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Einstein famously wrote that the most incomprehensible thing about the world is that it is comprehensible. He was thinking about mathematical and theoretical physics. The idea is an old one. Nobel prize winner Paul Dirac believed that mathematics was an especially well-adapted tool to formulate abstract concepts of any kind, and he also famously insisted that mathematical beauty is a key criterion for physical laws.[1] But one of the most famous presentations of that thought was by Dirac's brother-in-law, Wigner Jenő Pál, a.k.a. Eugene P. Wigner.

Wigner was a highly successful scientist. In mathematical circles he is best known for his contributions to quantum theory, pioneering the application of group theory to the discovery of fundamental symmetry principles—and, of course, for his 1960 paper "The Unreasonable Effectiveness of Mathematics in the Natural Sciences." Some passages of the 1960 paper are often quoted; here is one:

> The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or for worse, to our pleasure even though perhaps also to our bafflement, to wide branches of learning (Wigner 1960, 237/549).

Toward the beginning of his essay, Wigner writes that "the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and [...] there is no rational explanation for it" (1960, 223/535). It is telling that the word "miracle" appears twelve times in the text!

Surely Wigner's focus was more on the question of what it is *in the physicist's approach* to reality, as it has developed since Newton, that makes it possible to formulate mathematical laws.[2] But in fact his paper has been widely discussed in connection with a related question, which is our concern here; namely, what is it *within mathematics* that makes possible its highly successful application in physics? A good number of people have offered replies to Wigner, aiming to show that there is no miracle.[3] Here too we shall critically discuss elements of Wigner's presentation that unduly transform the relation between mathematics and physics into "a gift" or "miracle" that is very difficult to understand. Beyond that, we shall try to unveil the sources of Wigner's point of view. His discussion is characteristic of mid–20th-century images of mathematics, but it is hard to

[1] Dirac (1963) expressed this consideration in very strong terms; see also Kragh (1990).

[2] In a paper that came to my attention, Arezoo Islami (2016) suggests that this is the right understanding of Wigner's paper. This convincing reading would also help to explain the relatively careless presentation of ideas about mathematical theory, as opposed to physical theory, in the paper (see the following).

[3] An example in this same journal is Grattan-Guinness 2008; see also Lützen (2011) and Russ (2011).

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square with present conceptions or even with the views of the best experts from a generation before him.[4]

Wigner's Views and the New Practice of Mathematical Physics

I will distinguish three different parts in Wigner's 1960 paper. First, there are three sections devoted to generalities about mathematics and physics, in which the reflections regarding physics stand out as more relevant and insightful. Concerning physics, he lays emphasis on how the identification of regularities in the chaotic phenomena depends on packing a lot of the information into the "initial conditions."[5] Next is a section in which Wigner makes his strongest case, highlighting the success of mathematical laws in physical theories to underscore how it is "truly surprising." Finally, he moves on to question the uniqueness of physical theory, that is, the hope for a single foundation of all physics or even all science.[6] I will begin in the middle by explaining the strongest case Wigner makes for the astonishing effectiveness of mathematics as a central component of the methodology of physics.

In the section entitled "Is the Success of Physical Theories Truly Surprising?" Wigner offers three examples—which, he adds, could be multiplied almost indefinitely—to illustrate the appropriateness and accuracy of the mathematical formulation of the laws of nature:

- Newton's law of gravitation,
- Heisenberg's rules of matrix mechanics,
- and the theory of Lamb shift in QED.

The law of gravity "which Newton reluctantly established" and which he could verify to within an error of about 4%, has proved to be accurate to within an error of less than $^1/_{10,000}$ of 1%. It "became so closely associated with the idea of absolute accuracy that only recently did physicists become again bold enough to inquire into the limitations of its accuracy" (Wigner 1960, 231/543).

As for the second example, from the early years of quantum mechanics, Heisenberg established some quantum-mechanical rules of computation—which were to lead to matrix mechanics—on the basis of a pool of data that included the behavior of the hydrogen atom and its spectrum. When Pauli applied quantum mechanics to the hydrogen atom in a realistic way, the positive results were an expected success. But then, says Wigner, it was "applied to problems for which Heisenberg's calculating rules were meaningless." These rules presupposed that the classical equations of motion had solutions with certain periodicity properties, "and the equations of motion of the two electrons of the helium atom, or of the even greater number of electrons of heavier atoms, simply do not have these properties, so that Heisenberg's rules cannot be applied to these cases. Yet the calculation of the lowest energy level of helium, as carried out a few months ago [1959] by Kinoshita at Cornell and by Bazley at the Bureau of Standards, agree with the experimental data within the accuracy of the observations, which is one part in ten millions. Surely in this case we got something out of the equations that we did not put in." (Wigner 1960, 232/544)

Certainly Wigner has a point here. In his view, it is the theoretical separation between initial conditions of the system and the simple, mathematical "laws of nature" that have allowed physicists to attain such impressive levels of success. Wigner is right in that the actual empirical success of physical laws went far beyond anything that might reasonably have been expected at the outset. It is unconvincing to regard this as the outcome of mere chance, but is there "no rational explanation for it"?

As we shall see, the way in which Wigner framed his understanding of mathematics plays a large role in creating the "mystery," the impression of a miracle. The advancement of physical science shows undeniably that there are mathematical structures underlying natural processes and phenomena. (Of course we lack an a priori argument that it *must* be so, but science never offers ultimate answers.) Even if we admit that there is a common structure between our mathematical models and real phenomena, this does not force us to interpret realistically *all features* of the models. That is, one can still be critical and ponder the possibility that some features of the mathematics may be human artifacts that perhaps impute extra structure, complications which distance our physico-mathematical understanding from "the real" itself.

Wigner was an important figure in the emergence of the radically new mathematical toolbox of quantum physics, built on top of new, abstract, unintuitive representations. Some physicists resented the abandonment of the toolkit of classical analysis in favor of group-theoretic methods, abstract spaces, and so on. Around 1930, they described these innovations as a "group pest" or "plague of groups." The situation worsened when, instead of seeking explicit solutions by calculus, the new goal became to find invariants associated with structural representations. Higher levels of theorizing began to occupy center stage, a case in point being symmetry considerations one level above the mathematical laws of physics (see Scholz 2006).

Broadly speaking, an essential ingredient of the new type of work was the infusion of a new style of structural and qualitative methods (set theory, topology, symmetries) to replace the old quantitative spirit and its search for concrete solutions on the side of calculation. Little wonder that questions would arise about the new balance between mathematics and physics. Before we discuss his views on mathematics, I will argue that Wigner's formative years in Berlin seem to have been particularly relevant in shaping his philosophical views.

[4]For a broad and enlightening historical perspective on this topic, see Bottazzini and Dalmedico (2001).

[5]Wigner's views on physical theory are very interesting, but we cannot go into details here. The interested reader may consult his Nobel lecture, in which he amplifies these themes, and also Islami (forthcoming).

[6]He insists that it is conceivable that one will be unable to unify the fundamental physical theories, and even more so for theories of biology or of consciousness. This argument may well have been aimed at the Unity of Science movement, which was seeking to unify all science from a physicalist standpoint.

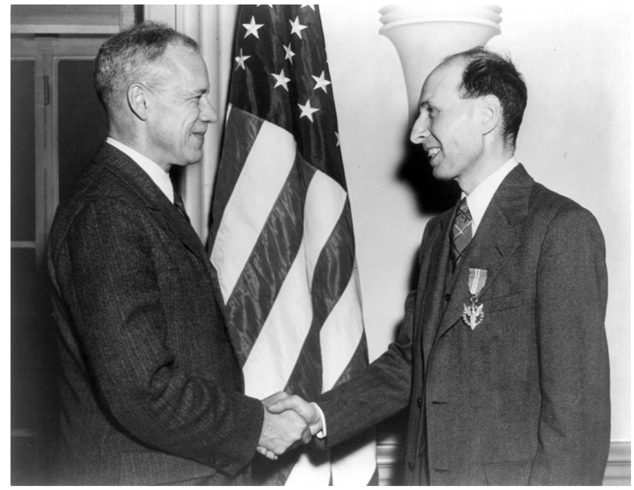
Some Biographical Elements

Eugene Wigner, who was born in Budapest in 1902, came from a well-to-do family. His long life included an extended period in Berlin until 1936, and a still longer one in the United States, mostly working at Princeton. He was awarded the Nobel Prize for Physics in 1963 "for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles."[7] Obviously the Nobel increased the visibility of Wigner's reflections on science, and in 1967 he published a selection of essays under the apt title *Symmetries and Reflections*. One of them was "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," originally published in the *Annals of Pure and Applied Mathematics* in 1960.

In Budapest, Wigner attended a secondary school (*Gimnázium*) where he obtained a sound training in mathematics. Two people were crucial in this respect, the noted mathematics teacher László Rátz, who knew how to care about promising students, and a fellow student who was one year younger, Neumann János, a.k.a. John von Neumann. Wigner's friendship with von Neumann was a lasting one and he would later acknowledge that he learned more mathematics from von Neumann than from anyone else.[8]

Wigner studied chemical engineering at the Technical University in Berlin, a choice strongly influenced by his father. He himself was more attracted to physics, and this led him to attend the Wednesday meetings of the German Physical Society, where he could see and hear luminaries such as Einstein, Planck, Sommerfeld, and Heisenberg. A noteworthy remark in his autobiography reads: "In my apartment, I read books and articles on chemical analysis, set theory, and theoretical physics" (Szanton 1992, 65). His independent reading on set theory is noteworthy, but this was probably because von Neumann was heavily engaged with the subject.

In the academic year 1926–1927, Wigner obtained a position in Berlin as an assistant to Karl Weissenberg, who worked on X-ray crystallography. Through his engagement with crystallography, Wigner was led to study group theory, taking up the algebra textbook by Heinrich Weber and then solving questions posed by Weissenberg.[9] The following academic year, he went to Göttingen to work as an assistant to David Hilbert. This might have been a momentous opportunity, yet things did not work out so well since Hilbert was seriously ill. Wigner was left to work on his own, and so decided to investigate the relation between group theory and the new quantum mechanics. Von Neumann had given him a crucial pointer, suggesting that he use group representations as found in the relevant papers by Georg Frobenius and Issai Schur.[10] Thus he became a pioneer in the new mathematical methods of theoretical physics.



Wigner receiving the Medal for Merit for his work on the Manhattan Project from Robert P. Patterson (left), March 5, 1946

Someone (presumably Wolfgang Pauli) characterized the period we are talking about as a time of "Gruppenpest" in physics.[11] The metaphor of a disease reflects the feeling of alienation experienced by many theoretical physicists, realizing that their traditional toolbox of classical analytical methods was being replaced by new and foreign "abstract" ideas. Wigner worked especially on the study of atomic spectra, which was to be the topic of his important book *Gruppentheorie und ihre Anwendungen auf die Quantenmechanik der Atomspektren* (1931). In the introduction he emphasizes how the precise solution of quantum mechanical equations by calculus is extraordinarily difficult, so that one could only obtain gross approximations. "It is gratifying, therefore, that a large part of the relevant results can be deduced by considering the fundamental symmetry operations [durch reine Symmetrieüberlegungen]." He adds,

> Against the group-theoretic treatment of the Schrödinger equation, one has often raised the objection that it is "not physical." But it seems to me that a conscious exploitation of elementary symmetry

[7]Half the prize went to Wigner, and the other half jointly to Maria Goeppert Mayer (the second woman to get the prize, after Marie Curie) and to J. Hans D. Jensen.

[8]"Jancsi von Neumann taught me more mathematics than any other of my teachers, even Ratz of the Lutheran gimnázium. And von Neumann taught not only theorems, but the essence of creative mathematical thought: methods of work, tools of argument" (Szanton 1992, 130).

[9]See Szanton 1992, 105–106; one of these questions was recognized by von Neumann to be related to group representations, and he told Wigner to study Frobenius and Schur (1905). On Weber's textbook, a crucial source for one or two generations of algebraists, see Corry 1996.

[10]See Hawkins (2000).

[11]Szanton 1992, 116–117. In an interview with Kuhn (1963), Wigner said: "I don't think [Pauli] liked it particularly … there was a word, Die Gruppenpest, and you have to chase away the Gruppenpest. But Johnny Neumann told me, "Oh these are old fogeys; in five years every student will learn group theory as a matter of course," and essentially he was right." (Arch. for Hist. of Quantum Physics. Eugene P. Wigner, Interview with T. S. Kuhn).

properties ought to correspond better to physical sense than a treatment by calculation.[12]

In 1928 Wigner became a *Privatdozent* at the Technische Hochschule in Berlin, but given the worsening political situation in Europe, in 1936 Wigner and von Neumann decided to settle permanently in "the New World." Nevertheless, the European years in the 1920s and 1930s had a particularly strong impact on Wigner's views. In Germany at the time, there was an intense sense of rupture, of new forms of life being created. Quantum mechanics was perceived as a radical break with the past. One spoke of "Knabenphysik," because its protagonists were all "youngsters" (except for Max Born and Niels Bohr). This tense social and intellectual atmosphere was alluded to in Wigner's reminiscences:

> Historians tell us that Berlin in the 1920s was a city in chaos. … Is a radical a man who repudiates the society of his parents and teachers? If so, then I was no radical in Berlin. I admired my teachers more with each passing year. I loved my parents and wanted to help them. To dream of pursuing a career that they had not chosen was a radical enough path for a youth of my background. I had no wish to be more radical than that. But if a radical is someone who regards a traditional subject in a revolutionary way, then perhaps I was a radical, because quantum mechanics had transformed physics and I embraced quantum mechanics fervently. (Szanton 1992, 84)

Wigner's early exposure to abstract mathematical theories led him to adopt some new and radical ideas about mathematics. These ideas were very different from the views of previous generations, and they came to be clearly expressed in his 1960 paper.

What Is Mathematics? To Be or Not to Be a Formalist

In the section "What is mathematics?" Wigner provides a surprisingly simple answer to this question:

> […] mathematics is the science of skillful operations with concepts and rules invented just for this purpose. The principal emphasis is on the invention of concepts. […] The depth of thought which goes into the formulation of the mathematical concepts is later justified by the skill with which these concepts are used (Wigner 1960, 224, W. 536).

Wigner here emphasizes the predominant role of intra-mathematical considerations, regardless of the potential for application to real phenomena. However, he makes a distinction. On the one hand, we have basic ideas such as the concepts and principles of elementary geometry, rational arithmetic, and even irrational numbers—which are directly suggested by the physical world. On the other hand,

> Most more advanced mathematical concepts, such as complex numbers, algebras, linear operators, Borel sets—and this list could be continued almost indefinitely—were so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty (Wigner 1960, 224).

Ingenuity, inventiveness, the skill of the virtuoso to develop interesting connections, guided by a sense of formal beauty and a basic concern for logical coherence, are what drive pure mathematics.

Such a description brings to mind the modernist work of von Neumann around 1930: developing axiomatic set theory in a way completely different from Zermelo's and introducing the notion of Hilbert space to redefine in a much more abstract setting the foundations of quantum mechanics.[13] Further, von Neumann was working on Hilbert's metamathematical program, and was invited to represent the foundational standpoint of formalism in a conference on the Epistemology of the Exact Sciences, organized jointly by the Berlin Society for Empirical Philosophy and the Vienna Circle in September 1930.

Strict formalism interprets mathematical systems as a game of symbols. The symbols have no other content than they are assigned in the calculus by their behavior with respect to certain rules of combination, the only requirement being consistency of the system. The network of relations thus codified in a formal calculus restricts possible applications or *interpretations* of the system. In the 1930s, this formalistic standpoint had the advantage of eliminating all "metaphysical difficulties" concerning mathematics, and in particular the need for positing any Platonic realm of mathematical objects. Formalism also incorporated traits of "conventionalism" about mathematics such as the insistence on simplicity and elegance or beauty as guides in the formulation of the basic principles of axiomatic systems.

In fact, Wigner's paper offers very little information on his sources about the idea of mathematics. We find a brief reference to Hilbert on foundations (1922), another passing reference to Karl Polanyi,[14] and mention of *Die Philosophie der Mathematik in der Gegenwart* (1932) by Walter Dubislav. Yet if we reflect on these sources, adding Wigner's time in Göttingen and his relation to von Neumann, links to the Hilbert School are predominant.

The case of Walter Dubislav (1895–1937) is particularly interesting. He was a member of the Berlin Association for Empirical Philosophy and one of the signers of the famous Vienna Circle manifesto. He began studying mathematics at

[12]Years later, when the English version was published, he wrote: "When the original German version was first published, in 1931, there was a great reluctance among physicists toward accepting group theoretical arguments and the group theoretical point of view. It pleases the author that this reluctance has virtually vanished in the meantime and that, in fact, the younger generation does not understand the causes and the basis for this reluctance. Of the older generation it was probably M. von Laue who first recognized the significance of group theory as the natural tool with which to obtain a first orientation in problems of quantum mechanics."

[13]According to Saunders Mac Lane (in Duren 1989, 330), after a lecture by von Neumann at Göttingen in 1929, Hilbert asked "Dr. von Neumann, ich möchte gern wissen, was ist dann eigentlich ein Hilbertscher Raum?" ("Dr. von Neumann, I would like to know, what after all is a Hilbert space?").

[14]There is not enough space here to develop, but one should emphasize that Polanyi was a very important influence, "my dearest teacher" who "decisively marked my life" (Szanton 1992, 76). A physical chemist, Polanyi was to become a philosopher of science and may have influenced Wigner insofar as he was heavily marked by matter/mind dualism (*op cit.* 76 ff). See also Esfeld (1999).

Göttingen, but World War I intervened. After military service he went to the University of Berlin, concentrating on philosophy and logic. The brief presentation of the philosophy of mathematics offered in his 1932 textbook is very clear, emphasizing mathematical logic, axiomatic thinking, and a form of empiricism in the case of applied mathematics. The imprint of the Hilbert School is undeniable here. Dubislav argued for the "character of calculation [*Kalkülcharakter*] in pure mathematics" and defended a strict formalism:

> Formalism states the following: that pure logic like pure mathematics are in the strict sense of the term not sciences, […] Pure logic and pure mathematics are calculi [*Kalküle*] which deal with this: obtaining from certain initial formulas, arbitrary in themselves, more and more formulas according to rules of operation that in themselves are arbitrary. Put grossly: pure logic and pure mathematics, taken in themselves, are games of formulas [*Formelspiele*] and nothing else (Dubislav 1932).

This was not yet a familiar point of view. As it turns out, Dubislav was a Privatdozent at the Technische Hochschule Berlin from 1928 and a colleague of Wigner there.[15]

Two issues deserve to be emphasized. First, logical empiricism would continue to be prominent in the philosophical context around Wigner and is visible in his (1960). But the second point is more directly interesting for my purposes. We have seen that Dubislav was a strict formalist, and that Wigner himself still defended a kind of formalism in his remarks about mathematics. The previous generation of physicists and mathematicians were not formalists and it was only the generation that matured in the 1920s that understood the new ideas about axiomatics, structures, logic, and foundations in a radical way. The situation is parallel to the radicalism of the new conceptions of the physical world among the "youngsters" who advanced quantum physics.

Hilbert was not a formalist at the level of epistemology. His celebrated formalism was a *method* adopted in the context of studies of the foundations of mathematics, for the goals of metamathematics (consistency proofs, decision procedures). Using the axiomatic method, one may begin by considering a particular field of work with concrete ideas. But there is much to gain methodologically by disregarding the particular meaning of the concepts, considering the axioms as schematic conditions, and adopting full freedom of interpretation. In foundational research, this attitude can be amplified to achieve strict formalization, but these methods do not expand into a full epistemological account, and such an account was not at all Hilbert's intention.

Incidentally, it is easy to find a thousand places in which Hilbert is alleged as saying, "Mathematics is a game played according to certain simple rules with meaningless marks on paper." The source of this (mis)quotation seems to be E. T. Bell, and it can nowhere be found in Hilbert's papers. What we can find in lectures of 1919–1920 is the following: "There is no talk of arbitrariness here. Mathematics is not like a game in which the problems are determined by rules invented arbitrarily, but a conceptual system [endowed] with inner necessity, that can only be this, and not any other way."[16]

I have previously mentioned the novelty of the work in mathematical physics around 1930 with its infusion of a new spirit of structural and qualitative methods in place of the old quantitative spirit. But this was not unknown to Henri Poincaré or Hilbert, so it cannot be simply regarded as the source of a formalist attitude. In this case, other more general sources have to be found, coming largely from the intellectual context.

As we discussed earlier, the young intellectuals in Berlin, during the 1920s, were living in a rather chaotic, rapidly changing, and heated cultural atmosphere. After wartime defeat and the political and economic turmoil (inflation, the Weimar republic), one could hear everywhere the call for a "new order," a new society, indeed a "new man," and of course new forms of science. Such a setting promoted forms of *modernism* in the sciences, modernistic tendencies that presented themselves as a radical break with the past.[17] Jeremy Gray (2008) offered a reconstruction of early 20th century mathematics as undergoing a "modernist transformation." He defines modernism, in science or mathematics, as the new conception of the field as "an autonomous body of ideas, having little or no outward reference, placing considerable emphasis on formal aspects of the work and maintaining a complicated—indeed, anxious—rather than a naïve relationship with the day-to-day world" (Gray 2008, 1). The interwar period was a particularly high time for such modernist tendencies.

Hilbert was basically right: formalism is very good as a method for studying foundations, but philosophical questions about the epistemic nature of mathematical knowledge require more sophisticated answers. Moreover, the examples Wigner presents from advanced mathematics do not support his formalist views. His list included complex numbers, algebras, linear operators, and Borel sets. His idea was that the elaboration of such concepts is guided by intramathematical considerations, disregarding considerations of the potential for application to natural phenomena. They are, according to him, "so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty."

What Is Mathematics? Remarks on the Reasonable Development from Physics

Wigner's most convincing example is that of complex numbers. Italian mathematicians introduced the square root of −1 in the 16th century to manipulate numbers and

[15]He may have given the 1930 book to Wigner as a gift. Wigner (1960, 237/549) also refers to Dubislav's *Natural Philosophy* of 1933, a text defending an empiricist philosophy of science.

[16]D. Hilbert, *Natur und mathematisches Erkennen*, lectures delivered in 1919–1920, ed. D. Rowe (Basel, Birkhäuser, 1992). The point has been made repeatedly by experts such as Corry 1996; Rowe, 2000; Mancosu 2010, 139–140.

[17]See Gray 2008, Epple and Müller 2017, and earlier work by Herbert Mehrtens.

expressions in algebraic equations. Astonishingly, the imaginary numbers turned out to play important roles in different places: the equation that some consider the most beautiful in all of math, $e^{\pi i} + 1 = 0$ (Euler), the fundamental theorem of algebra (D'Alembert and Gauss), Cauchy's integral formula, Riemann's mapping theorem, etc. Even so, the early results and procedures did not establish a secure position for imaginary numbers in the world of mathematics. Their full adoption occurred only in the 19th century and involved a reconception of the number concept as well as the establishment of geometric representations of the complex numbers. For Gauss and Riemann, considering the system of complex numbers as the *natural* general framework for number in general was a basic commitment and fundamental principle of pure mathematics.[18]

Wigner later emphasizes that quantum mechanics is formulated on the basis of *complex* Hilbert space. This is why theoretical physicists, such as Wigner and Roger Penrose, have placed great emphasis on the complex number structure. However, going somewhat against Wigner's thesis, the complex field inherits most of its properties from the real field. Wigner himself stressed that the real number system was devised so as to mirror the properties of measurable quantities. But let us concede that the story of complex numbers fits well with Wigner's viewpoint; their importance in connection with the theory of electromagnetic fields and, even more, quantum theory is astonishing.

Given that Wigner pioneered group-theoretic methods in quantum mechanics, it is noteworthy that group theory is not among his examples. However, it may well be the case that Wigner was aware of opinions like Hermann Weyl's (1928), that the group concept is in a sense "one of the oldest" mathematical concepts. The reasoning behind this statement is that group structures are implicit behind all kinds of ancient concepts and practices—symmetry considerations, operations of translation and congruence in basic geometry, measuring operations, and so on. Weyl's quite reasonable view is that 19th-century explorations and formalizations just made explicit and abstract what had been there, implicitly, throughout the history of mathematics.

Likewise, linear operators and matrices might have seemed a very novel feature to physicists around 1930, for the simple reason that they had not been part of their basic education, but in fact linear algebra arose naturally in different areas of mathematics *and* its applications. As Kleiner (2007, 79) remarks, "the subject had its roots in such diverse fields as number theory (both elementary and algebraic), geometry,[19] abstract algebra (groups, rings, fields, Galois theory), analysis (differential equations, integral equations, and functional analysis), and physics." Thus these examples were not good choices for Wigner's purposes.

Perhaps the oddest example in Wigner's list is his reference to Borel sets, given that these play no immediate role in physics. Probably Wigner chose the example of Borel sets as one of the central concepts of set theory in the first third of the 20th century—what better example of the purest in pure math?[20] Yet this case goes rather against his thesis. Borel sets are strongly linked with the function concept and their study was motivated by a desire to *restrain* the most general and arbitrary possibilities opened by set theory, to focus on concrete ideas closer to classical math.[21] The all-important notion of function is something that one misses in Wigner's list. But the study of functions has constantly been promoted by *extra*mathematical considerations, mostly physical.

As suggested previously, it is natural to compare Wigner's views with Poincaré's. Both were pioneers in the new mathematical methods and their use in physics—the group concept was a key guiding element—and both were highly influential in promoting new qualitative approaches and techniques. Also, both scientists were inclined to general philosophical reflection, and it is interesting that both emphasized the importance of the aesthetic element in guiding pure mathematics. Yet Poincaré never suggests a "miracle" in the role of mathematics in physics; on the contrary, he insisted on the interplay between mathematics and science, and he (unlike Wigner) emphasized the centrality of the continuum and the function concept. Thus in *The Value of Science* he writes:

> …physics has not only forced us to choose among problems which came in a crowd; it has imposed upon us problems such as we should without it never have dreamed of.[22]

A case in point might be Fourier's work in *Théorie analytique de la chaleur* (1822), in which he used trigonometric series in mathematical physics, also linked with the famous 18th-century discussion about vibrating strings. Fourier series were the background for Dirichlet's proposal of the notion of arbitrary function, as well as Riemann's study of highly discontinuous functions and his notion of the integral.

[18]On the history of complex numbers, see Nahin (1998), Ebbinghaus et al. (1991), and Flament (2003).

[19]Thus Grassmann in 1844 coming from geometry, and Dedekind in 1871 from algebraic number theory, were among the first to articulate modern ideas about the subject clearly (Kleiner 2007, 84–88).

[20]If Wigner studied Hausdorff's textbook in the 1920s, he must have learned about Borel sets. Hausdorff and Alexandroff proved in 1916 that the Continuum Hypothesis is true in the limited case of Borel sets; certain properties called "regularity properties" were established for them (e.g., Lebesgue measurability), and set-theorists were hard at work studying how far those properties applied.

[21]As a matter of historical fact, Émile Borel, René Baire, and Henri Lebesgue were all critics of Zermelo set theory. After 1905, they all criticized the most general notions of "arbitrary" set, "arbitrary" function, and the axiom of choice. It was their intention to obtain more clarity about the notion of set by focusing on sets that can be "constructed" by well-understood operations. See Ferreirós (1999, 315–316) and the letters from 1905 that were translated in the Appendix of the book by Moore (1982).

[22]"Analysis and Physics," Chapter V of Poincaré 1905, p. 80.

Conclusions

Even Wigner's friend, János von Neumann, who may have entertained modernist views akin to formalism around 1930, was no longer in agreement with him after World War II. In an interesting paper for the general public, published in 1947, he writes:

> The most vitally characteristic fact about mathematics is, in my opinion, its quite peculiar relationship to the natural sciences, or, more generally, to any science which interprets experience on a higher than purely descriptive level. […] Some of the best inspirations of modern mathematics (I believe, the best ones) clearly originated in the natural sciences. The methods of mathematics pervade and dominate the "theoretical" divisions of the natural sciences.

Contemporary Soviet mathematicians, who would have regarded Wigner's presentation as a quintessential example of bourgeois philosophy, were even more in favor of such views. I am led to mention this because Wigner, like his Hungarian friends Leo Szilard, Edward Teller, and von Neumann, was strongly anticommunist—and it may be the case that his political views colored his philosophical ideas.

Many of the great abstractions introduced in mathematics from the mid-19th century have strong roots in the (physically motivated) mathematics of functions, analysis, the real-number continuum, and geometry. Actually the 20th-century abstractions are often based on making the basic assumptions behind the earlier systems *more flexible*. And this increase in flexibility provides a very rational explanation of the applicability of mathematics! It is hardly surprising that a much more general and flexible theory of geometrical structures (e.g., Riemannian differential geometry) can be applied in many contexts in which the rigid structures of Euclidean geometry would not be applicable.

Yet perhaps the empirical success of mathematical laws in physics requires something else.

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