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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

[a] I. Hargittai
Budapest University of Technology and Economics and Hungarian Academy of Sciences
P.O. Box 91, Budapest 1521, Hungary
phone: +36 (0)1 463-4051
fax: +36 (0)1 463-4052
e-mail: istvan.hargittai@gmail.com

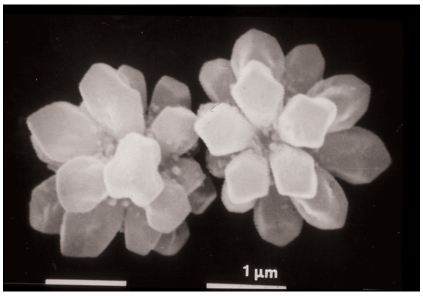

Figure 2. Flowerlike icosahedral quasicrystal in a quenched Al/Mn sample, courtesy of Ágnes Csanády, Budapest. Used with permission.

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

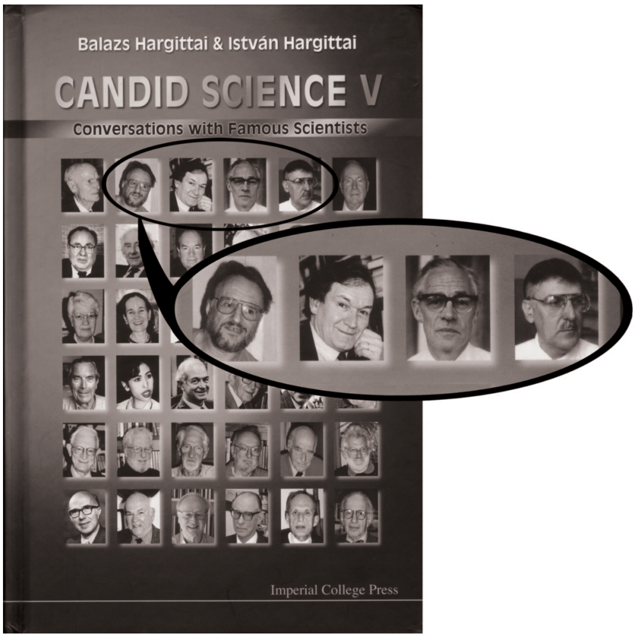

Figure 3. *Candid Science V* book cover highlighting John Conway, Roger Penrose, Alan Mackay, and Dan Shechtman (© I. Hargittai).

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]

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

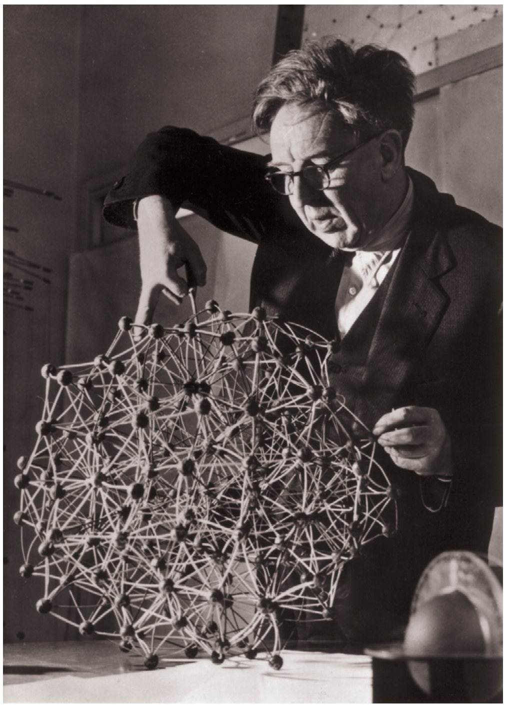

Figure 4. J. Desmond Bernal and his model of an ideal monoatomic liquid; courtesy of John Finney, University College London. Used with permission.

> 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-

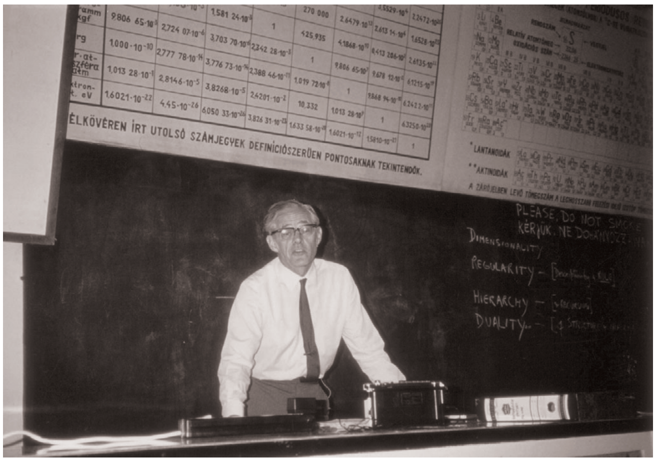

Figure 5. Alan L. Mackay lecturing on fivefold symmetry in September 1982, in Budapest; photo by and © I. Hargittai.

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 re-

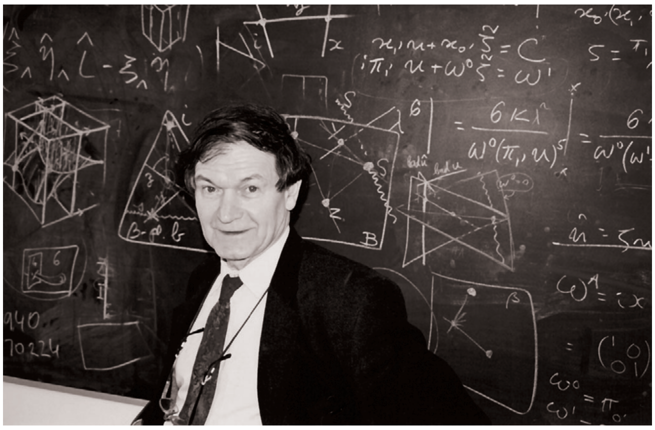

Figure 6. Roger Penrose in his office in Oxford in 2000; photo by and © I. Hargittai.

gretted. 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]

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]

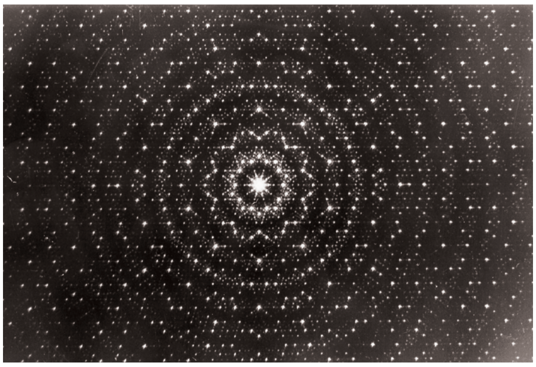

Figure 7. Simulated "electron diffraction pattern of three-dimensional Penrose tiling" in 1982, prepared for Professor Alan L. Mackay by Dr. G. Harburn at Cardiff University; courtesy of Alan Mackay, London. Used with permission.

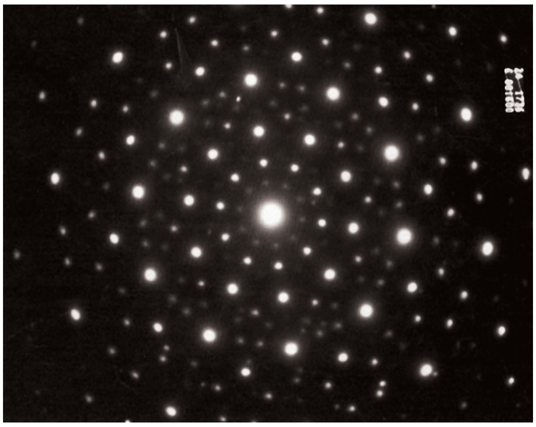

Figure 8. Dan Shechtman's electron diffraction pattern of an aluminum-manganese alloy with 10-fold symmetry; courtesy of Dan Shechtman, Haifa. Used with permission.

> 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]

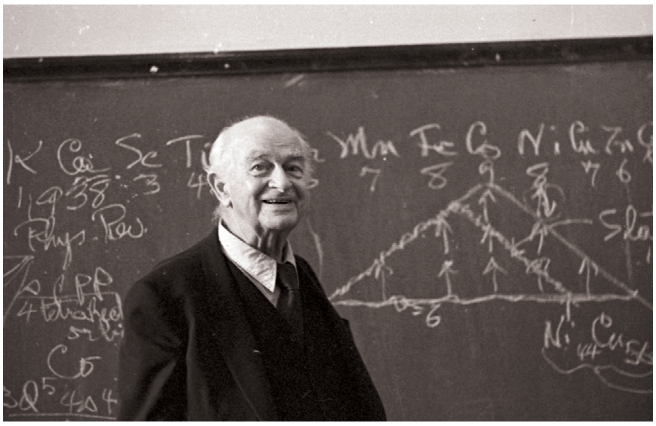

Figure 10. Linus Pauling in the early 1980s at Moscow State University; photo by and courtesy of Larissa Zasourskaya, Moscow. Used with permission.

> 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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