



EUGENE P. WIGNER

Eugene P. Wigner (1902, Budapest — 1995, Princeton) received the Nobel Prize in Physics “for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles.” He attended one of the famous Budapest high schools before his studies at the Berlin University of Technology, where, eventually he earned his doctorate in chemical engineering. When the Nazis came to power in Germany he left for the United States. He was Thomas D. Jones Professor of Mathematical Physics at Princeton University between 1938 and 1971, when he retired. Wigner worked on the Manhattan Project during World War II. He was a member of the General Advisory Committee to the U.S. Atomic Energy Commission, 1952–1957 and 1959–1964. Among his decorations, he received the U.S. Medal of Merit (1946), the Enrico Fermi Prize of the U.S. Atomic Energy Commission (1958), the Atoms for Peace Award (1960), the Medal of the Franklin Society, the Max Planck Medal of the German Physical Society, the National Medal of Science (1969), and others. He was a member of the National Academy of Sciences of the U.S.A., the American Academy of Arts and Sciences, and other learned societies. We met and had extensive conversations at the University of Texas at Austin in 1969. This narrative (by István Hargittai) is based on these conversations and on other interviews in this volume.

I spent a year at the Physics Department of the University of Texas at Austin in 1969 as a Research Associate. I worked in a research group loosely directed by Harold P. Hanson who was the chair of the Department.

We met in the previous year in Oslo. By the time of Wigner's visit though, Hanson had left for the next position in his distinguished career as a scientist and administrator. When I heard about Wigner's forthcoming visit — his lectures were advertised well ahead of time — I went to see his official hosts. I told them that I had known Wigner if only by correspondence and would like to see him while he was there. They declined, explaining it was very expensive for them to bring Wigner to Texas and they could not waste his time on me. However, they let me leave a note for Wigner in which I said hello and gave him my location in the department.

Wigner must have received my note because he came to see me in my office every morning during his stay in Austin and spent an hour with me from 8 a.m. to 9 a.m., when his official program started. Thus he spent some of his "private" time with me with no loss to his hosts. When he first came to see me having read my note, I might have ascribed his call as a courtesy. But his subsequent visits I took as a genuine expression of interest and magnanimity at the same time. Of course, I also experienced his politeness, but to a lesser degree than others might have. His legendary politeness and modesty on occasions reached such a degree that it irritated people and many saw through it as being a shield preventing others from getting close to him. Freeman Dyson (see elsewhere in this volume) and his wife lived very close to the Wigners in Princeton for many years. According to Dyson, they were friendly, but they never got close. About Wigner's politeness, this was Dyson's observation,

There is a professor here, whose wife is Japanese. She is a sociologist and writes about Japanese society. One of the papers she has written, and I think it is brilliant, is called "Politeness as a Tool of Repression". It is certainly true for the Japanese society and perhaps a little bit with Wigner, too.

In physics though, his modesty may have helped Wigner to recognize the principal tasks of physics. In his Nobel lecture¹ he stressed the limitations in the ambitions of physics and physicists:

... [P]hysics 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. In fact, the specification of the explainable may have been the greatest

discovery of physics so far. It does not seem easy to find its inventor, or to give the exact date of its origin. Kepler still tried to find the exact rules for the magnitude of the planetary orbits, similar to his laws of planetary motion. Newton already realized that physics would deal, for a long time, only with the explanation of those of the regularities discovered by Kepler which we now call Kepler's laws.

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 do not try to regulate all actions and behavior, the laws of physics also determine the behavior of its object of interest only under certain well-defined conditions but leave much freedom otherwise ...

According to another physics Nobel laureate (1979) Steven Weinberg (see elsewhere in this volume),

Wigner realized, earlier than most physicists, the importance of thinking about symmetries as objects of interest in themselves. In the 1930s, although physicists talked a lot about symmetries, they talked about them in the context of specific theories of nuclear force. Wigner was able to transcend that and he discussed symmetry in a way, which didn't rely on any particular theory of nuclear force. I liked that very much.

Wigner received the inspiration to look for regularities in Nature from his mentor in his doctoral studies in Berlin, Michael Polanyi. We know this from Wigner's two-minute speech at the Stockholm City Hall, following the award ceremony of the Nobel Prize in December 1963.² Wigner devoted this two-minute speech to his teachers. He said³ that

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.

Looking for regularities is very much the same as looking for symmetries. In this sense, Wigner's interest in symmetry may have originated from his



At the banquet of the Nobel Prize award ceremonies in Stockholm, 1963: in the middle of the picture, the three physics Nobel laureates, Maria Goeppert-Mayer, J. Hans D. Jensen, and Eugene Wigner (courtesy of Franca Natta Pesenti, Bergamo, Italy).

interactions with Polanyi. There was then a less direct impact by Polanyi in turning Wigner's attention to symmetries. After his doctorate Wigner returned to Hungary and worked in the same leather tannery where his father had a managerial job. Even decades later I sensed pride in Wigner's describing his knowledge of the chemistry processes involved in tannery and the different ways of preparing the leather. Some leather is prepared for the bottom of the shoe, some for the upper part of the shoe, yet some other leather is prepared for travel bags. Wigner had a tremendous knowledge of chemistry, especially materials. This came from his training as a chemical engineer and he made good use of this knowledge later in the Manhattan Project. During the two years he spent at the tannery, he kept subscribing to the German physical journal *Zeitschrift für Physik* and read it, in his words,⁴ "in the evenings industriously." Then,⁴

After two years, to my great surprise, I received a letter of invitation from Professor Becker, the newly appointed professor of theoretical

physics at the Institute of Technology in Berlin, offering me a position of assistant. I suspect that it was my teacher, the person I worked with for my doctoral dissertation, Michael Polanyi, who must have recommended me. He was a famous scientist and excellent man.

...

Before my job started with Professor Becker, I had two months in Berlin and I used that time to learn crystallography, and this was very useful later. In crystallography I learned about group theory, which became the center of my interest for several years. It was a great joy when Schrödinger's paper came out in which he described his equation and quantum mechanics, and which made the application of group theory to quantum mechanics so much easier.

Wigner was not only an excellent pupil; he became an outstanding mentor himself. He served as guide for several major figures in condensed matter physics in the United States. John Bardeen was one of them. Bardeen is the only person to have ever received two Nobel Prizes in physics (1956 and 1972). From Bardeen's description, Wigner emerges as a popular mentor and Bardeen notes that "Wigner was attracting a number of other students and postdoctoral fellows interested in problems of solid state physics."⁵ Among them were F. Seitz and C. Herring. Herring was later Philip Anderson's (Nobel Prize in 1977) mentor. However, Wigner may not have been equally good a pedagogue for everybody. Steven Weinberg (see next interview) was a graduate student at Princeton when Wigner was a professor there. This is how he remembers Wigner as his teacher

I took his course in nuclear physics. He was not a very good teacher because he was obsessively worried that there might be someone in the class who wasn't understanding him. So he went very slowly. But he still was very profound. He did me great compliment once, when he had to go out of town, of asking me to take over the class. I didn't learn much from Wigner when I was a graduate student at Princeton. But in the years following, I found that my point of view toward physics was very much in tune with Wigner's, much more so than with other people. Wigner had analyzed, especially in 1939, the nature of the elementary particles and, in particular, the significance of the spin, in a way far superior and much more well-grounded in fundamental principles than what I had earlier learned, say, from the treatment of the spin by Paul Dirac. In that sense I became a disciple of Wigner. In my book on quantum field theory I very much followed Wigner's 1939 paper.

At the time when Wigner received the Nobel Prize, I did not pay much attention to the Nobel Prizes. However, the next year, in the fall of 1964, I read an article by Wigner in the Hungarian literary weekly *Élet és Irodalom* (*Life and Literature*).⁶ In the wake of Wigner's Nobel Prize, they translated an earlier Wigner article into Hungarian and communicated excerpts from it.⁷ At that time I was a Master's degree student at Moscow State University. The topic of his article was the limits of science and it immensely interested me, so I wrote a response and sent it to the editor of *Élet és Irodalom*. To my surprise, it soon appeared and was quite a sizeable article.⁸ I had never written anything before let alone seen my writing and name printed. It was quite a thing for a student. Soon after the appearance of the article, something of even greater significance happened. I received a long letter from Wigner and some reprints of his papers. He agreed with some of what I had written and disagreed with other aspects, all in his most polite way.

In our conversations in Austin, Texas, we talked about many things, but the most remarkable for me was what he taught me about symmetry, and this is what had the longest-ranging impact on me. Wigner had contributed to the application of the symmetry principles in the most fundamental ways and the Nobel Committee stressed this in its one-sentence description of his achievements (*vide supra*). It was fortunate that at the time of our meeting, I was engaged in studying one of the most symmetrical molecules, called adamantane, $C_{10}H_{16}$. The name refers to its high stability. The structure of the molecule resembles the diamond structure. Wigner's interest was in the solid state rather than in molecules, but he found my molecules intriguing. Later Wigner sent me his essay book, *Symmetries and Reflections*,⁹ in which he had a diagram of the diamond structure. It dawned on me only later how lucky I was that, if only for a few days, I had Wigner as my mentor in symmetry.

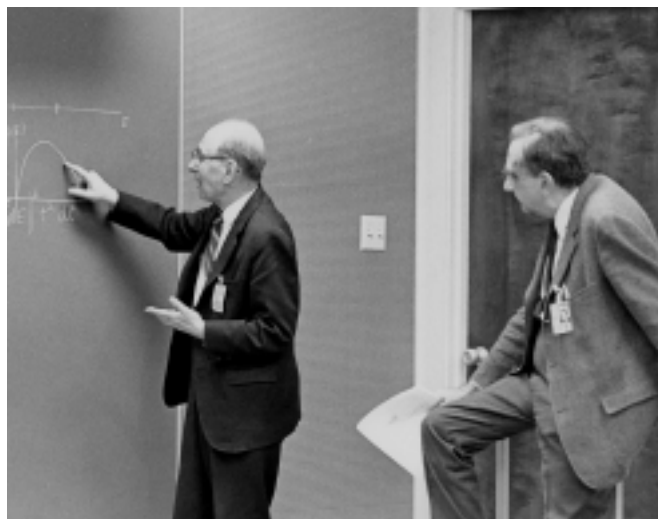
On that occasion we hardly went into specifics, but he had made seminal contributions to the utilization of symmetry in chemistry. Here I mention only a few examples. A fundamental property of the electronic wave function of a molecule is that it can be used as basis for irreducible representations of the point group of a molecule. This property establishes the connection between the symmetry of a molecule and its wave function. The preceding statement follows from Wigner's theorem, which says that all eigenfunctions of a molecular system belong to one of the symmetry species of the group.¹⁰ The first application of symmetry considerations to chemical reactions



John von Neumann (courtesy of Ferenc Szabadváry, Budapest Museum of Technology).

can be attributed to Wigner and Witmer.¹¹ The Wigner-Witmer rules are concerned with the conservation of spin and orbital angular momentum in the reaction of diatomic molecules. Although symmetry is not explicitly mentioned, it is present implicitly in the principle of conservation of orbital angular momentum. Wigner and Witmer prepared the ground for the breakthrough that followed almost four decades later through the works of Woodward and Hoffmann, Fukui, Bader, Pearson, and others. For two of them (Fukui and Hoffmann) it even resulted in a Nobel Prize in Chemistry in 1981.¹² In modern theory of chemical reactions invoking the symmetry principle, there is a consideration to connect levels of like symmetry without violating the so-called non-crossing rule. This non-crossing rule was introduced by Neumann and Wigner, and independently by Teller.¹³ According to this rule, two orbitals of the same symmetry cannot intersect in the correlation diagram. These diagrams provide valuable information about the transition state of the chemical reaction.¹⁴

Looking back to my conversations with Wigner about symmetry, two things have crystallized for me. Both are fundamental, almost trivial, and they provided a great introduction for me into symmetry considerations. One is that there is the geometrical kind of symmetry and then there is everything else, which includes, among others, the symmetry of molecules.



Eugene Wigner and Edward Teller at the Lawrence Livermore Laboratory during the 1970s (courtesy of György Marx).

The molecules are not rigid bodies and the larger-amplitude is their motion, the more they are away from structures with rigorous symmetry. The adamantane molecule has a rather rigid carbon cage and its T_d symmetry is unambiguous, but there are floppy molecules too that can be described much more loosely with point groups. Wigner then helped me see the yet much broader meaning of symmetry that Hermann Weyl had made broadly familiar through his Princeton lectures and his classic book¹⁵ that symmetry is also harmony and proportion.

The other characteristic of Wigner's approach to symmetry that has remained in my mind as sticking out from many other scientists' interpretations, is that Wigner did not distinguish between chemical symmetry and physical symmetry, and so on. For him the symmetry concept transcended man's subdivision of Nature into subject areas. In various fields and various applications, the models that we utilize may have different emphases on different aspects and may ignore different other aspects, but the symmetry concept itself is universal. Perhaps this universality is what has captivated me most about the symmetry concept and has encouraged me to write broadly about symmetry in various books.

Disregarding the boundaries of various branches of science served Wigner well. It has been noted repeatedly how useful his knowledge of materials

proved to be in the Manhattan Project. His studies with Polanyi about the mechanism of chemical reactions and the transition state have also proved useful in other branches of science. John Wheeler (see elsewhere in this volume) provided a nice example to illustrate this point:

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 oysters in 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 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;¹⁶ by strange coincidence the same day when Germany invaded Poland.

I thoroughly enjoyed my encounter with Wigner and his lectures in Austin were attended to capacity. He gave talks on different topics, his physics and his almost-obsession topic of civil defense. In the latter he mentioned the Budapest subway system as an example of civil defense in Eastern Europe, and he urged the United States to pay more attention to civil defense. Almost 30 years later, Wigner's infatuation with civil defense came up in my conversation with Steven Weinberg (see next interview). Weinberg mentioned that he and Wigner

... were quite different about politics. He got very angry with me. He edited a book about civil defense. ... He was a very committed believer in the Cold War, that America should prepare in every possible way for a nuclear confrontation with the Russians. ... The Cold War fed on itself in the sense that both sides were building more and more destructive weapons because the other side was. Wigner saw nothing wrong with that. He just wanted us to build everything we possibly



Princeton Physics Department Faculty, 1962 (photograph by and courtesy of Robert Matthews).

could. He wasn't interested in arms control, and I was. I wrote a rather negative review of his book on civil defense, and Wigner got very angry with me. He attacked me at a cocktail party in Princeton. He was quite hostile to me for a number of years.

Philip Anderson (see elsewhere in this volume) and Wigner never argued about politics but Anderson knew that they were quite different politically. Apparently Wigner became rather isolated in the department at Princeton, at least on one occasion. Anderson remembers when they discussed the David Bohm affair at a party, "Wigner was the only member of the department who was not in favor of keeping Bohm. The department voted for Bohm, but the President of Princeton turned down our recommendation and fired him. But Wigner had not voted for him."

Anderson told me an amusing story from 1954, which was about a trivial personal friction of no particular interest. What I found of interest is how Anderson characterized Wigner: “Wigner was not a Nobel-Prize-winner yet but he was Wigner.”

People in Austin in 1969 were very much in awe of him although there is a general opinion that by about the 1950s he had lost his creativity in physics. According to Anderson, he could no longer keep abreast with the latest developments and his influence started waning in the early 1950s. Weinberg told me an awkward story:

I gave a talk at a symposium in his honor, explaining how much I thought I'd learned from Wigner's approach to elementary particles and why his approach was superior to others. Wigner came up to me after my talk, and he didn't really understand what I'd said. Maybe what I interpreted as wignerism was not entirely wignerism. He may also have been already ill, so I don't know. Wigner is a person I have complicated feelings for.

Anderson's other encounter with Wigner was related to science and, sadly, it confirmed Weinberg's impressions of the late Wigner:

Then there was one more encounter around 1958 or 59, during one of my talks about the BCS (Bardeen-Cooper-Schrieffer) theory of superconductivity for which I had resolved the problem of gauge invariance. Wigner had written a paper in which he promulgated his super-selection-rules. One of these said that there can be no phase coherence between two states, which have different numbers of particles. A fundamental principle of the BCS theory allows phase coherence between states with different numbers of particles. It explicitly violates Wigner's super-selection-rule. There is though a way you can get around it, but I never believed much in its necessity. I don't think though that I was particularly dismissive of the super-selection-rules in my talk. In fact, I didn't mention them at all. But Wigner saw through me and he was very negative in his polite way. He made it clear that he didn't believe the BCS theory, and to his dying day he never accepted it. This was a deeper problem between us whereas the first two I mentioned were merely amusing incidents. Up until that point Wigner had been very central in theoretical physics but at about that time, in the 50s, he put his foot down and refused to go any further. At that point Wigner, as a person, seemed to become less relevant to physics.

Wigner went to the United States in 1930 for the first time. He had a half-time job in Princeton and a half-time job in Berlin. When Hitler came to power, Wigner decided to leave Germany for good and he helped his parents and sisters to go to the United States from Hungary too. In the mid-1930s, he lost his job at Princeton and spent two years at the University of Wisconsin. This is how he remembered it in 1986⁴:

In Princeton I never felt at home, but in Wisconsin I felt at home from the second day. After two years, I was asked to return to Princeton. The reason was that the people whom they had wanted to have, and for whom they had fired me, did not get the job in Princeton. Instead, they invited Van Vleck to Princeton but he preferred Harvard and recommended me in his stead. There was another reason for me to leave Wisconsin. I fell in love with a young lady in Madison and we got married, but after eight months of marriage she passed away. That made me very sad and I thought it was better for me to go away from the place where I had lost my better half. Thus I went back to Princeton in 1937.

In the video recording of 1986, Wigner talked to Clarence Larson about his participation in the Manhattan Project and, what I find especially interesting, about his later thoughts concerning the atomic bombs used in Japan in 1945⁴:

Many scientists came to the United States at that time from Germany, from Europe. Hitler did something very good for science in the United States. Niels Bohr was visiting in Princeton and gave a colloquium. I could not attend because I had to be in a hospital but a friend of mine, Dr. Szilard came to me and told me about it. Bohr talked about Hahn and Strassmann's discovery of nuclear fission. We thought a good deal about it and realized what enormous energies will be available this way. We started to think also of the danger of it. It became evident that those who had said that atomic energy could not be liberated were mistaken.

We soon began to realize also that it was quite possible that a bomb could be made with uranium, based on liberating such energy. We were very much afraid that the Germans would develop it because, after all, they had discovered all that. We were afraid that this would make it possible for Hitler to realize his dream of ruling the whole Earth. For most Americans, Hitler was so far away, in Germany, that they thought that they did not have to worry about



Eugene Wigner and Leo Szilard, winners of The Atoms for Peace Award (1960) (courtesy of György Marx).

that. Szilard and I did not think so. We knew that if we could come from Hungary to the United States, then Hitler would be able to come from Germany to the United States, and the distance was even shorter from Germany.

I persuaded two of my colleagues to work a little on this problem, and one of them, slightly annoyed, told me, “You are pleasantly disagreeable.” We also wanted to interest the United States as a whole because we thought that it was the duty of the government to defend the country. We talked with several people, but it was not very effective. Nobody took it very seriously. It was then Szilard’s idea to talk to Einstein about it. Perhaps Einstein could persuade the American government that it was an important question. So we went out to Long Island, where Einstein was vacationing. I expected that it would take a long time before we would be able to convince him that this was a serious problem. He did not know about the process, he was not interested in nuclear physics. But it took only 15 minutes and he understood the danger and he dictated a letter in German, which could be taken to the American authorities, even to the President. I took down the letter, brought it back to Princeton, had it translated into English, and had it typed, and I think it was Teller and Szilard who took the letter back to Einstein and he signed it. The letter then was taken to President Roosevelt, who then appointed people to study the question. Eventually, General Groves was put in charge of the program, and it was a great success.

As it turned out, it was not necessary at that time because Hitler, who had been told about the possibility of such a bomb, said that he would win the war long before such a weapon could work. Hitler did, in fact, lose the war before the bomb became effective. At that point we thought that it was not necessary to continue the work on the bomb, but the government was not of that opinion. General Groves also wanted to continue and he said that we could use it against the Japanese and it would shorten the war.

We proposed then to demonstrate the bomb in the presence of some Japanese scientists and military leaders. Groves once again disagreed and said that we should demonstrate it on a city. And that is what happened, but we were against it and were quite unhappy. We thought that many Japanese lives could have been saved if the bomb had been demonstrated on an uninhabited territory. But, apparently, I must admit, and I will admit, we were probably mistaken. Much later, I read in a book that the demonstrations in Hiroshima and Nagasaki may have saved many, many Japanese lives. Since I thought that a demonstration over an uninhabited territory in the presence of Japanese scientists and politicians could have sufficed, I went around and asked my Japanese friends about it. And with one exception, they said, “No, such a demonstration would have had no effect on the Emperor.” According to all my Japanese friends, with one exception, “It would not have had the same effect; it was very good that you demonstrated it this way.” Maybe that was the way to do it, but I did not think so at that time. Of course, they knew the Japanese politicians, the Japanese Emperor, and the Japanese military leaders much better than we did. But I was very surprised. They thought that many Japanese lives were saved this way even though it led to the extinction of many Japanese lives. Apparently General Groves was right, and the bomb had to be demonstrated the way it was.

In my conversations in 1969 with Wigner, apart from symmetry, we talked about many other things too, for example, poetry. His favorite was the Hungarian poet of the first half of the 19th century, Mihály Vörösmarty, and I introduced him to the poetry of the martyr Miklós Radnóti and later sent him a volume of Radnóti and corresponded with him about it. Wigner was interested in Hungarian politics, much more than I was. I did not think there was any political life in Hungary, but he carefully read even the speech of the defense minister, a truly insignificant figure, and made comments on it. I also noticed Wigner’s conservatism. At that time, in 1969, the first year of the Nixon Administration, the notion of



Eugene Wigner and István Hargittai in Austin, Texas, 1969 (by unknown photographer).

Lyndon Johnson's Great Society was still very close and the question of poverty in America came up. Wigner denied that there were poor people in America. As a proof he wanted me to come and see his neighborhood in Princeton! Both Anderson and Weinberg noted Wigner's political conservatism.

Apart from what Wigner taught me about symmetry, what especially stayed in my memory was what he told me about the Hungary of the early 1920s, as he experienced it. Sadly I recognized much of what he said in my own experience in the 1960s. His most important observation was how the Hungarian authorities treated the very Hungarians they were supposed to serve. They were the authority and the rest were the subjects. People stood in their offices, no matter who they were, and whatever they needed they were asking for it like beggars asking for a handout. Wigner said that whenever he felt any nostalgia about Hungary, he only needed to remember how the officials treated the people there, and it sufficed to sober him up.

References and Notes

1. Wigner, E. P. "Events, Laws of Nature, and Invariance Principles." In *Nobel Lectures: Physics 1963–1970*. Elsevier, Amsterdam, 1972, 6–17.
2. There were three physicists laureates in 1963 and they decided among themselves that Wigner should be the one giving the speech. There is one speech only in each category of the Nobel Prize on the occasion of that reception. Wigner had, of course, some preeminence among the three laureates since he received half of the prize while the other two, Maria Goeppert-Mayer and Hans Jensen shared the other half "for their discoveries concerning nuclear shell structure". The composition of the three laureates caused some headache to the people responsible for the protocol of the Nobel ceremonies that year. If there is one physics laureate, his wife goes into the dining room on the arm of the king. Further rules regulate the assignment of the other laureates and their spouses to members of the royal family when going into the dining room, and as regards their seating order. In 1963, according to alphabetical order, Goeppert-Mayer would have been the first among the physicists. However, the protocol people chose Wigner to be the first among the physicists, hence no new precedence had to be established. Wigner's primacy was justified by the above consideration [Hargittai, I., *The Road to Stockholm: Nobel Prizes, Science, and Scientists*. Oxford University Press, 2002, 10].
3. Wigner, E. P. "City Hall Speech — Stockholm, 1963." In Wigner, E. P. *Symmetries and Reflections: Scientific Essays*. Indiana University, Bloomington and London, 1967, 262–263.
4. Hargittai, I. "'You are Pleasantly Disagreeable': Eugene P. Wigner Remembers." *The Chemical Intelligencer* **1999**, 5(3), 50–52. This article is based on a video recording on March 4, 1986, by Clarence Larson.
5. Bardeen, J. "Reminiscences of early days in solid state physics." *Proc. R. Soc. Lond. A* **1980**, 371, 77–83.
6. Wigner, J. "Van-e a tudományak határa?" *Élet és Irodalom* 1964, VIII, 40. sz., 6–7. old. (1964 október 3). [Here J. stands for Jenő, Wigner's original first name whose English equivalent is Eugene.]
7. Wigner, E. P. "The Limits of Science." *Proceedings of the American Philosophical Society* **1950**, 94, No. 5 (October).
8. Hargittai, I. "Tudományok határán." *Élet és Irodalom* 1964, VIII, 51. sz., 6. old. (1964 december 19).
9. Wigner, E. P. *Symmetries and Reflections: Scientific Essays*. Indiana University Press, Bloomington, Indiana, 1967.
10. Wigner, E. P. *Group Theory and Its Application to the Quantum Mechanics of Atomic Spectra*. Academic Press, New York, 1959.
11. Wigner, E.; Witmer, E. E. *Z. Phys.* **1928**, 51, 859.
12. Hargittai, I. *The Road to Stockholm: Nobel Prizes, Science, and Scientists*. Oxford University Press, 2002.

13. Neumann, J. von; Wigner, E. *Phys. Z.* **1929**, *30*, 467; Teller, E. *J. Phys. Chem.* **1937**, *41*, 109.
14. For examples, see Hargittai, I., Hargittai, M., *Symmetry through the Eyes of a Chemist* (Second Edition). Plenum, New York, 1995, 301.
15. Weyl, H. *Symmetry*. Princeton University Press, Princeton, New Jersey, 1952.
16. Bohr, N.; Wheeler, J. A. *Phys. Rev.* **1939**, *56*, 426.