrate on various diseases in a piecemeal manner, one after the other and often by randomly looking for the genetic markers associated with their manifestations. It was soon recognized that everything could be done faster and more economically if the human DNA were deciphered in its totality at once. Crucial changes were taking place in the 1980s. This was not only in scientific techniques, but also in public awareness and, accordingly, in the political climate for considering the importance of

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what has come to be called “genomics.” The involvement of politics was justified not only due to the expected benefits from the new science but also because new dangers—real or perceived—had to be handled as well.

When recombinant DNA initially became a possibility in the mid-1970s, the scientists themselves initiated policing (see, the famous Asilomar meeting), accompanied sometimes by hysterical reactions in the general public, but things eventually returned to normal. With the accumulating information from mapping the genes and from sequencing DNA, it was increasingly apparent that a larger-scale effort would be needed and that the ultimate goal should be the complete sequencing of the human genome. Some of the opponents of such a project hastened to point out that it would not be the final goal because knowledge of the complete sequence could not yet mean medical applications. However, it became clear that this was an unavoidable step.

The work had to take place on several fronts simultaneously. Information was to be collected about the sequence, but technology improvement was to be continued if sequencing was not to be considered a single project since it was realized that for true applications, i.e., discovering the genetic basis of diseases, numerous individual DNAs would have to be sequenced.

Questions such as the relationship between government-funded work and private enterprise came into the forefront just as it did between university research and commerce. The HGP was to be enormous with a price tag of an estimated 3 billion dollars. Other questions arose as well, such as whether DNA base sequences were patentable or not. And reality was moving rapidly without waiting for the outcome of lengthy deliberations.

The first explicit calls for a project to sequence the human genome emerged as early as the mid-1980s. It was to become the task of a new generation, because the scientist most noted for his discoveries in sequencing, Frederick Sanger, retired in 1983, and the first big meeting about the HGP took place in 1985. When Sanger was asked at the time whether such sequencing should be done, he unequivocally supported such a project.

Everything seemed to be moving toward the HGP. Small organisms but of ever increasing complexity became subjected to complete sequencing and the best scientists were vocal in advocating the need for such a project and in actually participating themselves. This included people like Sydney Brenner, who had been one of the pioneers in establishing the genetic code, and Renato Dulbecco.

There were also opponents, to be sure. They were apprehensive about big science entering the traditionally small science arena of biology, but it was recognized that the scale of a possible HGP would truly have to be big science. On the other hand, there was no real danger of concentrating all the HGP in one huge center; rather, it was anticipated that big science in this case would mean a well-coordinated assembly of little science projects. Initially, there was a gap between the older scientists who supported the big project and the young ones who saw it not only as an infringement on their toiling ground but as a sponge absorbing the support that could have gone to more diverse projects. There was the promise that rather than depleting ongoing projects in biology, the HGP would be subsidized with additional funds.

One of the moving forces was another promise implicit in all the plans for the HGP and that was its potential in fighting cancer. Another was the benefits expected from the technological innovations for all biological science. Still another driving force—more influential than anybody else—was James D. Watson. Initially, in the mid-1980s, he was lukewarm toward such a project, however, he quickly warmed toward it, and he had excellent resources to back up his efforts. He had the Cold Spring Harbor Laboratory behind him and he had his ways to influence the general public and, in particular, the media as well as his fellow scientists. In addition, the presumably genetically based illness of one of his sons became publicized and added a personal touch to his involvement, and generated additional trust and sympathy toward what he advocated.

At one point, it was no longer a question of whether the HGP should be set up but rather at which institutions, with what framework, and under whose leadership it should operate. The Howard Hughes Foundation—the largest private organization of its kind—quickly bowed out. The U.S. Department of Energy (DOE) with its vast experience in big projects, such as those at Los Alamos and Livermore, was willing and interested. The DOE was not without prior involvement in biological research either. Nonetheless, the most logical choice seemed to be NIH due to its enormous funding of biomedical research and its responsibility for advances in human medicine where the HGP was expected to bring most of its benefits. Ruth Kirschstein, in charge of the National Institute of General Medical Sciences, was made responsible for studying the possibilities of the HGP on NIH’s part. It was realized that the HGP would be much less expensive than the space program and probably more directly benefiting human life.

One of the preconditions for a meaningful HGP was the recognition of the universality of the genetic code, which by this time had been established. This is something we take for granted but initially it could not have been. The universality of the genetic code itself was something that had to be established. It impressed me when Marshall Nirenberg, who had accomplished the first step in cracking the genetic code, told me that it was a profound moment when he realized this universality. It had an almost religious significance on the non-religious Nirenberg. He considered it an expression of the unity of Nature.

The preparations for the HGP involved U.S. legislation and Watson along with a few others got involved in informing members of Congress. Watson was such an important component that he appeared even to have a say in which of the two principal contending agencies should be involved, DOE or NIH, in administering the project. It was then also almost inevitable that he was chosen to initially direct the efforts with an appropriate title in the NIH administration. McElheny thoughtfully enumerates Watson's traits, both favorable and not so favorable, as a leader of the HGP. Although it could be doubted from the start whether Watson would have the stamina to carry on this function for long, it had great significance that he was at its helm at its very inception.

Watson's assuming the leader's role of HGP was advantageous because his fame added visibility to the project and generated additional trust. Now there was someone—someone well known, that is—who appeared to take responsibility for this great excursion into the unknown. Watson himself considered it important to have someone at the helm of the project who could take the blame in case of failure. An additional benefit was his authority among scientists who could be induced to join the project answering his call. He knew that success or failure depended to a large extent on the quality of cadres he would be able to attract. It was characteristic that a leading scientist suggested that offering sufficient cash would be attractive enough to recruit the right people. In contrast, Watson warned that such an incentive would attract the wrong people. He wanted to have people who were too busy to join, who had a lot of things going for them, and for whom merely a lot of money would not be decisive in making a career move. Watson's dedication to the HGP could not be demonstrated better than by pointing out that he continued his lobbying for it even after he had been "fired." One of his most telling personal imprints on the project was the allocation of a percentage of the HGP budget to studying its ethical, social, and moral issues. There was a lot of relevance to these questions and the emphasis on their studies from early on enhanced public trust in the project.

As the question of the scaling up of the genome project arose, various further considerations had to be addressed. They included multifaceted supports of the project, the dilemma between technological innovations and continuous use of more traditional methodology, and how the HGP functioned as a peculiar big science being constituted of numerous small-scale projects. Even though Watson's directorship did not last long, it appeared that it was crucial that he had had his imprint on it from the start. His scientific authority could not be questioned and his dedication was unconditional. Though he might have been forced out for reckless statements, it was his interest in some biotech companies that was used as a pretext to cause him embarrassment. Still, one has the impression that he might have been grateful for it is difficult to imagine him lasting too much longer in a bureaucrat's role. Amid bickering among various governmental branches, science kept going on, producing complete mappings and even sequences of organisms of ever increasing complexity.

Patenting appeared to be a crucial question, that is, whether the human genome and its portions could be patented at all. Curiously, there was not a clear-cut divide between those who supported patenting and those who did not. One might have expected the big pharmaceutical companies to prefer patenting, but they recognized its dangers and opted instead for the public domain approach. In contrast, I remember how dedicated Walter Gilbert appeared to me, during a personal encounter at a meeting "Frontiers in Biomedical Research" in Indian Wells, California, on February 2, 1998, to seeing his company earn money from genetic tests based on such patenting. I remember it because I had been on a low-salt diet from my youth due to my tendency toward elevated blood pressure. The test would determine whether people inclined to have such a condition would or would not benefit from a low-salt diet. When Gilbert told me about it the test had been on the market for just a few weeks.

An obvious player in scaling up was Craig Venter who had started at NIH, but moved out, and became the most conspicuous player in the private enterprise sector of the human genome race. At some point, Francis Collins succeeded Watson at the helm of the HGP. Even though Watson no longer occupied any formal position in the project, he continued his role behind the scenes and utilized his enormous authority for gathering support in Congress and elsewhere for the project. As Venter aggressively pursued his goals, he mobilized tremendous funds from the private sector for the project. It meant not only more monies but also more competition. The situation resembled the stimulating effects of excellent private universities on the state universities.

There were reasons for alarm as well. Venter's private sequencing company would release sequence data at 3-month intervals; thus, the private company would have considerable advantage in developing diagnostic tests and eventually drugs, and they would even patent genes and important functions. The British were especially vigilant in not letting patenting become a barrier to public access to the benefits of the HGP. Their efforts were financed by the Wellcome Trust, a private foundation.

It was realized from the start that deciphering the human genome would only be the beginning in revolutionizing the medical sciences. Various aspects were coming into the

A few of the principal players on the road to the Human Genome (all photos by and © of István Hargittai). From the top row, left, and in each subsequent row, from the left, Werner Arber, 2005; David Baltimore, 2004; Seymour Benzer, 2004; Paul Berg, 1999; Elizabeth Blackburn, 2003; Sydney Brenner, 2003; Erwin Chargaff, 1994; Francis Crick, 2004; Walter Gilbert, 1998; François Jacob, 2000; Ruth Kirschstein, 2000; Arthur Kornberg, 2001; Joshua Lederberg, 1999; Maclyn McCarty, 1997; Matthew Meselson, 2004; Kary Mullis, 1997; Daniel Nathans, 1999; Marshall Nirenberg, 1999; Richard Roberts, 2003; Frederick Sanger, 2001; Phillip Sharp, 2001; Maxine Singer, 2000; Hamilton Smith, 2001; Gunther Stent, 2003; John Sulston, 2003; Harold Varmus, 2002; Craig Venter, 2007; Robert Waterston, 2003; James Watson, 2000; Charles Yanofsky, 2006 forefront that would show the way to utilization of the information from the HGP in human medicine. One of them was the determination of disease-causing variation—the change of a single nucleotide for another, called also the single-nucleotide polymorphism or SNP. A whole new area of biomedicine, "pharmacogenetics," was emerging. In the meantime, private companies flourished on NASDAQ due to the promise of personalized medicine with the yet more attractive goal of preventive medicine appearing close to reality in the not so distant future. On March 14, 2000, President Bill Clinton and Prime Minister Tony Blair took a strong stand against patenting, and, as a consequence, the shares of private companies dropped and the NASDAQ index slashed considerably, after they had, previously, skyrocketed.

Various analogies have been introduced to stress the importance of deciphering the human genome. A parallel was drawn between the periodic table of the 100 chemical elements as the guiding principle in twentieth century chemistry and the knowledge of the tens of thousands of genes of the human body in the biomedical sciences of the twenty-first century. There seemed to be a competition of superlatives in characterizing the importance of the human genome in which Watson appeared to be among the most restrained when he declared "…it's the script of life. It's the information for the play of life" (p. 161). Watson's preeminence in the project was acknowledged by President Clinton when he turned to Watson saying, "Thank you, sir" (p. 165). The occasion was a joint, electronically linked White House–10 Downing Street event on June 26, 2000, declaring the next triumph of the HGP.

The flood of information from the HGP gave hope for attacking numerous diseases, but the data had broader implications as well. It was established that well over 99.9% of the genome is the same in all humans and in this light the concept of "race" was fast losing importance. At the same time, the 0.1% still represented the possibility of 3 million differences among the 3 billion nucleotides. The possibilities of utilization were enormous, ranging from diagnostic tools to drug development, to genetic screening, forensic applications, and coming to decisive information in paternity disputes. One of the goals was the mapping of genes associated with every inheritable disease. Parallel to the virtually limitless potentials of the benefits of the HGP, possible dangers were also emerging. The information might make genetic discrimination possible in employment, insurance, and elsewhere. It might be that skin color and gender would not be the most decisive factors in discrimination, but it could become the variations in a person's DNA. So far, seven diseases have been identified as linkable to DNA mutations that included manic depression (bipolar disorder); coronary artery disease; irritable bowel syndrome (Crohn's disease); hypertension; rheumatic arthritis; type I diabetes; and type II diabetes. This is in addition to the susceptibility genes already known for breast/ovarian cancer, colon cancer, and Alzheimer's disease that were found using the targeted disease-based approach.

It was an event announced with big fanfare when in 2007 Watson and Venter posted, simultaneously, their own full DNA sequences on the Internet. Their purpose was to combat the fear on peoples' minds when they thought about the genome project. This reality was also demonstrated by Watson. He said that he was glad he had sons rather than daughters because his genome showed familial tendency for breast cancer. Even more to the point, he held back one section of his DNA that might have revealed information to him about his chances for Alzheimer's. He declined to learn about his chances for developing this awful condition. Venter wrote a whole book in connection of the publication of his DNA, *A Life Decoded*. He called it his genomic autobiography.

Of course, for developing personalized medicine the DNAs of thousands of others will have to be sequenced. However, this appears increasingly realistic. Sequencing the first human genome cost 3 billion dollars, Watson's price tag was a mere 1 million—a drastic decrease within only a decade and a half. The most immediate goals of reaping the benefits of the HGP would be pinning down the causes of mental illness and autism. The next would be establishing diagnostic tools for various cancers and finding their treatments. Tall orders to be sure, but considering the pace of progress in recent biomedicine there is justified optimism about reaching these goals.

McElheny's book is a great service for a broad audience in disseminating reliable knowledge in an accessible way. His background eminently qualified him for producing such a book. He was a science journalist during the decades that led to the HGP and during its initial periods. He worked for years at the Cold Spring Harbor Laboratory. He wrote an excellent biography of James D. Watson, *Watson and DNA*. It appears, the importance of the topic and the preparedness of the author made a perfect match and the result is an informative and readable account of the most important scientific project of our time (the photo collage shows some of the principal players on the road to the Human Genome).

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EDITORIAL

Linus Pauling's quest for the structure of proteins

István Hargittai

Abstract Linus Pauling, arguably the greatest chemist of the twentieth century, never publicly admitted that there was a race for the determination of the structure of the most important biopolymers. But according to his competitors there was a race, in fact, there were two, and Pauling won one and lost the other. He had a tremendous amount of ideas, many of them worthless, but a few were spectacular. Not only did he make seminal discoveries, he was also a master of announcing them in a most dramatic way. Eventually, Pauling shifted toward politics and controversial issues, but his science ensured him his place among the greats. Here, we follow Pauling's route to the discovery of the alpha-helix; the defeat of the star-studded British team in the same quest; and a seemingly unrelated story about the fate of the theory of resonance that assured Pauling's victory yet at the same time it was excommunicated in the Soviet Union.

Keywords Linus Pauling · Structure of proteins · Theory of resonance · Peptide bond · Alpha-helix

For most of the first half of the twentieth century a large number of scientists were not even sure biopolymers existed. The view survived for long that the principal components of living matter were in a colloidal state, that is, conglomerates of smaller molecules. It was only in 1953 when Hermann Staudinger was awarded the Nobel Prize in Chemistry for his discoveries about macromolecules that the existence of polymers was irrevocable accepted. By then, though, a lot about the structures of the biologically important macromolecules had been discovered. Thus, during the first half of the twentieth century, the efforts to establish the nature of biological substances and to uncover their structures went in parallel.

The British father and son team, W. H. Bragg and W. L. Bragg, pioneered the technique of X-ray diffraction crystallography in 1913, with the son playing the leading role. When the two Braggs were awarded the Nobel Prize in 1915, the son became the youngest ever Nobel laureate and has stayed the youngest to this date. After a hiatus due to World War I, this field took off spectacularly in the realm of small molecular systems. As early as the 1920s, fibrous materials were subjected to X-ray diffraction for the first time by Michael Polanyi in Herman Mark's laboratory at the Kaiser Wilhelm Institute in Berlin. Polanyi's experiments on cellulose indicated the presence of crystallites in cellulose and they were oriented in the direction of the fiber axis. He could not have performed a full structure analysis at that time, but Mark and Polanyi observed characteristic changes on stretching the cellulose fibers. Mark was to become one of the century's foremost polymer chemists. When he was forced out of Germany, he moved to his native Vienna where he helped one of his students, Max Perutz to be accepted as a doctoral student in Cambridge, UK, in 1935. Perutz would become a key player in the quest for the structure of proteins. But he was only slowly moving to the area of his ultimate success when Linus Pauling was already a major force in the field.

Pauling came from a humble background, but he was ambitious. He lost his father when he was 9 years old and his mother found it difficult to cope with her obligations. She certainly did not appreciate her son's intentions to stay

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in school even when it was no longer mandated for him to continue his studies. Pauling's schoolings were not at top places, and when in 1922 he went to the California Institute of Technology (as it was later; Caltech in short) it was far from the preeminent research-oriented institution into which it would develop. But the school was as ambitious as its new student, and there were visionary movers of it who were set to making Caltech a top-notch institution of higher education and research. They were smart enough not only looking into the distant future and only for big names to recruit from faraway places, but recognized in Pauling the potentials of a star scientist who would even challenge British preeminence in the science of chemical structures.

When Pauling started his doctoral studies with Roscoe Dickenson, a fresh home-grown PhD in X-ray crystallography at Caltech, this field was less than a decade old. Pauling became engaged in the determination of the structure of many inorganic and organic molecules and amassed a large amount of information about them during the ensuing decade. What kind of information was that? It was about the geometrical arrangement of the atoms in the molecules and the arrangement of the molecules in the crystals.

Not all the modern knowledge was to be had at Caltech at the time, and not even in other laboratories in the United States. The leading country of science was Germany and a few other places in Europe, and Pauling—like many other aspiring American scientists—paid pilgrimage to a series of European research centers in order to learn from the likes of Arnold Sommerfeld in Munich and Erwin Schrödinger in Zurich. They were both physicists, but Pauling's aim was not to transform himself into a physicist. Rather, his goal was to apply the latest discoveries in physics, and above all the new quantum mechanics, to solving a wealth of problems in chemistry in which he proved to be unique.

The most intriguing question in chemistry at that time was about the forces that keep the atoms together in a molecule, that is, about the nature of the chemical bond. If there is anything truly associated with Pauling's name, it is the understanding the nature of the chemical bond. He used the achievements of modern physics, the experimental information about the geometry of molecules and his thinking, to put together a theory. He then kept refining it in accordance with the emergence of the latest experimental information. The science of chemistry has a great deal of intuitive approach in it, very often stemming from a desire to represent on paper what the chemists experience in the laboratory. Thus, for example, they started using a straight line connecting the symbols of two elements to represent their bonding without really understanding anything about what that straight line represented. Nowadays when we know so much about what it means, we still find this straight line an excellent representation of the chemical bond. Lewis's description of the covalent bond in 1916 was not much less intuitive than this; nonetheless he made a big step forward. He introduced the idea of the shared electron pair, meaning the covalent bond between two atoms. During the late 1920s two physicists, Walter Heitler and Fritz London used the new quantum mechanics and their sophisticated mathematical apparatus to rigorously describe this covalent bond. It was so rigorous that it was too sophisticated for most chemists to understand it let alone to apply it to solving their problems that were usually more complex than the hydrogen molecule for which Heitler and London had worked out their theory.

Linus Pauling bridged this gap in a series of brilliant articles in the *Journal of the American Chemical Society*. Eventually he developed his ideas and his repository of structural information into a bestseller *The Nature of the Chemical Bond* [1]. Its last, third edition appeared in 1960 and many of the later stars of chemistry benefited from it by getting their introduction to the intricacies of this branch of science. A new book would be timely, but nobody seems brave enough to try filling Pauling's shoes in producing a new comprehensive monograph about the chemical bond.

Had Pauling produced his series of articles about the chemical bond, and nothing else, he would have already written his name into the annals of the history of chemistry. However, he did not limit his interest to theoretical studies. He utilized X-ray crystallography broadly and was constantly on the lookout for new techniques. While in Europe, he visited Herman Mark's laboratory in Ludwigshafen, Germany (where he was at the time), and Mark introduced a new experimental technique to his visitor for the determination of molecular structure, gas-phase electron diffraction. It was similar to X-ray crystallography, but there were two major differences. It used electrons rather than X-rays and the target was not a crystal but a gaseous sample in which the molecules had no well-defined order in their mutual arrangements.

One of the great advantages of using electrons was the very high intensity of the interaction between electrons and molecules. Thus, the duration of the required interaction was measured in minutes rather than many hours as with X-rays. The other important advantage was that in the gaseous sample the molecules were by themselves and their structures were not impacted by the closeness of their neighbors. For the X-ray technique, the molecules were required to be able to form a crystal in the first place, and there was no such requirement for using the electron diffraction technique. The structures determined by the new technique depended only by the molecule itself and not by the way they were arranged relative to each other as was the case in the crystal. Other limitations of the new technique, however, have restricted it from becoming so widely used as X-ray crystallography, which truly has been the preeminent tool for uncovering the structures of biopolymers.

Mark's industrial laboratory was not the proper environment to expand the studies of molecular structures and he happily offered Pauling to take the new technique with him to Caltech. Mark even supplied him with the blueprints of his apparatus. Pauling not only introduced the gas-phase electron diffraction technique quickly in the United States, but he and his student, Lawrence Brockway further developed it. They added a mathematical step to handling the experimental data that made it possible to extract structural information in a graphically direct and attractive way from the probability density distribution of the internuclear distances in the molecule (usually it is referred to as the radial distribution curve, which is a misnomer). From the experiment to reading off the curves directly the distances between atoms in simple molecules took only a few days' work.

Pauling (Fig. 1) established relationships among various experimental facts and made predictions about structures not yet investigated. He then worked out a theoretical technique based on quantum mechanics, but simple enough for a broad circle of chemists, to describe molecular structures. It was called the valence-bond or VB theory and it was one of the two major theoretical approaches developed over the decades. The other is the molecular orbital or MO theory. The VB theory builds the molecules from individual atoms linked by electron-pair bonds. For chemists, the VB theory appealed as more straightforward, alas, it did not stand well the test of time. The MO theory has proved more amenable to computations, which itself has become a major thrust in modern structural chemistry. However, for a long time the VB theory dominated the field.

Fig. 1 Ava and Linus Pauling (photograph by and courtesy of Karl Maramorosch, Scarsdale, NY)

An important feature of the VB theory was that a molecular structure could be described by a set of "resonating" structures. This did not mean that each structure in such a set would be considered as present individually, but that the sum of these resonating structures represented the emerging structure better than any other description at the time. It needs to be stressed that what the resonance theory provides is merely a model, an approach, rather than a unique reflection of reality. There were proponents and opponents of the theory as is the case with most theories. Yet the resonance theory proved to be eminently useful for Linus Pauling—who was one of its initiators—in his quest for the protein structure. It happened so that this theory showed him the way and brought him a resounding victory over his competitors who lacked this tool and could not arrive at the right solution.

Pauling was advancing in a systematic manner in his quest for building up structural chemistry. First, he busied himself with inorganic substances and after the first 10 years he moved to organic substances. Among the organic molecules he often observed structures in which the lengths of the bonds between atoms were intermediate between single bonds and double bonds, so the theory of resonance came in handy in their understanding and description. Today, chemists no longer tend to think in terms of purely single bonds and double bonds, or triple bonds for that matter, and, accordingly, the utility of the resonance theory has largely disappeared, but in the 1930s it was considered to be of great help.

As Pauling was learning more and more about the structures of relatively simple molecules, in the mid-1930s, it occurred to him that he might as well make an attempt to learn about larger systems. He was aware of the importance of biopolymers and that the understanding of their structures might be a step toward understanding biological processes. Proteins were an obvious choice, because they were the most important biopolymers. At that time nucleic acids were already known, and their building blocks, the nucleotides, had been identified, but the nucleic acids were not considered to be of great significance. There was a hypothesis by Phoebus Levene about the tetranucleotide structure that was based on an erroneous observation that the four nucleotides in nucleic acid were present in equal amounts [2]. Hence, the nucleic acids were thought to be dull, uninteresting molecules, not capable of carrying any great amount of information.

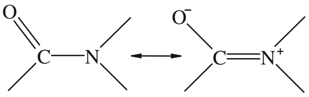
When Pauling started thinking about protein structures, the first protein to attract his attention was hemoglobin, which is the vehicle of carrying oxygen in our organism. Incidentally, the British group engaged in protein structure studies had also selected hemoglobin for their target; their choice was independent of Pauling's interest. At the end of the 1920s, Gilbert Adair in Cambridge, UK, showed that the hemoglobin molecule consists of four units each with an iron atom, and each iron could bind an oxygen atom. Pauling formulated a theory about the oxygen uptake of hemoglobin and the structural features of this molecule related to its function of disposing of and taking up oxygen.

His interest in protein structures was further whetted when a visiting scientist and protein specialist, Alfred Mirsky of the Rockefeller Institute, spent the academic year 1935–1936 in his laboratory. They jointly studied the phenomenon of denaturation of proteins by heat or chemical substances, and formulated a theory about it. In this theory, they described the native protein as having a regularly folded structure in which hydrogen bonds provided the stability of the structure. Hydrogen bonding was a recently discovered phenomenon; it was becoming recognized as a crucial mode of interaction in chemical structures and especially in those of biological importance. In retrospect, it was a pivotal discovery, but its significance emerged only gradually over the years. For many biological molecules it is the hydrogen bonds that keep their different parts together.

Pauling postulated that the subsequent amino acid units are linked to each other in the folded protein molecule not only by the normal peptide bond but also by hydrogen bonding that is facilitated by the folding of the protein, which brings the participating atoms sufficiently close to each other for such interactions. In Pauling's and Mirsky's conclusion, when the protein molecule is denatured it undergoes complete or partial unfolding accompanied by breaking the hydrogen bonds. This was a hypothesis, because they knew practically nothing about the nature of folding; finding more about it occupied Pauling's mind for the next 15 years.

By the time Pauling became engaged in this research it had been established from rudimentary X-ray diffraction patterns that there might be two principal types of protein structure. Keratin fibers, such as hair, horn, porcupine quill, and fingernail belonged to one, and silk to the other. The foremost British crystallographer of fibers, William T. Astbury showed in the early 1930s that the diffraction pattern of hair underwent changes when it was stretched. He called the one producing the normal pattern alpha keratin and the other, which was similar to the pattern from silk, beta keratin. In 1937, Pauling set out to determine the structure of alpha keratin. He did not just want to rely on a single source of information. He planned to use all his accumulated knowledge in structural chemistry and find the best model that would make sense on this background and would be compatible with the X-ray diffraction pattern.

There was one piece of information from X-ray diffraction that seemed to be a good point of reference and that was the structural unit—whatever it would be—along the axis of the protein molecules repeated at the distance of 5.1 angstrom. He also knew the dimensions of the peptide group, that is, the characteristic sizes of the group linking the amino acids to each other in the protein chain. The C–N bond in the peptide linkage was not simply a single bond, but it was not a purely double bond either. Pauling's involvement with the resonance theory taught him that the emerging structure could be represented by two resonating structures



Hence, the resonance theory suggested that the C–N bond in the peptide linkage had a partial double bond character. From the accumulated structural information he also knew that the bonds around a double bond are all in the same plane. This was a very important piece of information because rather than taking into account all kinds of rotational forms with respect to the peptide bond, he could assume that it was a planar configuration. This assumption greatly reduced the number of possible models he had to consider for describing the structure of alpha keratin. Nonetheless, at this time Pauling was unable to find a model that would fit the X-ray diffraction pattern and he postponed further study on protein structures.

During the ensuing years Pauling and his newly arrived associate, Robert Corey, an expert in X-ray crystallography, carried out a large amount of experimental work determining the structures of individual amino acids and simple peptides. At some time every doctoral student in Pauling's laboratory was supposed to determine the structure of an amino acid for his PhD dissertation. The study was interrupted by World War II, but continued vigorously upon its conclusion. Pauling returned to the question of the structure of alpha keratin in 1948 while he was a visiting professor at Oxford University in England.

Not only had the amount of experimental information in the meantime expanded considerably, but Pauling could take a more detached view of the problem in his renewed efforts. When he was looking for the solution more than a decade before, he was bothered by the knowledge that his model was supposed to accommodate the possible presence of 20 different amino acids in the protein chain. At this time, in 1948, he decided to ignore their differences and assumed them to be equivalent for the purpose of his model. This was yet another example of Pauling's ability to distinguish between essential features and those that could be ignored in building his models.

Pauling remembered a theorem in mathematics he learned about at Caltech a quarter of a century before. It stated that the most general operation to convert an asymmetric object into an equivalent asymmetric object is a rotation–translation and that repeated application of this operation produces a helix. Here the asymmetric objects are the amino acids constituting the protein chain; the rotation should take place about the molecular axis of the protein; and the translation is the movement ahead along the chain. The amount of rotation was such that took the chain from one amino acid to the next while the peptide group was kept planar, and this operation was being repeated and repeated all the time. An additional restriction was keeping the adjacent peptide groups apart at a distance that corresponded to hydrogen bonding. In Pauling's model the turn of the protein chain did not involve an integral number of amino acids—he did not consider this a requirement whereas his British counterparts did. This was yet another relaxed feature of the structure that served him well in finding the best model whereas it served as an unnecessary restriction for his competitors.

Pauling—ever the model builder—sketched a protein chain on a piece of paper and folded the paper while looking for structures that would satisfy the assumptions he had made (Fig. 2). He found two and called one the alpha helix and the other the gamma helix, the latter being much less probable than the former. He determined the distance between repeating units in the protein chain and noticed a marked difference between his estimation from the model and the experimental value from the diffraction pattern. This was disappointing but the model was so attractive and so sensible that Pauling had little doubt in its correctness. Nonetheless, he decided to wait with its publication until the discrepancy would be understood. His confidence was enhanced when he visited the British group involved also in the structure elucidation of proteins and Max Perutz showed him his diffraction patters. From the X-ray diagrams it was obvious to Pauling—though not yet to Perutz—that the structure was alpha helix. Pauling did not say anything to Perutz.

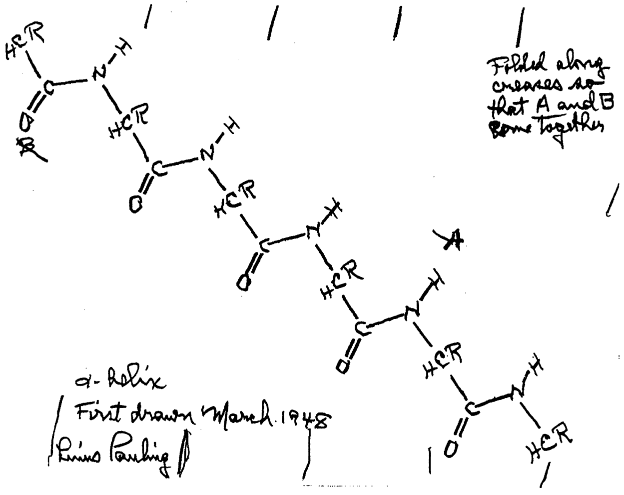


Fig. 2 Linus Pauling's sketch of the polypeptide chain in 1948. When he folded the paper along the creases, the alpha-helix appeared [3] (Fig. 3)

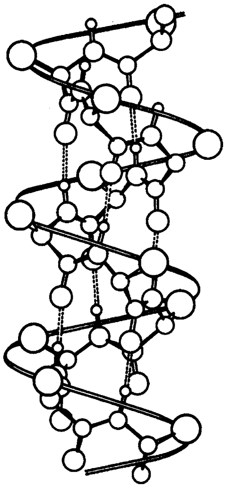


Fig. 3 Model of alpha-helix with 3.7 amino acid residues per pitch after [4]

When Pauling returned to Pasadena, he and his associates double checked all his calculations and found no errors in them. In the meantime, after about a year, Bragg, Perutz, and John Kendrew of Cambridge, UK, published a big article about protein structures and communicated about 20 models, none of which contained a planar peptide group and none of which described alpha keratin satisfactorily [5]. Finally, Pauling decided to ignore the discrepancy of the repeat distance between his model and the experimental observation and he and his associates published the alpha helix.

Eventually, the origin of the discrepancy was understood; it was caused by the alpha helices twisting together into ropes. This interaction between the chains caused a change in the experimental data as compared to what it would be for a single chain for which the model had been constructed. Thus, Pauling's alpha helix was confirmed even in this detail. The alpha helix has proved to be a great discovery because it is a conspicuously frequent structural feature of proteins.

Pauling's approach to solving this complex problem was exemplary in focusing on what was essential and ignoring what had little consequence. When it turned out that the turn about the chain did not correspond to an integer number of amino acids, hinting at less than perfect symmetry, he did not let himself bothered by this. He thus expanded the realm of crystallography toward structures that were not part of classical crystallography yet included literally vital substances. It was also noteworthy that he could skip a decade in working on this most important discovery without much danger of others scooping him.

They almost did, but only in their timing and not in knowledge, because his knowledge proved to be superior to anyone in his field at that time.

Pauling must have sensed the precarious nature of the situation and restrained himself from revealing crucial information to Perutz during his visit to Cambridge (vide supra). The Cambridge X-ray diffraction pattern showed the helical nature but Perutz did not think about it and thus did not notice it whereas for Pauling it provided additional evidence of the correctness of his model. This episode showed both his competitive spirit and his self-discipline. Finally, Pauling was sure enough in himself and his model that he went ahead with publishing the alpha helix without having yet resolved the remaining (apparent) discrepancy between his model and the available experimental evidence. First they published a short note [6], followed by a longer article [7] and soon they wrote seven more papers to report their findings.

Pauling was a master in creating publicity for his discoveries. When he prepared for announcing the discovery of the alpha helix it was to be in a big lecture hall at Caltech. The model stood on the rostrum, but it was under a cover, waiting to be unveiled, just as a sculpture would be, and it came toward the end of Pauling's lecture. When it was finally unveiled, the effect was dramatic and the audience was stunned by its beauty. I myself experienced the mesmerizing effect of Pauling's lecturing at the University of Oslo in 1982. He covered the board with complicated formulas and from time to time he looked at the audience as if checking whether we were duly impressed. Otherwise, the formulas were not at all necessary for us to understand the points he was making. He was already an octogenarian, but watching him gave an impression of a young assistant professor who came for interview and was presenting his research with the usual arrogance of such scenes. During the lunch following the lecture he was more vigorous than the rest, led the discussion, and fired away questions, mostly answering them himself.

In research publications there is no place for the human sides of the discoveries and Pauling wrote up the story of his alpha helix discovery separately, but it never appeared while he was alive. It was published 2 years after he died when I was running a chemical magazine and his former secretary of his last 20 years, Dorothy Monro, suggested to bring it out there. Research papers usually lack the human element and the blind alleys in research, so this paper by Pauling was especially valuable for our understanding how this particular discovery happened [3].

The Cambridge group suffered a defeat in this case, which was especially heavy for W. Lawrence Bragg to bear, because he was the pioneer of X-ray crystallography and the American group came out on top in their undeclared race. It was not possible to pinpoint a single reason for this defeat, but it was a crucial difference that Pauling could limit the number of possible models because of his superior knowledge of structural chemistry. The Cambridge group had no such guideline although it could have. It turned out that Lord Todd the soon to be Nobel laureate organic chemist who worked in the next building to Perutz's and Kendrew's laboratory had told Bragg that the peptide bond had some double-bond character. Bragg, however, could not from this piece of information make any conclusion about the configuration of the peptide bond, namely, that it was planar.

Years after this fiasco, Perutz complained about their lack of knowledge of the planarity of the peptide group. He blamed the Medical Research Council (MRC) for having him denied the use of a Rockefeller Fellowship for travel to America in 1948. The Secretary of the MRC thought that rather than going to learn from the Americans, the Americans should come and learn from the British. In hindsight, Perutz thought that he could have learned about the peptide bond planarity from Pauling had he been allowed to travel [8]. Of course, he could have just walked across the street to visit Lord Todd for the same information.

It is not at all sure whether had Perutz visited Pauling he would have learned from Pauling as much as he might have supposed in retrospect. We have seen Pauling withholding his observation from Perutz that he had noticed the evidence of helical structure on Perutz's X-ray diffraction diagram. During his Oxford sojourn, Pauling wrote to Corey back to Pasadena that he felt uncomfortable about the English competition. In their turn, the British considered protein crystallography their own territory. It was not only that the Braggs discovered X-ray crystallography and that Astbury was a pioneer in taking X-ray pictures of proteins. It was also J. Desmond Bernal who had prepared the first ever X-ray diffraction diagrams of a single-crystal protein—a pepsin single crystal—that clearly showed the possibility of deducing atomic positions from it. This was in 1934. In the future Nobel laureate X-ray crystallographer Dorothy Hodgkin's description, “that night, Bernal, full of excitement, wandered about the streets of Cambridge, thinking of the future and how much it might be possible to know about the structure of proteins if the photographs he had just taken could be interpreted in every detail” [9]. The British self-confidence in dominating this field reached such proportion that Astbury and Bernal divided it by a gentlemen's agreement between the two of them. They decided that Bernal would take up the investigation of the crystalline substances and Astbury the fibrous ones [10].

Perutz on his part, for their failure blamed Astbury's X-ray diffraction picture, which showed a discrepancy between the repeat distances as compared with reasonable structures, a discrepancy—as we have seen—Pauling daringly disregarded. Perutz was disheartened when he found

Pauling's paper about the alpha helix model. He devised an additional X-ray experiment that gave further evidence for the correctness of Pauling's result, something that Pauling had missed. When Perutz reported his finding to Bragg, Bragg asked him, "How did you think of that?" Perutz's response was that it was because he was so angry that he hadn't thought of the structure himself. To which Bragg replied coldly, "I wish I'd made you angry earlier" [11]. Perutz told me this story in 1997, and he used this phrase as the title of his next book. Perutz might have thought that Pauling would be pleased that he provided additional evidence for alpha helix, but was disappointed by Pauling's reaction, which was clearly dismissing.

Pauling's fascination with proteins served him well in his focusing his attention to their structures at a crucial period in twentieth century science. However, he continued his protein bias even when the next big task appeared before structural chemistry that was the structure of nucleic acids. Pauling entered that race too, but there is ample evidence that Pauling did not concentrate on it with the intensity and dedication as he had done for the protein structures. In case of the quest for the structure of nucleic acids he was defeated by the British teams. Pauling published an erroneous triple helix and he was not in possession of the best X-ray diffraction patterns of nucleic acids that were available at the early 1950s either. As is well known, those patterns were produced at King's College in London and the winning double helix model came out from the Cavendish Laboratory in Cambridge, UK, but this is a different story.

We can add a footnote about Pauling's theory of chemical resonance, which served him so well in the above story. At about the same time, this theory was in the center of attack by rabid ideologists in the Soviet Union [12]. The culmination was a 4-day conference in Moscow in 1951, organized by the Soviet Academy of Sciences. Leading Soviet chemists, physicists, philosophers, and others attended the meeting. A small but vocal group of chemists attacked the theory of resonance as an ideological aberration and together with it quantum theory and the science of the West. They insisted on returning to traditional Russian values and offered their own worthless theories. Excellent scientists suffered ruthless criticism for having applied the theory of resonance in their work, and they, in turn, offered humiliating self-criticism.

The affair has been referred to as the great Soviet resonance controversy and it was a chapter in the anti-science events following World War II that touched biology even more severely. Physics was spared in the last minute due to its decisive role in producing nuclear weapons. Stalin's terror did everything to protect his empire from even the slightest influence by the West, the purest sciences included. There was irony in this story in that Pauling was a friend of the Soviet Union and suffered persecution in the McCarthy era, but this was not yet known in the Soviet Union. In 1993, I asked Pauling for his comments about this affair. He appeared as if he misunderstood it or did not want to understand it. He wrote that it took years "for the chemists in the Soviet Union to get a proper understanding of the resonance theory" ([12], p 5). In reality, they understood it well enough and applied it with great success, that is, until 1951, when the main proponents of the theory lost their jobs. If it was a consolation, their lives were spared in contrast with some of their biologist colleagues in a similar ideological controversy.

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Dan Shechtman's Quasicrystal Discovery in Perspective

Istvan Hargittai*[a]

Abstract: Dan Shechtman's discovery of quasicrystals brought about a paradigm change in chemistry, physics, materials science, and other areas of science and engineering. Although superficially it could be looked at as a serendipitous event, Shechtman's curiosity and drive played equal parts with serendipity in this discovery. Shechtman was a lonely discoverer, again, seemingly detached from the main stream of generalized crystallography for which his contribution was a milestone. Generalized crystallography is the science of structures without restrictions — "structures beyond crystals."[1] The discovery of quasicrystals can be seen as written into the history of ideas that have much extended our views about the tools of our scientific inquiry and the materials we aim at producing and utilizing. This review augments a recent Editorial in the August 2011 issue of *Structural Chemistry* about the lessons of the quasicrystal discovery[2] and a book chapter about Dan Shechtman's traits as a discoverer and about his road to the discovery.[3]

Keywords: Dan Shechtman · generalized crystallography · Nobel Prize 2011 · quasicrystals · scientific discovery · symmetry

1. Introduction

Dan Shechtman (Figure 1), winner of the 2011 Nobel Prize in Chemistry for the discovery of quasicrystals, exemplifies how curiosity and drive can lead to major scientific breakthroughs. Shechtman was preparing and investigating rapidly solidified aluminum-manganese alloys possessing properties that would make them useful for applications. He examined them under the electron microscope, and he varied their compositions within reasonable limits, looking for the most useful ones. At one point he reached the limit of the manganese content above which he knew the alloys would become too brittle for application and where he was supposed to limit his inquiry toward the larger manganese contents. This is what he should have done in a purely applied laboratory. In 1981, Shechtman had arrived for his first sabbatical at the National Bureau of Standards (NBS; today, National Institute of Standards and Technology (NIST)). His stay was sponsored by the US Defense Advanced Project Agency (DARPA, later, ARPA). When he started his studies, the person with whom he was supposed to check his plans for research told him to feel free to go in any direction he found worthwhile. This instruction gave Shechtman freedom when he reached the upper reasonable limit of manganese content. He did not feel he had to stop, and indeed, he started probing alloys with ever-increasing manganese content to satisfy his curiosity. Both his conditions of work and his personal traits carried Shechtman in this direction.

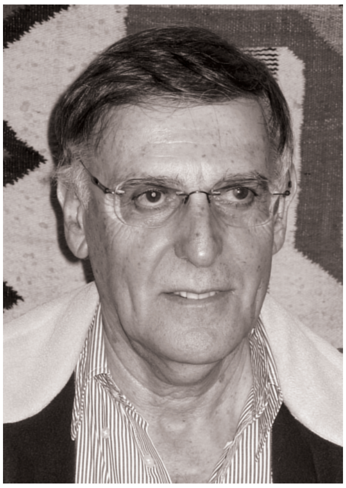

Figure 1. Dan Shechtman in 2007 in Budapest; photo by and © I. Hargittai.

Shechtman's drive manifested itself when he did not let benevolent colleagues, as well as those who ridiculed him, talk him out of pursuing the idea that what he had observed was what classical crystallography had deemed impossible symmetry (Figure 2). This drive kept him functioning in an intellectually belligerent world. Linus Pauling, the most authoritative chemist of his time, with great renown as far as structural chemistry was concerned, also found Shechtman's claims impossible. Despite Pauling's own reputation as innovative and a maverick, he could not come to terms with Shechtman's interpretation of the diffraction photographs. For example, Pauling in his quest for the protein structures was not bothered by the non-integer repetition of amino acid units along the molecular

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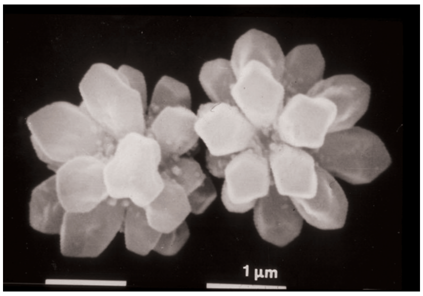

Figure 2. Flowerlike icosahedral quasicrystal in a quenched Al/Mn sample, courtesy of Ágnes Csanády, Budapest. Used with permission.

axis, because the presence of intra-chain hydrogen bonding precluded integer repetition.[4] It would be hard to imagine a more powerful opponent to recognizing Shechtman's discovery than Linus Pauling, but even this could not stop Shechtman's drive.

Shechtman was honored with many awards for his discovery, among which the Aminoff Prize occupied a special place because it was awarded by one of the most authoritative bodies of science, the Royal Swedish Academy of Sciences, expressly for recognition in the field of crystallography. This was in the year 2000, and many thought that while this was a very special distinction, it was also a subtle way to position Shechtman's discovery among important events in crystallography, without elevating it to the category of discoveries of more general significance. It has happened, but very rarely, that an Aminoff Prize laureate would later be awarded a Nobel Prize. I doubt that Shechtman did this consciously, but he dressed too formally for the prize-awarding ceremony, as if it were an event of higher importance. The unwritten dress code for the event prescribed a much less formal appearance. Secondly, he started his presentation by listing three discoveries related to new materials, of which two had already been awarded a Nobel Prize (high-temperature superconductivity and buckminsterfullerene). The third was the discovery of quasicrystals, and the implication was obvious.

Shechtman's Nobel Prize finally arrived in 2011. It is significant that he received it unshared, and — although some might have thought that the circle of awardees could have been expanded — no displeasure was expressed among the scientific community following this judgment. At this point, however, it is equally appropriate to view Shechtman's discovery in the context of the intellectual process that led to the development of what is called "generalized crystallography." The most august scientific body has now put its "stamp of approval" on this development.[5]

2. The Story

The quasicrystal story begins with John Desmond Bernal, who was the first to recognize the confining nature of classical crystallography, and he initiated generalized crystallography (Figure 3). He noticed that there are arrangements, especially among the low-coordination cases, both among organic and inorganic structures, where the classical restrictions of symmetry to two-, three-, four-, and sixfoldedness no longer hold.[6] He stressed that icosahedral geometry is not capable of forming regular extended arrangements, although it could provide close-packed structures. The absence of long-range order would account for the much greater variation of properties of such structures than the corresponding classical crystals. Bernal's conclusion was, "We clung to the rules of crystallography…, which gave us the 230 space groups, as long as we could. Bragg hung on to them, and I'm not sure whether Perutz didn't too, up to a point, and it needed Pauling to break with them with his irrational helix."[7]

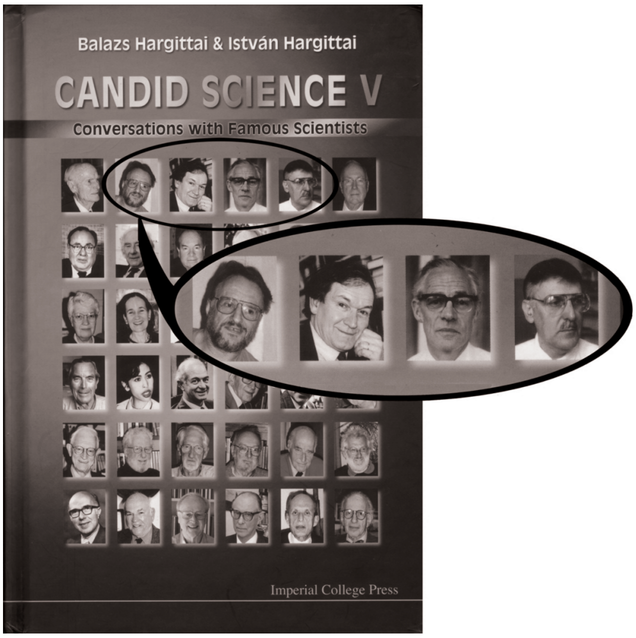

Figure 3. *Candid Science V* book cover highlighting John Conway, Roger Penrose, Alan Mackay, and Dan Shechtman (© I. Hargittai).

Looking back to Bernal's teachings (Figure 4) and the developments since, up to the quasicrystal discovery, a *fictional* story could be compiled of how the discovery *might* have happened — although it did not go this way:[8]

> For centuries excellent minds, including Johannes Kepler and Albrecht Dürer, have tried to employ regular pentagons for covering the extended surface with a pattern of repetitive fivefold symmetry without gaps or

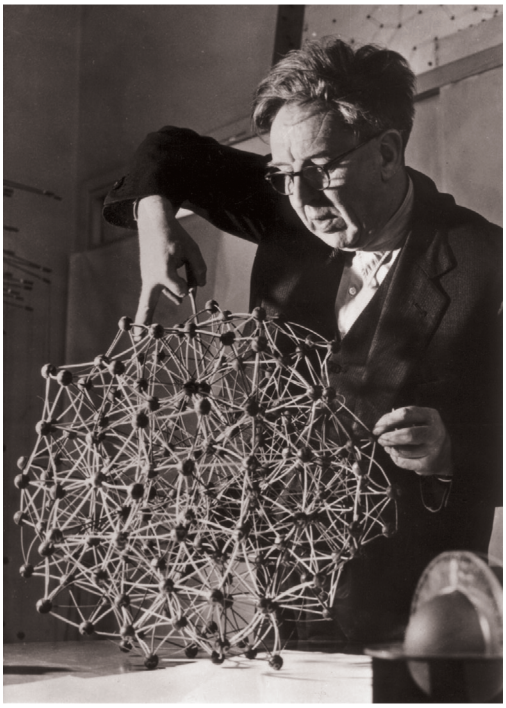

Figure 4. J. Desmond Bernal and his model of an ideal monoatomic liquid; courtesy of John Finney, University College London. Used with permission.

> overlaps. In the early 1970s, finally, Roger Penrose came up with such a pattern. Alan Mackay extended this pattern into the third dimension, and, by showing it was possible theoretically, he urged experimentalists to be on the lookout for such solids in their experiments. Taking up Mackay's challenge, Dan Shechtman then made such an observation. Shechtman used metal alloys of various compositions in rapid solidification. He anticipated that this rapid solidification of the alloys would produce the predicted structures. His experimental observations were published promptly and were embraced instantly by the leading scientists dealing with structures. His experimental observations were also interpreted right away by Paul Steinhardt and many others with various theoretical models. As a result of these *concerted* activities, the science of structures has fast expanded considerably.

In reality, everything was different: there were no concerted efforts, Shechtman was not aware of the previous attempts, and he made his observations serendipitously. Also, there was a long gestation period, two and a half years between April, 1982, and the fall of 1984, before Shechtman could publish his findings. That is when the broader scientific community learned about his discovery and responded with an avalanche of papers. The peculiarity of fivefold symmetry in this story is explained in Mackay's statement:[9]

> The main significance of fivefold symmetry for science is that it furnishes us with an explicit example of frustration, which has proved a most fertile concept in the physics of condensed matter… Neither we or nature can have everything simultaneously — not all things are possible,… We have only the freedom of necessity. "Nature must obey necessity", as Shakespeare (*Julius Caesar* IV: iii), Democritos, Monod, Bernal, and many others have also recognized. Science probes the limits of necessity and, in the case of fivefold symmetry, has found a corridor that leads us to a new territory.

My personal interest in fivefold symmetry remained at the hobby level, because in my research of molecular structures there was no restriction on this or other symmetries. But I found the issue intriguing and invited Alan Mackay to talk to us in Budapest about fivefold symmetry. In September, 1982, he gave us two lectures on this topic (Figure 5) and issued a warning that we should be aware of the possibility of extended structures of fivefold symmetry, because if we thought them impossible, they might go unnoticed and unrecognized. Mackay did not know, and, obviously, neither did we, that by then Dan Shechtman had already observed such structures. I will always remember our amazement at what Mackay told us, especially looking back; it felt as if we were present at creation.

Mackay was always interested in noncommensurate structures, and he considered simple things, like printing wallpaper. "…[S]uppose you are printing two motifs from two rollers of different diameter. Then you get a non-repeating pattern. I wasn't able to think of producing an aperiodic two-dimensional pattern in this way. … I was really interested in hierarchic patterns… It came directly from Bernal… I produced a hierarchic pattern of pentagons."[10] Mackay heard about the Penrose pattern, and contacted Penrose to discuss it. Mackay's interest in hier-

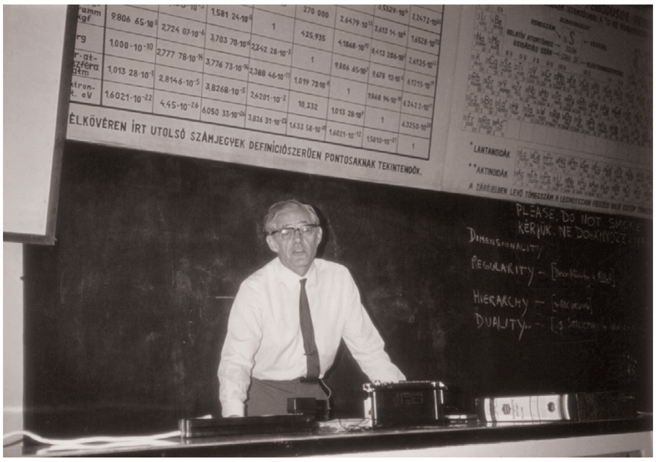

Figure 5. Alan L. Mackay lecturing on fivefold symmetry in September 1982, in Budapest; photo by and © I. Hargittai.

archic structures and Penrose's interest in forcing aperiodicity turned out to be very similar.

Roger Penrose started playing around with tile shapes and tiling problems. He was interested, for example, in the shape Maurits C. Escher used in his picture titled *Ghosts*. Penrose showed Escher his tiling, but for the time being these were periodic patterns. Then he became interested in hierarchical tiling and noticed a logo in a letterhead. The logo had a pentagon in the middle, surrounded by five others within a larger pentagon. He decided to iterate this, and sought a way of filling the gaps in a systematic way. In Penrose's words:[11]

> The only interesting thing is how to fill the gaps up. Thus I produced this pattern, which I designed partly to show somebody, who'd been in hospital, just as an amusement. A little later I realized that you could actually force that pattern by making it a jigsaw. There are pentagons, little rhombuses, five-sided, what I call jester's caps, which are half of them. The problem was to find a way forcing that pattern by some local matching rules. Having three versions of the pentagons and one of each of the others you could force it, so it was a six-piece tiling, which was non-periodic and which happened to have this fivefold quasisymmetry. But I wasn't thinking particularly to refute crystallography. It was just like an amusement.

Once Penrose had produced this tiling pattern, he published an article about it in 1974 in the *Bulletin of the Institute of Mathematics and Its Applications*.[12] The paper grew out of a lecture he gave on aesthetics. His lecturing about his patterns prompted him to think about possible applications in crystallography. Penrose thought that a generalization might be possible, and fivefold symmetry and icosahedral symmetry might occur in crystals. He thought an obstacle would be the impossibility of spotting mistakes and such events would prevent continuation. There were no local assembly rules and this is why he thought that it would be impossible to spot natural occurrences of what later became known as quasicrystals. This was at the time of our conversation in March, 2000, in Oxford (Figure 6). Within a decade, though, quasicrystals were found in nature.[13]

Penrose's paper in the obscure mathematical journal did not generate much interest. However, when Martin Gardner wrote about the Penrose patterns in *Scientific American*, interest was aroused.[14] Gardner had started corresponding with Penrose and he decided that these patterns deserved more exposure. The cover of the *Scientific American* issue in which the Gardner article appeared was designed by the mathematician John Conway.[15] At the time of the preparation of the magazine cover, Conway and Gardner conjectured about the possibility of crystallization, but they never published anything about their discussion, which Conway later regretted. In his words, "I remember that I wondered to myself how many different substances have been studied with respect to crystallization, and my guess was less than ten to the seventh power. Then I thought of the probability that something will crystallize in this manner and one in ten to the seventh power seemed to be a reasonable guess; therefore such crystallization should happen."[16]

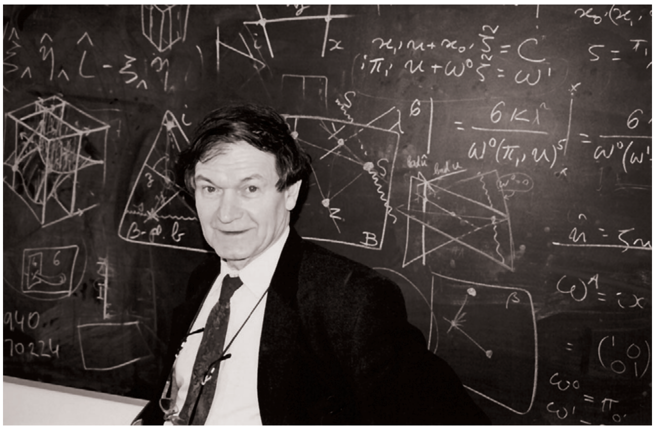

Figure 6. Roger Penrose in his office in Oxford in 2000; photo by and © I. Hargittai.

Alan Mackay continued to be intrigued by the possibility of the natural occurrence of three-dimensional Penrose patterns and, with assistance by others, he produced a simulated diffraction pattern from them (Figure 7). These simulated patterns would be found to be similar to the diffraction patterns in Shechtman's experiments in which he discovered "forbidden" symmetry (Figure 8). Shechtman and his colleagues were producing a series of aluminum–manganese alloys with increasing amounts of manganese in them. In Shechtman's own words from a conversation we recorded on May 14, 1995, in Balatonfüred, Hungary, during an international school on quasicrystals:[17]

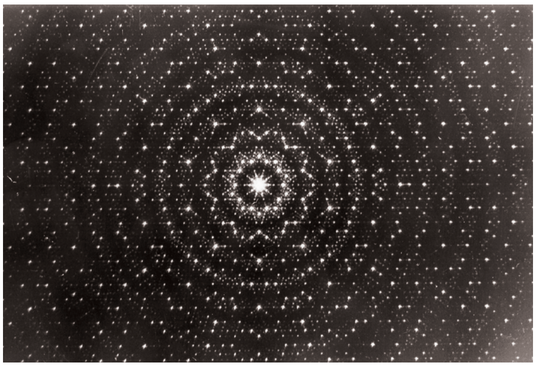

Figure 7. Simulated "electron diffraction pattern of three-dimensional Penrose tiling" in 1982, prepared for Professor Alan L. Mackay by Dr. G. Harburn at Cardiff University; courtesy of Alan Mackay, London. Used with permission.

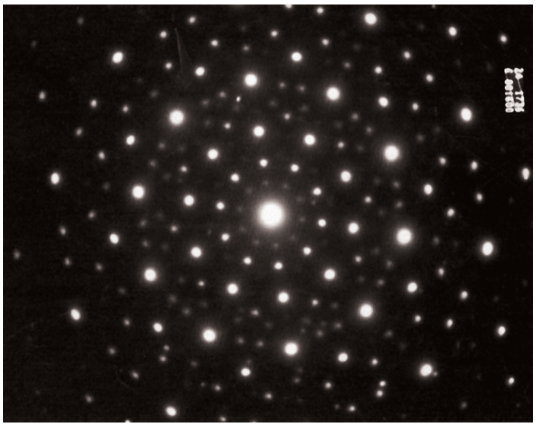

Figure 8. Dan Shechtman's electron diffraction pattern of an aluminum-manganese alloy with 10-fold symmetry; courtesy of Dan Shechtman, Haifa. Used with permission.

> Eventually I ran wild, from a practical point of view, since beyond a few percents of manganese the rapidly solidified alloy becomes brittle and therefore useless. Among the alloy ribbons which I have prepared with Frank Biancaniello by melt spinning, there were alloys which contained over 25 weight percent manganese. On April 8, 1982, as I was doing electron microscopy on a rapidly solidified aluminum alloy which contained 25% manganese, something very strange and unexpected happened. It is worthwhile to look at my TEM [transmission electron microscope] logbook records of that day. For plate number 1725 (Al-25%Mn) I wrote, "10 Fold???" There were bright spots in the selected area diffraction pattern, equally spaced from the center and from one another. I counted them and repeated them and repeated the count in the other direction and said to myself: "There is no such animal," in Hebrew, *Ein chaya kazo*. I then walked out to the corridor to share it with somebody, but there was nobody there, so I returned to the microscope and in the next couple of hours performed a series of experiments. Most of the needed experiments were performed at that time. A few days later all my work was complete, and everything was ready for the announcement. Then it took two years to publish it.

Shechtman first consulted his NBS colleagues, but they told him that he had either observed something else, or suggested to him to refresh his knowledge of diffraction theory. He knew what he was talking about and how powerful the dogma was about symmetry restrictions in the condensed state. He once had to prove it during an examination at the Technion. Shechtman knew that his observation was a lucky break, but was astonished over the years that a large number of knowledgeable scientists could not come up with an explanation. It might have helped him if he had known about Mackay's simulated diffraction experiment, but he did not (Figure 9).

Figure 9. Dan Shechtman and Alan Mackay in 1995 in the Hargittais' home in Budapest; photo by and © I. Hargittai.

There was a long, lonely period for Shechtman, and only his stamina and perseverance saved him from giving up. Eventually, his loneliness was eased by Ilan Blech, another Technion scientist, and the two produced a manuscript which they sent to the *Journal of Applied Physics*; it was returned by the editor with a note saying that their report would not be of interest to physicists. Later Shechtman also judged this manuscript as poorly written; a variation of it appeared later in *Metallurgical Transactions*.[18] The principal report about Shechtman's observation appeared under his name with three co-authors in late fall 1984.[19]

The announcement of the discovery was followed by frantic activities and an extraordinary number of publications in the years that followed. It appeared as if the scientific world had been ready for the discovery; thus, for example, theoreticians published models right away following the publication by Shechtman and his colleagues. The report by Dov Levine and Paul Steinhardt stood out not only because of their speed and their attractive model, but also because they coined the name "quasicrystals," which then stuck.[20]

The most conspicuous doubter of Shechtman's discovery was Linus Pauling (Figure 10); it was not the experiments he doubted, but the interpretation. Shechtman had several encounters with Pauling, but Pauling would not budge. He suggested that the observation originated from twin crystals. It is worthwhile to quote a sample of Pauling's statements in order to appreciate the formidable barriers Shechtman was facing in getting his discovery accepted. Following the success of a multidisciplinary symmetry volume in 1986, I edited a second volume in 1989, and Pauling wrote a paper for it with a long title: "Interpretation of So-called Icosahedral and Decagonal Quasicrystals of Alloys Showing Apparent Icosahedral Symmetry Elements as Twins of an 820-Atom Cubic Crystal." His stand was obvious already from the title, and he concluded his discussion with the following paragraph:[21]

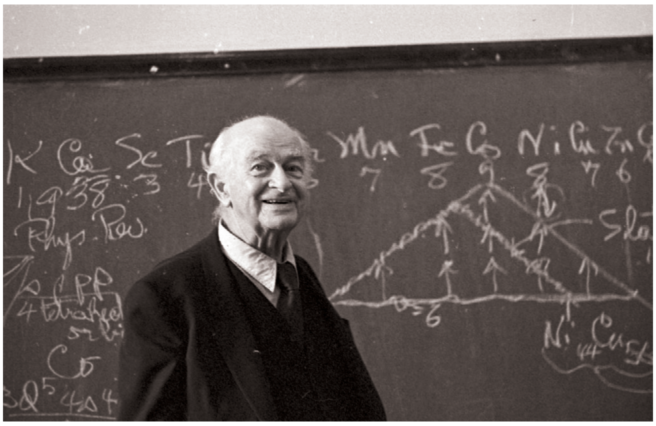

Figure 10. Linus Pauling in the early 1980s at Moscow State University; photo by and courtesy of Larissa Zasourskaya, Moscow. Used with permission.

> As a crystallographer, with 65 years of experience in X-ray crystallography, I am pleased that the problem of the so-called icosahedral quasicrystals has been resolved in this way. Crystallographers have believed for many years that crystals cannot have five-fold axes of symmetry. In my model the grains with apparent icosahedral symmetry consist of cubic crystals that have a conventional structure, but that have, by repeated twinning determined by the approximate icosahedral structure of the 104-atom clusters, arranged themselves into an aggregate of microcrystals that shows icosahedral symmetry.

In the fall of 1993, I asked the 92-year old Pauling again about his opinion of the quasicrystal discovery, and as it happened this may have been his last statement about this issue. Soon afterwards, he died. My questions referred to both the C_{60} and the quasicrystals discoveries, but of his responses I am quoting here only the one concerning the quasicrystals.[22]

> *Question*: Recent discoveries such as the quasicrystals and the fullerenes seem to have caught the solid state and chemical communities by surprise. Were these exceptional events or should we be getting prepared to seeing more of these kinds of unexpected findings in the future?
>
> *Linus Pauling*: As to the quasicrystals, you know that I contend that icosahedral quasicrystals are icosahedral twins of cubic crystals containing very large icosahedral complexes of atoms. It is not surprising that these crystals exist. The first one to be discovered was the MgZnAl compound reported by my associates and me in 1952. We did not observe quasicrystals of this compound, but they have been observed since then.

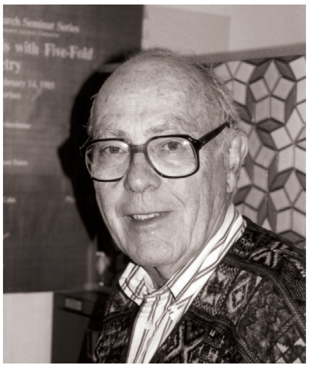

Figure 11. John Cahn at NIST in 1995; photo by and © I. Hargittai.

John Cahn (Figure 11) was a senior scientist at the time of Shechtman's stint at NBS, and for a while he also resisted accepting the quasicrystal discovery. He described how he heard about the discovery from Shechtman for the first time:[23]

> One day he came into my office, and said, "John, what do you think of a 10-fold axis?" I said, "Don't bother me, Danny, this is clearly twinning," and he said, "I don't think so." Then we discussed a number of experiments to decide this question. I didn't know much about twinning but I did know that through twinning you could get unexpected symmetries.

Two years after their first encounter about Shechtman's experiments, the two talked about it again when Shechtman returned for another visit at the NBS. By then the paper in *Metallurgical Transactions*[18] was already in production, and Shechtman showed the manuscript to Cahn. Cahn told Shechtman that the paper did not articulate Shechtman's discovery in any adequate way. Then, the following occurred, according to Cahn:[24]

> … in our conversation Danny at one point said, "If you feel so strongly about it, can you write this paper?" I said, "Danny, this is your work, you're making me an enormous gift." He said, "I don't mind." I began writing this paper for *Physical Review Letters.* I just wanted the data to speak for themselves, to show that they were not consistent with the paradigm of periodicity. The published paper is two and a half pages, and there are few things too many in it. One of the things I'm sorry about is that we said we couldn't fit the diffraction pattern to that of a periodic crystal; it couldn't be indexed. We should have said we cannot fit it to a periodic crystal up to a lattice parameter of a few nanometers. We should have been more specific because Linus Pauling noticed this and said that you can always fit something if you pick a large enough lattice parameter…

In the process of writing the paper, Cahn invited a young French theoretician, Denis Gratias, to join the team. This is how the four authors, Shechtman, Blech, Gratias, and Cahn, came together.[19] When the manuscript was ready, it had to be reviewed by the NIST Editorial Review Board. Since NBS had been burned in the polywater story, they were very careful. One of Cahn's friends warned him: "John, you have a wonderful reputation. Why ruin it by putting your name on something like such a paper."[25] Finally, however, the Board approved the manuscript and it could be sent off to the journal. As Cahn circulated preprints of the paper, it reached, among others, the theoretical physicist Paul Steinhardt (then) of the University of Pennsylvania who happened to be visiting at IBM at the time, and he showed it to his graduate student Dov Levine. Again, in Cahn's narrative: "… this was the first inkling that there was actually an explanation for the patterns we were seeing. Things moved very fast, and Steinhardt … was rushing his paper with Levine to *Physical Review Letters* and it appeared about a month later. I remember when I saw Steinhardt's copy of our manuscript it was almost illegible because it was a copy of a copy of a copy."[25]

Levine summarized the essence of their paper as follows: "We sought to elucidate the symmetries of quasicrystals by generalizing the Penrose pattern. We showed that orientational symmetries forbidden to periodic crystals are allowed for structures with quasi-periodic translational symmetry."[26]

Conclusions

In October 1994, I was having a conversation in London with Alan Mackay about the significance of the quasicrystal discovery. Mackay considered it as part of the bigger picture, on the background of Bernal's teachings about generalized crystallography, and said that the discovery might be considered to be "a bogus discovery because it arose simply because our definitions of crystallinity were drawn up rather carelessly. Therefore, it's a kind of legalistic discovery. It's a discovery of a material which breaks the laws that were artificially constructed. They were not laws of nature; they were laws of the human classificatory system."[27] Of course, with such an approach many other important discoveries might be considered merely legalistic if they uncovered phenomena that had not been covered by previous human description of nature, like superconductivity. It was obvious that Mackay's intention was not to belittle Shechtman's discovery. When, on the same occasion, I asked him about the Nobel Prize, he considered the various kinds of Nobel Prize and related Shechtman's discovery to other discoveries that had already been awarded this distinction. He described the discoverer of quasicrystals as[28]

> someone who turns over a stone and finds something really important, and recognizes that he has got something really important, maybe like superconductivity or the scanning tunneling microscope or the Mössbauer effect. There isn't any enormous amount of work but someone was in the right place at the right time, and recognized what he's done. I think Shechtman would come in [this] category. There is actually some new evidence that Shechtman's discovery may be more important than it had been believed. It has been mostly followed by a tremendous amount of mathematics, an Ivory Tower of mathematics and little more. Now it appears, however, that the very low thermal conductivity of quasicrystals may be useful for something more than the non-stick frying pan but also important as turbine blades, internal combustion engines, and so on. People are producing effectively quasicrystal surfaces by glazing metal with a laser. So Shechtman's discovery may be eventually related to a process of great economic importance.

Some have expressed surprise that Shechtman was awarded the Nobel Prize in chemistry rather than in physics. Apart from thinking in terms of Nobel Prize categories, or school subjects for that matter, his discovery could be assigned in modern terms to materials science, which is at the borderline between chemistry and physics with considerable overlap. The 2010 Nobel Prize in Physics for the discovery of graphene, for example, could have just as well been awarded in chemistry as in physics.[29] We are very much conditioned according to our school education, which with its "division into subjects creates the image of a compartmentalized world,"[30] whereas "Nature is not organized in the way universities are."[31]

What truly matters is that Shechtman's discovery was par excellence the kind of achievement that, in Eugene P. Wigner's formulation, was the task of scientific inquiry (when he mentioned physics, it was not a compartmentalized branch of science, but Science itself). The chemical-engineer-turned-theoretical-physicist Wigner (Figure 12) stated:[32]

> Physics does not endeavor to explain nature. In fact, the great success of physics is due to a restriction of its objectives: it only endeavors to explain the regularities in the behavior of objects. This renunciation of the broader aim, and the specification of the domain for which an explanation can be sought, now appears to us an obvious necessity. …
>
> The regularities in the phenomena which physical science endeavors to uncover are called the laws of nature. The name is actually very appropriate. Just as legal laws regulate actions and behavior under certain conditions but not try to regulate all actions and behavior, the laws of physics also determine the behavior of

Figure 12. Eugene P. Wigner and I. Hargittai in 1969, in Austin, Texas; by unknown photographer; © I. Hargittai.

> its objects of interest only under certain well-defined conditions, but leave much freedom otherwise.

In the main body of the present treatise much attention was paid to symmetry considerations. Indeed, "Symmetry is a stunning example of how rationally derived mathematical argument can be applied to descriptions of nature and lead to insights of the greatest generality."[33]

On a personal note, I was infinitely lucky that in 1969, while I was a research associate at the Department of Physics of the University of Texas at Austin, Wigner gave me one-on-one tutorials on symmetry during his stay in Austin. This experience has impacted me and those close to me during my entire research career, in which the determination and modeling of molecular structures have always been combined with symmetry considerations.[34] Part of this was the fascination with fivefold symmetry[35] and the sensitivity toward all its appearances around us.[36] It was also in 1969 in Austin that I met Michael Polanyi, the medical-doctor-turned-physical-chemist-turned-philosopher, who had also influenced his doctoral student Eugene Wigner. Recognizing regularities in properties, be they structural or other, has always been a principal tool in chemistry. Suffice it to recall the discovery and development of the Periodic Table of the Elements. Wigner learned about the importance of observing regularities from Polanyi, and he stressed this in his brief statement at the Nobel Prize award banquet in 1963 in Stockholm:[37]

> 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.

Returning to the "bigger picture," scientists and artists since Johannes Kepler and Albrecht Dürer have wondered about fivefold symmetry and both about its conspicuous presence and absence in nature. Classical crystallography and X-ray crystallography have had tremendous successes in uncovering the secrets of nature through the 1980s and beyond. J. Desmond Bernal and his disciples as well as others attempted to expand the science of structures to embrace more of fivefold symmetry and other "forbidden" symmetries in the extended world of solid state materials. Dan Shechtman's discovery arrived as an integral part of a unique succession of research and ingenuity.

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EDITORIAL

Marie Sklodowska Curie and the Year of Chemistry

István Hargittai

Abstract Madame Curie was awarded her second Nobel Prize in 1911 and on this occasion it was in chemistry. Honoring the centennial of this event, the world celebrates chemistry in 2011. Chemistry serves the world every minute every year and the world reciprocates with paying attention to this often cursed but uniquely useful science during this particular year.

Keywords Marie Curie · Chemistry · Year of chemistry · Nobel Prize

Marie Sklodowska (Fig. 1) was born in Warsaw, which was part of czarist Russia at the time and is the capital of Poland today. She left her homeland for France to study; married the physicist Pierre Curie; and together they made seminal discoveries for which in 1903 they shared one of the first Nobel Prizes in Physics. The prize was divided, and one half was given to Antoine Henri Becquerel "in recognition of the extraordinary services he has rendered by his discovery of spontaneous radioactivity." The other half was given jointly to Pierre Curie and Marie Curie (Fig. 2), "in recognition of the extraordinary services they have rendered by their joint researches on the radiation phenomena discovered by Professor Henri Becquerel."

Marie Curie became a widow very early; she carried on, raised their two daughters, and did seminal research on her own. She received a second Nobel Prize in 1911, this time in chemistry, and this time it was an unshared prize. The motivation stated that she was awarded "in recognition of her services to the advancement of chemistry by the discovery of the elements radium and polonium, by the isolation of radium and the study of the nature and compounds of this remarkable element."

Fig. 1 Marie Sklodowska Curie (George Grantham Bain Collection, Library of Congress, Washington, DC, LC-DIG-ggbain-06354)

She was fiercely attacked by many for the independent demeanor in her life, but she prevailed and became

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Fig. 2 Bust of Marie and Pierre Curie in Paris (photograph and © by the author)

a heroine. Wherever she is presented in talks and movies, children become fascinated with science and especially girls decide to seek a career in science. Not many have the stamina of these early converts to carry it through, but a few do, and it is like a chain reaction, they often become the nuclei of additional converts for science. To give additional luster to the celebration of the centennial of Marie Curie's second Nobel Prize by naming the year 2011 the Year of Chemistry was a marvelous decision.

Marie Curie belongs to a special group even among Nobel laureates. Very few recipients have been awarded two Nobel Prizes, very few women have received Nobel Prizes, very few Nobel laureates' children have also received Nobel Prizes, only one person beside Marie Curie received Nobel Prizes in two different categories (Linus Pauling, chemistry and peace), and what is absolutely unique for Mari Curie that she is the only recipient who has received two Nobel Prizes in *two different science categories* (physics and chemistry).

It was a fortunate choice to connect the international year of chemistry with Marie Curie, a legendary scientist. This connection may throw some additional warmth onto chemistry. There could not be too much effort and too many opportunities to try to make chemistry appear friendlier than it is often projected to and perceived by the great majority of the population. The problems start with the pedagogy through which children more often than not get alienated from chemistry. When they become adults, they propagate this alienation to their children. I learned to love chemistry from a book. Had it been left to my first chemistry teachers, I might have learned to hate it or at least become indifferent to it.

The second source of the negative attitude of the great majority of the population toward chemistry is the blame placed on chemistry for pollution. The blame should be placed where it belongs, the polluters, but somehow the whole of chemistry is usually portrayed as the polluter forgetting even the fact that chemistry more than any other branch of science saves us from the consequences of pollution.

Structural chemistry in a way is exempt from the scorn that usually engulfs chemistry, mostly because structural work usually does not spill chemicals onto the floor or elsewhere, and so on. But this should not make us lay back and observe the fate of the rest of chemistry with indifference. Also, in structural chemistry we can observe how easy it is to expropriate beautiful and useful chemical discoveries by other branches of science, like—with some exaggeration—the discovery of the double-helix structure of DNA by biology.

Sweden has issued stamps commemorating Marie Curie's two Nobel Prizes among the series of stamps devoted to all Nobel Prizes. Other countries have also issued stamps honoring her, notably, Poland and France, the two countries that she counted as her own and vice versa. An example of each is presented in Fig. 3. There has been a call for issuing stamps to commemorate Marie Curie and the year of chemistry. At the time of writing this Editorial, we know of three countries, viz., Indonesia, Slovakia, and Spain that have declared plans of issuing such stamps, and other countries may eventually augment the list.

Fig. 3 Marie Sklodowska Curie on Polish, French, and Swedish stamps

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EDITORIAL

Aleksandr Mikhailovich Butlerov and chemical structure: Tribute to a scientist and to a 150-year old concept

István Hargittai

Abstract In a pioneering move, one hundred and fifty years ago the Russian organic chemist Aleksandr Butlerov (1828–1886) coined the term “chemical structure.” He called for basing our understanding of the chemical composition of substances on the concepts of atomicity and structure.

Keywords Aleksandr Butlerov · Chemical structure · Stereochemistry

In the 1850s and 1860s, excellent chemists worked on elucidating the composition of organic substances, such as August Kekulé, Hermann Kolbe, Archibald Couper, and others. Concepts such as valence in general and the tetravalence of the carbon atom in particular were born. It was felt intuitively that there should be a correlation between the chemical properties of organic compounds and the distribution of bonds between their atoms. However, it was not yet possible to understand and, accordingly, to depict this correlation properly.

Fig. 1 Aleksandr Mikhailovich Butlerov (1828–1886). The portrait is from Ref. [1]

From this background is it only possible to appreciate the importance of Aleksandr Butlerov’s (Fig. 1) presentation one hundred and fifty years ago this year at the meeting of the chemistry division of the 36th congress of the German physicians and scientists. The title of his presentation on September 19, 1861, was “Einiges über die chemische Structur der Körper,” which was then printed in the German journal Zeitschrift für Chemie und Pharmacie [2] (Fig. 2). Note the spelling of „Structur” which is „Struktur” in today’s German. Butlerov at the time was professor at Kazan University and his paper was reprinted next year in Russian translation in the journal of Kazan University [3]. Figure 3 shows the first page of this paper in which the asterisked footnote refers to the original German publication.

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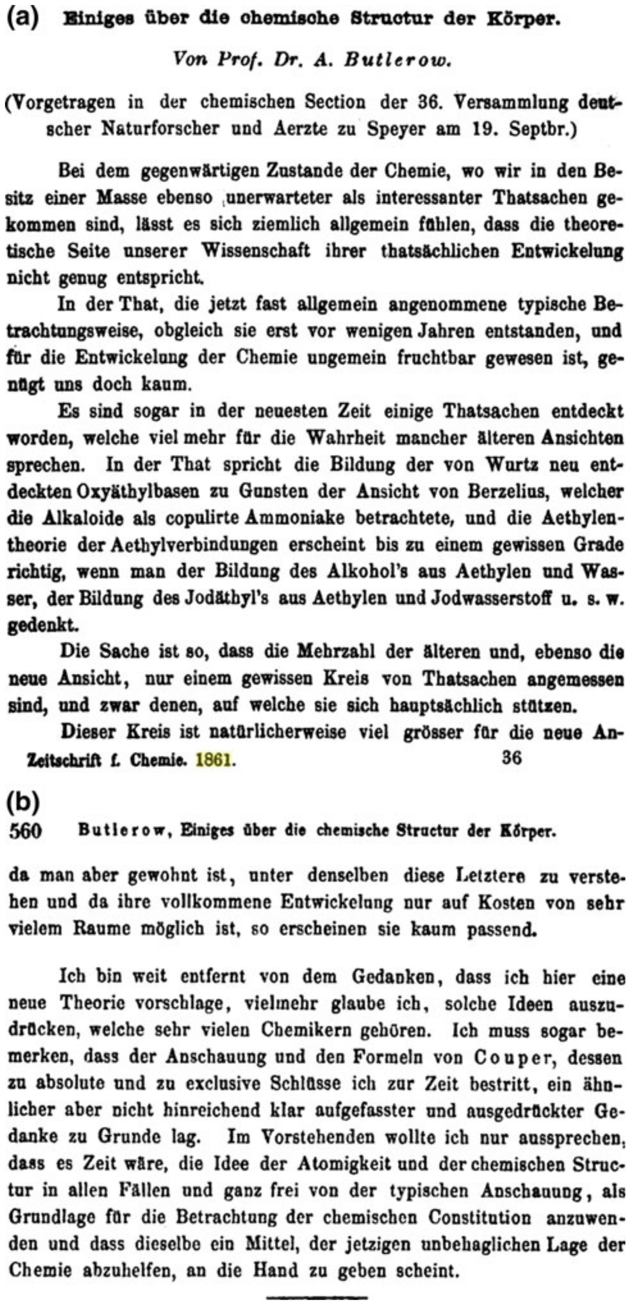


Einiges über die chemische Structur der Körper.

Von *Prof. Dr. A. Butlerow.*

(Vorgetragen in der chemischen Section der 36. Versammlung deutscher Naturforscher und Aerzte zu Speyer am 19. Septbr.)

Bei dem gegenwärtigen Zustande der Chemie, wo wir in den Besitz einer Masse ebenso unerwarteter als interessanter Thatsachen gekommen sind, lässt es sich ziemlich allgemein fühlen, dass die theoretische Seite unserer Wissenschaft ihrer thatsächlichen Entwickelung nicht genug entspricht.

In der That, die jetzt fast allgemein angenommene typische Betrachtungsweise, obgleich sie erst vor wenigen Jahren entstanden, und für die Entwickelung der Chemie ungemein fruchtbar gewesen ist, genügt uns doch kaum.

Es sind sogar in der neuesten Zeit einige Thatsachen entdeckt worden, welche viel mehr für die Wahrheit mancher älteren Ansichten sprechen. In der That spricht die Bildung der von Wurtz neu entdeckten Oxyäthylbasen zu Gunsten der Ansicht von Berzelius, welcher die Alkaloide als copulirte Ammoniake betrachtete, und die Aethylentheorie der Aethylverbindungen erscheint bis zu einem gewissen Grade richtig, wenn man der Bildung des Alkohol's aus Aethylen und Wasser, der Bildung des Jodäthyl's aus Aethylen und Jodwasserstoff u. s. w. gedenkt.

Die Sache ist so, dass die Mehrzahl der älteren und, ebenso die neue Ansicht, nur einem gewissen Kreis von Thatsachen angemessen sind, und zwar denen, auf welche sie sich hauptsächlich stützen.

Dieser Kreis ist natürlicherweise viel grösser für die neue An-

Zeitschrift f. Chemie. 1861. 36

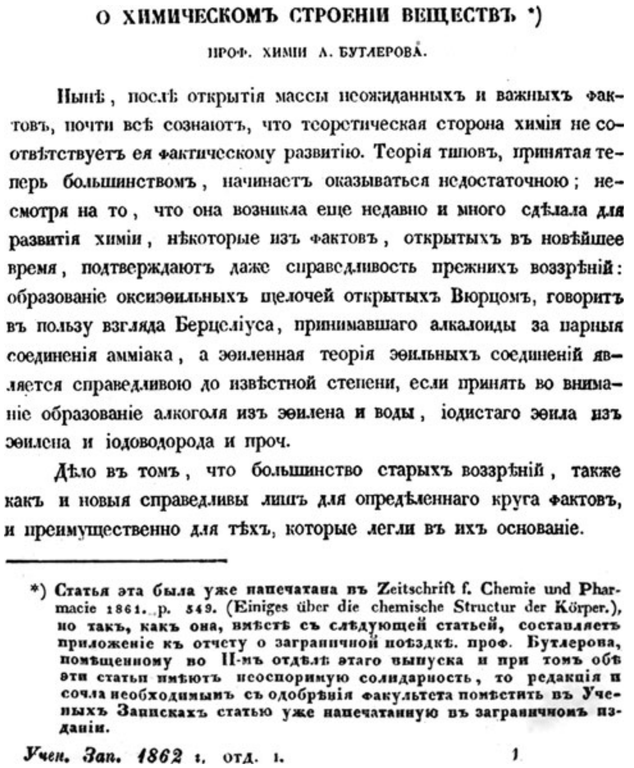
560 Butlerow, Einiges über die chemische Structur der Körper.

da man aber gewohnt ist, unter denselben diese Letztere zu verstehen und da ihre vollkommene Entwickelung nur auf Kosten von sehr vielem Raume möglich ist, so erscheinen sie kaum passend.

Ich bin weit entfernt von dem Gedanken, dass ich hier eine neue Theorie vorschlage, vielmehr glaube ich, solche Ideen auszudrücken, welche sehr vielen Chemikern gehören. Ich muss sogar bemerken, dass der Anschauung und den Formeln von Couper, dessen zu absolute und zu exclusive Schlüsse ich zur Zeit bestritt, ein ähnlicher aber nicht hinreichend klar aufgefasster und ausgedrückter Gedanke zu Grunde lag. Im Vorstehenden wollte ich nur aussprechen, dass es Zeit wäre, die Idee der Atomigkeit und der chemischen Structur in allen Fällen und ganz frei von der typischen Anschauung, als Grundlage für die Betrachtung der chemischen Constitution anzuwenden und dass dieselbe ein Mittel, der jetzigen unbehaglichen Lage der Chemie abzuhelfen, an die Hand zu geben scheint.

Fig. 2 Opening (**a**) and closing (**b**) pages of Butlerov's 1861 paper in the German journal [2]

The term „khimicheskoe stroenie" in Russian and „chemische Struktur" in German means chemical structure and was probably used for the first time. It did not go into general usage very soon, and the the term stereochemistry became more popular after it had been coined in 1890 by Victor Meyer to describe the relative three-dimentional positions of the atoms in the molecule [4]. Even in Butlerov's native Russian, „struktura" and „stroenie" are used alternatively, and the word „stroenie" has a connotation of being related with the building industry. This connotation though subtracts nothing from its being appropriate for chemical nomenclature since, knowingly or not, Butlerov introduced a term that was supposed to stress the three-dimensional nature of chemical entities. Incidentally, the bonds in Butlerov's paper linked groups of atoms rather than atoms. In this connection we note that even in the 1930s, when electronic theory of bonding was already in the vogue, the arrows in chemical texts, supposed to be indicating electron movements in chemical transformations, were often placed wherever it served the printers' convenience rather than expressing meaningful chemistry.



О ХИМИЧЕСКОМЪ СТРОЕНІИ ВЕЩЕСТВЪ *)

ПРОФ. ХИМІИ А. БУТЛЕРОВА.

Нынѣ, послѣ открытія массы неожиданныхъ и важныхъ фактовъ, почти всѣ сознаютъ, что теоретическая сторона химіи не соотвѣтствуетъ ея фактическому развитію. Теорія типовъ, принятая теперь большинствомъ, начинаетъ оказываться недостаточною; несмотря на то, что она возникла еще недавно и много сдѣлала для развитія химіи, нѣкоторые изъ фактовъ, открытыхъ въ новѣйшее время, подтверждаютъ даже справедливость прежнихъ воззрѣній: образованіе оксиэѳильныхъ щелочей открытыхъ Вюрцомъ, говоритъ въ пользу взгляда Берцеліуса, принимавшаго алкалоиды за парныя соединенія амміака, а эѳиленная теорія эѳильныхъ соединеній является справедливою до извѣстной степени, если принять во вниманіе образованіе алкоголя изъ эѳилена и воды, іодистаго эѳила изъ эѳилена и іодоводорода и проч.

Дѣло въ томъ, что большинство старыхъ воззрѣній, также какъ и новыя справедливы лишъ для опредѣленнаго круга фактовъ, и преимущественно для тѣхъ, которые легли въ ихъ основаніе.

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*) Статья эта была уже напечатана въ Zeitschrift f. Chemie und Pharmacie 1861. p. 549. (Einiges über die chemische Structur der Körper.), но такъ, какъ она, вмѣстѣ съ слѣдующей статьей, составляетъ приложеніе къ отчету о заграничной поѣздкѣ. проф. Бутлерова, помѣщенному во II-мъ отдѣлѣ этаго выпуска и при томъ обѣ эти статьи имѣютъ неоспоримую солидарность, то редакція и сочла необходимымъ съ одобрѣнія факультета помѣстить въ Ученыхъ Запискахъ статью уже напечатанную въ заграничномъ изданіи.

Учен. Зап. 1862 г. отд. I. 1

Fig. 3 Opening page of the Russian version of Butlerov's German paper in which he introduced the term "chemical structure" [3]

The tetrahedral arrangement of chemical structures goes back to Louis Pasteur who had to suppose it in order to account for dissymmetry and the optical activity of substances. There was another, little known pioneer of the tetrahedral bond arrangement of carbon, Emanuel Paternò. He published his ideas in an obscure Sicilian journal in 1869 [5]. From what he wrote though it is possible to derive what we call today conformational isomerism [6].

The year of the birth of stereochemistry, however, is considered to be 1874. The basic concepts were proposed by J. H. van 't Hoff and J. A. Le Bel, and in 1875 van 't Hoff published a booklet La Chimie dans l'Espace (Chemistry in Space). Van 't Hoff's and Le Bel's ideas were not readily accepted. The most vocal of their opponents was Hermann Kolbe whose vitriolic words illustrate the barrier the concept of three-dimensional chemistry had to overcome [Ref. 4, p. 93]:

> …A Dr. J. H. Van 't Hoff, of the Veterinarian College, Utrecht, appears to have no taste for exact chemical reserch. He finds it a less arduous task to mount his Pegasus (evidently borrowed from the Veterinary College) and to soar to his Chemical Parnassus, there to reveal in his La Chimie dans l'Espace how he finds the atoms situated in the world's space.
>
> It is not possible, even cursorily, to criticize this paper, since its fanciful nonsense carefully avoids any basis of fact, and is quite unintelligible, to the calm investigator…

Butlerov did not participate in the controversy. When he gave his lecture in 1861, he did not even claim credit for any new thoughts. Rather, in the conclusion of his paper he stressed that he was expressing ideas that had occurred to many of his colleagues, and he mentioned in particular Couper. Butlerov added that the ideas he was presenting had not yet been expressed with sufficient clarity. In the last sentence of his paper he stresses that „it is time to base our understanding about the chemical composition of substances on the concepts of atomicity and chemical structure…" [translated from the Russian original, Ref. 1, p. 74].

Recently, David E. Lewis reviewed the significance of Butlerov's contribution to the science of organic structures accompanied by a brief description of his career [7]. Butlerov's complete works have appeared in a monumental series of four volumes in Russian, the first volume being referred to in our Ref. [1]. In the subsequent volumes, his organic chemistry text book was reproduced (Vol. 2), along with his science-popularizing works, correspondence, reviews, and other writings (Vol. 3), and, finally, treatises concerning agriculture and other studies not related to chemistry (Vol. 4).

Butlerov's activities in organic chemistry beyond the structural aspects were also significant (see e.g., [8]), but the evaluation of his oeuvre has been clouded with interference from politics. In the Soviet Union, he was at times considered as the founder of the science of organic chemistry, and his memory was kept alive (Figs. 4, 5, 6). In contrast, in the West, his contributions were often underestimated. The blatant actions to politicize his chemistry occurred most conspicuously during the big controversy about the theory of resonance in the Soviet Union in the early 1950s. Butlerov explicitly stated that each compound had one chemical structure and only one. The critics of the theory of resonance used Butlerov's teaching to discard the possibility of resonance structures as it would allow two or

Fig. 4 Butlerov's statue in Kazan, courtesy of Boris Solomonov, Kazan

Fig. 5 Butlerov's statue in front of the Chemistry Department, Moscow State University (photograph by the author)

Fig. 6 Soviet stamp issued in 1951, at the height of the Soviet resonance controversy, and it says, "A. M. Butlerov, great Russian chemist creator of the theory of chemical structure of organic compounds"

more structures to coexist. Had this been part of a sober scientific discussion it could have been considered a reasonable argument.

Unfortunately, in the early 1950s, this grew into an ideological and even nationalistic controversy with grave consequences for the proponents of the theory of resonance. The critics of the theory of resonance contrasted Butlerov's true Russian values with the cosmopolitan views of those who had bowed slavishly to Western values, etc. The proponents of the theory of resonance had to exercise humiliating self-criticism and lost their jobs [9]. The minutes of a meeting in Moscow on June 11–14, 1951, were published in a 440-page hardbound volume [10]. Four hundred and fifty chemists, physicists, and philosophers attended the meeting, including the top chemists from all over the Soviet Union. There was a report on "The status of chemical structure theory in organic chemistry" compiled by a special commission of the Chemistry Division of the Soviet Academy of Sciences. It was followed by forty-three oral contributions. The report consisted of eight chapters and the first was titled "Butlerov's teachings and their role in the development of chemistry."

Linus Pauling was among the Western scientists attacked in the Soviet resonance controversy, and he seemed rather puzzled by these attacks and even after many years appeared as if he had misunderstood the situation in Soviet Union in the 1950s [11]. Today, we should not let the unprincipled past misuse of Butlerov's teachings mask the values of his pivotal contributions to organic chemistry as well as to structural chemistry.

Acknowledgments I thank Professor David E. Lewis (University of Wisconsin-Eau Claire) for a copy of his treatise referred to in Ref. [7] and Professor Boris N. Solomonov (Kazan State University, A. Butlerov Institute of Chemistry) for the image of Butlerov's statue in Kazan.

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History: Dreaming of the bomb[1]

Ray Monk, Inside the Centre: The Life of J. Robert Oppenheimer
Jonathan Cape, 2012, 832 pp

Istvan Hargittai

A towering yet enigmatic figure among theoretical physicists, J. Robert Oppenheimer directed the US laboratory in Los Alamos, New Mexico, that, between 1943 and 1945, built the first atomic bombs. He earned the label 'father of the atomic bomb' and worldwide fame, and features in numerous books. In the latest, Inside the Centre, Ray Monk — biographer of Bertrand Russell and Ludwig Wittgenstein — brings a philosopher's nuanced perception to Oppenheimer's life and work.

Oppenheimer grew up in a privileged upper-west-side Manhattan family, but felt burdened by being Jewish and “tried to pretend that he wasn't”, in the words of his friend, the Nobel-prizewinning physicist Isidor Rabi. A lonely childhood was followed by a troubled youth; he even showed signs of destructive tendencies. Oppenheimer was trying, as he would all his life, to discover an identity and an avocation.

Oppenheimer followed the customary path of budding US scientists of the time, completing his education in Europe. In 1925, he joined Ernest Rutherford's Cavendish Laboratory in Cambridge, UK, where he was mentored by future Nobel prizewinner Patrick Blackett. Rumours persist of a bizarre incident in which Oppenheimer left an apple laced with a chemical — believed to be cyanide — on Blackett's desk. In any case, Oppenheimer was unhappy: he had little aptitude for experimental physics. Moving to Max Born's lab in Göttingen, Germany, a hotspot of theoretical physics, he became a top player.

In 1929, Oppenheimer returned to the United States for good. He worked at the California Institute of Technology in Pasadena and the University of California, Berkeley, building up an American school of theoretical physics. Soon, an influx of brilliant scientists fleeing the Nazi takeover in Europe arrived to bolster his efforts. Among the glowing successes were contributions to what later became known as the black-hole concept and astrophysics. By the time the field could contribute to the war effort, he and his colleagues were ready.

For a long time, the well-to-do Oppenheimer was oblivious to the economic difficulties around him and had little interest in world affairs. His political awakening in the mid-1930s occurred as a consequence of the hardship he observed during the Great Depression and the intensifying persecution of Jews in Germany. He was drawn to the Communist Party, although he always denied having been a card-carrying member.

When nuclear fission was discovered in Germany in 1938, the Manhattan Project was initiated to develop an atomic weapon. Its final phase was bomb production — for which

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[1] Nature 2012, 491:670

the Los Alamos Laboratory was created in 1943. This powerhouse drew in other Manhattan Project resources: brainpower from the Metallurgical Laboratory in Chicago; uranium-235 from Oak Ridge, Tennessee; and plutonium from Hanford, Washington. Oppenheimer, however, seemed an odd choice as leader, having never directed anything. What no one foresaw was his remarkable ability to inspire associates.

Oppenheimer never regretted his role in making the bombs. He saw their deployment against Japan as helping to end the Second World War quickly, saving millions of lives, despite having killed some 150,000 Japanese in Hiroshima and Nagasaki. In 1947, he declared that “physicists have known sin”. Later, he clarified that he meant the sin of taking pride in their achievements rather than the sin of having caused destruction.

Once involved with the Manhattan Project, Oppenheimer gradually dissociated himself from communism. However, even while directing Los Alamos, he was constantly being investigated by US security organs over his communist activities and connections. In his eagerness to demonstrate loyalty to his country, Monk reveals, Oppenheimer lied despicably about friends and former pupils. For example, he unjustly accused his gifted former student, Bernard Peters, who had participated in anti-Nazi street-fights in Germany, of being a dangerous Red.

After the war, Oppenheimer was in great demand, and seen as a hero scientist. He chaired several committees, including the General Advisory Committee of the Atomic Energy Commission (AEC), which sometimes caused conflict of interest. For example, the Pentagon gave up the idea of the hydrogen bomb after Oppenheimer told them it was technically unfeasible. He then told the AEC that the Pentagon wasn't interested in developing the bomb. Spreading himself too thin also impaired his judgment: he humiliated others, made powerful enemies and hurt his chances of maintaining a leading role in government affairs, which he craved.

During the McCarthy era between 1950 and 1954, Oppenheimer's leftist past caught up with him. His concocted stories surfaced, and his only explanation was: “I was an idiot.” Monk's presentation of the well-known story of the 'Oppenheimer hearing' before an AEC security panel is a highlight of the book.

Oppenheimer had the highest level of security clearance because of his sensitive position. By the time his clearance was about to expire, his loyalty and trustworthiness had been questioned by a number of people. The AEC set up a personal security board to decide on an extension and, in 1954, many scientists testified before it. The damaging testimony of nuclear physicist Edward Teller is often held responsible for Oppenheimer's downfall. The most relentless advocate for a US hydrogen bomb, Teller viewed Oppenheimer as an obstacle to his efforts. But the 'prosecution' had already destroyed Oppenheimer's veracity by the time Teller stepped into the witness stand. Teller's testimony ultimately harmed him more than it did Oppenheimer.

Oppenheimer was both a brilliant physicist and a poor politician; a sophisticated speaker and an inconsistent debater; an inspirational colleague and a disloyal friend. In this highly readable book, Monk makes great strides towards fully understanding the phenomenon that was J. Robert Oppenheimer.

A Cold War Puzzle Persists[1]

Simone Turchetti, The Pontecorvo Affair: A Cold War Defection and Nuclear Physics
University of Chicago Press 2012, 292 pp

Istvan Hargittai

I was a teenager in Hungary when I first heard that the nuclear physicist Bruno Pontecorvo had defected from the West to the Soviet Union. The communist press praised his defection as a testament to the superiority of Soviet science and Soviet life, but to us it was a great puzzle, and it has remained one for more than 60 years. His action was unique -- no other well-known scientist ever defected from the West to the East – defections in the opposite direction were less extraordinary.

The latest attempt at fathoming his actions is *The Pontecorvo Affair*. Written by the University of Manchester historian Simone Turchetti, the book provides an informative account of Pontecorvo’s life up to his defection. Although it does not offer an unambiguous explanation for the event itself, it does go some way towards satisfying the historian’s curiosity about Pontecorvo’s motivations. Curiosity about the second half of the physicist’s life, however, is left entirely unsatisfied, as the book more or less avoids discussing how he adapted to life behind the Iron Curtain.

Pontecorvo’s early years contained little indication of the turmoil that would befall him later in life. He was born on 22 August 1913, near Pisa in Italy. His was a large and well-to-do Jewish family, composed of entrepreneurs and intellectuals. Young Bruno was good at tennis and science, and he became a member of Enrico Fermi’s exceptional team in the Physics Department of the University of Rome while still a teenager. He would remain in the group for five years, gaining experience in looking for applications of the fundamental discoveries being made there.

Perhaps the most remarkable event during his tenure in the Fermi group was the 1934 discovery of slow neutrons, which would have far-reaching consequences for world history and for Pontecorvo personally. The discovery yielded both a patent and a research paper by a stellar group of authors, including two future Nobel laureates, Fermi in 1939 and Emilio Segrè in 1959 (1935 E Fermi, E Amaldi, O D’Agostino, B Pontecorvo, F Rasetti, E Segrè, “Artificial Radioactivity Produced by Neutron Bombardment, Part II”, *Proc. Royal. Soc. Lon. Series A* **149** 522-558), and it is unfortunate that Turchetti does not cite the paper in his book.

In the early 1930s, Italian Jews like Pontecorvo experienced relatively few problems from the country’s fascist government. During the second half of the decade, however, Mussolini began to adopt Germany’s anti-Semitic policies, which had previously been alien to Italian society. In 1936, Pontecorvo responded to the increased tensions by

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[1] Physics World 2012, August: 44–45

moving to Paris. There he worked with Frédéric Joliot-Curie, and he also became politically aware for the first time, in concert with several of his relatives who were already card-carrying members of the communist movement.

In 1940 Pontecorvo and his family emigrated again, this time finding refuge in the US from the advancing Nazis. He got a job in Tulsa, Oklahoma, using his expertise in nuclear physics to develop novel technologies for oil exploration. Eventually, his acumen proved equally useful in prospecting uranium -- the crucial raw material for producing atomic bombs. His next move came in 1943, when he became a member of the British-Canadian efforts to build a nuclear reactor at Chalk River, Ontario. The reactor reached criticality in 1947, and in 1948 Pontecorvo moved for a fourth time, this time to Harwell, England, where he began working for the UK Atomic Energy Research Establishment.

By the time he arrived in Harwell, two developments were causing Pontecorvo increasing worries. One was an intensifying investigation by the US and UK security organs into his associations with friends and family members who were involved in communist politics. The other was an unsettled compensation claim that the holders of the slow neutron patent had lodged against the US government. As Turchetti describes, the complex legal proceedings of the patent dispute put Pontecorvo and his colleagues in the spotlight that made Pontecorvo increasingly uncomfortable.

His troubles culminated in the summer of 1950. It was in many ways a peculiar year, one that witnessed US President Harry Truman's decision to go ahead with the development of the hydrogen bomb; the unmasking of Klaus Fuchs as a Soviet atom spy in the UK; the start of the Korean War; and the development of McCarthyism in the US. All of these events conspired to make Pontecorvo's communist connections appear a considerably heavier burden than they had been just a few years before. Under pressure from these developments -- and maybe something else that we are still not aware of -- Pontecorvo cracked, and he fled, together with his family, to the Soviet Union.

Turchetti gives a meticulous account of Pontecorvo's movements, his excellence in nuclear science and its applications, and the fate of the patents filed by Fermi and colleagues in the US. He also offers some useful insights into what may have been Pontecorvo's value to the Soviet Union as a scientist. In addition, he demonstrates how both British and Americans authorities attempted to make Pontecorvo's flight appear to represent a next-to-negligible breach in national security.

Ultimately, however, we are still left with an uncertain picture of the motivations that led to Pontecorvo's decision to flee. There is also very little about Pontecorvo's life in the Soviet Union; it is not promised, to be sure, yet the absence of any real analysis of this period inevitably leaves the reader with a void. There are some hints that Pontecorvo was much appreciated by the Soviets, though Turchetti mistakenly states that Pontecorvo had an honorary membership in the Soviet Academy of Science (p. 180). The "honorary" designation would have implied being a foreigner, whereas Pontecorvo became a Soviet citizen, and in 1958 he was elected corresponding member of the Science Academy and in 1964, full member—the pinnacle in Soviet scientific life. He enjoyed the perks and privileges of the highest echelon of Soviet society to the end of his life. He died in 1993. His name does not figure prominently among the movers of the Soviet nuclear projects—the impression is that to the end he was to some extent kept in the shadow.

There are some trivial inaccuracies in the book that are disturbing. Here is a sampler: Brien McMahon was not a member of the US Atomic Energy Commission (p. 109); rather, he was a US senator much involved in legislation of nuclear matters. William Borden was not the prosecutor in the Oppenheimer case (p. 130), but the author of an accusatory letter against Oppenheimer. The US decision in 1950 to develop the hydrogen bomb did not impel the Soviets to follow suit (p. 185); they had already embarked on this path. The book *The Vavilov Affair* did not have two authors, Mark Popovski and Mark Aleksandrovich (p. 273); the author was Mark Popovsky and his patronymic was Aleksandrovich.

Readers of *The Pontecorvo Affair* will find that the book boosts their appreciation of the importance of Fermi's group and of Pontecorvo's work in applied nuclear physics. Turchetti offers a good account of Pontecorvo's later discoveries and contributions, including his work in prospecting, and vividly conveys the difficulties that he and other inventors encountered in their efforts to be compensated for patents that were amply utilized for defense purposes. His description of how Western security organizations attempted to belittle the significance of Pontecorvo's flight, hints that the Soviets were not the only experts in the art of propaganda. Turchetti shows meticulously Pontecorvo's movements leading to his flight to the Soviet Union, but much less his motivations. The result is that we are still not clear on the complete picture of Pontecorvo's defection, though, thanks to this book, our ignorance has now reached a higher level of sophistication than before.

Struct Chem (2013) 24:1397–1400
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EDITORIAL

Los Alamos and "Los Arzamas"

Istvan Hargittai

Abstract Marking the seventieth anniversary of the Los Alamos Laboratory provides an opportunity for comparison with its Soviet counterpart, Arzamas-16 (nicknamed "Los Arzamas"). There were similarities and differences, but in their principal motivations and treatments of their scientists, they diverged irrevocably. This Editorial is based on an invited presentation on June 12, 2013, at the Norris E. Bradbury Science Museum, Los Alamos National Laboratory, in Los Alamos, New Mexico.

Keywords Atomic bomb · Hydrogen bomb · Los Alamos · Arzamas-16 · J. Robert Oppenheimer · Yulii B. Khariton

This year, the Los Alamos Laboratory celebrates its seventieth anniversary. It came to life in 1943 as the concluding segment of the Manhattan Project to produce the atomic bombs for the US Army. In August 1945, these bombs were dropped over Hiroshima and Nagasaki. Apart from the devastation and human tragedies they caused, their immediate consequences included the surrender of Japan and the conclusion of World War II (Fig. 1). The Los Alamos Laboratory had importance well beyond World War II and the scientists working for the Soviet nuclear program at the secret Soviet installation, Arzamas-16, nicknamed their laboratory "Los Arzamas." This note focuses on some similarities and differences between Los Alamos and Arzamas-16.

The two laboratories had a one-way direct connection through espionage due to which the first Soviet atomic bomb was a copy of the American plutonium bomb. Only the leadership of the Soviet project was aware of the source of information, the scientists were merely given the tasks of what solutions to work out. It proved to be a good exercise for them, but a frustrating experience since they could not bring in their own ideas. For the hydrogen bomb, with less intelligence, the Soviet physicists could utilize their innovative talents. Even later, the shadow of Los Alamos over Arzamas-16 did not disappear entirely. In 1983, the long time scientific director Yulii Khariton wondered loud in a critical moment whether they could guess what the Americans might do in a similar situation. His colleagues sometimes called Khariton the Soviet Oppenheimer [1].

J. [Julius] Robert Oppenheimer (1904–1967), the first scientific director of Los Alamos, filled the post for only two years yet his name has become synonymous with Los Alamos (Fig. 2a). Oppenheimer trained for physicist, but his early achievements included seminal discoveries in chemical physics; suffice it to mention the Born–Oppenheimer approximation worked out jointly with his Göttingen mentor, Max Born. Oppenheimer was an unlikely choice for the post of Los Alamos scientific director, but the military commander of the Manhattan Project, General Leslie Groves, had the right instinct in making it. Oppenheimer had the intellectual capacity to oversee a complex project; possessed the talent in theoretical physics to wield authority over his colleagues, and was eager to prove himself. His past involvement in leftist politics made him feel insecure, and Groves probably sensed that this made Oppenheimer pliable. The physicist came from an upper-middle-class nonreligious Jewish family. His youth was at the time when anti-Jewish discrimination was still

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Fig. 1 "Whose son will die in the last minute? Minutes Count!" The poster refers to the deployment of the atomic bombs in anticipation of the expected huge sacrifices of the invasion of Japan in 1945. Photograph of the legendary Ed Westcott; courtesy of Oak Ridge National Laboratory

widespread in American academia. According to the renowned physicist Isidor I. Rabi, "Oppenheimer was Jewish, but he wished he weren't and tried to pretend that he wasn't" [2]. This must have contributed to Oppenheimer's feeling vulnerable. Oppenheimer performed impeccably against all odds and in spite of the harassments he suffered from the security services that did not trust him.

There was a conspicuous concentration of Jewish refugee scientists from Europe at Los Alamos. By the time the laboratory came to life, most other scientists had already been engaged in war-related projects. The refugees were latecomers in becoming US citizens to allow them participation in other classified projects. The atomic bomb project was a latecomer among war-related research projects. The refugees had been kicked out of their home countries, and in the US, they were welcome and were found needed. The physics of nuclear weapons was challenging, and the refugee scientists were dedicated to the fight against Germany. The anti-Nazi Jewish resistance expressed itself not only in the uprisings of the Warsaw Ghetto and the Vilna Ghetto, but in the Manhattan Project as well [3]. The Hungarian refugee Eugene P. Wigner, later, Nobel laureate, reasoned that if he could come to the US, surely, so could Hitler. The scientists in the US were not unique in recognizing the potentials of the new nuclear physics for defense. Their German, Japanese, and Soviet colleagues came to similar conclusions, but their circumstances were different.

The Soviet nuclear weapons project had its roots at the time right after the discovery of nuclear fission in December 1938 in Berlin. Just as in the US, a few scientists began a project before it could have been sanctioned and financed by the government. Yakov Zeldovich and Yulii Khariton were the principal protagonists and they worked at Nikolai Semenov's Institute of Chemical Physics in Leningrad. Khariton started his career in chemical physics and he and Semenov had co-discovered the branched chain reactions in chemistry in the early 1920s (for which Semenov would receive the Nobel Prize in Chemistry in 1956). In 1933, the Hungarian refugee Leo Szilard in London came to the idea of the analogous nuclear chain reaction; he patented it in 1934, and deposited his patent with the British Admiralty.

Even the small-scale Soviet attempts came to a halt between 1941 and 1943 when the scientists had to work on improving traditional weapons, among them the famous Katyushas. The German troops were still fighting on Soviet territory, however, when the nuclear program, by now as a state project, resumed. Soon after the war ended, the Soviet government established the secret nuclear installation, Arzamas-16, some two hundred and forty miles east of Moscow.

Many of the most prominent Soviet physicists happened to be Jewish and some joined Arzamas-16. The nuclear weapons project protected the physicists during the difficult period of 1948–1953 when Stalin's paranoia developed into active anti-science as well as anti-Semitic persecution. When Zeldovich got into trouble in Moscow, he found refuge at Arzamas. Another Jewish physicist, Ovsei Leipunskii, found shelter at the even more distant Semipalatinsk Proving Ground in Eastern Kazakhstan, to ride out a crisis. Under Stalin, as well as under subsequent Soviet leaders, if there was a project deemed exceptionally important, it was exempted to observe quotas or even complete ban on hiring Jewish scientists.

Fig. 2 **a** Upper part of the statue of J. Robert Oppenheimer in Los Alamos, New Mexico (photograph by I. Hargittai). **b** Yulii B. Khariton on Russian postage stamp, 2004

Yulii Khariton (1904–1996), the long-time director of Arzamas-16, himself was a conspicuous exception (Fig. 2b). His year of birth and his first name were not the only similarities with Oppenheimer (Yulii being the Russian equivalent of Julius). They both spent years in Western Europe for postgraduate studies. For both, this included Ernest Rutherford's Cavendish Laboratory in Cambridge, England. There, each had a future Nobel laureate for mentor; Patrick Blackett for Khariton and, a little later, James Chadwick for Oppenheimer. Khariton blended well into the Cavendish program and he earned his doctorate there whereas Oppenheimer did not, and left for Göttingen. Later as scientific director of Arzamas, Khariton tried to emulate what he experienced at the Cavendish—without success—but at least that was on his mind.

Like Oppenheimer, Khariton was Jewish, a life-threatening condition under Stalin and a definite disadvantage under the subsequent Soviet leaders. There was substantial difference between American anti-Semitism in academia—while it existed—and anti-Semitism in the Soviet Union. In the US, it was discrimination; in the Soviet Union it often developed into persecution. Khariton's situation was especially difficult. His mother lived in Palestine and his father had been kicked out of the Soviet Union and lived in a Baltic state. When in 1940, the Soviet Union annexed the Baltics, he was arrested and directed to the Gulag. Every time Khariton had to submit an autobiography, he painstakingly described his family background—known to the authorities in more detail than to him—lest he be accused of hiding it.

It was for Khariton's exceptional talent and abilities that in spite of his circumstances he was made, and retained for forty-six years, the scientific leader of the nuclear weapons installation. It was forty-six years of luxurious isolation, a "golden cage," with his private railway car for travel and other perks and the highest decorations.

There was a superficial similarity in the motivations of the American and Soviet programs. With few exceptions, the Soviet scientists were dedicated to their nuclear weapons program, at least initially. They were past a bloody war called with good reason the Great Patriotic War, in which their nation literally fought for survival. In the early 1950s, they were taught that a yet more dangerous foreign enemy might attempt their annihilation. This is why even the future fearless human rights fighter Andrei Sakharov could propose murderous schemes to destroy densely populated foreign ports with Soviet thermonuclear devices.

Gradually, however, the Soviet scientists came to the realization that placing nuclear weapons into the hands of a dictator could have led to unforeseeable tragedies. Clashes between Sakharov and the Soviet leader Nikita Khrushchev demonstrated the blatant recklessness of the Soviet leadership in connection with the nuclear arms race. When during the 1967 war between Israel and its neighbors, Zeldovich heard about the consideration of dropping a nuclear bomb over Israel, he deposited a suicide note in secure hands (he knew the authorities would destroy such a note if they found it) and decided to kill himself if the bombing happened. Fortunately, it did not. The nature of relationship of the scientists toward the Soviet nuclear program changed. Los Alamos and "Los Arzamas" diverged irrevocably.

Khariton, on his part, never expressed dissidence. However, when in 1990, amid the great political changes in the Soviet Union, the octogenarian Khariton greeted the first US visitors at Arzamas-16, he told them: "I was waiting for this day for forty years" [4].

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EDITORIAL

ACS memorial plaques in New York City

Istvan Hargittai[1]

Abstract As of spring 2015, five American Chemical Society (ACS) “National Historic Chemical Landmark” plaques have commemorated outstanding achievements in chemistry in New York City. These are at New York University (NYU), Columbia University, Rockefeller University (all in Manhattan), and the Polytechnic School of Engineering of NYU (in Brooklyn). The fifth used to be at the Pfizer plant in Brooklyn. The ACS Division of the History of Chemistry has its own program of commemorative plaques of “Citation for Chemical Breakthrough” and those erected in New York City are also introduced here.

Keywords “National Historic Chemical Landmark” plaques · “Citation for Chemical Breakthrough” plaques · New York University · Columbia University · Rockefeller University · Polytechnic School of Engineering of NYU · Pfizer Inc

Introduction

Cultivating tradition and remembering the past serves honorable purposes. Chemistry has a great deal to remember and to be proud of, but there is yet a lot to do for increasing awareness of the benefits for humankind from this branch of science.

The American Chemical Society (ACS) has a wonderful program of commemorating seminal discoveries and outstanding pioneers with memorial plaques. On a recent visit during fall 2014 to New York City, we have found four “National Historic Chemical Landmark” plaques, and learned about a fifth as well. Five memorial plaques is not a large number if considering the vastness of science and its achievements in this great city.

In addition to the National Historic Chemical Landmark program, the ACS Division of the History of Chemistry has its “Citation of Chemical Breakthrough” program of memorial plaques. The program recognizes seminal publications. As of spring 2015, we are aware of three such plaques in New York. We present these eight plaques below and one more that predated the systematic programs. All photographs of the memorial plaques are by Istvan and Magdolna Hargittai, except the Urey plaque at Columbia University and the Pfizer plaque.

Foundation of ACS

In 2001, the ACS erected a plaque (Fig. 1) of National Historic Chemical Landmark on the wall of the science center of New York University (NYU). The title of the plaque is, “John W. Draper and the Founding of the American Chemical Society,” marking the event on April 6, 1876, at NYU. It says, “On this site, 35 chemists formed the American Chemical Society intending to stimulate original research, awaken and develop talent throughout the United States, provide fellowship, and ensure a better appreciation of the science by the general public. The Society’s first president, New York University Professor John W. Draper, was noted for his pioneering work in photography and photochemistry, as well as his writings in history and education. In his inaugural address, Draper challenged his chemical colleagues to ‘deliver

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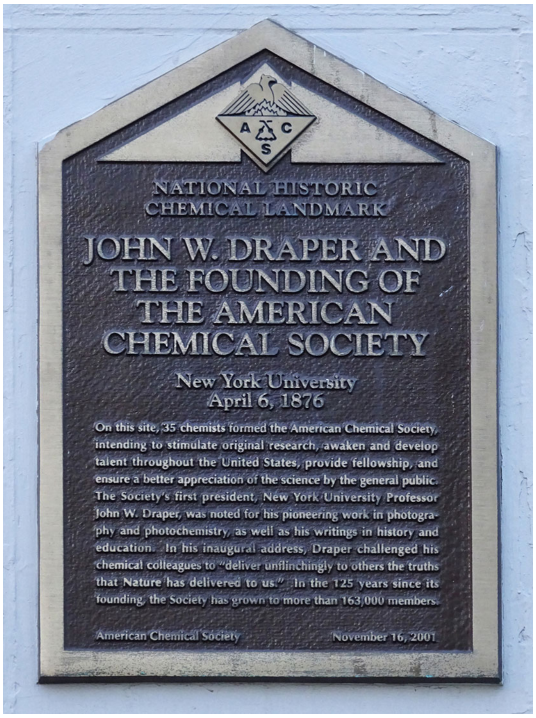

NATIONAL HISTORIC
CHEMICAL LANDMARK

JOHN W. DRAPER AND THE FOUNDING OF THE AMERICAN CHEMICAL SOCIETY

New York University
April 6, 1876

On this site, 35 chemists formed the American Chemical Society, intending to stimulate original research, awaken and develop talent throughout the United States, provide fellowship, and ensure a better appreciation of the science by the general public. The Society's first president, New York University Professor John W. Draper, was noted for his pioneering work in photography and photochemistry, as well as his writings in history and education. In his inaugural address, Draper challenged his chemical colleagues to "deliver unflinchingly to others the truths that Nature has delivered to us." In the 125 years since its founding, the Society has grown to more than 163,000 members.

American Chemical Society November 16, 2001

Fig. 1 "National Historic Chemical Landmark" plaque commemorating the 125th anniversary of the foundation of the Society, at 32 Washington Square N, Manhattan. ©Hargittai

unflinchingly to others the truth that Nature has delivered to us.' In the 125 years since its founding the Society has grown to more than 163,000 members." This plaque is beneath a round plaque (Fig. 2) commemorating the centennial of the founding of the American Chemical Society.

The two ACS plaques are on the wall of at the NE corner of Washington Square, on the western façade of the Silver Center for Arts and Science, College of Arts and Science, NYU (32 Washington Square N). There is yet another plaque next to the ACS plaques, but on the northern façade of the Silver Center; this one is "In honor of the seven public school-teachers who taught under Dutch rule on Manhattan Island." It lists the names of the seven teachers; the time-period was between 1633 and 1674, and the plaque was erected in 1909.

Fig. 2 ACS plaque commemorating the centennial of the foundation of the Society ©Hargittai

Columbia University

This "National Historic Chemical Landmark" plaque of the ACS (Fig. 3) was placed in 1998 on the wall in the entrance lobby of Havemeyer Hall on the campus of Columbia University. The occasion was the centenary of the completion of Havemeyer Hall, which was built under the leadership of Charles F. Chandler. There is a Chandler bust in the same hallway where the ACS plaque is. The plaque makes reference to seven Nobel laureates who did research in Havemeyer Hall and singles out two by name. One is Irving Langmuir, "the first industrial chemist to be so honored," in 1932. The other is Harold C. Urey who discovered deuterium for which he received the Nobel Prize in 1934.

Havemeyer Hall provided research and teaching facilities for faculty and students specializing in industrial,


NATIONAL HISTORIC
CHEMICAL LANDMARK

HAVEMEYER HALL

Columbia University
in the
City of New York
1896–1898

Havemeyer Hall was built between 1896 and 1898 under the leadership of Charles Frederick Chandler. It provided research and teaching facilities for faculty and students specializing in industrial, inorganic, organic, physical, and biological chemistry. Pioneering research done here led to the discovery of deuterium, for which Harold Clayton Urey received the Nobel Prize in 1934. Six others who did research here subsequently received the Nobel Prize, including Irving Langmuir, the first industrial chemist to be so honored, in 1932. The grand lecture hall in the center of Havemeyer remains the signature architectural feature of Charles Follen McKim's original design.

American Chemical Society October 9, 1998

Fig. 3 "National Historic Chemical Landmark" plaque in the entrance lobby of Havemeyer Hall dedicated to the centenary of the opening of Havemeyer Hall housing the chemistry department of Columbia University (1998) at 3000 Broadway, Manhattan ©Hargittai inorganic, organic, physical, and biological chemistry. The "Citation of Chemical Breakthrough" plaque (Fig. 4) honors Urey and his associates' pioneering paper, viz., H. C. Urey, F. G. Brickwedde, and G. M. Murphy, "A Hydrogen Isotope of Mass 2." *Phys. Rev.* 1932, 39, 164–165. The members of the team were associates of Columbia University (Urey and Murphy) and the National Bureau of Standards (Brickwedde).

Rockefeller University

The ACS placed this "National Historic Chemical Landmark" plaque (Fig. 5) at the entrance to Flexner Hall of Rockefeller University (RU) in 2000. It says, "For more than a century, scientists at Rockefeller University have enhanced our understanding of the molecular basis of life—specifically the relationship between the structure and function of nucleic acids and proteins. They showed that DNA transfers genetic information, and that the sugars ribose and deoxyribose are the key building blocks of the nucleic acids RNA and DNA. Furthermore, Rockefeller University scientists established that enzymes are proteins, crystallized the enzyme ribonuclease, determined the sequence of its amino acid building blocks, and then chemically synthesized it."

Division of the History of Chemistry
American Chemical Society

Citation for Chemical Breakthrough

The isolation of the first isotope: Deuterium

Urey, H. C.; Brickwedde, F. G.; Murphy, G. M.
Phys. Rev. 1932, *39*, 164-165.

A Hydrogen Isotope of Mass 2

We find that the vapor pressures for these three molecules in equilibrium with their solids should be in the ratio of $p_{11}:p_{12}:p_{13}=1:0.37:0.29$. The theory of the liquid state is not so well understood but it seems reasonable to believe that the differences in vapor pressure of these molecules in equilibrium with their liquids should be rather large and should make possible a rapid concentration of the heavier isotopes, if they exist, in the residue from the simple evaporation of liquid hydrogen near its triple point.

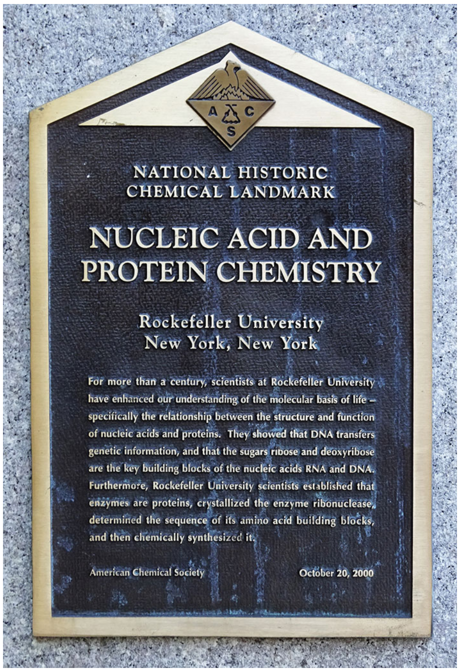
Accordingly two samples of hydrogen were prepared by evaporating large quantities of liquid hydrogen and collecting the gas which evaporated from the last fraction of the last cubic centimeter. The first sample was col-

Line	H_α	H_β	H_γ	H_δ
ΔΔ calc.	1.793	1.326	1.185	1.119
ΔΔ obs.				
Ordinary hydrogen	—	1.346	1.206	1.145
1st sample	—	1.330	1.199	1.103
2nd sample	1.820	1.315	1.176	—

The distillation was carried out at the Bureau of Standards by one of us (F.G.B.), who is continuing the fractionation to secure more highly concentrated samples. The spectroscopic work was done at Columbia University by the other two (H.C.U. and G.M.M.) who are working on the molecular spectrum.

Presented to the Department of Chemistry, Columbia University
and the National Institute of Standards and Technology
2007

Fig. 4 "Citation for Chemical Breakthrough" plaque marking the seminal paper by Urey et al. reporting the 1932 discovery of deuterium



NATIONAL HISTORIC
CHEMICAL LANDMARK

NUCLEIC ACID AND
PROTEIN CHEMISTRY

Rockefeller University
New York, New York

For more than a century, scientists at Rockefeller University have enhanced our understanding of the molecular basis of life – specifically the relationship between the structure and function of nucleic acids and proteins. They showed that DNA transfers genetic information, and that the sugars ribose and deoxyribose are the key building blocks of the nucleic acids RNA and DNA. Furthermore, Rockefeller University scientists established that enzymes are proteins, crystallized the enzyme ribonuclease, determined the sequence of its amino acid building blocks, and then chemically synthesized it.

American Chemical Society October 20, 2000

Fig. 5 "National Historic Chemical Landmark" plaque commemorating the seminal discoveries of Rockefeller scientists in the chemistry of nucleic acids and proteins, at the entrance to Flexner Hall at Rockefeller University, 1230 York Avenue, Manhattan ©Hargittai

Division of the History of Chemistry
American Chemical Society

Citation for Chemical Breakthrough

Discovery that DNA is the Material of Genes

Journal of Experimental Medicine, 1944, *79*, 137-158.

STUDIES ON THE CHEMICAL NATURE OF THE SUBSTANCE INDUCING TRANSFORMATION OF PNEUMOCOCCAL TYPES

INDUCTION OF TRANSFORMATION BY A DESOXYRIBONUCLEIC ACID FRACTION ISOLATED FROM PNEUMOCOCCUS TYPE III

BY OSWALD T. AVERY, M.D., COLIN M. MACLEOD, M.D., AND MACLYN MCCARTY,* M.D.

(From the Hospital of The Rockefeller Institute for Medical Research)

The Inactivation of Transforming Principle by Crude Enzyme Preparations

Crude enzyme preparations	Enzymatic activity			
	Phosphatase	Tributyrin esterase	Depolymerase for desoxyribonucleate	Inactivation of transforming principle
Dog intestinal mucosa	+	+	+	+
Rabbit bone phosphatase	+	+	−	−
Swine kidney "	+	−	−	−
Pneumococcus autolysates	−	+	+	+
Normal dog and rabbit serum	+	+	+	+

The present study deals with the results of an attempt to determine the chemical nature of the substance inducing specific transformation of pneumococcal types. A desoxyribonucleic acid fraction has been isolated from Type III pneumococci which is capable of transforming unencapsulated R variants derived from Pneumococcus Type II into fully encapsulated Type III cells. Thompson and Dubos (21) have isolated from pneumococci a nucleic acid of the ribose type. So far as the writers are aware, however, a nucleic acid of the desoxyribose type has not heretofore been recovered from pneumococci nor has specific transformation been experimentally induced *in vitro* by a chemically defined substance.

CONCLUSION

The evidence presented supports the belief that a nucleic acid of the desoxyribose type is the fundamental unit of the transforming principle of Pneumococcus Type III.

Presented to The Rockefeller University, 2013.

Fig. 6 "National Historic Chemical Landmark" plaque commemorating Avery et al.'s seminal paper of 1944 establishing that DNA is the substance of heredity, at the entrance to the Rockefeller University Hospital ©Hargittai

The ACS Division of the History of Chemistry placed a plaque (Fig. 6) of "Citation for Chemical Breakthrough" in 2013 at the Rockefeller University Hospital. It commemorates the "Discovery that DNA is the Material of Genes." The plaque symbolically reproduces the essence of the paper by Oswald T. Avery, Colin MacLeod, and Maclyn McCarty, "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types: Induction of Transformation by a Desoxyribonucleic Acid Fraction from Pneumococcus Type III." *Journal of Experimental Medicine*, 1944, 79, 137–158.

This ACS plaque is beneath another plaque that the Rockefeller University erected as part of the centennial celebration of the RU Hospital in 2010. This plaque has the title: "The Discovery of DNA as the Molecule of Heredity." It says, "In 1944, Drs. Oswald T. Avery, Colin M. MacLeod, and Maclyn McCarty reported their landmark research conducted on the sixth floor of this building demonstrating that pure DNA from a virulent strain of pneumococcus could stably transform a non-virulent strain into one that was virulent. This research, which grew out of the studies of patients with pneumonia who were treated in this Hospital, established that DNA is the molecule of heredity, one of the greatest discoveries in the history of biology."

The ACS Division of the History of Chemistry placed a plaque (Fig. 7) of "Citation for Chemical Breakthrough" in 2006 in the Founders Hall of Rockefeller University. It is "For the development of chemical synthesis on a solid matrix, a new approach to organic synthesis." The plaque symbolically reproduces the essence of the paper by R. Bruce Merrifield, "Solid Phase Peptide Synthesis. I. The Synthesis of a Tetrapeptide." *Journal of the American Chemical Society*, 1963, 85, 2149–2154.

Division of the History of Chemistry
American Chemical Society

Citation for Chemical Breakthroughs

For the development of chemical synthesis on a solid matrix,
a new approach to organic synthesis.

R. B. Merrifield *J. Am. Chem. Soc.* 1963, 85, 2149-2154

[Contribution from the Rockefeller Institute, New York 21, N. Y.]

Solid Phase Peptide Synthesis. I. The Synthesis of a Tetrapeptide[1]

By R. B. Merrifield

Received January 31, 1963

The general concept underlying the new method is outlined in Fig. 1. It depends on the attachment of the first amino acid of the chain to a solid polymer by a covalent bond, the addition of the succeeding amino acids one at a time in a stepwise manner until the desired sequence is assembled, and finally the removal of the peptide from the solid support.

Reprinted with permission. ©1963 American Chemical Society.

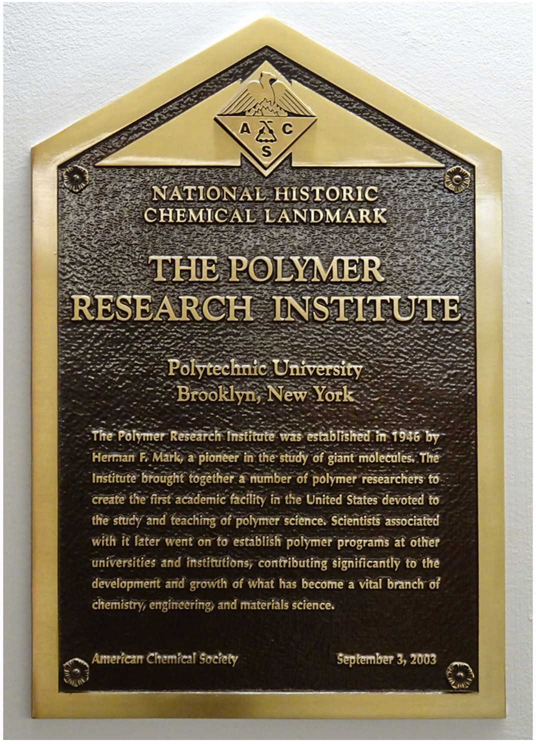
Presented to The Rockefeller University
2006

Fig. 7 "National Historic Chemical Landmark" plaque commemorating Merrifield's seminal paper about the discovery of solid-phase peptide synthesis, on the fourth floor of Founders Hall ©Hargittai

Also in 2006, the American Peptide Society presented a plaque to RU as a tribute to R. Bruce Merrifield, "an exceptional scientist, a wonderful colleague and an inspiring mentor." Merrifield's actual experimental setup is on display at the exhibition of historic equipment at RU.

NYU Polytechnic School of Engineering

This plaque (Fig. 8) is a tribute to the Polymer Research Institute of Polytechnic University, Brooklyn, now, the Polytechnic School of Engineering of NYU. The ACS erected it in 2003. It says, "The Polymer Research Institute was established in 1946 by Herman F. Mark, a pioneer in the study of giant molecules. The Institute brought together a number of polymer researchers to create the first academic facility in the United States devoted to the study and teaching of polymer science. Scientists associated with it



NATIONAL HISTORIC
CHEMICAL LANDMARK

THE POLYMER
RESEARCH INSTITUTE

Polytechnic University
Brooklyn, New York

The Polymer Research Institute was established in 1946 by Herman F. Mark, a pioneer in the study of giant molecules. The Institute brought together a number of polymer researchers to create the first academic facility in the United States devoted to the study and teaching of polymer science. Scientists associated with it later went on to establish polymer programs at other universities and institutions, contributing significantly to the development and growth of what has become a vital branch of chemistry, engineering, and materials science.

American Chemical Society September 3, 2003

Fig. 8 "National Historic Chemical Landmark" plaque commemorating Herman F. Mark's seminal discoveries in polymer chemistry and the achievements of the Polymer Research Institute, in the entrance hall of the Joseph J. and Violet J. Jacobs Building, Polytechnic Institute of New York University, 305–315 Jay Street, Brooklyn ©Hargittai later went on to establish polymer programs at other universities and institutions, contributing significantly to the development and growth of what has become a vital branch of chemistry, engineering, and materials science.” There is a Herman F. Mark bust in a secluded office of the Joseph J. and Violet J. Jacobs Building.

Pfizer, Inc

There used to be a “National Historic Chemical Landmark” plaque (Fig. 9) at the Founding Site of Pfizer Pharmaceutical Company in Brooklyn. The site is no longer operational and lately, the plaque has been in Pfizer’s Archives. It was for “The Development of deep-tank fermentation.” The ACS erected this plaque in 2008. Its inscription says, “In the early twentieth century Pfizer developed innovative fermentation technology, applying it first to the mass production of citric acid. In subsequent years, under the direction of James Currie and Jasper Kane, Pfizer perfected deep-tank fermentation, an aseptic process for growing large quantities of microorganisms, which require oxygen for survival. When scientists in England were unable to produce penicillin on a large scale during World War II, Kane suggested trying deep-tank fermentation. In a major feat of chemical engineering, the company rebuilt an old ice plant, which had the refrigeration machinery required for submerged fermentation, and opened the world’s first large-scale penicillin facility on March 1, 1944. Pfizer manufactured other antibiotics, notably Terramycin, and vitamins using deep-tank fermentation techniques.”

NATIONAL HISTORIC
CHEMICAL LANDMARK

THE DEVELOPMENT
OF DEEP-TANK
FERMENTATION

Pfizer’s Founding Site
Brooklyn, New York

In the early 20th century Pfizer developed innovative fermentation technology, applying it first to the mass production of citric acid In subsequent years, under the direction of James Currie and Jasper Kane, Pfizer perfected deep-tank fermentation, an aseptic process for growing large quantities of microorganisms which require oxygen for survival. When scientists in England were unable to produce penicillin on a large scale during World War II, Kane suggested trying deep-tank fermentation. In a major feat of chemical engineering, the company rebuilt an old ice plant, which had the refrigeration machinery required for submerged fermentation, and opened the world’s first large-scale penicillin facility on March 1, 1944. Pfizer manufactured other antibiotics, notably Terramycin, and vitamins using deep-tank fermentation techniques.

American Chemical Society June 12, 2008

Fig. 9 The National Historic Chemical Landmark plaque honoring the development of deep-tank fermentation technology in 1944 used to be at Pfizer’s Brooklyn plant (courtesy of Pfizer, Inc)

Acknowledgments In spring 2015, it took quite some detective work by Bob Weintraub, Director of the Library of Sami Shamoon College of Engineering (Beersheva and Ashdod, Israel) to determine the current whereabouts of the Pfizer plaque. I thank him and Jeff Brand of Corporate Affairs, Pfizer, Inc, New York, for a high-quality image of the plaque. Furthermore, I thank Vera V. Mainz (Urbana, IL) for a high-quality image of the Urey plaque. This Editorial was based on the preparations of a book tentatively titled *New York Scientific* by Istvan Hargittai and Magdolna Hargittai.

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EDITORIAL

James D. Watson 88—the discovery of the double helix was an iconic event in structural chemistry

Istvan Hargittai[1]

Abstract The ingenuity of James D. Watson and Francis Crick, the convergence of the advances in X-ray crystallography, the accumulated knowledge of structural chemistry, and the breakthroughs in chemical methods of analysis led to the discovery of the double helix structure of DNA. The discovery catapulted Watson to a career that helped DNA and the applications of the knowledge about its structure triumph in biomedical sciences. Watson's eighty-eighth birthday is an occasion to have a look at his path to success, his personality, and assess his legacy.

Keywords James D. Watson · Double helix · Francis Crick · DNA · Human Genome Project

> *It is structure that we look for whenever we try to understand anything.*
> Linus Pauling (1950)

Introduction

The discovery of the double-helix structure of DNA in 1953 was a seminal event in the history of science and a great achievement for structural chemistry [1]. The discoverers, Francis Crick (1916–2004) and James D. Watson (1928–), *suggested* a structure; they did not say they had determined it. It took another two decades of painstaking research when Crick and Watson's proposal received hard experimental evidence.

It happens often, when a scientist makes an important discovery in his or her youth, a less remarkable career follows. In contrast, Crick and Watson remained at the top of science for the next half century. This alone would warrant a closer examination of their activities. In this Editorial, I am going to have a closer look at the lessons Watson's personality and career might offer.

I have been interested in twentieth-century scientists and their discoveries and this has included a fascination with James D. Watson. We met for the first time when my wife Magdi (short for Magdolna) and I visited him in 2000 in his office at the Cold Spring Harbor Laboratory (CSHL). I was recording our conversation and I had an uneasy feeling that everything appeared superficial in our exchange when we had already passed half an hour of the planned one-hour taping. Then, suddenly, things changed and the exchange became meaningful and exciting. We could not stretch much the planned one-hour meeting because we had to start for the airport—it was the last day of our visit in the United States. Watson took us to the train station and made us promise that we would return for a more substantial visit. I had learned enough about him to know that he would not say such things out of politeness.

Later in the same year Watson and his wife Elizabeth—Liz—visited us in Budapest. Their brief stay included sightseeing, lunch in a Hungarian restaurant, sampling of ice-cream, dinner in our home followed by a meeting, still in our home, with leading Hungarian intellectuals—just as Watson had requested.

We next met in 2002 when Magdi and I spent three months at CSHL as the Watsons' guests. The purpose of

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Dedication This Editorial is dedicated to the great scientific partnership of Francis Crick and James D. Watson on the occasion of Francis Crick's birth centennial and Watson's 88th birthday.

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Photo 1 Double Helix—sculpture by Bror Marklund in front of the Biomedical Center of Uppsala University (© 1997 Istvan Hargittai)

the stay was to facilitate my work on my semi-autobiographical book, *Our Lives* [2]. During the subsequent years, we had brief meetings, such as in 2003 during the fiftieth-anniversary celebration of the discovery of the double helix in Cambridge, UK; in 2004 during our visit with Matthew Meselson in Woods Hole, MA; and other occasions. When in the spring of 2007, I was working on my small book, *The DNA Doctor* [3], based on previous conversations with Watson, I experienced some hesitancy in our interactions. When I asked him to give me permission to quote from among his statements in other publications, he declined. Moreover, he did not do this himself but asked one of his associates to call me and tell me about this. This associate was embarrassed conveying Watson's message. Watson's decision, however, was consistent in that he preferred using his material in his own books as he had told me.

Watson's *Avoid Boring People* appeared later in 2007 [4]. As he was preparing for launching the book, he gave an interview to a journalist, who had worked before at CSHL. They spent several hours together. During the interview, Watson made disparaging comments about Africans. When these statements appeared in print, the reactions were devastating for Watson. The CSHL reacted by attempting to dissociate itself from him. When Watson later told me about this experience, he repeatedly used the word "sordid" in characterizing the reaction from CSHL. As I was reading about Watson's humiliation, I wrote him a letter expressing my friendship.

The next time we met was in the spring of 2008 during another of our visits in the United States. This was the first time Magdi and I had been in their Manhattan home. It was then that I fully understood that his ordeal was heavier than I had suspected and it was not over yet. A few days later, Watson asked me to be present at an interview arranged for him by the publicist who had been hired for him. This turned out to be a depressing experience. I knew that CSHL had retired him from his position and the circumstances of the interview with an apparently ignorant journalist were such as if even his independent thinking had also been taken away from Watson.

In contrast, our next meetings in the spring of 2010 and in the fall of 2014, both in their Manhattan home, were uplifting. My impression was that Watson was recovering from his ordeal.

It is possible to view Watson's life in a consistent way, which I attempt below by breaking it into eight periods.

Preparation, 1928–1951

Watson was born April 6, 1928, into a non-practicing Christian family with mostly Irish and Scottish roots. He left his mother's Catholic faith by the age of twelve. The family lived in a not very well-to-do neighborhood of the south side of Chicago. The parents were determined to get a good education for their two children—Watson had a sister, Elizabeth. Watson in his succinct style referred to this as growing up in a quasi-Jewish atmosphere where books were more important than material goods.

Watson went to schools that were not especially remarkable and he breezed through them at an accelerated pace. Although no child prodigy, he was successful in quiz programs on television. He graduated from high school at the age of fifteen and enrolled at the University of Chicago under its maverick president Robert Hutchins who placed the Great Books in the focus of instruction. This broad-based education proved beneficial to Watson. He was more ambitious than most of his peers. When he found a subject that interested him, he was keener to learn about it than anybody else. He did not mind seeing others that were more talented than he was; on the contrary, he sought out their company. He learned from others if there was something to learn, and imitated others when he found people worthy of imitation.

Photo 2 James D. Watson with a double helix model in his left hand in June 1953 at Cold Spring Harbor Laboratory (photo by and courtesy of Karl Maramorosch)

He read Erwin Schrödinger's *What Is Life?* and this book more than anything contributed to Watson's transformation from a bird-watcher zoologist into a geneticist. He completed his undergraduate college education by the age of nineteen and began looking for a graduate school. The big-name schools were not kind to him, perhaps because they could not see anything remarkable about him—eagerness can hardly come through in written applications. He ended up at Indiana University in Bloomington in 1947, but Indiana at that time was probably the best place for his further development. It could offer him top graduate education in modern biology. It had the recent Nobel laureate Hermann J. Muller and two future Nobel laureates—three if including Watson—in the same department. This department provided Watson a diverse international environment with a strong European flavor. Watson had a compressed youth because his and Crick's seminal discovery catapulted him early into the big league of science and world fame. His maturity followed more slowly.

Photo 3 James D. Watson in 2000 in the Hargittais' home in Budapest (photo by I. Hargittai)

Double helix—the discovery, 1951–1954

Upon having earned his doctorate, Watson left for Denmark for postdoctoral studies. He was not lucky with his first assignment so he moved to another laboratory, but the project there did not go well either. In the spring of 1951, he attended a meeting in Naples where he listened to Maurice Wilkins talking about the X-ray work on DNA at King's College in London. Watson glimpsed at Wilkins's photograph of an X-ray diffraction pattern, and decided to work on the structure of DNA in Britain.

This was not a decision taken lightly. The funding agency for Watson's postdoctoral fellowship opposed his move, yet Watson was undeterred even when he lost the support that was supposed to sustain him. At this point, he hardly knew anything about X-ray crystallography, let alone its application to biological macromolecules. This was the time when some giants of science were struggling with solving the structure of proteins at the edge of feasibility.

In hindsight, Watson's decision was a sign of genius, but his ignorance must have contributed to making it. Of course, he was not ignorant in many aspects of his subsequent research and it could not be ascribed to ignorance either that he recognized the importance of uncovering the structure of DNA beyond the importance of DNA, the substance. However, he was not clear about the possibilities and limitations of structural chemistry at the time and in particular about those of X-ray crystallography. A certain amount of ignorance is useful when a scientist embarks on an ambitious project. Rita Levi-Montalcini might have had Jim Watson in mind when she stressed in her autobiography the importance of underestimating the "difficulties, which cause one to tackle problems that other, more critical and acute persons instead opt to avoid" [5].

Once Watson had decided on his project, he had to choose the venue for it, and he ended up in the best place for his purpose, in the Cavendish Laboratory in Cambridge. The change from the periphery of science in Denmark (periphery, that is, in molecular biology and not in Niels Bohr's physics) to a world center was to Watson's liking. No sooner had he arrived than he teamed up with Francis Crick, who had a background in physics, was full of ideas, and had been engaged in an unexciting project. They formed one of the most remarkable partnerships in the history of science.

In April of 1952 in Oxford, Watson—as a proxy—presented the results from the experiments of Hershey and Chase of the CSHL. These results reinforced Avery et al.'s findings that DNA was the substance of heredity. Also in 1952, the biochemist Erwin Chargaff visited the Cavendish Laboratory, and told Watson and Crick about his seminal experiments. The essence of Chargaff's discoveries had direct relevance to them: DNA was organism-specific, but the DNA bases adenine (a purine) and thymine (a pyrimidine) occurred in roughly equal amounts as did the bases guanine (a purine) and cytosine (a pyrimidine) in all DNAs, regardless from which organisms they had been extracted.

Scientists congregated in Cambridge, and were anxious to share their latest findings with the researchers there, as if seeking their approval. It was another fortunate circumstance that Linus Pauling had sent his son, Peter, there, and he became friendly with Watson and Crick. The young Pauling was happy to carry the news from his father about progress at Caltech to his new friends. Then, Watson and Crick received a roommate at the Cavendish in the person of the American chemist, Jerry Donohue, who put them on the right track about the preferred chemical forms of the bases in DNA. Watson hardly knew any chemistry at the time of the double helix discovery, but he was always ready to learn what he needed to know.

Watson and Crick did not do experiments, but had access to Rosalind Franklin's diffraction pattern. When Wilkins shared Franklin's observations with Watson, he did so as part of his angry revenge against her rather than in an altruistic move for the sake of advancing science. Wilkins considered Franklin an intruder into his research turf and resented her style. Then, through Max Perutz, Watson and Crick had access to the laboratory report with Franklin's discussion of her experiments. There has been much effort to demonstrate that there was nothing wrong with having communicated Franklin's data to Watson and Crick, but it has been questionable at least whether it was "legal" or not [6]. Nobody has ever suggested that the way Watson went about it was "moral."

In addition to Franklin and her student Raymond G. Gosling's X-ray patterns, Watson and Crick utilized Pauling's approach of relying on all available structural chemistry in their quest for the DNA structure. This was, of course, perfectly legitimate and constituted a brilliant example of how the next discovery builds on previous discoveries about which it utilizes published data and techniques. What Watson and Crick needed to do was "only" to put together all the relevant information after they had done the most crucial act by having posed the right question.

Watson and Crick's paper in April 1953 [1] was barely longer than one page in *Nature* and it stressed that its authors merely *suggested* a structure. However, it had important novel features. One was that it consisted of two helical chains, each coiling around the same axis, but having opposite direction, and thus complementing each other. The other novel feature was the manner in which the two helices were held together through hydrogen bonds between the purine and pyrimidine bases. The bases were joined in pairs, a single base from one helix paired with a single base from the other helix. The two bases in a pair lay side by side, and the complementary pair of a purine base was always a pyrimidine and vice versa. A majestically simple sketch illustrated the report. The structure was consistent with all the information available by then: X-ray crystallography, model building, and chemical analysis of DNA.

Watson and Crick's approach to research was very efficient, but unusual at that time. It was using other people's measurements, techniques, experimental results, and conclusions. Science works this way. Isaac Newton explained that he saw farther than his predecessors, because he stood on the shoulders of others. This is what Watson and Crick did, except that Franklin was their contemporary and they failed to inform her that they had stepped onto her shoulder. In any case, Watson and Crick did not want to let themselves get bogged down with details.

Watson and Crick's working style appeared unorthodox to many. They seemed sloppy, did not seem hard working, and appeared as if they had plenty of free time for entertainment. At times they behaved as if they were underemployed—not the usual image of the mad scientist who lives for his work day and night. Furthermore, they seemed too interested in scientific gossip and not enough in learning from the scientific literature. However, there is no definition of what constitutes the most efficient approach to research, and the unconventional features of Watson's and Crick's approach turned out to be an excellent way to attack the problem they were working on.

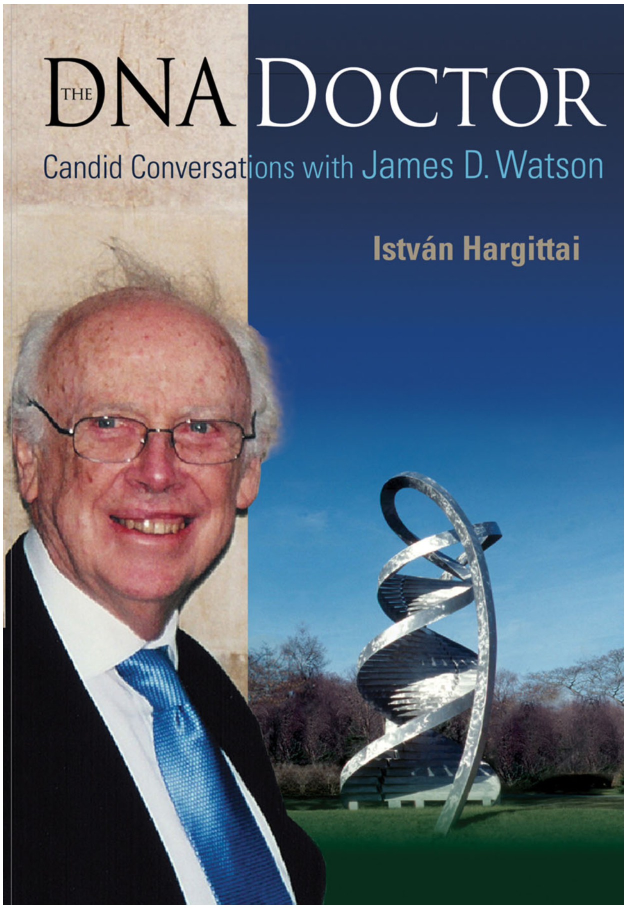

Photo 4 James D. Watson's portrait of 2003 in Cambridge by Magdolna Hargittai on a book cover [3]

Max Delbrück formulated his idea about the usefulness of limited sloppiness, according to which if one is very sloppy that is bad, but thriving for too much rigor might hinder advances. Crick formulated his idea about the advantages of listening to gossip because the grapevine might bring in crucial information that had not yet reached the degree of perfection that would fit publishing it. Finally, hard work and hard thinking do not necessarily appear the same on the surface while the latter may not be less needed in research than carrying out yet another experiment or computation. Not all environments in the world would have so easily tolerated Watson and Crick's way of doing science as the Cavendish Laboratory.

Transition, 1954–1962

During the period from 1954 to 1962, Watson was seeking his role for the rest of his life. It was a transition between the great discovery and Watson's becoming an impresario of science. He first tried to emulate his and Crick's big success in research, but it did not work. He was a good researcher, but unremarkable if compared with his early achievement. He distinguished himself as a professor at Harvard University, but just being a Harvard professor did not satisfy him (while for most it would be a dream position). He did not seem comfortable in a situation, in which however distinguished he could be, there were others around him similarly distinguished. He built up an excellent laboratory at Harvard and attracted to it first-rate scientists, among them Walter Gilbert, a theoretical physicist and future Nobel laureate for his biological discoveries.

Watson was increasingly recognized for the 1953 discovery by such road posts as the Lasker Award and membership of the US National Academy of Sciences. In 1962, Watson, along with Francis Crick and Maurice Wilkins received the Nobel Prize for the double helix. By then Franklin had died. Had she lived, it is not at all certain that she might have been included in the award (a three-person limit in any category of the Nobel Prize is rigorously observed). In the early 1960s, her contribution to the double-helix discovery was not yet recognized to the extent that it has since.

At this time, Watson embarked on textbook writing that would result in his exceptional *Molecular Biology of the Gene* [7]. It was a first both for its subject and for its unusual, creative style.

The Double Helix—the book, 1962–1968

The book *The Double Helix* [8] was long in the making, and the story of its publication is symptomatic of Watson and of the environment in which he operated. It appeared in 1968, following clashes with fellow discoverers and with the Harvard authorities for his unconstrained and subjective style. The book became a success and a defining contribution to twentieth-century literature on science. His negative portrayal of the late Rosalind Franklin sparked a re-evaluation of her contribution to the double-helix story and led to its enhanced recognition. The end of this period brought Watson a long-awaited marriage and his initial appointment to the Cold Spring Harbor Laboratory (CSHL).

Cold Spring Harbor Laboratory, 1968–

Initially, Watson was CSHL's part-time director, but in 1976, he left Harvard and became full-time director of CSHL. He transformed CSHL from a dilapidated and impoverished laboratory to an institution of world leadership in biological and cancer research. Ever since Watson's dedication to it, the CSHL has enjoyed the fruits of his exceptional fund-raising abilities.

Photo 5 James D Watson lecturing on June 15, 2010, at Moscow State University. Courtesy of MASTER-MULTIMEDIA Ltd. © 2010 Felix O. Kasparinsky

Watson reshaped not only CSHL's scientific profile but also its physical appearance to universal satisfaction. In this, his architectural historian wife, Elizabeth L. Watson, proved to be a creative and dedicated partner. Simultaneously with his taking command of CSHL, Watson was one of the leaders in molecular biology whose importance had been reinforced by the fast emerging biotechnology. Watson contributed to the movement of scientists that publicly faced the potential hazards of genetic engineering. This movement led to the memorable Asilomar meeting in 1975 that discussed the scientific safety and ethical ramifications of biotechnology. Subsequently, he was instrumental in calming the runaway hostile sentiments by some segments of the public toward genetic engineering. In 1988, Watson stepped onto the national scene in a major way for his next undertaking.

Human Genome Project (HGP), 1988–1992

The HGP became central to Watson's thinking and efforts from the mid-1980s. It is an oversimplification to ascribe the roots of the Human Genome Project to the discovery of the double helix, but it is easy to do so because the structure has such an easily perceived and beautiful appearance. Other factors, most notably the cracking of the genetic code by Marshall Nirenberg and others as well as Frederick Sanger's (and to a smaller extent, Walter Gilbert's) works in creating the techniques for sequencing complex biological macromolecules, played decisive roles in this.

From the mid-1980s, increasingly loud voices called for deciphering the human genome, pointing to the potential benefits in biomedicine. When the project became a national program in the United States, Watson assumed its administrative leadership in 1988, which proved crucial for the success of the HGP. It was characteristic for Watson's anticipatory thinking and innovative approach that from the start, he had a percentage of the budget of the HGP assigned to the study of societal and ethical issues related to the project. Although Watson was forced out of the HGP leadership in 1992, he has remained a staunch supporter.

Elder Statesman, 1992–2007

For the next decade and a half, Watson continued in a somewhat reduced role at both the CSHL and nationally. In 1993, he resigned from his directorship of the CSHL and became its president, thus removing himself from the day-to-day running of the Laboratory. There was no doubt, however, that he could get involved in micromanaging at any point at the CSHL, and he often did. His dominating presence prevented other strong personalities to consider a leading position at the CSHL. But the Laboratory has thrived. At some point, Watson even felt the presidency superfluous for him and he became chancellor, continuing fundraising and being a major presence but without administrative duties. He had book writing projects of recording everything in minute detail about his own life. This was to change along with everything else in his life in October 2007.

Exit and Twilight, 2007–

In October 2007, there was the scandal that I have already referred to in the Introduction and that had been in the making for many years if considering Watson's recklessness in making politically-not-correct statements. This time, however, he overstepped an important boundary and appeared as if he were a racist, which he definitely was not. Watson underwent the most critical period of his life. He appeared to be no longer master of his fate, and not even of his thoughts. This state continued for several long months. Lately, the situation has slowly consolidated, but Watson's fierce independence seems to be gone for good. In time, Watson has resumed his fundraising activities for CSHL.

Assessment and legacy

Any student of Watson's life may seek to answer a plethora of questions. Here is a sampler, but no attempt raising all possible questions, let alone answering them all. It will be the task of a future biography.

What does it mean that Watson is a genius (something few would doubt)?
How could someone, not obviously a great scientist, rise to the top in science?
How could Watson stay at the top in science for half a century?
What explains his tremendous authority in spite of his lack of oratorical abilities and in spite of his lack of many positive human qualities?
What is the explanation for the tremendous popularity of the double helix?
Did Watson "make DNA" or did DNA make Watson?
How did it happen that Watson has become identified not just with the double helix, but also with DNA itself?
What kind of role model does Watson represent?
What will his legacy be and how far will his influence extend into the future?

The closing sentence of Watson and Crick's seminal paper about the double helix has become a celebrated quotation in the scientific literature: "It has not escaped our notice that the specific pairing [of the bases] we have postulated immediately suggests a possible copying mechanism for the genetic material" [1]. Today, this is commonplace whereas in 1953, it was revolutionary. The double helix structure of DNA came within a decade after the discovery that DNA was the genetic material. When Oswald Avery and his two associates first pronounced it in 1944, few people noticed it and it impacted yet fewer. When, in 1952, Alfred Hershey and Martha Chase showed the same, its acceptance was enthusiastic and broad.

The discovery of the double helix structure of DNA opened a new era in science with a direct route to the Human Genome Project four decades later, and its beneficial consequences in human medicine we cannot yet fully fathom. For years, Watson had doubts about the structure. Only in the early 1970s did reliable crystal structure determinations of DNA, finally, confirm Watson and Crick's original suggestion. It was only then that Watson, finally, had his first good night's sleep about the double helix.

The 1953 discovery catapulted the twenty-five year old Watson to the pinnacle of twentieth-century science. He was an ambitious young man who himself wondered in retrospect about how could it happen to him to "go beyond [his] ability and come out on top" [9]. He had doubts about whether he was bright enough, whether he would at all be able to solve a problem, and whether he would ever have original ideas. He was much sooner a genius than a great scientist, and what happened to him was the fortunate confluence of many factors of being at the right place at the right time, and above all, of being the right person for his self-ordained task.

It certainly was not sheer luck, because it was his decision about what to do and where to continue his career when he faced branching points. Circumstances, too, favored what he decided doing. Watson was very lucky, but he worked hard at finding his luck. He always had the right mentors; supporters; partners; ultimately, the right wife; the right venues for remaking a research place into his own image; and most of all, the right shoulders to stand on in order to look farther. Peter Medawar, the great immunologist, remarked, "Lucky or not, Watson was a highly privileged young man" [10]. It was less his background at home than the environments he eventually sought out for himself that made him privileged.

Watson and Crick never explicitly acknowledged that Watson had had access to Franklin's data, not even in the April 1953 *Nature* paper, and this omission was as much a breach of ethics as snatching the information itself. Watson ignored—whether knowingly or just because he did not care—many minor and not so minor societal conventions. Some of this was on purpose. Legend has it that he was so absent-minded that he often forgot to tie his shoelaces, but

Photo 6 James D. Watson with Istvan Hargittai in 2010 in the Watsons' home in Manhattan, New York (photo by and courtesy of Magdolna Hargittai)

it has been observed, when arriving at a party, just before entering the house, Watson untied his shoelaces.

His idiosyncrasies might have made Watson unwanted company, but the opposite happened; they enhanced his popularity. So did many of his manners that went against accepted norms. He mumbled in his lectures, often speaking to the blackboard rather than to the audience, and in a voice hardly audible, yet his audiences eagerly awaited and attended his talks. He was a poor dresser, but was sought out to attend gatherings. He was clumsy and awkward with girls, but the Cambridge ladies threw themselves into helping him find dates and girlfriends.

For six decades, Watson basked in success and it was not a casual relationship, because he thought a great deal about how to succeed in science. He wanted success and he thought about the Nobel Prize already early in his career. Fame was a driving force for him; he set up rules that assured success, and he practiced them. Watson summarized his prescriptions in over a hundred rules in his book *Avoid Boring People*, and that title was one of his favorite rules [4]. The near-obsession has remained with him and on a recent, June 2010, visit to Russia, he enumerated his rules to his eager Russian audience. The students of Moscow State University took his advice very seriously.

When I used to lecture about Watson in my course on the great discoveries in the twentieth century, I told my students that if Watson opened the door to our auditorium and looked for a place to sit down, he would single out the person in the audience whom he would find most interesting. At this moment, usually there was a little commotion; my students looked around as if assessing themselves and their peers, and sometimes one of them shifted in his seat as if making room for Watson (it was invariably a he rather than a she).

It is a Watson maxim that if you are the smartest person in the room, you are not in the right room. Watson and Crick were roommates at the Cavendish Laboratory in Cambridge and Watson felt comfortable about it. They fortunately complemented each other. Their contributions blended to such a degree that when Crick had to decide about the topic of his long overdue dissertation, he better thought of choosing something from protein structural work rather than the discovery of the double helix where it proved impossible to disentangle their contributions. Watson and Crick were very different not only as human beings but even more so as researchers. For example whereas both were curious and ambitious, Crick's curiosity was stronger than Crick's ambitions whereas Watson's ambitions were stronger than Watson's curiosity. Crick was a great scientist willing to attack even risky problems if he was sufficiently curious about them. Watson was a great scientist whose ignorance contributed to his decision to study the structure of DNA—he was not fully cognizant of the then possibilities of X-ray crystallography and even of the state of analyzing biologically important macromolecular structures. He was, however, fully aware of the importance of elucidating the structure of the substance of heredity. His going for it against all odds was a stroke of genius.

Watson's keys to success are comprised of a broad domain of traits. They included the ability to distinguish between the important and the unimportant, and he always found time for relaxation. He economized with his time, but when he was doing something that he judged truly needed doing, he spent his time on it liberally. He was very patient when he was cutting out his paper models of the bases for his model as he was on the verge of the discovery of base-pairing in DNA. He paid meticulous attention to the minutest details in writing his textbooks. He devoted a lot of time to the back-and-forth exchanges with his colleagues and friends as he was preparing the publication of his book *The Double Helix*. He paid the most careful attention to all aspects of the planning of new constructions and renovating old buildings at Cold Spring Harbor Laboratory.

It is equally noteworthy what he did *not* do. There are scientists who once they find a fertile area of research, exploit it to the fullest; once they establish a new methodology, apply it to whatever it may be applicable. Others may feel in retrospect that they had moved away too quickly after they had made a discovery. For Watson, it was never a problem to determine when his work would become repetitious without, however, under-utilizing the potentials of an area. After the discovery of the double helix, but only after having made sure that everybody saw its biological implications, he moved on. His negative experience with the study of the structure of RNA and with the quest for the messenger RNA strengthened his determination that instead of trying to top his previous feat in research, he should be seeking his success elsewhere.

He became immensely successful in his new avocations, directing science and authoring books. His next success, his textbooks, covered new grounds and were innovative not only for their contents but for their style as well. His account of the double helix discovery showed the process of scientific research in a way that nobody before him had been capable of or dared. Cold Spring Harbor Laboratory did not merely become singularly successful, including its Watson Graduate School; it has also become Watson's shrine. However, only time will show whether it will become a lasting success after Watson is gone. He had generated hostility at CSHL due to his methods of enticing success through competition between members of the same group, between groups of the same laboratory, and so on. On occasion, it seemed to his associates that nothing was too sacred to him for the sake of success.

Watson was seldom a player in politics at the national level, but there were exceptions. When President Nixon declared his "War against Cancer," Watson pointed out the futility of the project. He showed that they could spend the money more wisely if they first reached a basic understanding as to the causes of the different cases and the mechanism of actions. He acquired a prominent role in the Human Genome Project between 1988 and 1992, a brief period, but crucial as it was the start of the project. Otherwise, he was seldom involved in politics. His public appearances made headlines for some shocking, but inconsequential statements like the one that fat women have better sex lives than slim women do. Mostly, he was restrained as one who knew what he could say publicly and where to draw the line to keep his views private, with due consideration for his fundraising role for the Cold Spring Harbor Laboratory. This restraint was absent in his 2007 debacle. Due to his age, the scandal could have signified the closing of his career and would have made a sad ending.

Watson, however, was not done yet: he persevered. He managed a comeback. The former whiz-kid, now an octogenarian, has lately been active again, traveling, giving talks, and raising funds—for CSHL. James D. Watson is still going strong. He continues shaping his legacy, which he sees primarily in his books and in CSHL. His image building has long focused on making him identified with DNA. He knows that the fame of an individual based on scientific discoveries is fragile. His haunting experience in 2007 reinforced the necessity of a stronger basis for his legacy than an institution. Sydney Brenner, one of the architects of modern molecular biology, stated: "Worrier or Warrior, Jim has been the guardian of DNA for the past 50 years" [11]. Watson's legacy may be dependent on his success in having become identified with DNA, not just its structure, but also the substance. Nobody could ever destroy DNA—it is eternal.

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EDITORIAL

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

Istvan Hargittai[1]

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"

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]

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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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*.

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)

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. 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)

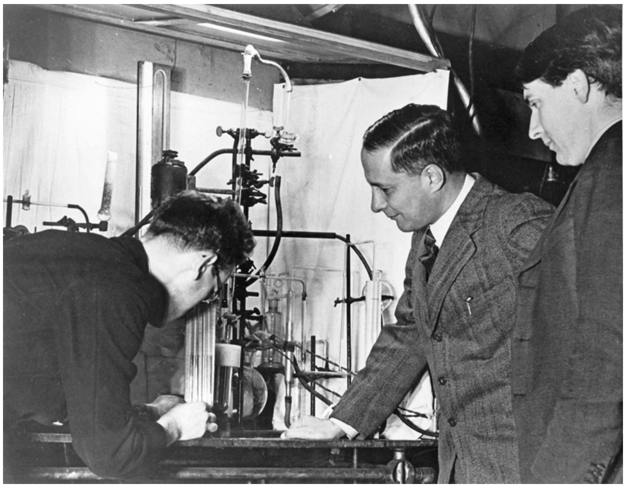

Fig. 11 Michael Polanyi (*middle*) and Alwyn G. Evans (*right*) in 1940 in Manchester (courtesy of John C. Polanyi)

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. 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

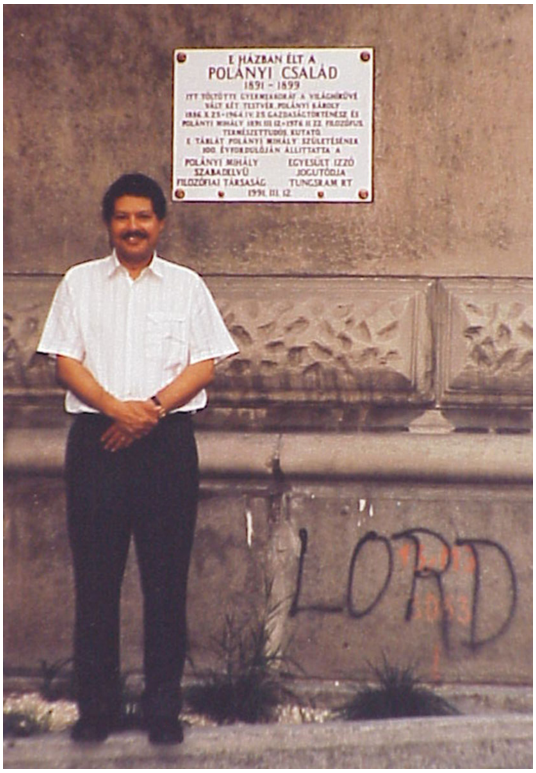

Fig. 18 Memorial plaque honoring the Polanyi Family and especially Karl Polanyi and Michael Polanyi at 2 Andrássy Avenue in Budapest in the 1980s with the Egyptian-American chemistry Nobel laureate (1999) Ahmed Zewail. Courtesy of Ahmed Zewail

E HÁZBAN ÉLT 1891–1899 KÖZÖTT
A POLÁNYI–CSALÁD
ITT TÖLTÖTTE GYERMEKKORÁT
A VILÁGHÍRŰVÉ VÁLT KÉT TESTVÉR
POLÁNYI KÁROLY (1886–1964)
GAZDASÁGTÖRTÉNÉSZ
ÉS
POLÁNYI MIHÁLY (1891–1976)
TERMÉSZETTUDÓS, FILOZÓFUS
A TÁBLÁT
POLÁNYI MIHÁLY SZÜLETÉSÉNEK
100. ÉVFORDULÓJÁN ÁLLÍTTATTA
A POLÁNYI MIHÁLY
SZABADELVŰ FILOZÓFIAI TÁRSASÁG
ÉS AZ
EGYESÜLT IZZÓ JOGTUDÓJA
A TUNGSRAM RT.
1991
HELYREÁLLÍTOTTA
A POLÁNYI MIHÁLY
SZABADELVŰ FILOZÓFIAI TÁRSASÁG
2008

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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EDITORIAL

Structures and mechanisms in chemical reactions: George A. Olah's life-long search of chemistry

Istvan Hargittai[1]

Abstract The Hungarian-born American chemistry Nobel laureate George A. Olah used superacids to give longer life to carbocations. He resolved a long-standing debate on reaction mechanism in organic chemistry and, more importantly, opened new vistas in hydrocarbon chemistry to produce hosts of new compounds. The concerted utilization of organic synthesis, physical techniques, and computational methods led to spectacular achievements in hydrocarbon chemistry. Olah has always been on the lookout for the practical applications of his discoveries in fundamental chemistry. He continued his research after his Nobel award and has worked out the idea, which he labeled "the methanol economy." Olah's example shows that a great researcher can also be a devoted and caring human being.

Keywords George A. Olah · Carbocations · Superacids · Hydrocarbon chemistry · Reaction mechanism · Methanol economy

> *The realization of the electron donor ability of shared electron pairs could one day rank equal in importance with G. N. Lewis' realization of the electron donor unshared pairs.*
>
> George A. Olah [1]

Introduction

In 1962, George A. Olah (Fig. 1) delivered an invited talk at the Brookhaven Organic Reaction Mechanism Conference. He had immigrated only 5 years before to North

Fig. 1 George A. Olah in 1995 in the author's office at the Budapest University of Technology and Economics (photograph by the author)

This contribution is dedicated to George A. Olah in celebration of the forthcoming 90th birthday of this great scientist and wonderful friend.

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Fig. 2 The Olah family lived at 13–15 Hajós Street, District VI (photograph by the author), where George was born. It is just across the street from the Budapest Opera House

America and was working in a Canadian industrial laboratory. It was at the time of the famous debate about the reaction mechanism: whether the 2-norbornyl ion—an intermediate in the hydrolysis of the 2-norbornyl esters, for which there was significantly higher rate for the 2-exo versus the 2-endo derivatives—had a "non-classical" or a "classical" structure. Saul Winstein suggested that the "non-classical" ion had a bridged structure as a consequence of the sigma participation of the C1–C6 bond leading to electron delocalization. Herbert C. Brown ascribed the observed difference in the rate of hydrolysis to steric hindrance of the endo side causing rapidly equilibrating "classical" trivalent ions. Winstein and Brown were giants of organic chemistry and their public debates were popular spectacles of organic chemistry meetings.

In his lecture, Olah reported to have applied a new method of producing long-lived carbocations by means of superacids. Thus, he gave hope of resolving the long-standing 2-norbornyl ion controversy. The experimental observations concerning the rate difference in the hydrolysis of the 2-exo versus 2-endo-norbornyl esters had never been questioned. They were well established facts. The debate concerned the mechanism of the reaction. Uncovering the mechanism of a chemical reaction has been compared to uncovering Hamlet's story between the opening and closing acts of Shakespeare's drama [2]. Often, only the identities of the reactants and the products are known and the mechanism of the reaction leading from the reactants to the products need to be understood. There was solid evidence about the presence of cationic species in the reaction of hydrolysis of 2-norbornyl esters, but they were short-lived, "elusive," hence their nature and structure could not be determined. This is why Olah's claim of giving longer lives to such ions was so stirring. The two protagonists of the debate, Winstein and Brown each, separately, told Olah to be careful with his claim, citing the ease in which unsubstantiated claims could ruin a young chemist's promising career. Winstein and Bown also told Olah that should his claims prove true they expected him to come up with evidence supporting the "non-classical" (Winstein) and the "classical" (Brown) nature of the 2-norbornyl cation.

Eventually, and with the help of NMR spectroscopy and theoretical calculations, Olah provided unequivocal evidence in favor of the "non-classical" nature of the 2-norbornyl cation. The resolution of the famous debate was by itself not a pivotal achievement, but it enhanced Olah's visibility among his peers. Its real significance was in using superacids to produce long-lived, "persistent" carbocations. It pointed to the creation of a whole new chemistry involving hardly reactive hydrocarbons. The development of Olah's new chemistry happened in stages rather than in outbursts of earthshaking discoveries. Olah took his growing fame with attractive humility. He must have felt enormous inner satisfaction though when looking back to the road leading to this exalted status in his science. That road was anything but easy and uneventful.

Fig. 3 George A. Olah as a high school student (courtesy of George A. Olah)

The beginnings

George A. Olah was born (as György Oláh, on May 22, 1927) into an upper-middle-class intellectual family. His father was a lawyer and the family lived in downtown Budapest. The house (Fig. 2) in which they lived in an apartment stood across from the Budapest Opera House. George attended good schools. In particular, for high school, he attended the Gimnázium of the Piarist Order (Fig. 3). The Catholic Piarist Order has taught in Pest since 1717 (Figs. 4, 5). The school boasts another chemistry Nobel laureate among its graduates, George de Hevesy (or Georg von Hevesy, depending on the language he was using). Hevesy attended this school between 1895 and 1903. Hevesy was Jewish and his family converted at around the time of his graduation or soon after.

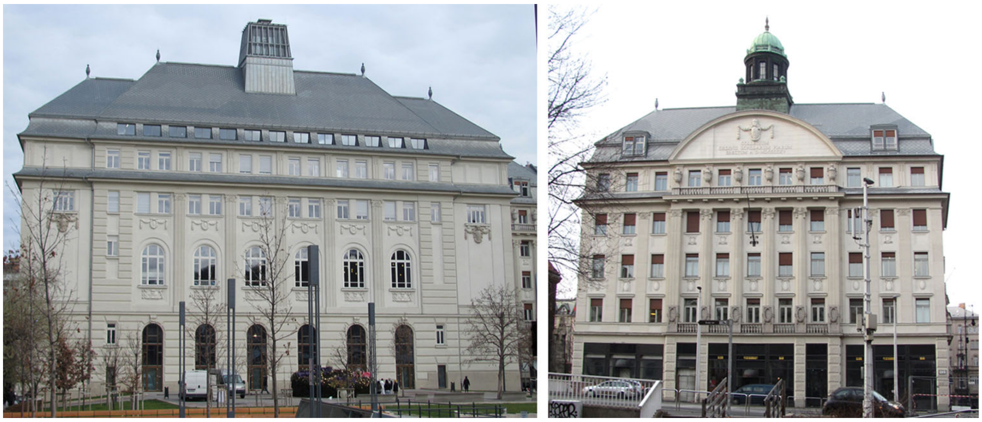
Olah was looking forward to a pleasant and fulfilling life with whatever he would choose for a profession. Everything was given for him, except security as Europe and Hungary were rapidly moving toward World War II. As he was growing up, especially during his upper classes in high school, racial laws of increasing severity were threatening not only his well-being, but eventually even his life. We are circumspect in describing these years of his life in accordance with his own tacit wishes. When in 1957, he, his wife, and their first child immigrated to North America, he and his wife felt they could leave behind all the unpleasantness and horrors of their lives in Hungary. He expressed this in a letter to a friend in Budapest in 2003, "… My life is a life of an American of Hungarian origin, and I am no longer living in the shadow of the [anti-Semitic] Nuremberg Laws" [3]. In his autobiography, he devotes a single, though poignant, sentence to this period: "I do not want to relive here in any detail some of my very difficult, even horrifying, experiences of this period, hiding out the last months of the war in Budapest" ([4], p 45).

In 2003, Olah received an award from the University of Szeged, the Klebelsberg Prize, honoring the memory of the long-time minister of religion and public education. Kuno Klebelsberg had a broad vision for the dominance of Hungary in the region through Hungarian 'cultural superiority,' which was an expression of blatant nationalism. This included a desire to regain territories referred to as Greater Hungary, racism and in particular anti-Semitism.

He aimed at bringing back some of the talent that had left Hungary, but he did not include the Jewish expatriates in the circle of those he wanted to return to Hungary. There was an irony in Olah's receiving the Klebelsberg Prize. It is highly doubtful whether Olah could have had a career in academia under Klebelsberg's reign of culture and education in the anti-Semitic Horthy regime that lasted in Hungary for 25 years, between 1920 and 1944.

In their tolerance, the Piarists built on their liberal traditions. At the time of the Holocaust, the school meant to exclude persecution within its walls and had its Jewish students remove the yellow star from their clothing. However, eventually, Olah had to seek refuge outside the school and Olah's above quoted sentence referred to this last period.

During the Hungarian Holocaust, representatives of a number of nations distinguished themselves in saving lives, and there were a number of Hungarian saviors as well. Gábor Sztehlo was a Lutheran minister who responded to the Lutheran Bishop Sándor Raffay's call to save persecuted Jewish children who had converted, and Sztehlo organized protective homes for them (Figs. 6, 7). The high school student Olah was among his charges. Soon, Sztehlo extended his efforts to all Jewish children and eventually to all children that he found abandoned as he continued his activities after liberation. In 1972, Yad Vashem granted Sztehlo the title "Righteous among the Nations" [5].



Figs. 4, 5 The two principal buildings of the recently renovated Gimnázium of the Piarist Order at the Pest bridgehead of Erzsébet [Elizabeth] Bridge (photographs by the author)

Figs. 6, 7 Two views of the Gábor Sztehlo statue (erected in 2009) on Deák Ferenc Square, District V, by Tamás Vigh and Barnabás Winkler. Photographs by the author

George's brother, Peter, three years his senior, did not survive the war. He had also been a student of the Piarists, which he attended between 1934 and 1942. His name is listed among the martyrs of the school of the period 1938–1958 on a memorial plaque in the lobby of the school.

When the Hungarian legislation had ordered the Hungarian high schools to restrict the number of Jewish pupils for the academic year 1943/1944, the Piarists ignored these restrictions except for the first-graders. They did not send away any of the upper-class pupils. The Arrow-Cross (Hungarian Nazi movement) took over the government on October 15, 1944, and the school closed for a few days. The instruction stopped entirely from October 25. It resumed on March 12, 1945. There were some new teachers and some classes were combined. The yearbook of the school lists the names of the pupils that never returned. George completed his studies at the Gimnázium of the Piarist Order in the spring of 1945 (Fig. 8). The school, keeping with its academic standards and regardless of the immediate post-war conditions, instituted a demanding final examination.

The sentence quoted above from Olah's autobiography about placing the painful experience of his Hungarian period behind him should not be interpreted as indifference to political and other developments in Hungary. He has remained conscious and proud of his Hungarian roots. He has observed keenly and critically the recent political developments in Hungary that include the restoration of much of the spirit of the Horthy regime between the two world wars. He finds it especially painful that the Hungarian responsibility for past tragedies has still not been faced [6].

Fig. 8 Graduates of Olah's Class 1945; Olah's portrait is in the lower left corner (courtesy of the Gimnázium of the Piarist Order)

Start of a career

Up to his graduation from high school, Olah had been especially interested in literature and history and he had not planned a career in the hard sciences. When in 1996, soon after his Nobel award, the periodical *Chemistry & Industry* asked him, “If you hadn’t become what you are, what else would you most likely to have been?,” Olah’s response was, “Writer, historian” [7]

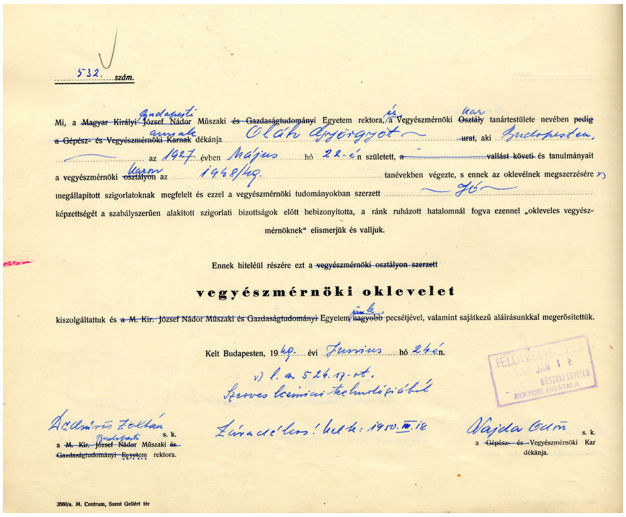
His experiences and the post-war conditions in Hungary, however, prompted him to rethink the direction he was going to take. One generation before him, other future great scientists had to face similar dilemmas. Eugene P. Wigner, for example, was interested in becoming a physicist and John von Neumann, a mathematician. However, parental advice directed each of them to earn a diploma in chemical engineering first as it was offering a more secure future than physics or mathematics at the time as far as jobs were concerned. Olah chose chemical engineering rather than history or literature, and once he became engaged to chemistry, he never left it (Figs. 9, 10).

The Budapest Technical University (today, Budapest University of Technology and Economics) had an academically very strong Faculty of Chemical Engineering (today, Faculty of Chemical Technology and Biotechnology). There was and has been as much emphasis on learning basic chemistry as on the subjects more directly related to technology. By all available information, Olah enjoyed his studies and valued the direct interactions with his teachers. A reviewer of his autobiography noted: “Lectures, albeit compulsory, by active professors so inspired him that he continues to advocate a historical perspective in teaching and, despite the accessibility of electronic communication, direct teacher-student interaction in informal lectures” [8].

It took four years—eight semesters—of structured and intensive studies to earn the Diploma of Chemical Engineer (Fig. 11). Immediately upon graduation, in June 1949, Olah was appointed assistant professor at the Institute of Organic Chemistry of the Technical University. Géza Zemplén (1883–1956, Fig. 12), a former disciple of the great German organic chemist Emil Fischer in Berlin did postdoctoral studies (using today’s term) with Emil Fischer in 1907 and 1908–1910. Zemplén was Professor of Organic Chemistry at the Budapest Technical University from 1913 until his death, and from 1950, he was the head of the Institute of Organic Chemistry of the University. He was the principal figure in organic chemistry in Hungary. His main interest was carbohydrate chemistry and did a great deal of work for pharmaceutical companies as well. Zemplén proved to be a good mentor who could serve as a knowledgeable example but who let Olah go his own way

Fig. 9 The middle section of the central building “K” of the Budapest University of Technology and Economics, 1–3 Műegyetem Quay, District XI (the building was inaugurated in 1909). Photograph by the author

Fig. 10 The chemistry building “CH” of the Budapest University of Technology and Economics, 4 St. Gellért Square, District XI (it was built in 1902 and houses about half of the chemistry faculty). Photograph by the author



532. szám.

Mi, a ~~Magyar Királyi József Nádor~~ Budapesti Műszaki ~~és Gazdaságtudományi~~ Egyetem rektora, és a Vegyészmérnöki ~~Osztály~~ kar tanártestülete nevében ~~pedig~~ ~~a Gépész- és Vegyészmérnöki Karnak~~ annak a dékánja Oláh Györgyöt urat, aki Budapesten, az 1927 évben Május hó 22-én született, ~~a vallást követi~~ és tanulmányait a vegyészmérnöki ~~osztályon~~ karon az 1948/49. tanévekben végezte, s ennek az oklevélnek megszerzésére x) megállapított szigorlatoknak megfelelt és ezzel a vegyészmérnöki tudományokban szerzett Jó képzettségét a szabályszerűen alakított szigorlati bizottságok előtt bebizonyította, a ránk ruházott hatalomnál fogva ezennel „okleveles vegyészmérnöknek“ elismerjük és valljuk.

Ennek hiteléül részére ezt a ~~vegyészmérnöki osztályon szerzett~~

vegyészmérnöki oklevelet

kiszolgáltattuk és ~~a M. Kir. József Nádor Műszaki és Gazdaságtudományi~~ Egyetem [illegible] ~~nagyobb~~ pecsétjével, valamint sajátkezű aláírásunkkal megerősítettük.

Kelt Budapesten, 1949. évi Junius hó 24-én.

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Szerves kémiai technológiából

FELÜLBÉLYEGZÉS JAN 1 MŰSZAKI EGYETEM REKTORI HIVATALA

[illegible] Zoltán s. k.
a ~~M. Kir. József Nádor~~ Budapesti Műszaki ~~és Gazdaságtudományi Egyetem~~ rektora.

Záradékolás! kelt: 1950. III. 18.

Vajda Ödön s. k.
a ~~Gépész- és~~ Vegyészmérnöki Kar dékánja.

3560/a. M. Centrum, Szent Gellért tér

Fig. 11 Copy of George A. Olah’s (György Oláh) Diploma of Chemical Engineer dated June 24, 1949; the grade is “good,” the final examination was in organic chemistry technology. The many amendments of the printed form were due to the fact that in 1949, the school was still using pre-war printed forms (courtesy of the Library of the Budapest University of Technology and Economics)

Fig. 12 Bust of Géza Zemplén in the aula of the central building "K" of the Budapest University of Technology and Economics (photograph by the author)

when Olah wanted to develop an independent research line in fluoro-organic chemistry.

Olah excelled from the start of his research career and in several aspects. He published papers that caught the attention of foreign researchers; he, with a colleague, compiled the index to an organic chemistry text; embarked on writing a book on theoretical organic chemistry; and did his share of teaching. In the years 1950 and 1951 Olah's primary research focus was in carbohydrate chemistry—the area of Professor Zemplén's studies. From 1951, Olah developed his independent line of research.

Soon after graduation at the Technical University, Olah applied for and was granted a scholarship for doing his postgraduate work in a structured framework. This led to the scientific degree, which used to be called "Candidate of Science" following the Soviet example. For all practical purpose, it was equivalent to a PhD degree in a good western university, but it was not granted by a university; rather it was granted by a special degree-granting institution of the Hungarian Academy of Sciences. Olah did all his research at the Technical University, submitted his dissertation in 1953, and defended it in 1954. The dissertation was about the chemistry of organic fluorine compounds; it is in Hungarian; and a copy of it is stored in the Manuscript Collection of the Hungarian Academy of Sciences, available for inspection. It consists of 186 pages with a vast amount of hand-drawn formulae and reaction equations, and it reports about a tremendous amount of innovative synthetic work. The working conditions were poor, the reactants that elsewhere might have been readily available often had to be prepared from scratch, but the work is overwhelmingly impressive.

In his thesis work, Olah applied techniques and procedures of organic fluorine chemistry practiced already elsewhere and invented new techniques and procedures as well. He constructed what he called a Freon reactor (Fig. 13), a technique for fluorinating carbon tetrachloride and chloroform under ultraviolet irradiation using NaF, KF, and CaF_2. He produced chlorofluorocarbons in continuous operation. Olah listed 16 entries as publications containing the materials of his dissertation. They include a series of 12 papers most of which appeared parallel in Hungarian and in English (the latter in the English-language chemistry journal of the Hungarian Academy of Sciences, *Acta Chimica Hungarica*, which no longer exists). In most of these papers, Attila Pavlath, then Olah's student, much later, President of the American Chemical Society, was a co-author. Two entries referred to Olah's inventions of new techniques and procedures for producing organic fluorine compounds, filed one each in 1952 and in 1953. In his summary, Olah stressed the importance of his inventions for the industrial production of Freon compounds. In addition, two entries among Olah's publications referred to papers co-authored with colleagues at the medical school about the impact of organic fluorine compounds on

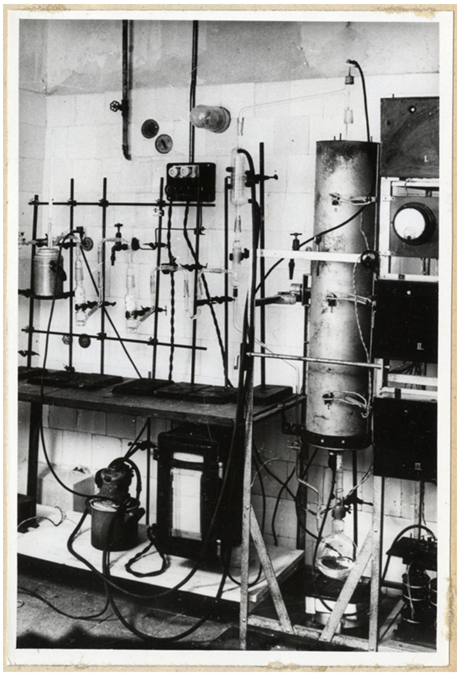


Fig. 13 Olah's "Freon reactor" constructed at the Budapest Technical University in the early 1950s as part of his thesis work (from George A. Olah's PhD-equivalent dissertation; courtesy of George A. Olah and the Library of the Hungarian Academy of Sciences)

experimentally induced tumors in animals. One of these two papers appeared in a German tumor research journal, the *Archiv für Geschwulstforschung*. Olah defended his dissertation and was granted the PhD-equivalent Candidate of Science degree in June 1954.

Olah's interactions with his colleagues in the medical school were anything but superficial. He signed up and completed the first 3 years of the subjects in the medical school, passed the examinations and fulfilled other requirements. All this, he was doing in the years 1951–1953. Also in this period he studied the Russian language and passed the exam for the PhD candidates with flying colors. He had to take also the obligatory political subjects prescribed for the PhD candidates during this same period. Olah listed his working engagements as 64 h weekly, which included 14 contact hours with students. In reality his engagement was most probably more than 64 h per week.

In 1954, Olah was appointed deputy director of the newly organized Central Research Institute of Chemistry of the Hungarian Academy of Sciences. There, Olah found another worthy mentor in the director of the Institute, the physical chemist Géza Schay (1900–1991, Fig. 14). He was a former disciple (postdoc) of the great Hungarian-born physician-turned physical chemist (and later turned philosopher) Michael Polanyi in Berlin, in the years 1926–1928 and 1930. Schay was Professor of Physical Chemistry of the Budapest Technical University (1949–1965) and in 1954, he was appointed director of the new institute. Schay's primary interest in physical chemistry was thermodynamics and reaction kinetics, and in particular, adsorption.

Fig. 14 Relief of Géza Schay in building "F" at the Department of Physical Chemistry of the Budapest University of Technology and Economics (photograph by the author)

Olah must have been working with improbable high intensity and efficiency. Within 2 years from 1954, having been awarded the Candidate of Science (PhD-equivalent) degree, in 1956 he submitted his dissertation for the Doctor of Science degree. This has no exact equivalent in the American system; it is not the same as the British DSc and it is more than the German habilitation. This was a degree in which substantial scientific research production had to be demonstrated and served as prerequisite for a professorial appointment. Furthermore, only those who possessed this degree could be considered for getting elected to the Science Academy. Olah was not yet 29 years old at the time. It sometimes happens that mathematicians produce such a dissertation at an early age, but I know of no other chemist having completed the Doctor of Science work at that age. Olah's dissertation is available for inspection at the Archives of the Hungarian Academy of Sciences. The Manuscript Collection and the Archives are two separate sections of the Library of the Academy. The Manuscript Collection stores the dissertations that had been defended, whereas the dissertations that have not been are stored in the Archives. Olah left Hungary before he could have completed the process of the defense.

Just like Olah's Candidate of Science dissertation, his Doctor of Science dissertation is also substantial. It is in Hungarian and its title in English translation is "Data for the mechanism of electrophilic reactions of aromatic substitution." Almost all publications on which his previous thesis was based appeared in Hungarian journals (even if they were published in English). In contrast, most of the papers for his DSc thesis appeared in important western periodicals, such as the British *Nature* and the *Journal of the Chemical Society* and the German *Chemische Berichte* and *Naturwissenschaften*. Almost all of these papers were co-authored by Olah and his students, Attila Pavlath and Istvan (later, Steven) Kuhn.

The period between 1949 and 1956—the years during which Olah operated in Hungary after graduation—were busy. He established joint research with colleagues at the medical school in Budapest, attended meetings and visited research laboratories in Switzerland, East Germany as well as West Berlin (as they were then), and the Soviet Union, among others. He met with outstanding scientists, such as the Germans Weygand and Bohlmann, the Czech Wichterle, the Romanian Nenitzescu, and the Russian Reutov, Nesmeyanov, Semenov, and Kitaigorodskii. All these Russian scientists were among the top in Soviet science, and not only scientifically. Nikolai N. Semenov was the founding director of the Institute of Chemical Physics of the Soviet Academy of Sciences and in 1956 he was to receive the Nobel Prize in Chemistry. Aleksandr N.

Nesmeyanov was Professor of Organic Chemistry at Moscow State University, the director of the Institute of Organic Chemistry between 1939 and 1954, and from 1954, the director of the Institute of Element-Organic [Heterorganic] Compounds of the Soviet Academy of Sciences. More importantly, he was the President of the Soviet Academy of Sciences between 1951 and 1961. It was at the start of his tenure as president that there was a big conference in Moscow organized to condemn Linus Pauling's resonance theory and condemn its Soviet followers as well. It was part of Stalin's anti-science policies as the theory of resonance was considered as being against Marxist-Leninist dogma. Stalin's anti-science policies were in concurrence with his paranoiac anti-Semitic policies [9]. Olah sensed the impossibility of doing science freely in such an atmosphere and he mentions this in his autobiography.

Olah applied for and was awarded the Dutch van 't Hoff Fellowship (but had no opportunity to utilize it). Reviewing his activities and interactions at the time, his situation may be called exceptionally favorable among his peers. His complaint that "Isolation clearly was a most depressing aspect of pursuing science in Communist-dominated Hungary" ([4], p 62) becomes understandable only if considering the flurry of his later activities under freedom in Canada and the United States.

Toward the summit

In 1949, Olah married a colleague at the Technical University, Judit Lengyel (born in Budapest in 1929; Judit was later changed to Judith, Fig. 15). She was at the time a secretary at the University, but soon she studied chemical engineering and graduated from the Budapest Technical University. They shared a heavy burden of the recent past. In 1944, the Red Cross helped Judit and her 22-year old sister hide in a convent. On December 17, the Hungarian Nazis took them and others and marched them through the city. In a brave moment Judit escaped from the column, went into hiding, and survived; her sister stayed in the column and perished.

Fig. 15 The Olah family in 1962: George, Jr, George, Judith (Judy), and Ronald (Ron). Courtesy of George A. Olah

Judith and George had a boy in 1954 and another boy after they immigrated to Canada. The Olahs left Hungary in November, shortly after the Soviet tanks suppressed the Hungarian Revolution of October 23, 1956, but the borders to the West remained open for a short while. After brief stops in Vienna and London—where Olah initiated valuable interactions with colleagues—they moved on to Canada. Olah started looking for a suitable job already in London and in this he was assisted by Ms. Esther Simpson of the Academic Assistance Council (AAC).

The AAC was formed in 1933 to help refugee scientists from Germany and it was initiated by William Beveridge, Leo Szilard, and a group of internationally renowned British scientists, with Ernest Rutherford as its first president. Ms. Simpson was already working for the organization at that time. The AAC had been renamed to the Society for the Protection of Science and Learning by the time Ms. Simpson was trying to help Olah. Today, the successor of AAC is the Council for At-Risk Academics and it still performs a much needed function.

The Olahs did not intend to stay in England; they were headed to Canada because they had close family connections there. Olah did not find employment in academia and started working in an industrial laboratory of the Dow Chemical Company in Sarnia, Ontario. Years later, when Olah had already become an internationally renowned scientist, a professor of organic chemistry at the University of Toronto apologized to him for opposing Olah's appointment to the University in 1957. Olah was unknown and this professor thought he was not worth the risk of employing him at the University of Toronto.

Olah found an industrial position and he has maintained ever since that "it is good to be challenged" [10]. Beside fulfilling the obligations of his job at the Dow laboratory, Olah continued his fundamental research he had begun in Hungary. In this industrial laboratory, he discovered ways to prolong the lifetime of carbocations and reached results that—again, showing Dow's magnanimity—he was allowed to publish. Olah's publications made an impact and resulted in his invitation to give a talk at the 1962 conference in Brookhaven I referred to in the Introduction. Furthermore, still at the time he was with the Dow Canadian laboratory, the American Chemical Society conferred upon him in 1964 its Award in Petroleum Chemistry.

The broader chemistry community recognized Olah's achievements over the years and the first impulses came through his decisive contribution to the resolution of the

Winstein-Brown controversy (Figs. 16, 17). Gradually, a whole new chemistry was emerging from Olah's discoveries. His first pivotal results came from his works in the Dow laboratory in Canada, from where he moved to another Dow laboratory in Massachusetts. While in industry, Olah was doing everything to maintain his fundamental research and never lose connection with academia. His colleagues in academia responded to his efforts; they visited Olah's laboratory, invited him to participate in seminars and meetings, read and appreciated his papers, and attended the seminars he organized. Dow was good to him, but within limits. His research director did not recognize the significance of NMR spectroscopy and Olah had to bring or send his samples for NMR recording to university laboratories. However, this was another opportunity to enhance his interactions.

Back in his brief sojourn in London in 1957, he established interactions with the English chemist Ronald J. Gillespie (Fig. 18), one of Christopher Ingold's disciples. Ingold was Gillespie's mentor who helped enormously his associates, which was very good in the beginning of a research career, but became burdensome when it was time for Gillespie to establish his independence. Ingold was not the exploiting type and did not let his name figure on Gillespie's papers even when Ingold did write Gillespie's manuscript on the basis of Gillespie's investigation. However, Gillespie was told by Ingold who his graduate students should be, what apparatus to acquire next time, and so on. When an opportunity arose for a fully independent position at McMaster University in Canada, combined with the possibility of acquiring the most up-to-date equipment, such as a high frequency NMR machine, Gillespie moved [11]. He then welcomed Olah's technician to run Olah's samples on his NMR equipment. Gillespie's laboratory was not the only one that assisted Olah and his group with NMR spectroscopy during Olah's industrial activities.

Fig. 16 Saul Winstein in 1951 (photograph by and courtesy of J. D. Roberts)

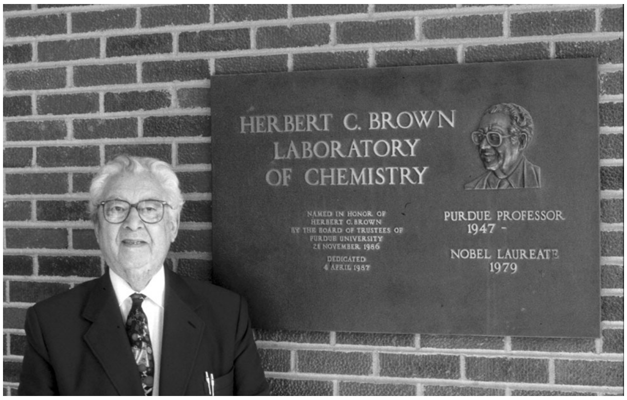


Fig. 17 Herbert C. Brown in 1995 in front of the plaque of the Herbert C. Brown Laboratory of Chemistry at Purdue University (photograph by the author)

Fig. 18 Ronald J. Gillespie in 1998 in Austin, TX (photograph by the author)

Gillespie's and Olah's research interests had an important overlapping area, and that was the superacids. The Harvard professor James B. Conant coined the name superacids for very strong acids as early as 1927, but he did not define their strength. Gillespie did just that in the 1960s, and according to him superacids are protic acids stronger than 100 % sulfuric acid. Gillespie did a great deal of pioneering work in the superacid field. Olah recognized this to the extent that according to him, "Had the Nobel Prize been given for superacids, Ron in my opinion—as he well knows—should have certainly been included" [12]. However, the Nobel Prize was given for Olah's discoveries in carbocation chemistry, and it was, most deservedly, an unshared award.

In hindsight, it was almost inevitable that sooner or later Olah would find his way back to academia. This happened in 1965 when he moved to Cleveland as Professor and Chairman of the Department of Chemistry of Western Reserve University and he stayed in Cleveland for a decade (Fig. 19). He showed his acumen as an organizer, but he never slowed down in his research and fulfilled enthusiastically his teaching duties as well. The five portraits on the wall of his Cleveland office demonstrated his loyalty and his sense for the importance of continuity. When in 1996, he was asked the question: "Who is your biggest influence/hero and why?," his response was: "Hans Meerwein, who never considered himself a 'hyphenated' chemist and contributed much to synthetic, as well as mechanistic chemistry" [7].

Olah strengthened the chemistry department and by far not only through his own activities and those of his group. For example, he invited his fellow émigré chemist Miklos Bodanszky (Fig. 20), well known for his research in peptide chemistry and for his monographs in the field. As an organizer, Olah had an eye for the obvious that is sometimes the most difficult to notice: Across from his department, there was another chemistry department with only a parking lot between them. The other chemistry department belonged to the Case Institute of Technology. Within a couple of years, at Olah's initiative, the two departments joined and the merger was so successful that subsequently the two schools joined as well, creating Case Western Reserve University as it is well known today. Olah served as chair for the joined department for a while, but then he let others run it.

Fig. 19 George A. Olah in 1976 in his office in Cleveland. The pictures on the wall are of Hans Meerwein, Christopher K. Ingold, Saul Winstein, Herbert C. Brown, and Frank Whitmore. Courtesy of George A. Olah

Fig. 20 Miklos Bodanszky in 1999 in Princeton (photograph by and courtesy of Eszter Hargittai)

Olah's acumen as researcher manifested itself also in bringing together all the techniques that he found necessary for solving the problems he was working on. It was not only a task of finding the right instrumentation but finding the right experts as well. At some stage it became obvious—at least to Olah—that the reliable solution of the carbocation problems could not happen without high-level quantum chemical computations. This is how his life-long cooperation and friendship developed with Paul von Ragué Schleyer (Fig. 21) [13]. Schleyer's computations contributed significantly to Olah's discoveries as the application of physical techniques and computation become jointly much more powerful than the sum of the two approaches when applied independently of each other. Schleyer's commitment to Olah's research lasted his entire research career and he returned to the question about the structure of the 2-norbornyl cation in one of his last papers. In it, he and his colleagues showed unambiguous X-ray crystallographic evidence, in concert with high-level computations, for the bridged, non-classical geometry of this carbocation [14].

At the top

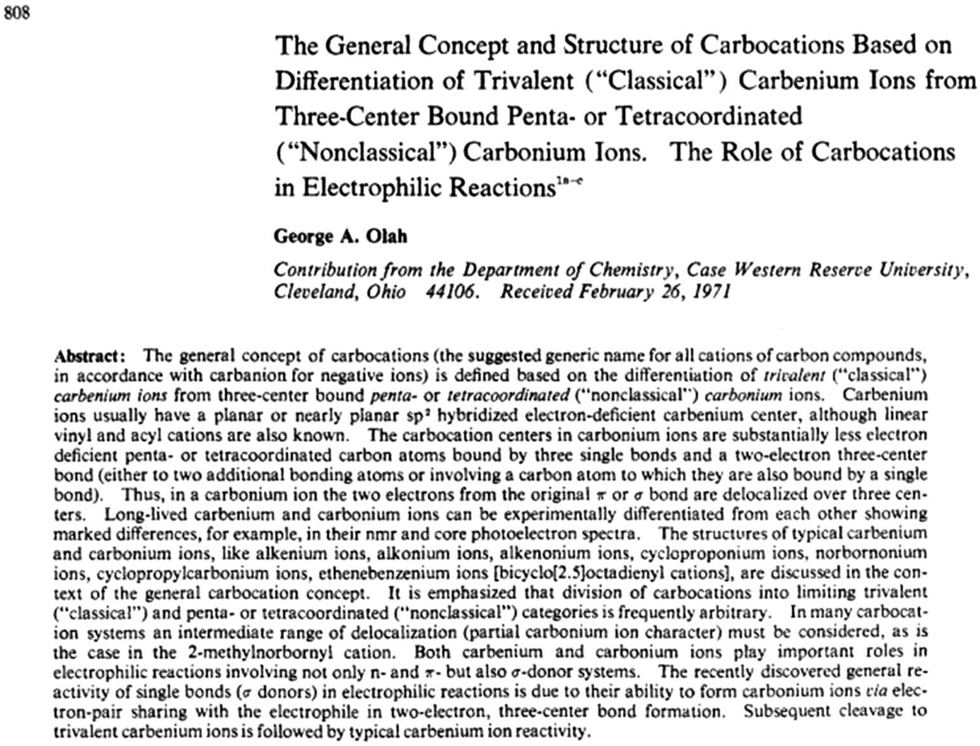
In 1972, Olah published a seminal paper in the *Journal of the American Chemical Society* (Fig. 22) in which he described the general concept of carbocations. His

Fig. 21 Paul von Ragué Schleyer in 1995 in Vicksburg, MS (photograph by the author)

discovery of the reactivity of sigma donor single bonds in electrophilic reactions was nothing short of a revolution in hydrocarbon chemistry. The reactivity of these single bonds was "due to their ability to form carbonium ions via electron-pair sharing with the electrophile in two-electron, three-center bond formation." ([15], p. 808) There are a few characteristic drawings to illustrate a few aspects of the production and structure of carbocations after Olah. These are from a book series of the chemistry division of the Hungarian Academy of Sciences in which these drawings were reproduced (Figs. 23, 24, 25) [16].

In 2015, Olah published the second, updated edition of his autobiographical book, *A Life of Magic Chemistry* [4]. In it, he once again evaluated the significance of the norbornyl controversy. He did not formulate his views on this anew; rather, he referred to a conversation I recorded with him in 1996 in his office at the University of Southern California. The conversation was printed in full in 2000 ([10], pp 273–274) and the quoted passage was printed again in 2014 [17]. This is what Olah reproduced in 2015:

> I came into it because around 1960 I discovered methods to generate positive organic ions, called now carbocations, as long-lived species, and we were able to take all kinds of spectra and establish their structure, including that of the norbornyl cation. In the course of this work I realized, however, that the problem has much wider implications. In the norbornyl ion the C–C single bond acted as an electron donor nucleophile. In this particular case this happens within the molecule, that is, intramolecularly. This delocalization, which had been originally suggested by Winstein, was indeed there and we were able to



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The General Concept and Structure of Carbocations Based on Differentiation of Trivalent ("Classical") Carbenium Ions from Three-Center Bound Penta- or Tetracoordinated ("Nonclassical") Carbonium Ions. The Role of Carbocations in Electrophilic Reactions^{1a-c}

George A. Olah

Contribution from the Department of Chemistry, Case Western Reserve University, Cleveland, Ohio 44106. Received February 26, 1971

Abstract: The general concept of carbocations (the suggested generic name for all cations of carbon compounds, in accordance with carbanion for negative ions) is defined based on the differentiation of *trivalent* ("classical") *carbenium ions* from three-center bound *penta-* or *tetracoordinated* ("nonclassical") *carbonium* ions. Carbenium ions usually have a planar or nearly planar sp^2 hybridized electron-deficient carbenium center, although linear vinyl and acyl cations are also known. The carbocation centers in carbonium ions are substantially less electron deficient penta- or tetracoordinated carbon atoms bound by three single bonds and a two-electron three-center bond (either to two additional bonding atoms or involving a carbon atom to which they are also bound by a single bond). Thus, in a carbonium ion the two electrons from the original π or σ bond are delocalized over three centers. Long-lived carbenium and carbonium ions can be experimentally differentiated from each other showing marked differences, for example, in their nmr and core photoelectron spectra. The structures of typical carbenium and carbonium ions, like alkenium ions, alkonium ions, alkenonium ions, cycloproponium ions, norbornonium ions, cyclopropylcarbonium ions, ethenebenzenium ions [bicyclo[2.5]octadienyl cations], are discussed in the context of the general carbocation concept. It is emphasized that division of carbocations into limiting trivalent ("classical") and penta- or tetracoordinated ("nonclassical") categories is frequently arbitrary. In many carbocation systems an intermediate range of delocalization (partial carbonium ion character) must be considered, as is the case in the 2-methylnorbornyl cation. Both carbenium and carbonium ions play important roles in electrophilic reactions involving not only n- and π- but also σ-donor systems. The recently discovered general reactivity of single bonds (σ donors) in electrophilic reactions is due to their ability to form carbonium ions *via* electron-pair sharing with the electrophile in two-electron, three-center bond formation. Subsequent cleavage to trivalent carbenium ions is followed by typical carbenium ion reactivity.

Fig. 22 Part of the first page of the pivotal paper in the *Journal of the American Chemical Society* in which Olah suggested the carbocation name and described the general concept and structure of carbocations [15] (© 1972 American Chemical Society)

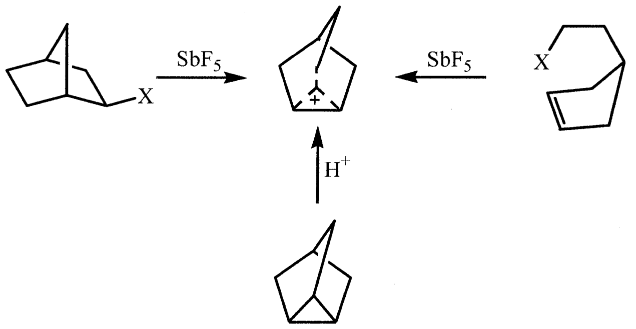


Fig. 23 A variety of routes to the long-lived non-classical 2-norbornyl ion ([16], p 35)

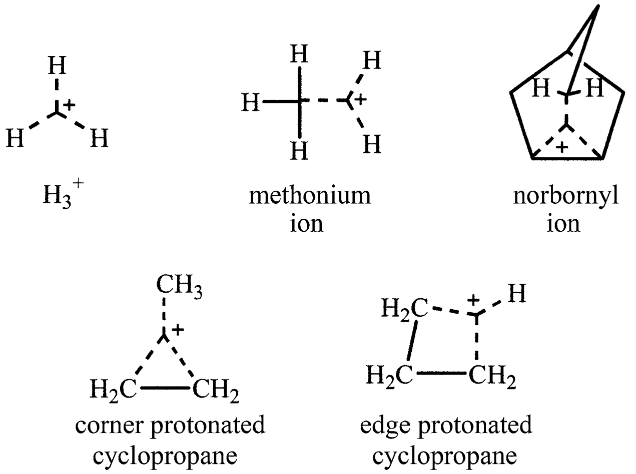


Fig. 24 Characteristic bonding examples in non-classical ions ([16], p 33)

> see it directly for the first time. Later came, what I thought was a logical idea. The question what I asked myself one day was, if this can happen within the molecule, why can't it happen between the molecules? This led to the discovery of a wide range of electrophilic reactions of saturated hydrocarbons, that is, of C–H or C–C single bonds and the realization that carbon, under some conditions, can indeed bind five or even more neighboring groups ([4], pp 152–153).

Structure of carbocations: The case of CH_5^+

The overlapping interests of Olah and Gillespie were manifest also in the application of Gillespie's qualitative model for molecular geometry for testing some of Olah's unusual structures. Gillespie's valence shell electron pair repulsion (VSEPR) model or theory predicts the geometry of the molecule on the basis of the number of electron domains (bonding pairs, lone pairs, multiple bonds) in the valence shell of its central atom [18]. The predicted shapes and symmetries depend not only on the general number of electron domains but to various extents also on the nature of those domains, whether they are single bonds, lone pairs or multiple bonds. For the shape of CH_5^+, that is, for five electron domains in the valence shell, Gillespie's model would predict a trigonal bipyramid or a tetragonal pyramid—these two configurations are too close in energy to be distinguished unambiguously.

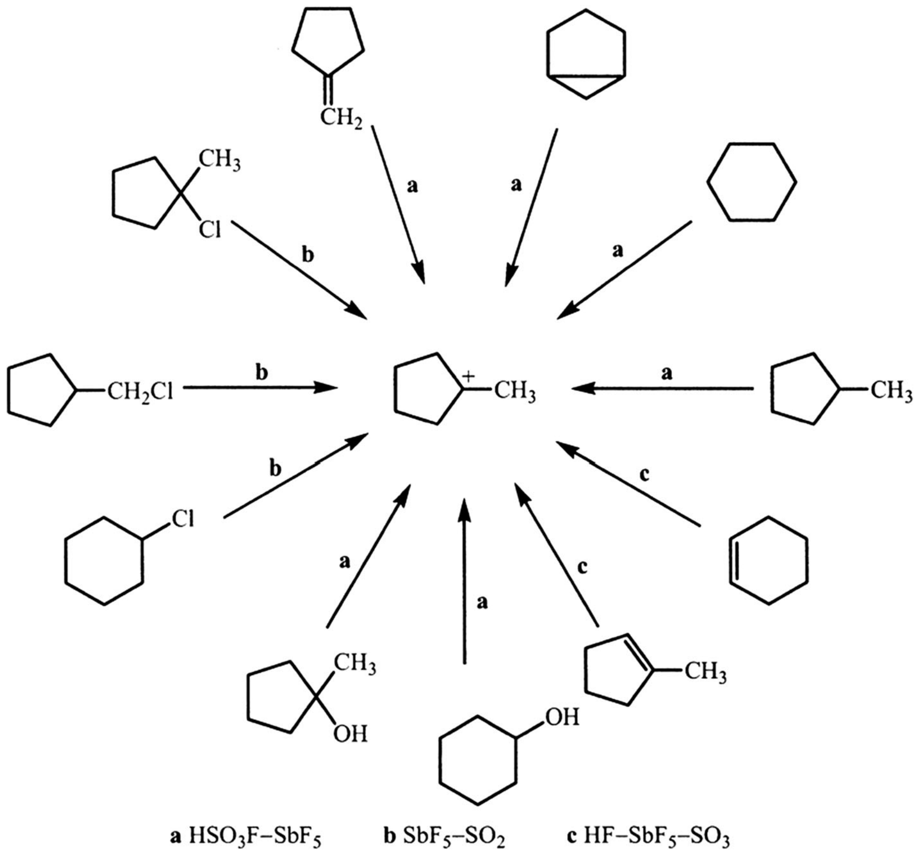
From the beginning, however, computations predicted a C_s symmetry structure for the CH_5^+, which would correspond to the presence of three two-electron two-center bonds and one two-electron three-center bond (see, e.g., [4], p 158). This structure can be viewed either as having a high-degree of localization, or as having a fluxional character by exchanging the positions of the two-electron two-center bonds and the two-electron three-center bond. If the C_s symmetry structure corresponds to a sufficiently deep energy well, it can be observed in experiments, provided that the life-time of this structure is sufficiently long as compared with the interaction time for the physical technique employed. In this respect, the spectroscopic techniques, and NMR spectroscopy especially, are at disadvantage as compared with the diffraction techniques. The interaction times of the former are orders of magnitude longer than those of the latter. Alas, for practical reasons, the structure determination of CH_5^+ by diffraction techniques would not be feasible.

Highly sophisticated high-resolution spectroscopic experiments on CH_5^+, as recent as 2015, have suggested the preeminence of geometries fully consistent with Olah's description of the structure (Fig. 26) [19]. There is a caveat though, because all the spectroscopic evidence point to the highly fluxional character of this carbocation: "the five proton swarm around the central carbon" [20].

Structural studies and considerations for the CH_5^+ carbocation have a rich history (going back much before this nomenclature had been introduced) [21]. Historically, the structure of CH_5^+, called also the methonium ion, has presented a puzzle ever since it was first observed experimentally in 1952 in a mass spectrometric investigation [22]. Before the first computational studies, it used to be an assumption that the five hydrogens around the carbon would be equivalent or close to equivalent, which means a a VSEPR-type geometry. The early computational studies suggested this carbocation consisting of two parts; one, a pyramidal CH_3^+ unit and the other, a hydrogen molecule bound to it. In other words, they were consistent with Olah's model of three two-electron covalent bonds and one two-electron three-center bond.

Thus, the VSEPR model could not predict the geometry of CH_5^+. In contrast, the geometry of monopositively charged carbocation $\{[(C_6H_5)_3PAu]_5C\}^+$ containing five-coordinate carbon has been found to be trigonal

Fig. 25 The utilization of a variety of precursors for the preparation of the methylcyclopentyl carbocation ([16], p 27)



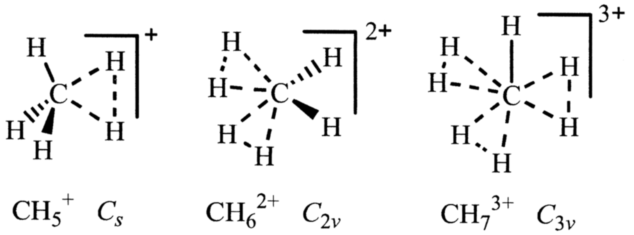
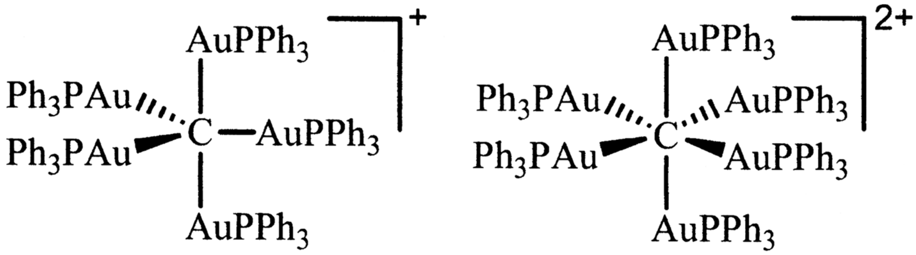


Fig. 26 Two-electron two-center bonds and two-electron three-center bonds in protonated alkanes ([16], p 38)

bipyramidal in agreement with VSEPR predictions (Fig. 27). As Olah has suggested, this gold complex represents an isolobal analog of CH_5^+, hence the isolobal analogy would favor a trigonal bipyramidal geometry for CH_5^+ as well, which, as we have seen, was not the case. Considering the highly fluxional character of the CH_5^+ carbocation, it means not only the exclusion of VSEPR-type configurations but also a distinct CH_3^+ plus H_2 configuration in which three distinguished hydrogens form two-electron two-center bonds and two hydrogens participate in one two-electron three-center bond.

A discussion similar to the CH_5^+ carbocation could be provided for the CH_6^{2+} carbocation and the $\{[(C_6H_5)_3PAu]_6C\}^{2+}$ carbocation. Six equivalent electron domains would favor a regular octahedral geometry (of O_h symmetry). In the gold complex, indeed, the six bonding directions point to the vertices of a regular octahedron in agreement with the prediction of the VSEPR model. In contrast, for the CH_6^{2+} carbocation, again, the computations have suggested the presence of two two-electron covalent bonds and two two-electron three-center bonds.

Fig. 27 The trigonal bipyramidal monopositively charged carbocation $\{[(C_6H_5)_3PAu]_5C\}^+$ and the octahedral dipositively charged carbocation $\{[(C_6H_5)_3PAu]_6C\}^{2+}$ ([16], p 38)



There is then the CH_7^{3+} carbocation with one two-electron covalent bond and three two-electron three-center bonds.

Here, we are dealing with hypercarbons, though not with hypervalent carbons! The distinction is important. Carbon has no d orbitals available, hence it cannot extend its valence shell; it can only extend its coordination. Thus the correct reference to it is hypercoordinated carbon rather than hypervalent carbon. ([4], p 160)

We still do not know the structure of CH_5^+ *in full*, but by now we do not know it at a much higher level of sophistication than before. One of the leading spectroscopists of this and similar species, Takeshi Oka of The Enrico Fermi Institute of the University of Chicago, called CH_5^+ "the 'enfant terrible' of chemical structures." According to Oka, its theoretical understanding "will take at least a few more decades"! [20].

The Nobel Prize

In 1977, Olah and his group moved to the University of Southern California (USC) at Los Angeles. There he developed a research institute known today as the Loker Hydrocarbon Research Institute. It focuses its research efforts in a single well-defined area. This is unusual for the American university setting, but it is not unlike some European research laboratories of science academies. Olah has emerited from the directorship, which G. K. Surya Prakash took over from him, and Olah has continued with the title of Founding Director. Donald P. and Katherine B. Loker were the principal benefactors of the Institute but others have contributed generously to it as well. Olah's Nobel Prize brought a great deal of publicity to the Loker Institute, but the Institute had been operating with great intensity and efficacy for years.

George A. Olah received the Nobel Prize in Chemistry for 1994. It was an unshared award, which is not too common as far as recent science Nobel Prizes go. On the other hand, awarding Olah the Nobel Prize and an unshared one at that met with universal satisfaction, which is also not too common. It was obvious to all that he deserved it and deserved receiving it alone. The official motivation for the prize was terse and even sounded a little noncommittal: "for his contribution to carbocation chemistry." I would not just call his works "contributions;" rather, they were bona fide discoveries.

The presentation speech by Salo Gronowitz, the Chairman of the Nobel Committee for Chemistry at the award ceremony gave a more direct description of why Olah received the award. He said, in part, "Olah's discovery resulted in a complete revolution for scientific studies of carbocations, and his contributions occupy a prominent place in all modern textbooks of organic chemistry." Further, Gronowitz noted: "Olah demolished the dogma that carbon in organic compounds could at most be tetracoordinated, or bind a maximum of four atoms" [2].

Gronowitz stressed that "Olah found that superacids were so strong that they could donate a proton to simple saturated hydrocarbons, and that these penta-coordinated carbonium ions [in Olah's nomenclature, carbocations] could undergo further reactions" [2]. In other words, even a carbon-carbon single bond or a carbon-hydrogen single bond could become an electron donor under the impact of an extremely strong proton donor, that is, in the presence of superacids. There is no doubt that Olah recognized the significance of his discovery for the whole of chemistry as he stated what we chose as the motto for this overview: "The realization of the electron donor ability of shared electron pairs could one day rank equal in importance with G. N. Lewis' realization of the electron donor unshared pairs" ([1], first pronounced in Olah's 1972 paper [15]).

The Nobel Prize in Chemistry is supposed to be given specifically "to the person who shall have made the most important chemical discovery or improvement" according to Alfred Nobel's Will (Fig. 28). This stipulation is preceded by a general one applicable to all categories of the prize: the awardees "shall have conferred the greatest benefit on mankind." Of course, even a discovery of purely fundamental nature with no foreseeable practical applications may qualify for conferring great benefits on mankind (the more so that seldom are there even purely fundamental discoveries that would not become eventually the roots of practical applications). However, it is always an advantage for the award selection if a discovery will have already shown direct benefits for improving the lot of humankind by the time of the Nobel Prize. Salo Gronowitz pointed out such features of Olah's discoveries in his presentation speech. Here are two examples from that speech: One that [Olah's] "discoveries have led to the development of methods for the isomerization of straight chain alkanes, which have low octane numbers when used in combustion engines, to produce branched alkanes with high octane numbers." Another is "With superacid catalysis it is also possible to crack heavy oils and liquefy coal under surprisingly mild conditions" [2].

This synergy of fundamental discoveries and their practical applications has continued shaping Olah's activities ever since the Nobel Prize as well. He decided not to sit on his laurels but continue his work and the practical applications may have taken up an even greater share of his activities since the Nobel distinction than before. This was so not necessarily by design, but because he had reached a stage in his research when the application of the vast amount of accumulated fundamental knowledge became almost inevitable. Thus, for example, to utilize the possibilities of "hydrogen economy," and to utilize them safely,

Fig. 28 A group of Nobel laureates at the Award Ceremony in Stockholm on December 10, 2001 (Photo by Hans Mehlin, © The Nobel Foundation, reproduced with permission). *Row 7* Klug, X, Huber, X, Varmus (partly hidden), Bishop; *Row 6* Blobel, M.S. Brown, Goldstein, H.C. Brown, Neher, Gilman; *Row 5* Doherty (partly hidden), Heeger, Pople, Lewis (partly hidden), Nüsslein-Volhard, Wieschaus; *Row 4* MacDiarmid, Shirakawa, Olah, Kroto, Schally (partly hidden), Mullis; *Row 3* Nirenberg, Crutzen, E.H. Fischer, E.G. Krebs, Watson; *Row 2* Benaceraff, Walker, Gilbert, T.N. Wiesel, Rowland; *Row 1* Samuelsson, Vane, Edelman, Jacob; *Row 0* Two members of the Royal Family: Prince Carl Philip and Princess Lillian

Olah proposed storing the hydrogen in the form of methanol—this has become known as his "methanol economy." He recognized the utility of this approach at several levels. It is not only good storage; its production by reduction of the carbon dioxide in the atmosphere helps to counter global warming. The development of the direct methanol fuel cell was a natural outcome of these efforts. Olah's updated autobiographical volume provides detailed and readable accounts of the methanol economy and the methanol fuel cell.

Secrets, not classified

One wonders if there is any secret that would be the clue to Olah's great success. In fact, there appear to be many such secrets, fortunately, none of them is classified, and here we mention a few. Of course, every scientist must travel his or her own road and there is no generally valid single route to recommend. Olah benefitted from a broad-based education. That his interest in chemistry came relatively late in his youth meant the development of his great interest in reading and in history.

It is instructive to follow the carbocation story in perspective with Olah as a knowledgeable guide. Even in possession of an unshared Nobel Prize he is sufficiently humble to give credit where it belongs and go back to the roots of his science ([4], pp 72–76). As early as 1899, Julius Stieglitz at the University of Chicago raised the question of the possibility of ionic carbon compounds. This was an isolated episode that did not generate any follow up although Stieglitz was an influential chemist. Just as an aside, Herbert C. Brown, who has figured in Olah's story above, attended Stieglitz's lectures years later. Brown referred to his interactions with his professor in this way "That began my acquaintance with Julius Stieglitz, one that changed my life" ([10], p 255).

In the early 1900s, subsequently, several researchers produced and described compounds that could be interpreted as having ionic carbon in them. Hans Meerwein discovered in the 1920s that there are reactions that, while both the reactants and the products are covalent compounds, may have ionic intermediates (carbocations, in today's nomenclature). Still in the 1920s, Ingold, Hughes and their associates discussed further the role of carbocations in reactions.

Olah gives much credit to Frank Whitmore who in the 1930s established the transient role of the ionic intermediates that could not be observed directly, but just had to be there. Sadly, Whitmore's ideas met with so much disbelief that he could not use the trivial notation of cationic carbon species in his papers published in the *Journal of the American Chemical Society*. No wonder, there is loneliness for true discoverers.

In subsequent years, the scenery was changing and broadly recognized chemists took up the problem of cationic carbon species to which they ascribed the presence of transient intermediates in some organic reactions. The famous debate between Winstein and Brown developed, and the conditions had gradually become ripe for Olah's discoveries. With Saul Winstein's untimely death in 1969, Olah had to take up Winstein's role and the discussions continued to 1983 when there was no longer any doubt that there was nothing more to argue about; the idea of the non-classical ion presence had been proved unambiguously.

Olah though did not find the debates superfluous, because they have contributed to a better formulation of his discoveries. Olah appreciated the utility of criticism and he fully embraced what another Hungarian-born American Nobel laureate Georg von Békésy advocated in 1960 about the need of a few selfless enemies: "[One] way of dealing with errors is to have friends who are willing to spend the time necessary to carry out a critical examination of the experimental design beforehand and the results after the experiments have been completed. An even better way is to have an enemy. An enemy is willing to devote a vast amount of time and brain power to ferreting out errors both large and small, and this without any compensation. The trouble is that really capable enemies are scarce; most of them are only ordinary. … Everyone, not just scientists, needs a few good enemies" [23]. Olah noted that the term adversaries would be a more proper term than enemies in this case. As it happened, nobody could stay long even to be an adversary to Olah, and his former adversaries have become his friends.

"The idea that ionization of alkyl fluorides to stable alkyl cations could be possible with an excess of strong Lewis acid fluoride that also serves as solvent first came to me in the early 1950s while I was still working in Hungary…" ([4], p76) Here Olah magnanimously dates the origin of his road to success back to his tenure at the Budapest Technical University. His direct observation of the long-lived carbocations, called also persistent, happened in the late 1950s at the Sarnia, Ontario, Dow Chemical laboratory. Thus, these two pivotal steps came about in two supposedly unlikely places for important fundamental research. It is not surprising that Olah in his autobiography shares his wondering about the advantages and disadvantages of the famous research universities and the venues he had labored in from Budapest via Sarnia and Cleveland, to Los Angeles. He offers encouraging words to those that, like himself, were not born with a silver spoon as far as research conditions were concerned.

It is a proof of Olah's greatness that if the conditions were not around, he created them. Witness to this the Loker Hydrocarbon Research Institute—a unique institution for an American university setting. He must have been a persuasive individual who could share his enthusiasm and dedication with people of means that were outside of chemistry and outside of science. Hydrocarbon chemistry may have not sounded too exciting even to many chemists and yet Olah could convince people of business that it was. He was right, of course; it is easy to see this in hindsight. Olah has had loyal friends and supporters and he has always been a loyal mentor, teacher, and colleague. When he was escaping from Hungary with his wife and little child, he was also thinking of his associates. I have had limited personal interactions with Olah, but even being far outside of the center of his activities I felt his care in the warmest of ways. How privileged it could have been to be closely associated with him. Olah states it unequivocally, "I have always put great importance on loyalty" ([4], p 85).

Olah always gave priority to research over positions and was "never bitten by the bug that makes many people feel important by exercising power." ([4], p 87) He rarely held administrative positions, except for the chair in Cleveland and the directorship of the Loker Institute. He held that the people who do not really want to give up their research and teaching make the best university administrators, because they have the intention and a place to return to their natural calling when their administrative tenure is over. He did not decline invitations to join various committees. However, while in the committee, he freely spoke his mind and this he found to be a foolproof method of never getting asked back to the committee again.

Olah always took teaching duties seriously and in his teaching, he conveyed his personal experience as a researcher. He maintains that good teaching supposes successful research activities. He makes this general comment that monies for education should not be considered expenses; rather, they should be considered investment [24]. He spoke about this at the University of Szeged in Hungary where the sources for education are in the state budget and politicians tend to decrease these sources for education first when the budget has to be cut.

Legacy

Olah has continued his interest and his participation in his science and human affairs, but approaching 90 it is inevitable that the question of legacy comes to mind. Olah is leaving a multifaceted and rich legacy. When reviewing his legacy it becomes clear that his resolving the famous Winstein-Brown dispute is fading away. In contrast, his discoveries that made the resolution of the dispute possible shine in ever increasing intensity. Olah applied the extremely strong superacids to prolong the lifetime of carbocations and his realization of the electron donor ability of shared electron pairs opened a new direction in organic chemistry—one may wonder whether the same realization would not open new vistas in inorganic chemistry as well. Olah's new chemistry led to the creation of countless new compounds and he has enhanced the practical applications of newly synthesized substances. This did not just happen; Olah has always had an eye for and interest in practical applications. The culmination of his efforts in this aspect was the emerging methanol economy for which future developments will be the measure of the scale of its success.

The mention of success brings us to an exchange with Olah I had in 2003 about its meaning for him. This is what he had to say in response to my query:

> Success in science it looks to me means different things to different people. Many judge it in outside recognition of someone's work (prizes, membership in academies, honorary degrees, quotation numbers etc.). These may please the ego, but frankly are only trimmings. What I always felt is important is your inner satisfaction. After all, you should know best, if you are honest about it, whether you had achieved something in your scientific field, which has some lasting importance to our knowledge and understanding. If unexpectedly this can have also some application and benefit to society it adds to the feeling of success. However, most scientists are generally quite selfish and are inquiring because of their personal interest in a topic, which drives them not necessarily because they want to do something for society. Some of course judge success also based on material aspects (i.e. making money) but frankly, this never tempted me [25].

His books have a considerable place in Olah's legacy as this is similarly valid for many scientists. When I asked James D. Watson of the DNA double-helix fame about what he expected to be his longest ranging impact, his response was: "Probably my books" [26]. Watson thought that the DNA discovery "was just waiting to be made," but as for his *Double Helix* book, it "was probably unlikely to have been written by anyone beside myself." The relationship of books and discoveries may be different in Olah's legacy, but his books have undeniable importance. They have closely followed his progress in research. Whenever he completed his work in one research area, a summarizing monograph or an edited volume followed. This makes it possible to compile an approximate progression of his research career on the basis of these books. It started with his treatise on theoretical organic chemistry, on which he worked in Budapest and completed in Canada, to appear then in German in 1960. This was also part of his learning process. A selection of more research oriented volumes follows here without co-authors and co-editors and without full bibliographic references, which can be found elsewhere, for example in Olah's autobiographical volume. These books started appearing right from the start of Olah's research career.

Friedel–Crafts and Related Reactions (edited, in four volumes, 1963–1965)
Carbonium Ions (edited in four volumes, 1968–1973)
Carbocations and Electrophilic Reactions (1973)
Friedel–Crafts Chemistry (1973)
Halonium Ions (1975)
Superacids (1985)
Hypercarbon Chemistry (1987, updated 2011)
Nitration: Methods and Mechanisms (1989)
Cage Hydrocarbons (edited, 1990)
Electron Deficient Boron and Carbon Clusters (edited, 1991)
Chemistry of Energetic Materials (edited, 1991)
Synthetic Fluorine Chemistry (edited, 1992)
Hydrocarbon Chemistry (1994; 2004)
Onium Ions (1998)
A Life of Magic Chemistry (2000, updated 2015)
Across Conventional Lines (edited, selected papers, two volumes, 2003)
Carbocation Chemistry (2004)
Beyond Oil and Gas: The Methanol Economy (2006, updated 2009)
Superelectrophiles and Their Chemistry (2008)
Superacid Chemistry (2009)
Across Conventional Lines (edited, selected papers, third volume, 2014)

The synergy of fundamental science and the applications of its achievements is another important component in Olah's legacy. One might think that hydrocarbon chemistry is such a field that by its nature is close to practical aspects. However, some scientists in purely fundamental areas may also be more interested in practical applications than others. Eugene P. Wigner and John von Neumann, for example, were theoreticians, yet they were eager to find challenges related to applications, especially when they sensed the need for them [27]. For von Neumann, it meant primarily building computers and for Wigner, to use his knowledge of materials to help developing nuclear reactors. I am not suggesting that the shared origin and the shared life experience of Olah, Wigner, and von Neumann played a role in their shared interest in turning their scientific acumen into practical use, but the thought has crossed my mind.

I consider it part of Olah's legacy, the example he has set before others with his human demeanor. He has demonstrated that even a great scientist can stay a caring human being, a loyal friend, and an individual dedicated to assist others. He demonstrated tremendous inner strength when just surviving the last life-threatening months of persecution in 1945, he caught up with his school duties and passed his matriculation examinations at the Gimnázium of the Piarist Order. He then began his studies at the Budapest Technical University without any interruption let alone skipping a year, which could have been understandable. He succeeded in overcoming all barriers in the gradually hardening communist dictatorship of the early 1950s. As a refugee, he used the London sojourn of his family to build interactions with fellow scientists that would soon become useful especially during his tenure at the Dow Chemical industrial laboratory. When it became clear that no academic appointment would be available, he adjusted himself to the conditions of an industrial laboratory; built up his fundamental research after-hours; and in addition to fulfilling his duties, he functioned also as if it were an academic research venue, holding research seminars and attracting world-renowned scientists for visits.

His associates and disciples have always had a special place in Olah's life and activities. It was so during his Budapest years and it was so when he was making his escape with his family and his associates. It was also so from the start of his career in North America, and later in the Loker Hydrocarbon Research Institute. His care for his associates was always present regardless of his difficulties in securing a job, raising a young family, and overcoming all barriers that came his way. He concerned himself with the professional progress of his disciples and about the well-being of his students. Just an atypical example of the use of prize monies for an American professor: In 1979, the American Chemical Society presented Olah with the Award for Creative Work in Synthetic Organic Chemistry. He "used the prize money to send his students and postdoctoral researchers on a vacation to Hawaii" [28].

Wisdom and gaiety helped Olah to overcome difficult situations in his career. Although he states that "Human nature helps to block out memories of hardship and difficulties," ([4], p 294) some crept through even into his autobiography. It is telling that he considered hardship as character-building, but there was a limit to how much of it he wanted to tolerate. He remembers that "personal attacks and criticism which frequently came along were at times not easy to take" ([4], p 268). Early on, Olah received the lesson that being successful, even mildly successful, will generate envy. There were always some who would enjoy seeing him and his group failing. It may have helped him that he had experienced this kind of responses a great deal before his immigration. It is quite telling that the English language has no succinct equivalent to the German word "Schadenfreude" (enjoyment obtained from the troubles of others). As Olah took setbacks and difficulties in stride, he did not allow Schadenfreude to get in the way.

Fig. 29 George A. Olah and Istvan Hargittai on February 20, 1996, the day of the Inaugural George A. Olah Lecture at the University of Southern California (photograph by and courtesy of Magdolna Hargittai)

Olah has found great joy in chemistry, and chemistry has remained the focus of his attention throughout. However, this did not prevent him from seeing the beauty of the rest of the world surrounding us. I single out his interest and fascination with the concept of "Symmetry," which is present in both science and the arts, and serves to connect them. This played a role in our personal interactions, which started before his Nobel Prize, a fortunate circumstance, because the number of a Nobel laureate's friends usually grows exponentially after the award, but the friendships that had begun before it usually prove to be stronger.

When in the spring of 1995 George and Judy visited us in Budapest, we talked, among others, about the symmetry concept. My wife and I had already been producing books about symmetry, first about symmetry in chemistry and, eventually, about symmetry everywhere else [29]. George invited me to give a lecture on symmetry at his university in February 1996 (Fig. 29). By the end of December 1995, everything was settled for this event planned for February 20, 1996. At that point, the chair of the USC chemistry department informed me that they were setting up a George A. Olah annual lectureship and decided to transform my presentation into its inaugural lecture. This is how it happened that my talk as "The 1st George A. Olah Lecture in Chemistry" on February 20 at USC was about symmetry. Plenty of chemistry found its way into this presentation [30].

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EDITORIAL

Sydney Brenner (1927–2019)—One of the greats of our science on new frontiers

Istvan Hargittai[1]

Abstract

Sydney Brenner was among those who created the modern science of molecular genetics. He was primarily an experimentalist yet his philosophical views on science will also have long-time impact on those practicing this noble trade. His views and teachings are worthy of sharing. This Editorial is based on our recorded conversation in 2003 when we met in Cambridge as part of the celebrations of the 50th anniversary of the discovery of the double-helix structure of DNA.

Keywords Sydney Brenner · Francis Crick · Genetic code · Computability · Mentoring · Future of nature research · Human Genome Project

Introduction

Sydney Brenner (Fig. 1) was born in 1927 in Germiston, South Africa, to East European Jewish immigrant parents. They were poor and the public library played an important role in Brenner's education. He studied at and received his first degrees from the University of Witwatersrand. An "1851 Exhibition Scholarship" brought him to Oxford, England, where he earned his DPhil degree. For decades he was an associate of the MRC Laboratory of Molecular Biology (LMB) in Cambridge for which he even served as director in the years 1979–1986. He directed the MRC Molecular Genetics Unit in the years 1986–1991. In 1995, he founded the Molecular Sciences Institute in Berkeley, California, from which he retired in 2000. Then, he spent a period of time with the Salk Institute in La Jolla. He had a long association with Singapore science. There, he helped launch the Institute of Molecular and Cell Biology and called Singapore his home during the last period of his life.

Sydney Brenner was awarded the Nobel Prize in Physiology or Medicine in 2002 together with H. Robert Horvitz and John E. Sulston, who both could be considered

Fig. 1 Sydney Brenner in 2003 in Cambridge, England (photograph by István Hargittai)

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his disciples, "for their discoveries concerning genetic regulation of organ development and programmed cell death." In this connection, I note, that many of us had felt for quite some time that Brenner should have received the Nobel Prize for his many discoveries in molecular genetics. It was known that Francis Crick and Brenner used to share an office at the LMB for a long time, and it occurred to me that this close relationship might have hindered Brenner's recognition. In spring 2001, I asked Crick about this, and I received Crick's response, as it turned out, 18 months before Brenner's Nobel Prize was announced. Crick wrote [1]:

> "Although Sydney Brenner and I shared an office for 20 years, for most of that time I worked in the office (not always the same office) whereas Sydney worked mainly in the lab. However we did talk together for an hour or more on most days.
> The adaptor hypothesis was my idea, but Sydney coined the name for it. Sydney had the idea that acridine mutants were probably the addition or subtraction of bases. I did all the initial work on the phase-shift mutants, but Sydney designed the special genetic cross to show that +++ mutants were like wild-type. I worked out that shifts to the left were different from shifts to the right. Sydney did almost all the work to establish the stop-chain codons. Sydney realized that the Volkin-Astrachan DNA was really messenger RNA, though I immediately saw it too. Sydney, with Meselson and Jacob, established the existence of mRNA experimentally. Sydney (and another group) established experimentally the co-linearity of gene and protein. My recollection is that all this is fairly accurately described in Horace Judson's book "The Eighth Day of Creation."
> All the initial work on the nematode was conceived and carried out by Sydney, and he organized the study of its cell lineage and its detailed neuroanatomy."

Brenner was Fellow of the Royal Society (London, 1965); foreign member of the National Academy of Sciences of the U.S.A. (1977) and of the French Academy of Sciences (1992); and was a member of many other learned societies. His numerous awards included the Albert Lasker Medical Research Award (1971), the Albert Lasker Award for Special Achievement in Medical Science (2000), the Royal Medal of the Royal Society (1974), the Gairdner Foundation Award (Canada, 1978, 1991), the Krebs Medal of the Federation of European Biochemical Societies (1980), the Kyoto Prize (1990), the King Faisal International Prize for Science (1992), among many others.

We recorded a conversation at King's College in Cambridge on April 22, 2003 [2], a few segments of which I am quoting below.

Genetic code

> *I asked about the origin of the question that has become known as the genetic code. It is known that George Gamow raised this puzzle almost immediately following the Watson-Crick discovery of the double-helix structure of DNA. It is interesting for the history of science how Brenner saw Gamow's contribution in this development.*

Brenner: Gamow defined the problem although Jim [Watson] and Francis [Crick] had thought about it and I had thought that it was a one-dimensional sequence that could be translated into a three-dimensional structure. … I went to see Francis in April 1953, before their paper appeared, we were already talking about what came out in their second paper, which appeared in May. We talked about some way to translate the DNA information into the amino acid sequence. What Gamow did was to propose a form of the code. He introduced a kind of terminology with which one could begin to discuss it. In fact, everything what he did was wrong. … He defined the problem; he took the view that the amino acids were assembled directly on the DNA in what he called the diamond-shaped cavities. That was his physical model, but the big mistake about this was that he did not realize that DNA has a polarity, it has a chemical polarity that reads in one direction. There is only one message because the second strand will be derived from the first by the rules of complementation. Gamow thought that you could read DNA equivalently in either direction. That was one of his degeneracies.

> *You said something to the effect that it is not enough to say that the future of an organism is written in its genes* somehow*, we should know* how. *Has it been solved yet?*

It hasn't actually because we can't read it. All we know is this linear script and we have known it for some time and we know that regions of it are translated into amino acid sequence. We also know that some other regions carry information for products, which are themselves nucleic acids. We know about transfer RNA and we know about lots of other small RNAs, which have been discovered, but which we don't know so much about. Then, of course, we know about the ribosome and several other entities in the cell. We also know that in some way the regulation is written there. But we don't have a lexicon and we can't interpret it. So if you would ask, Could we compute organisms from their DNA, the answer is No. Not now.

The significance of computing

Von Neumann said that If you can't compute it, you don't understand it.

Von Neumann said that there were two ways of explaining complex things. One was to explain them in terms of essentially the level above, that is, in a matter language, in other words. Then he said, certain things were so complex that effectively we could not give an explanation of them and we had to define a prescription for constructing objects that perform the same behavior; in other words, to give an algorithmic explanation of them. He accepted that as a scientific explanation. As an example he quoted pattern recognition. Now, how to explain pattern recognition itself; what you can do is describe the essential features of pattern recognition, to describe an object with this internal structure. Usually now it is a computer program rather than just the solution. So the answer is, I believe, the following. We can describe everything in the Universe today, we have the power to give atom by atom a description of everything, but that's just data, that's just description. What science depends on is taking not the morest data but the leastest best and predicting the remainder from some other information. In other words, it is the classic technique of science to effectively form a theory from the facts as ascertained and then you can predict it. For explaining such things as behavior of organisms, we could essentially make a description of how an organism behaves under all circumstances, but that is description. The best thing, I believe, is to know what generates the behavior, the machine, the structure, and then we can predict the behavior. Once you have that you have the explanation of it.

People and books in mentoring

We have corresponded about your two-minute speech at the Nobel banquet after the award ceremony on December 10, 2002. You summarized the ingredients of winning the Nobel Prize. Your description contained all the ingredients that I conjectured from talking to many laureates and summarized in my book The Road to Stockholm *except that you did not mention the role of a mentor. Didn't you have any?*

I had colleagues, but I didn't have mentors. I had an early teacher Dr. Gilman when I was a student. I had a good friend who taught me mathematics. I can't identify someone whom I would consider my mentor.

Were you the mentor of any pupils who later turned out to be outstanding scientists?

I've acted as mentor for lots of people, including both the people who got the Nobel Prize with me. And for many others who came through my lab. Many.

You have mentioned the impact of two books on you in your youth, The Young Chemist *and* The Science of Life. *In my conversations with others, the most often mentioned books are* Microbe Hunters *for younger children and* What Is Life? *for the age when people embarked on their research careers.*

I've read them too, but the ones I mentioned were the ones that stayed in my mind. I read *Microbe Hunters* a little later than when it might have had the most impact on me. As for *What Is Life*, I read it early, I knew a lot about chromosomes, but I don't understand what it was really about, this aperiodic crystal, so it conveyed nothing to me.

In hindsight,…

In hindsight, there is a terrible error in Schroedinger's book, which is a fundamental mistake. There is a section in which he states that the chromosomes contain the plan or the program of the organism and the means to execute it. They do not contain the means to execute it. They contain a description of the means to execute it. That's a fundamental error, which I saw only later when I read von Neumann's theory on self-reproducing machines. The code in his model does not contain the means to execute the program, it only contains a description and you have to use the old machine to make the new machine. Schroedinger went wrong there. So I read it and in hindsight, in going through it again, that is a fundamental error. My copy of *What Is Life* has a quotation in it from Michael Faraday, which I penciled in when I read the book in 1946 and which I don't quite remember now, but its essence is that you go out and do experiments. This is important and especially in biology where a theory is a theory, so what?

What follows next?

What is the next step in finding out about nature?

What we have now is the effect of enormous capacity to obtain sequence information plus all this insane sort of rise of what's called omics science, such as genomics, proteomics. In other words, more is better, make multiple observations, and this spirit in science is that in order to do science you just have to make a lot of observations. The idea is to create a computer program, which will tell you the answer.

To which question?

That's the problem; there is no question. The data generate the answer. I think this is rubbish. This is a modern period. It's the great Baconian view of science, of biological science; the journals are full of papers full of stuff of so-called emerging phenomena. Our great task here is, our ultimate task is, to be able to simulate biological activity that has a theoretical model, and then you can compute what happens. Nobody can compute it at the moment. Our task is to solve how to convert data into knowledge. Knowledge will imply that old thing, causal relationships. You can't understand how twenty thousand genes work in a single cell by simply categorizing them, by simply saying that these genes are involved in energy production and these genes are involved in something else. The cell must have its own grammar, which is not what you impose on it. People are trying to do this, but we are still very far from doing this. What we now have to do is basically what I call computational or theoretical biology, and we even have a task before that, which I call pre-computational biology, which is to find the correct level of abstraction. In this correct level of abstraction should be embedded the correct level of analysis and description.

Let me give you one example. Many people say that this is going to be all these proteins interacting with each other and we'll have to calculate these interactions. The question is this, Is this going to be a whole set of partial differential equations in order to find out how does the thing work in fact? We can, by demonstration, simply show that many of the systems proposed would not be stable enough because if you only have one good gene and one bad one, you still can be apparently normal. We know that with one good gene we only make half of the products and it does not regulate. So the system must be robust; the system must not be sensitive to changes of concentration of twofold. There have to be ways around that. Otherwise we should fall apart instantly.

The ways around it are the following. Roughly speaking, there are two levels of protein interaction, which I call strong and weak. The strong interaction is that no protein or hardly any, protein or polypeptide chain, act alone. Most of the proteins inside of a cell form assemblages, complexes, they form little machines, what I call gadgets, devices. It is the device, which works. These devices can have as many as 65 different genes to contribute to that. Usually it is of the order of ten. The argument is as follows. Once you understand how a given device works, then you can begin to compute the output, and you can then condense that into one object. For example, there is a device, which can be analyzed in detail and essentially what it does is that it takes away cyclic AMP [adenosine 3′,5′-phosphate], converts it into a pulse, and through a lot of machinery, it converts it into a pulse of calcium. When we understand how all that is set by the detailed properties of the components, by the affinity constants of an enzyme, how it can also be set by the local diffusion constants, and so on, and once we have that, we can place all this information in a form and we can compute the response of the object. Someone can then say that I put this drug on this receptor, and the receptor, which is another piece of machinery, generates so much cyclic AMP, I can eventually compute what the effect of the drug would be on, let us say, the contraction of the heart muscle. I don't have to think that there is a lot of kinetic equations to influence the outcome because what you find is that in biology things are done by counting. It has to be done by counting because of the characteristics of the nature of the interactions. Nobody has actually sat down and analyzed it in this detail. I'm just giving you a glimpse of this. That's what I'm working on now. Once you do this, you can have a framework for dealing with all this information.

Isn't it becoming exceedingly complex?

It has taken about a billion years or more to evolve a human being; you can't expect to do it in a weekend.

How will we cope with it?

We'll cope with it by simple things; otherwise, the biological system itself could not cope with it unless it's simplified in some way. My view of complexity is that we've added to this because the actual elementary phenomena have to be simple or else the thing would just break down. What we've got to do is to find, if you like, the principle of natural engineering; we impose too much on them the concept of designed objects, which we design, the machines that we design. That's wrong because all the systems have different properties; they've got to evolve, that's one of the cardinal things and therefore we must discard our preconceptions of artificial engineering, which is what we do, and start to think in natural engineering. Let me give you one example. If you take insulin and look at its interaction with its receptor, that interaction is irreversible. It has such a high affinity constant, essentially there is no dissociation constant. So how do you measure the amount of insulin inside your blood without having a dissociation constant?

So in any way you measure it, it is the following. Each interaction is converted through some machinery, which we can specify, into an activated receptor, which essentially has tyrosines phosphorylated. There's an immediate explanation for why a receptor is always a dimer. It is because a dimer is a closed thing; it is a handshake model. I activate you, you activate me, end of story. I am both the substrate and the enzyme. That means that you are counting molecules of insulin, you are counting collisions, because from every collision you will convert that into an activated receptor. The number of activated receptors can be converted into a linear rate of production of a small molecule simply by having enzymes that make the small molecule bound to the receptor and be activated there; you convert the count to the product. This is a whole bit of machinery and there are dozens of examples where things are done by counting and not by classical Michaelis-Menten kinetics, like normal enzymes do. You count so many molecules converted to this.

You can now see that this can make it essentially concentration-independent because you have only half of the amount of protein or you introduce a change in the induction period, but the output, provided it's not saturated, remains the same. This system is very much buffered against fluctuations in the concentrations of the actual molecules participating, which does this essentially by counting. Once I just sketched the data in, but now there is an enormous simplification because we can simply say, this transforms into this and this transforms into that, and we have many such things. Sometimes we can convert the front levels of concentrations into frequencies of pulses. It might be easier for a cell rather than measuring 10 % change in a concentration, which, as you know, is very difficult to make a discrimination like this, to measure the difference between ten and eleven pulses in a fixed amount of time. Understanding the nature of signaling inside the cells immediately gives you a means of simplifying it. That's the answer to not to be defeated by complexity. A lot of the complexity is something we can't think about. We can't think about twenty thousand things going one and the same time, but the cell has means of making this difference.

I've written a paper on this and I once explained it to Gödel, about the act of center of enzymes, the fact that enzymes and products can coexist in the same cell in solution. In other words, you don't have to send the product through a pipe to the next enzyme at the scale of bacterium collisions. Of course, collisions are highly frequent events, so that basically at most of the time a molecule is hitting the wrong protein and most of the time it hits the right one it hits at the wrong place. But all other things are ignored and the system has not to worry about anything. When I told about this to Gödel, he looked at me and he said, "That is the end of vitalism." It was an interesting remark.

The importance of names

You seem to have always found it important to coin names. Was it a conscious effort?

I'm very interested in words; that's something that I do.

A few names come to mind, such as molecular genetics, adaptor hypothesis, messenger RNA, codon.

A good name can carry a lot of message. Most of these names have just evolved. I thought, let's be a little bit sophisticated about it. I have introduced another word, instantiation. It's a tough word, meaning an example. A gene becomes instantiated because what we call a gene is now no longer one gene; it can have many different modes of expression, and genes carry much more information than just the amino acid sequence. So we talk about a gene being instantiated in five different ways; they are five different instantiations. That encompasses when it is expressed, in which cells it is expressed at, and where it goes in the cell. Genes carry little addresses with them. Once you realize that all this has been encoded, we better go and find out what all that is, because that's the key to putting this into a computational form.

*****

This is the end of the excerpts I selected to present here from our conversation. During the celebrations of the 50th anniversary of the double helix, Brenner gave a talk. At the end of the conversation, I presented my summary of his talk [3]. He did not prepare a power-point presentation because, as he said it, "one good phrase is worth a thousand power points." It was a thought-provoking presentation. He spoke about the unique contribution of LMB to the DNA revolution, not only by discoveries, but also by creating the tools of investigation.

On the Human Genome Project

Brenner devoted the concluding segment of his presentation to his thoughts about the Human Genome Project as he considered it in 2003, and here is my summary of this segment of his presentation [3]:

The question is often asked whether after the Human Genome, will be our life the same? Yes, it will be because the Human Genome is only a telephone book; but try to find out from a city telephone directory how the city works. There is need for a theory. It is the wrong approach to collect the data, stuff them into the computer, and wait for the *emerging* results. This is not artificial intelligence, rather, it is artificial stupidity. We have to have a theory.

Before molecular biology, chemistry dealt with matter and energy. Molecular biology has brought information into the picture; it made information also into a chemical problem. Sequence is information and it is chemical information (it may also be considered low-energy physics problem). DNA gave a framework to think about information.

The next level of challenge is organization, to understand how inventory is organized, we should find out about the organization of the cells, how they work and interact, etc., and we have to find out how the genome maps human behavior. But we should still be concerned with causation. We should ask questions like: What causes things? What is the chain of causation? Knowing causation would simplify representation.

Biology is different from many other fields because in biology we have the possibility to interfere whereas we can't change the weather or the origin of the Universe. At the same time biology should remain a predictive science and this is why we need to worry about causality.

We have to continue collecting data; we have to collect a lot of data, but, remember, when you are collecting data, you are collecting a lot of noise too. Nonetheless, we need to get back from the hangover of the Human Genome to experimentation.

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EDITORIAL

Year of the periodic table: Mendeleev and the others

Balazs Hargittai [1] **· Istvan Hargittai** [2]

Abstract

On the occasion of the Year of the Periodic Table of the Elements, the authors look back at the original discovery, its simultaneity and the difficulties of the discoverers in their own countries, the missing Nobel recognition for this discovery, and the abundance of memorials honoring Mendeleev in Russia and elsewhere.

Keywords Periodic table · Dmitry Mendeleev · John Newlands · Lothar Meyer · Stanislao Cannizzaro

Introduction

The General Assembly of the United Nations has proclaimed 2019 as the International Year of the Periodic Table of the Elements (Fig. 1). Similarly, 2011 was the Year of Chemistry and 2014 the Year of Crystallography. There is no exclusiveness, and several other designations may be proclaimed for a given year. Nonetheless, it is remarkable that, within a single decade, three times topics that belong to the scope of Structural Chemistry have been declared to be the subjects of International Years.

The year 2019 marks the 150th anniversary of the birth of Dmitry I. Mendeleev's (1834–1907) first periodic table. The date of its first compilation is considered to be February 17, 1869. However, this could not be a sudden Eureka moment and the date must refer to the compilation of an already presentable version of the table. On this day, Mendeleev sent his table to the printers and it was printed on March 1, 1869. Once Mendeleev received the printed version, he mailed copies at once to Russian and international colleagues. However, there must have been several draft versions leading to the presentable one, and such an early version is reproduced in Fig. 2 [1]. We are showing this early, hardly intelligible version to indicate how painstaking the process must have been.

Mendeleev was preparing for his general chemistry lectures and his text Foundations of Chemistry (*Osnovi Khimii*) when he made the observation of periodicity in the properties of the elements. In this he provided an example *par excellence* of how the physical chemist turned philosopher Michael Polanyi described the process of scientific discoveries. His idea was conveyed in the brief speech by Eugene P. Wigner on December 10, 1963, at the Stockholm City Hall. The occasion was Wigner's Nobel Prize in Physics. Wigner was Polanyi's doctoral student four decades before in Berlin. Wigner said, among others, "… 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" [2]. Mendeleev noted the regularities and the coherence even though his observations were based on the atomic masses (rather than the then yet not known quantities of the atomic numbers) and even though some of these atomic masses needed to be corrected eventually.

From the start, Mendeleev recognized the importance of making his discovery known internationally. Another crucial feature of his achievements was that he made predictions of not yet known elements on the basis of his observations of coherence and regularities. Eventually, his predictions proved to be correct. Not only did he mail his table to colleagues in Western Europe, in 1871, he visited chemistry hubs in order to inform his colleagues first hand about his periodic table. During these months and years he kept improving his table.

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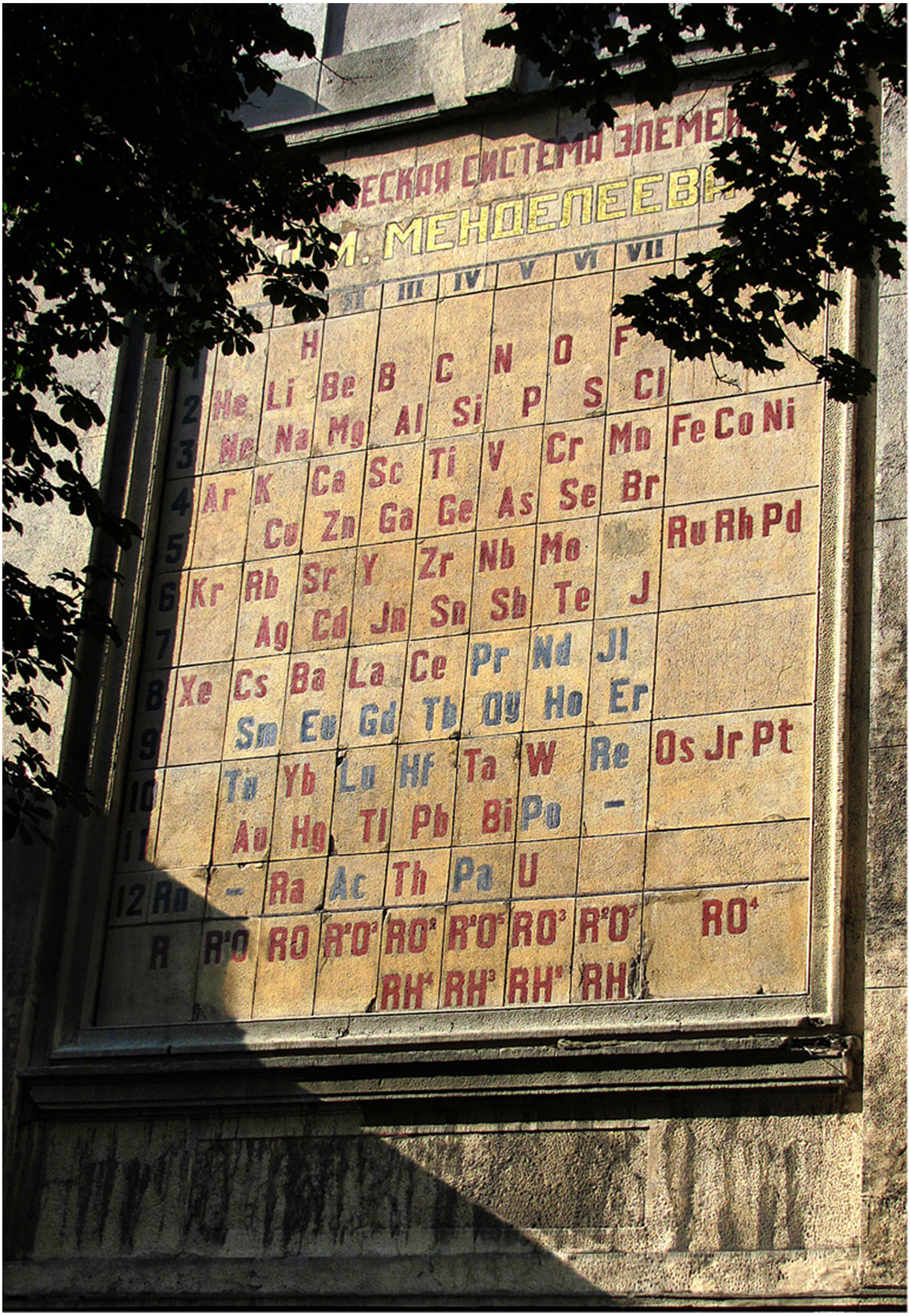
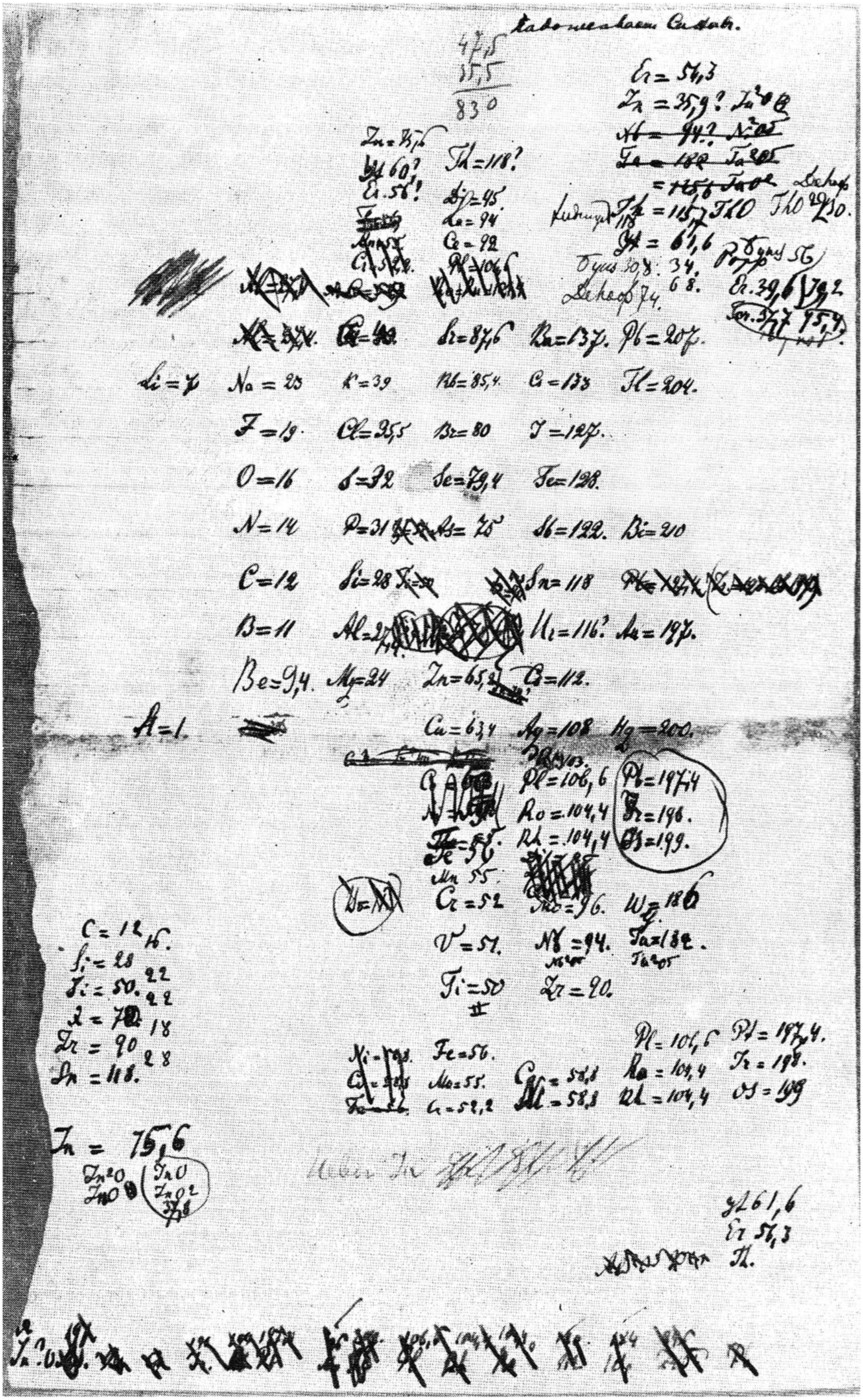


Fig. 1 Mendeleev's Periodic Table of the Elements as mural on the façade of the Mendeleev Institute of Metrology in Saint Petersburg (photograph by Magdolna Hargittai; reproduced by permission)



Sr=87,6 Ba=137. Pb=207.
Li=7 Na=23 K=39 Rb=85,4 Cs=133 Tl=204.
F=19 Cl=35,5 Br=80 J=127.
O=16 S=32 Se=79,4 Te=128.
N=14 P=31 As=75 Sb=122. Bi=210
C=12 Si=28 Sn=118
B=11 Al=27,4 Ur=116? Au=197.
Be=9,4 Mg=24 Zn=65,2 Cd=112.
H=1 Cu=63,4 Ag=108 Hg=200.
Pl=106,6 Pt=197,4
Ro=104,4 Ir=198.
Rh=104,4 Os=199.
Mn 55.
Cr=52 Mo=96. W=186
V=51. Nb=94. Ta=182.
Ti=50 Zr=90.

C=12
Si=28
Ti=50.
Zr=90
Sn=118.

Pl=106,6 Pt=197,4.
Ro=104,4 Ir=198.
Rh=104,4 Os=199
Fe=56.
Mn=55.
Cr=52,2
Co=58,8
Ni=58,8

In=75,6

Fig. 2 Facsimile of an early version of Mendeleev’s Periodic Table of the Elements [1]

Simultaneity of the discovery

As it often happens with significant discoveries for which the time had become ripe, several scientists more or less about the same time came to the conclusion of periodicity in the system of the chemical elements. The best known of them were Lothar J. Meyer (1830–1895) in Germany and John A.R. Newlands (1837–1898) in England. Newlands did not fare well in his home territory and he did not expose his discovery internationally. When he revealed his observation of periodicity in the properties of the elements, some of his colleagues ridiculed him—it was so alien to them to absorb his revolutionary idea. Someone even asked him whether he had tried to classify the elements according to the initial letters of their names. Newlands also made predictions for elements not yet known, but his ideas did not meet interest or approval. His accounts reflect his justified bitterness [3].

The professional environment of Newlands in England did not embrace his discovery whereas it welcomed that of Meyer's and Mendeleev's. They were awarded jointly the prestigious Davy Medal of the Royal Society (London) in 1882 "For their discovery of the periodic relations of the atomic weights." A few years later, in 1887, Newlands also received this distinction "For his discovery of the periodic law of the chemical elements." Then, in 1905, Mendeleev received the highest award of the Royal Society, the Copley Medal "For his contributions to chemical and physical science." Mendeleev was elected a foreign member to the Royal Society, the National Academy of Sciences of the U.S.A., and the Royal Swedish Academy of Sciences.

No *prophet* in his own country

Considering the milestone significance of the discovery of the Periodic Table of the Elements, there is scarce remembrance of Newlands and Lothar Meyer. Newlands has a blue plaque in London, on the façade of the building at 19 West Square. Its text is "J.A.R. Newlands, 1837–1898, chemist and discoverer of the Periodic Law for the chemical elements, was born and raised here." It was erected by the Royal Society of Chemistry. Meyer's birthplace, Varel, Germany, remembers him with a plaque and with a memorial consisting of three columns with the sculpted heads of Meyer, Mendeleev, and Cannizzaro. The Italian chemist Stanislao Cannizzaro (1826–1910) made seminal contribution to the discovery of the Periodic Table providing the most accurate atomic weights at the time. The Royal Society awarded him the Copley Medal in 1891 "for his contributions to chemical philosophy especially for his application of Avogadro's theory." He has a memorial plaque at the headquarters of the University of Genoa (Palazzo Balbi, 5 Via Balbi) and a bas relief at the old site of the University of Palermo (172 Via Maqueda).

Mendeleev was not spoiled by recognition in Russia during his lifetime. He became a professor at the Saint Petersburg Institute of Technology in 1864 and the Saint Petersburg State University in 1865. Subsequently, however, he lost his professorships ostensibly because of his support of the student movements aiming at improving their conditions of life and studies. For the last period of his life he continued as Controller of the Board for Weights and Measurement, which then evolved into today's Mendeleev Institute of Metrology. He was elected a corresponding member of the Russian Academy of Sciences in 1877, but in 1880, he was voted down when his full membership was being decided. This happened in spite of his international fame and his having made Saint Petersburg and international hub of chemistry. Ostensibly, the controversy of his second marriage, which took place some time before his divorce from his first wife, contributed to his negative treatment by the Academy. This was a conspicuous humiliation as the full membership is so much more important than being a corresponding member in the two-tier system of the Russian Academy of Sciences.

Missing Nobel Prize

Mendeleev was nominated for the Nobel Prize in Chemistry in 1905, 1906, and 1907. None of the nominators were from among Mendeleev's colleagues in Russia. Although the Nobel Prize is supposed to be awarded for recent discoveries, it was argued successfully that the Periodic Table of the Elements gained added significance recently. One of the most telling examples of the continuing timeliness of Mendeleev's discovery was how easily the newly discovered inert (today, noble) gases could be accommodated in the Periodic Table. In 1905, the top nominees were the German organic chemist Adolf von Baeyer and the French inorganic chemist Henri Moissan in addition to Mendeleev and the prize was awarded to von Baeyer. In 1906, the Nobel Committee of Chemistry recommended Mendeleev for the prize to the general meeting of the prize-awarding body, the Royal Swedish Academy of Sciences. The vote in the committee was by a 4:1 majority. The single dissent vote went for Moissan. The dissenting member of the committee, Peter Klason, argued forcefully for Moisson. At the same time, he did not belittle Mendeleev's achievement, but stressed that without Cannizzaro's accurate atomic weights the discovery of the Periodic Table could not have happened. He suggested Cannizzaro's recognition along with Medeleev's. This was a reasonable stipulation, but the inclusion of Cannizzaro was not possible in 1906 because only those for whom nominations had been made by January 31 could be considered. Cannizzaro was not among the nominees in 1906. According to the decision by the Royal Swedish Academy, in 1906, the prize went to Moisson. In 1907, both Mendeleev and Cannizzaro were among the nominees, but Mendeleev died early in the year and the rules of the Nobel Prize exclude posthumous awards.

Mendeleev's missing Nobel Prize is one of the most conspicuous omissions in the roster of Nobel laureates. Given Mendeleev's milestone contribution to science, his subsequent fame, and the fact that his Periodic Table of the Elements hangs in classrooms all over the world wherever chemistry is taught, his name is above all worldly recognitions. It is the institution of the Nobel Prize that suffers from his absence from among its awardees. As the Academie Française wrote

Fig. 3 Dmitry I. Mendeleev on the façade of the former ICI headquarters, 9 Millbank, SW1, London (photographs by Magdolna Hargittai; reproduced by permission)

Fig. 4 Mendeleev's statue by Matvei G. Manizer and Elena A. Yanson-Manizer in one of the two entrance lobbies of the Ceremonial Hall of the Tower of the Lomonosov Moscow State University (photograph by Istvan Hargittai; reproduced by permission)

of Molière (attributed to Bernard-Joseph Saurin), who was never elected to its membership, “Rien ne manque à sa gloire, il manquait à la nôtre” (Nothing was missing from his glory; ours lacked only him).

Mendeleev remembered

There is a full figure Mendeleev statue in his birthplace Tobolsk. Not far from Tobolsk, there is a Mendeleyevo, one of several localities in Russia named after him. There are stamps with Mendeleev’s portrait and his periodic table in Russia and internationally, as well as statues, busts, and memorial plaques (see some in [4, 5]). Here we display two memorials that are less known than some others. One is at the former headquarters of the Imperial Chemical Industries (ICI) in London. There is a giant niche at the center of the fifth level of the main facade of the building facing the western bridgehead of the Lambeth Bridge over the Themse. This niche is dedicated to Mendeleev with his portrait carved into the keystone (Fig. 3). The other sides of the building commemorate other greats of chemistry and chemical industry. The other memorial we show here is Mendeleev’s full figure statue in the Tower of the Moscow State University [6]. As entrance into the Tower is by permit only, this statue is not very well known (Fig. 4).

Apart from his ubiquitus periodic tables, Mendeleev’s name is immortalized by Element 101, Mendelevium, Md.

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His Fate Was Larger than Himself:

To Andrei D. Sakharov's Centenary

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The world-renowned physicist Andrei D. Sakharov (1921–1989) was "the father of the Soviet hydrogen bomb," and as such an architect of the Soviet superpower. He developed into a fierce fighter for human rights distinguished by the Nobel Peace Prize. In his words, "my fate was larger than what would have followed from my personality. I was merely trying to be worthy of my fate."[ii] His life and career provide thought-provoking lessons and is worthy of review on the eve of his centennial.

Andrei Sakharov behind the microphone in 1989 (photograph by Anna D. Kudryavtseva, FIAN; courtesy of the Moscow Sakharov Archives)

Under Soviet Reign

Andrei D. Sakharov (1921–1989) was born into a Moscow family of intellectuals. His physicist father wrote physics texts and knew Igor Tamm, the future Nobel laureate theoretical physicist. Tamm was an associate of the Lebedev Physical Institute of the Soviet Academy of Sciences (Fizichesky Institut Akademii Nauk, known as FIAN) and later helped Andrei to launch his career. The young Sakharov studied physics at the Lomonosov Moscow State University and although it was evacuated to Turkmenistan during World War II, the instructions continued at a high level. After graduation, Sakharov was directed to a plant producing ammunition where he excelled with innovations. He met his future wife at this plant, Klavdia, “Klava,” Vikhireva (1919–1969). She had an incomplete degree in chemical technology and worked in chemical analysis. They married in 1943 and had three children, Tatyana, “Tanya,” in 19545; Lyubov, “Lyuba,” in 1949; and Dmitry, “Mitya,” in 1957. Tanya became a research biologist and Lyuba a librarian (trained as a physicist). Mitya grew up being fraught with personal problems, worked as a photographer and held entrepreneurial jobs. In 1968, Klava was diagnosed with advanced cancer to which she succumbed the next year.[iii]

Following the war, Sakharov landed a position at FIAN. He was lucky to remain untouched by the Stalinist terror raging in full force during the last years of the dictator’s life. In previous terrors outstanding scientists perished, such as the brilliant physicist Lev Shubnikov, the world-renowned biologist Nikolai Vavilov, and many others. The future Nobel laureate theoretical physicist Lev Landau was brutally incarcerated. Many scientists and technologists ended up in slave labor camps, such as Sergei Korolev, the future leader of the Soviet space program.

Sakharov was assigned to be a member of the group of experts, charged with developing the Soviet hydrogen bomb without having been asked whether he wanted to participate in this project or not. He moved, along with Tamm, to Sarov, the closed atomic city, and started work at the secret nuclear laboratory Arzamas-16. By then, the Soviet atom bomb had already been produced as a copy of American design. For the hydrogen bomb, genuine Soviet contribution was required.

Sakharov participated in the project with dedication and great success. Three decades later, in the 1980s, at the time of his internal exile, he tried to understand his own unreserved enthusiasm in the 1950s for this horrible project. He did not serve in WWII, which is called the Great Patriotic

War in Russian, and in the 1950s, he felt like being a soldier in a new scientific-technological war. He considered the hydrogen bomb a necessary evil to protect his country against a yet more powerful enemy than Nazi Germany had been.

Two of the three basic ideas leading to the Soviet hydrogen bomb were Sakharov's; the third was Vitaly Ginzburg's, also a FIAN associate and also a future Nobel laureate. Ginzburg participated in the nuclear project only for a short while as he was not given security clearance. This was on account of his wife, Nina Ermakova, being in internal exile for having, ostensibly, participated in an anti-Stalin conspiracy, long before she met Ginzburg. The accusation was unfounded and her exile ended after Stalin's death.

Prior to moving to Sarov, the secret nuclear installation, the official defense of Sakharov's candidate of science (PhD-equivalent) dissertation took place in 1947 at FIAN. His research was about the theory of transitions in the atomic nuclei. The President of the Academy of Sciences, Sergei Vavilov, presided and two future members of the Academy, Arkady Migdal and Isaac Pomeranchuk, acted as the official referees at the defense. Igor Tamm and another luminary of physics, academician Grigory Landsberg, took part in the discussion. Tamm emphasized Sakharov's two traits essential for a theoretical physicist that occur rarely in the same person. One was the ability to grasp the problem qualitatively and arrive at an approximate estimate of the solution. The other was to solve the problem by exact mathematical tools. The Scientific Council of FIAN voted unanimously to award Sakharov's scientific degree.

According to Sakharov's *Memoirs*, in spring 1953 the Soviet atom tsar, Igor Kurchatov, initiated Sakharov's election to corresponding member of the Academy. Sakharov was asked to compile the necessary documents for the election anticipated in fall of the same year. The prerequisite higher doctorate, the D.Sc. degree, was missing, and he acquired it in haste during the hectic work on the development of the hydrogen bomb. This higher doctorate in the Soviet, now Russian, system is also a prerequisite for a professorial appointment. Sakharov did not write a full dissertation, only a summary of his most important results and the defense took place in June at the secret laboratory. Fortunately, it employed more than the sufficient number of qualified scientists to form a Scientific Council for granting his higher doctorate. Igor Tamm was one of the referees whose report was never made public, and is kept in Sakharov's private archive. It does not show a date and does not contain the customary listing of the new scientific results. Yuly Khariton, the scientific director of Arzamas-16, was another referee, and a sanitized version of his report has appeared, dated November 9, 1953, as if it had been compiled after the Academy elections. Khariton mentions Sakharov's achievements in connection with the development of the thermonuclear device.

Sakharov's cientific acumen was amply manifested in 1947 in his candidate of science dissertation and in its defense. He could have been granted the higher doctorate, skipping the candidate's degree, which is not common, but not too extraordinary. His higher doctorate was arranged for during the critical period of the preparation for the test of the experimental thermonuclear device during the summer of 1953. This shows how important his election to the Academy was considered at the time. The first Soviet thermonuclear device—not yet a full-scale hydrogen bomb—was tested on August 12, 1953, with complete success. Sakharov was elected full member rather than corresponding member in the two-tier system of the Soviet Academy of Sciences, on October 23, 1953. In this two-tier system, the corresponding membership preceeds the full membership and many corresponding members never reach full membership. Skipping the corresponding membership is most exceptional. Prior examples were Igor Kurchatov in 1943 and Lev Landau in 1946.

A few months following the successful August 1953 test, Sakharov received his first gold star of Hero of Socialist Labor. Subsequently he was given twice more this highest recognition, in 1956 and 1962, on both occasions following successful tests of nuclear explosions. He became one of the most decorated Soviet citizens—referred to often as the father of the Soviet hydrogen bomb—one of the prime architects of the superpower status of the Soviet Union.

Personal Transition

His first collision with the powers that be happened in 1955. Following a successful test—it was the first truly Soviet hydrogen bomb—there was a festive celebration. In his toast, Sakharov expressed his hope that the successful explosions will always happen over proving grounds and never over cities. All those present sensed that the scientist wandered onto slippery grounds. The representative of Soviet officialdom hastened to correct him with a cautionary tale to warn the scientist that he should leave politics to the politicians. This was the first time, but not the last that he was shown his place in Soviet society. When the Soviet Union was preparing for exploding the world's most powerful bomb, in June 1961, the supreme Soviet leader, Nikita Khrushchev, conveyed a meeting of the atomic scientists. On this occasion, Sakharov argued that there was no real dividend in deploying bombs of ever increasing power, whereas their testing carried various dangers. This time, Khrushchev himself reprimanded the scientist and humiliated him publicly in front of the leading scientists and politicians.

Sakharov continued his work at Arzamas-16 for years and was returned to Moscow only in 1968 following his complete alienation from the weapons project. His actions were not determined by his emotions. From the mid-1950s, he was concerned about the possible biological consequences of the nuclear tests. He understood that the biological damages of the tests are non-threshold events, that is, there was no minimum dose beneath which any possible damage could be excluded. The impossible situation of the science of biology in the Soviet Union further enhanced his worries. He was among those physicists and chemists who felt an increasing responsibility to do something to counteract the tragedy of biology and biologists. They were suffering from the iron grip of the charlatan T. D. Lysenko who had enjoyed first Stalin's, then Khrushchev's virtually unlimited support.

On the one hand, Sakharov recognized the biological hazards of testing and felt horrified witnessing the recklessness of the Soviet leadership in misusing the tests in their international power play. On the other hand, he was concerned to ensure the best possible utilization of the weapons he helped creating. At some point he considered the most efficient ways for deploying his horrific invention and approached a Soviet rear admiral, by the name of Fomin, with a proposal.[iv] He suggested to equip a submarine with a hydrogen bomb-torpedo that could be directed to a most important Western harbor for maximum destruction. One might dismiss this story as improbable had it not been narrated by Sakharov in his *Memoirs*. (Sakharov, 1992, p. 221) Obviously, this tormented Sakharov in hindsight. It is just another example of the long road he covered from the creator of the tools by which Stalin and his successors might have held the democratic world hostage to a most dedicated fighter for democracy.

Sakharov carried out calculations to estimate the possible damages of nuclear tests, including the long-term impacts of the radioactive isotopes they produce. He estimated that for every megaton (one million ton) TNT-equivalent (2,4,6-trinitrotoluene-equivalent) nuclear explosion, there are ten thousand human victims.[v] By 1957, the nuclear explosions in the world reached fifty megaton TNT-equivalent. The estimated number of human victims reached half a million. It was a quirk irony of history that Sakharov's estimates, at the time, helped Khrushchev's political interests. The Soviet leader had declared a temporary moratorium on nuclear testing whereas the Americans continued testing. Somewhat later, the Soviets renewed their nuclear testing when Khrushchev's political interests so dictated. Sakharov proved powerless in his attempts to block them.

Sakharov was still an associate of the Arzamas-16 laboratory when he took an activist role during the Academy elections in 1964. He took a stand against the election of an unworthy Lysenko protege who had secured Party support. As it turned out, other physicists had also formed an opposition and the candidate was not elected. It was an unprecedented action in the history of seamless Party domination in the life of the Science Academy as in everything else in the Soviet Union. Sakharov was removed from Arzamas-16 in 1968 and returned to FIAN to continue where he started twenty years before. By then, he became actively interested in politics, initially directing his attention to general issues. He signed a protest against Stalin's rehabilitation; joined a movement protesting the pollution of Lake Baikal in Siberia; and attended a silent demonstration, organized on the Day of the Constitution, to protest unlawfulness. His participation—he was an academician, three-time Hero of Socialist Labor—added weight to any movement that had him among its midst. The authorities arrested demonstrators and protesters, but never touched him.

For human rights

Gradually, Sakharov's attention moved from general issues to the protection of the human rights of individuals. He attended trials of activists, fought against the use of psychiatric incarceration and for the freedom of religion, against anti-Semitic discrimination, for the right of emigration, supported numerous other causes, and was on the lookout for more. The Western media helped enhancing his fame and he recognized the power of the press.

It was a milestone when in 1968, he published his pamphlet, smuggled out to the West, *Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom*. It was a tumultuous year of the student movements and the Prague Spring and its ruthless supression, which dissipated any hope for creating "socialism with a human face." The world was thirsty for direction and many discovered it in Sakharov's words. He warned the human race of the dangers of „thermonuclear extinction, ecological catastrophe, famine, uncontrolled population explosion, alienation, and dogmatic distortion of our conception of reality." (Sakharov, 1992, p.282) The pamphlet sold in at least eighteen million copies; only Mao Zedong's and Vladimir Lenin's books preceded it on the bestseller list whereas he left behind star authors like Georges Simenon and Agatha Christie. In many places those in power felt threatened by Sakharov's views, and nowhere as much as in the Soviet Union. The Soviet authorities dreaded the intellectuals and their views; their fear bordered paranoia. They persecuted those who read the pamphlet let alone those who disseminated it. There was a long history of the Soviet authorities considering the writers, poets, sociologists, and environmentalists their enemies even though they possessed nothing except their ideas. Now, a world renowned physicist, academician, a principal architect of Soviet superpower joined these powerless forces. Although he was a singularity, a lonely hero, he shattered this invincible and eternal—or so believed—empire.

The widower Sakharov met Elena, “Lusia,” Bonner (1923–2011) in 1970 at a human rights event. She was a divorsee, a pediatrician, war hero, and a human rights activist. They married in 1972. Her children, Tatyana, “Tanya,” and Aleksei became close to him. Bonner was his faithful partner in his heightened human rights activities. When he was awarded the Nobel Peace Prize in 1975, he was prevented from attending the prize awarding ceremony in Oslo where his wife represented him. Instead, and this could be taken as symbolic, he was attending a trial of another human rights activist in Vilnius. In subsequent years, Sakharov multiplied his efforts in his fights for human rights and for freeing incarcerated human rights activists. He did not shrink even from such drastic measures as going on hunger strike.

It says a great deal about the nature of the Soviet regime that among their many attempts to discredit Sakharov they intimated that he may be Jewish and spread stories that might encite anti-Semitic attacks against him—and they did. In his words, these attempts were “calculated to arouse envy, malice, and all the instincts of the pogrom-makers.” (Sakharov, 1992, p. 431) Sakharov followed rigorously Igor Tamm’s views on anti-Semitism, according to which “one foolproof way of telling if someone belongs to the Russian intelligentsia. A true Russian *intelligent* is never an anti-Semite. If he’s infected with that virus, then he’s something else, something terrible and dangerous.” (Sakharov, 1992, p.123)

Sakharov’s dedication and determination rendered the authorities powerless, and in January 1980, they resorted to an extreme action in curbing his activities. They revoked all his awards and distinctions and, without any legal foundation, exiled him to the city of Gorky—now, as before, Nizhny Novgorod—which was a closed city for foreigners. They did not dare though to revoke his membership in the Academy of Sciences.

During the next seven years the authorities kept him, and his wife, who joined him, in isolation. They allowed only once or twice annually one or two of his fellow physicist academicians at FIAN to visit him. An army of KGB agents kept harrassing him, stole his manuscripts, scared away his would-be visitors, and did everything to make the Sakharovs' life as hard as possible. Even under these inhuman circumstances Sakharov did not give up taking a stand in defense of others. He was followed on every step, spied on all the time, they tapped his apartment, and listened to when he talked with others. On one occasion, when he and his rare visitors were to discuss some physics that included classified information, he stopped the conversation. He noted that although he and his interlocutors possessed the highest security clearance, the KGB officers listening clandestinely to their exchange might not. Was he was serious or was he sarcastic? Probably both.

The Academy of Sciences in the background

Statue of Andrei Sakharov with his bound hands behind his back (by L.K. Lazarev, unveiled in 2003) on Sakharov's Square in St. Petersburg with university buildings in the background (photograph by the author)

Sakharov's inhuman treatment went on during the reign of subsequent supreme leaders, Leonid Brezhnev, Jury Andropov, Konstantin Chernenko, and Mikhail Gorbachev. Sakharov continued his resistance, including firing off letters of protest to these leaders. Sadly, the Academy of

Sciences was among the instruments the authorities enlisted in their efforts of breaking Sakharov's resolve. It was still under Brezhnev, in 1973, that forty academicians signed a published letter, which condemned Sakharov's activities. The signatures were collected unscrupulously. Some of the signatories were not even asked; their names were just added. They could not do this though with everyone. The internationally renowned physicist Petr Kapitsa refused to sign. Yakov Zeldovich, Sakharov's long-time colleague at Arzamas-16, was not even approached. When the president of the Academy, Anatoly Aleksandrov, was called, his wife picked up the receiver and told the caller that her husband was drunk and could not come to the phone, so his signature was also missing. Publishing such letters was a common practice in Soviet times and had become routine. There was a very different letter published in 1983 at the time of Sakharov's exile. This letter was signed by four academicians only, and they did indeed sign it. This letter condemned Sakharov in extreme terms. One of the signatories was the Nobel laureate (1964) laser pioneer Aleksandr Prokhorov and this action left a stain on his brilliant career. It is interesting to note that the current Russian officialdom is looking back on Prokhorov's public demeanor with pointed appreciation. Alone among the great generation of Soviet-time physicists, Prokhorov was honored recently (2015) with a large statue-memorial at one of Moscow's busiest intersections.

As alluded to above, the authorities did not revoke Sakharov's Academy membership, but at one point, during his exile, he himself raised this issue. By 1984, in the fourth year of his exile, he found his situation hopeless. He was willing to resign from the Academy if the institution proved unable to assist him. This was a drastic proposition, threatening even his livelihood as the considerable allotment as a full member of the Academy was his principal income at the time. Fortunately, he did not have to resort to this drastic step.

Sakharov and Gorbachev

The 54-year-old Mikhail Gorbachev ascended to be the new Soviet leader in March 1985. Whether he was set to dismantle the Soviet regime or was being forced to agree to one change after another, has been a question of contention. It is a fact though that during Gorbachev's reign, Sakharov was kept in exile for 18 more long months. During this time there were negotiations between Sakharov and the Gorbachev Administration about the terms of his liberation and return to Moscow. Even during these 18 months, Sakharov's harassment continued, and it happened that he had to resort to the extreme action of hunger strike. Gorbachev was still hesitant about letting Sakharov free when his advisers urged him to do so. In most accounts, Gorbachev "invited" Sakharov back to Moscow in December 1986, but in reality, and in Sakharov's own words, Gorbachev "allowed" his return.

Upon Sakharov's return to Moscow he re-joined FIAN, but for the remaining three years of his life, politics took over and physics played a diminishing role. His path and Gorbachev's intersected to an ever increasing degree. Initially, the almighty secretary general, then, president, was almost unapproachable for the "meddlesome" and "impertinent" intellectual, who rapidly had become an important player of the Moscow political scene. Sakharov had to be reckoned with unless a politician was ready to ignore the entire intellectual class. At that time this was impossible and unthinkable—today though this is a different matter, as we are observing its diminishing role. Sakharov's statements and criticism is worthy of remembering lest we let the distance in time alter history, belittle Sakharov's role in advancing democratic change, and camouflage Gorbachev's resistance to it.

Sakharov sharply criticized Gorbachev when in February 1986 the Secretary General declared that there were no longer political prisoners in the Soviet Union and no one was persecuted for political views. This was a false statement as Sakharov himself still being in exile was a direct proof of the opposite. There were still numerous political prisoners. One of their most outstanding representatives, the 48-year-old Anatoly Marchenko died in prison later in 1986. Sakharov protested when Gorbachev's administration initially treated the Chernobyl catastrophe as an insignificant accident, misleading even Sakharov himself. Sakharov later narrated events—by then he was their witness upon his joining the political scene—in which Gorbachev behaved dictatorially at various gatherings and debates, and tended to apply non-democratic approaches, allegedly, in order to protect democracy. Sakharov recorded his observations about Gorbachev's tendency to concentrate power in his own hands and observed a deep gap between Gorbachev's words and deeds both in his economic and social policies. He was a political leader who did not yet get used to acquire political leadership via elections and let his prejudices influence his decisions. Sakharov's uncompromising pro-democracy stand often irritated Gorbachev who aired his irritation. He was unable to apply his policy of *glasnost'* to his own demeanor and tended to limit *openness* in his own political activities. Sakharov tried to curb Gorbachev's attempts of grabbing all power while he also recognized that the new political leader represented a token of progress.

Sakharov's Science

Sakharov was an internationally renowned physicist whose achievements earned him broad recognition. Alas, he could devote only a fraction of his time to science, and even less to basic science. Such an unobtrusive period was the three years at FIAN immediately after the war when he was doing his research in preparation for his candidate of science degree. Some of his work on the thermonuclear bomb he also considered to be true physics. In this, he was not alone. Enrico Fermi did not think it a waste of time what he spent on developing the nuclear bombs. I am not referring to the importance of the nuclear weapons in preserving peace through mutual deterrence. Rather, much of it was *interesting* physics (Fermi's expression was *good* physics), full of challenges for *bona fide* researchers. During his two decades at the secret atom laboratory, however, Sakharov had hardly any chance to do physics other than what was connected to thermonuclear science. The only opportunity was what came through his fellow physicist Yakov Zeldovich who was eight years his senior. These eight years of difference meant that Zeldovich had built up a network of connections with other physicists in Moscow prior to WWII. This helped him stay alert as far as the rapid progress in physics was taking place in the 1950s and 1960s. His engagement provided stimulus for Sakharov who was rather slow in building interactions with his peers. His 1968 return to Moscow and to FIAN meant also his return to fundamental physics. However, his involvement with human rights issues was gradually taking away an increasing amount of his time from research. Then came the exile, 1980–1986, making it almost impossible to continue doing his physics. It is a manifestation of his extraordinary talent and dedication that during the 1968–1986 period he produced new results and weighty publications that added to his international recognition as a most significant contributor to his science. After his return to Moscow in December 1986, he hardly had any opportunity to continue in physics.

In light of the above, it may seem surprising that his original scientific contributions amounted to a five-hundred-page densely printed volume, published by FIAN. (Sakharov, 1995) His achievements in the following three areas are especially noteworthy: plasma physics, the physics of elementary particles, and cosmology. Sakharov was the first among Soviet physicists who suggested the application of lasers for controlled thermonuclear reactions. Also, he was the first to suggest the utilization of neutrons from fusion reactors to produce fission fuels for nuclear reactors. He suggested techniques for the production of extremely strong magnetic fields.

Along with fellow Soviet physicists, he initiated the development of *tokamak,* which is the Russian term for a hot plasma confined to a torus-shape by a powerful magnetic field, which could lead to energy production by controlled thermonuclear reaction of fusion.

In the physics of elementary particles (today, more often referred to as fundamental particles) he estimated the masses of some of these particles on the basis of the structure of the most fundamental building blocks of matter, the so-called quarks. He communicated his most important, certainly his best known, result involving the interpretation of the so-called baryon asymmetry of the universe. The protons and the neutrons are the most common baryons and they constitute much of the known mass of the universe. The baryon–antibaryon asymmetry is part of the fundamental issue of our universe consisting of matter rather than antimatter. This issue could be formulated as why does antimatter exist at all? At the moment of the Big Bang, when the universe was formed, it was extremely hot, representing enormous energy, and it produced both particles and antiparticles. As the temperature kept decreasing, the particles and antiparticles annihilated each other in pairs. Had they been present in equal amounts, this would have led to emptying the universe. Apparently, there was some excess of matter over antimatter in the early universe, and this meant the baryon asymmetry and from this followed that the universe now consists of matter. The big puzzle is the origin of the initial imbalance between matter and antimatter, and there has been no solution yet for this puzzle. Sakharov did not provide the solution either, but, in 1967, he set up three requirements that the solution, when it is found, should satisfy. One is that there must exist processes that are capable of altering the number of baryons. The next is the existence of some shift in the natural laws that favors matter over antimatter. And the third is that the processes altering the baryon number must form under the absence of thermal equilibrium—this corresponds to the process of permanent cooling of the universe ever since the initial Big Bang.

The theory of baryon asymmetry links the physics of fundamental particles to cosmology and Sakharov’s works played a role in the emergence of the new science of ”CosmoMicroPhysics.” He investigated the problem of the expanding universe, the non-uniform distribution of matter, the reversal of the direction of time, the negative curvature of space, and the finite cosmological constant. Sakharov had a publication on an alternative theory of gravitation and his discussion differed from Albert Einstein’s approach with all the long-ranging consequences of this difference.

Sakharov and Teller

The authorship of the hydrogen bomb connects the names of these two scientists forever. Edward Teller has been called the father of the American hydrogen bomb and Andrei Sakharov of the Soviet hydrogen bomb. It is doubtful whether such a label is appreciative or condemning. There is though quite broad consensus that the policy of mutually assured destruction (MAD), however horrible it sounds, restrained the two superpowers for decades from attacking one another. When in 1985, at the time of Sakharov's exile, an anthology in his honor was published in New York (*Andrei Sakharov and Peace*), Teller wrote one of its chapters in which he noted that there were similarities between them though he found their differences more significant, hence their stories could not be viewed as running in parallel. (Lozansky, 1985) Sakharov and Teller met in person only once, at a banquet honoring Teller on November 16, 1988, in Washington, DC. They had a brief private exchange followed by Sakharov speaking to the gathering. He condemned the Strategic Defense Initiative (SDI), after which he had to leave immediately in order to catch the last plane to Boston. When Teller's turn came to speak, Sakharov was no longer there. Teller expressed his disagreement with Sakharov in the matter of SDI, but did not go into the details of their disagreement, saying merely that Sakharov was ill-informed. This was a typical tellerian approach to debate—Teller should have known that the issue was of principal concern for Sakharov who never addressed any issue without having been thoroughly prepared.

Their differences in opinion manifested themselves most conspicuously in how they viewed the possible biological consequences of nuclear tests. The danger of biological damage was the principal reason why Sakharov opposed further testing. As for Teller, on some occasions, he characterized the danger of testing negligible as compared with other sources of possible biological consequences. On other occasions, he emphasized that the unavoidable birth defects as a consequence of testing was an affordable price for enhancing security.

They differed also concerning the responsibility of scientists in finding solutions to the most pressing political problems. Sakharov assigned responsibility to scientists and felt uneasy about nuclear destruction and the moral responsibility of scientists in preventing it. Teller emphasized the responsibility of scientists for creating new technical solutions, but shifted the responsibility to Society (or their elected representatives) in their utilization. Teller in this appeared in concert with the Soviet leadership that was critical toward Sakharov when he appeared meddling in nuclear policy. In reality though, Teller was unstoppable in his attempts influencing politicians to sway their decisions in matters he himself felt strongly about (Hargittai 2010).

Both declared that they did not create their respective hydrogen bombs alone and that it was the work of many people. Sakharov's assessment was realistic when he mentioned Vitaly Ginzburg's suggestion as one of the three fundamental ideas, in developing the Soviet bomb and the participation of Yakov Zeldovich and others. In contrast, Teller belittled Stanislaw Ulam's contribution, which may have triggered Teller's approach that moved the project to completion.

Both had excellent ability to make qualitative estimates when facing a problem and arriving at a qualitative solution; only then did they elaborate the details. Both reconciled fundamental research and applications; in fact, both devoted themselved to seeking applications of the fruits of basic research. Both were dedicated to the utilization of nuclear science for energy production. Teller in his time played a leading role in creating safe operational protocols of nuclear power plants in the United States. This needs emphasis as this aspect of his career is hardly known. Both advocated the importance of operating the nuclear power plants under ground. It is now over thirty years that both declared this mandatory for new nuclear reactors and their strong recommendations appear to have been unheeded.

They were different in their public appearances. Sakharov had a withdrawing personality; he did not like improptu interviews; he was not a practiced debater. Teller thrived on public appearances, enjoyed live interviews, and when it was a recording, he insisted that it should not be edited; rather, the transcripts should appear unaltered or not at all. He was an excellent debater; most of his interlocutors thought him invincible in debate. Sadly though, he did not alway operate with fairness; and liked to intimate knowledge that was in his favor, but that he was not at liberty to divulge.

Sakharov respected Teller and his principal arguments regardless whether he agreed with Teller or not. A critical comparison of the careers and views of the two should be instructive. Whether they ran in parallel is questionable. To me, considering the directions of their careers and views, they followed, rather, anti-parallel paths.

Demise

Sakharov's grave in the Vostryakovskoe Cemetery in Moscow (courtesy of Aleksandr Verny). Sakharov's second wife, Elena Bonner, is buried in the same grave

Sakharov died in 1989 just as the Soviet Union was soon dissolving. His status and his contributions to his country's having become a superpower, as the „father of the Soviet hydrogen bomb," would have made him eligible for a most prestigious burial place. He might have been buried in the Kremlin Wall as Mstislav Keldysh and Sergei Korolev of the space program were. He certainly could have been buried in the most exclusive Novodeviche Cemetery, as Igor Tamm, Yakov Zeldovich, and Vitaly Ginzburg of the nuclear weapons program were (Hargittai and Hargittai, 2019). When Sakharov's first wife died, he arranged for her burial at the Vostryakovskoe Cemetery, anticipating that, when the time comes, he would also be buried there. Indeed, there is his final resting place, not far from his first wife's, together with his second wife.

Epilogue

Statues of Feliks Dzerzhinsky (left, by E. V. Vuchetich, 1958) and Andrei Sakharov (right, by G. V. Pototsky, 2008) in the Muzeon Park, Moscow (photograph by the author)

The Muzeon Park is a beautiful and popular center of art and entertainment close to downtown Moscow and it includes a huge collection of statues. It began right after the collapse of the Soviet Union and the intention was to collect the memorials of the discredited regime. After a while though the direction of the sculpture park changed. Many of the memorials that should have become part of the collection were left in their original locations whereas many other statues that had nothing to do with the Soviets were exhibited at the Muzeon Park. This is how the statues of Feliks Dzerzhinsky and Andrei Sakharov happen to stand now in each other's vicinity. Dzerzhinsky founded the predecessor of the infamous KGB in the 1920s and his monumental statue used to be a landmark on Lubyanka Square in front of the KGB (today, FSB) Headquarters. Sakharov's statue was created in 2008 and had it not been labeled properly it could be taken merely for an old man sunbathing peacefully. In juxtaposition, the two statues form a symbolic ensemble; there is Sakharov as David, and Dzerzhinsky as Goliath.

David/Sakharov for years fought for human rights, which for a long time was thought to be hopeless, against an invincible regime represented here by Goliath/Dzerzhinsky. Then, finally, this frail man defeated the mighty order, which collapsed like a house of cards. Following his death, the respect for and reputation of Sakharov grew enormously. It appeared as if the process of democratization in Russia, symbolized by Sakharov, scored—using a favorite expression of Soviet times—a complete and final victory over the forces symbolized by Dzerzhinsky. Alas, the development in Russia during the past years and decades demonstrates that Sakharov's victory may have not been complete, nor final (this is as of Fall 2020). There may be need for new Sakharovs!

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Notes

[i] Istvan Hargittai is Professor Emeritus (active) at the Budapest University of Technology and Economics. He is a member of the Hungarian Academy of Sciences and the Academia Europaea (London) and a foreign member of the Norwegian Academy of Science and letters. Among his distinctions are honorary doctorates of Lomonosov Moscow State University and of the Russian Academy of Sciences. He is the author of *Buried Glory: Portraits of Soviet Scientists* (2013) and co-author, with Magdolna Hargittai, of *Science in Moscow* (2019).

[ii] "... судьба моя оказалась крупнее, чем моя личность. Я лишь старался быть на уровне собственной судьбы ..." Андрей Сахаров 1988 г. From a poster at the Moscow Sakharov Archives.

[iii] She died of stomach cancer. There is no hard evidence, only anecdotal evidence, according to which many wives and daughters of Arzamas scientists died of cancer. Sakharov's family lived in Sarov from 1950 till 1968. Klavdia Vikhireva may have also had chemical poisoning at the ammunition plant where she worked during the war. That poisoning caused stomach ulcers from which she suffered for years. Those stomach ulcers may have also turned cancerous.

[iv] Sakharov does not give the initials of the rear-admiral, but considering his career, he must be Petr F. Fomin (1904–1976) who commandeered the Soviet atomic submarines when they were getting first deployed.

[v] 1 megaton = 1,000 kiloton, and for comparison, the atom bomb over Hiroshima was of 15-kiloton TNT-equivalent. For the number of human victims, Hiroshima could not serve for comparison, because an atom bomb targeting a big city kills directly an enormous number of people.