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THE UNREASONABLE EFFECTIVENESS. THE PHILOSOPHICAL PROBLEM OF THE APPLICABILITY OF MATHEMATICS

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ABSTRACT. In this article I will sketch a general introduction to the problem of the applicability of mathematics as it is conceived in contemporary philosophy of mathematics. After some brief considerations concerning the historical reason why this problem was dismissed by the philosophical analysis during the first half of 20th century, I will expound Wigner's puzzle, and I will make some theoretical considerations about it. In particular, I will show that the problem of applicability is independent from the ontological framework we can eventually assume and that it depends rather on the existence of an epistemological gap between mathematics and physics. Finally, I will try to sketch a possible line of inquiry for understanding the applicability of mathematics in physics.

KEYWORDS. Applicability of Mathematics, Wigner's Puzzle, Ontology.

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1 Applicability: a neglected problem

The problem of the applicability of mathematics has always been considered a central topic within the framework of philosophical reflection. From Plato to Frege, moving through Berkeley, Descartes, Kant, and many others, philosophy has always been interested in the reasons why it is possible to employ abstract concepts (such as mathematical ones) in order to count, to forecast empirical phenomena and, more generally, to explain reality.

However, it seems that contemporary philosophy (especially during the second postwar period) has forgotten the problem. This omission is exemplified by the fact that one of the most important 20th century anthology in philosophy of mathematics, Putnam and Benacerraf's Philosophy of Mathematics, includes no article concerning mathematical application or applicability. This is just what Mark Steiner reproached Benacerraf for: when he asked why they don't give this topic space in their anthology, Benacerraf answered that the lack of material was the reason.[1] Thus, we can agree with Steiner when he notes that «the disregard by the philosophical community of issues of mathematical application is quite recent».[2]

Wilholt (2006) tries to give an answer on why and how this problem has been progressively dismissed by philosophical analysis. He finds a cause for it in the history of logical positivism. He examines the idea of the early logicians who held that the analyticity of mathematics could account for its applicability. This idea has been transformed during Carnap's efforts to establish a consistent philosophy of mathematics within the framework of Logical Empiricism.

In Frege's conception, the analyticity of mathematics was conceived as adequate to explain mathematical applicability. According to his reductive logicism, mathematics could be reduced to logical concepts and this logical concepts could be applied to things in the world. The applicability of mathematics was so reduced to the applicability of logical concepts – something that after all we could even assume as a brute fact. But fregean reductive logicism could not be maintained for long, as is well known.

In Carnap's view, mathematical truths are analytically true in virtue of our adoption of a form of language, and the question why this language is applicable has not a theoretical but a pragmatic answer. Namely, nothing that could be demanded to philosophical analysis. But analyticity by itself does not grant an explanation of why mathematics is applicable: it needs to go logical reduction. So, Carnap's logicism saves analyticity of mathematical truth but loses applicability as one of the problems among the ones assigned to philosophical research.

Wilholt's analysis shows a possible cause for the neglect of applicability problem from the side of philosophy of science, but doesn't say much about this neglect from the side of philosophy of mathematics. On this side, probably, we can find a cause for it in the almost exclusive foundational interest that dominated the philosophy of mathematics during all the first half of the 20th century. This foundational interest shifted the philosopher's focus mainly on the internal relationships of mathematics, thus neglecting the external relationship between mathematics and other disciplines. As a result, the problem of applicability became just a test bed for the resilience of a foundational account. So, for example, logicist account of mathematics is better than formalist one because the former manages to account for applicability of mathematics in a way that the latter doesn't. As Russell points out:

> [W]e want our numbers to be such as can be used for counting common objects, and this requires that our numbers should have a definite meaning, not merely

[1] See (Steiner 1998, p. 14n).

[2] (Steiner 2005, p. 625).

> that they should have certain formal properties. This definite meaning is defined by the logical theory of arithmetic.[3]

Anyway, however this neglection could be accounted for, this oblivion lasted at least until 1960, when the physicist Eugene P. Wigner published an article titled "The unreasonable effectiveness of mathematics in natural sciences". In this article, Wigner underlines the existence of a problem that nevertheless, as the article's title suggests, seems to be unsolvable. The suggestive way in which he closed his article has became very famous in the literature on this topic:

> 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 of for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning.[4]

That goes without saying, this conclusion left many unsatisfied. Since the most part of the current philosophical reflection about the applicability of mathematics takes Wigner's article as its starting point and arises generally as a reaction to it, it is a good idea to take a deeper view into it.

2 Wigner's miracle

There are two points emphisized by Wigner:

> The first point is that mathematical concepts turn up in entirely unexpected connections. Moreover, they often permit an unexpectedly and accurate description of the phenomena in these connections. Secondly, just because of this circumstance, and because we do not understand the reasons of their usefulness, we cannot know whether a theory formulated in terms of mathematical concepts is uniquely appropriate.[5]

But what does he mean by "mathematical concepts"? His answer is very simple: «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».[6] Mathematical concepts, according to him, are *invented*. His claim is better clarified by his next words:

> 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 subject on which the mathematician can demonstrate his ingenuity and sense of formal beauty. [...]. The principal point [...] is that the mathematician could formulate only a handful of interesting theorems without defining concepts beyond those contained in the axioms and that the concepts outside those contained in the axioms are defined with a view of permitting ingenious logical operations which appeal to our aesthetic sense both as operations and also in their results of great generality and simplicity.[7]

[3](Russell 1993, p. 10).
[4](Wigner 1960, p. 14).
[5](Wigner 1960, p. 2).
[6](Wigner 1960, p. 2).
[7](Wigner 1960, p. 3).

According to Wigner, the main characteristic of mathematics consists in its "being of some interest". Part of the concepts are comprised among (defined by) the axioms and part are instead invented by the mathematician only in order to satisfy her "sense of formal beauty". So aesthetics comes out to be the main, metatheorical leading criterion that mathematicians follow.

Of course, to use the word "aesthetics" in this context does not helps very much. Actually, we would know which these aesthetical criteria are and what is their role in invention. Anyway, Wigner doesn't give any satisfactory answer to these questions.

Having this conception of mathematics in mind, he passes to analyze the role of mathematics in physical theories. He points out two different roles:[8]

1. evaluating the consequences of already established and already mathematically formulated theories; and

2. contributing to the (mathematical) formulation of physical theories.

The first role is the role generally assumed by applied mathematics, where mathematics merely serves as a tool – probably no more than a calculus. That's what happens when, for example, we want to know the exact position of a star in the sky at a certain time t: by means of the appropriate astronomical theory, we make the relative computations and we find the wanted result. In this case, we already have a mathematically formulated theory and we use mathematics just in order to evaluate the consequences of this theory.

The second role is the most intriguing. It consists in the fact that physicists choose certain mathematical concepts for the formulation of the laws of nature. This is something that comes out with strong evidence even at a first glance to the physicist's practice. But the very question is: Why does the physicist use these mathematical concepts to formulate the laws of nature?

> A possible explanation – Wigner answers – [...] is that he is a somewhat irresponsible person. As a result, when he finds a connection between two quantities which resembles a connection well-known from mathematics, he will jump at the conclusion that the connection is that discussed in mathematics simply because he does not know of any other similar connection. [...]. However, it is important to point out that the mathematical formulation of the physicist's often crude experience leads in an uncanny number of cases to an amazingly accurate description of a large class of phenomena. This shows that the mathematical langauge has more to commend it than being the only language which we can speak; it shows that it is, in a very reak sense, the correct language.[9]

The three examples he gives for substantiating his words are: (A) the use of second derivatives in Newton's law of gravitation, (B) the use of matrices in elementary quantum mechanics, and (C) the quantum theory of the Lamb shift. These examples show with great incisiveness the «appropriateness and accuracy of the mathematical formulation of the laws of nature in terms of concept chosen for their manipulability, the "laws of nature" being of almost fantastic accuracy but of strictly limited scope».[10] At this point, he proposes to refer to this fact as to the "empirical law of epistemology". This law, together with the laws of invariance of physical theories, is an indispensable foundation of these theories. Thus, according

[8]See (Wigner 1960, p. 6).

[9](Wigner 1960, p. 8).

[10](Wigner 1960, p. 10).

to Wigner's account, the astonishing ability of mathematical concepts in describing reality, and the amazing high degree to which it does that, is something that we should accept as an empirical fact – and that's all! Actually, the above quoted Wigner's conclusion is just a pompous phrasing for that.

For easing our discussion, we can sum up Wigner's argument as follows:

(A) Mathematicians *invent* their mathematical theories in order to satisfy their own *aesthetical* criteria $x_1, \dots, x_n$.

(B) Phisicists elaborate their physical theories in order to satisfy their own criteria $y_1, \dots, y_m$.

(C) Criteria $x_1, \dots, x_n$ are different from criteria $y_1, \dots, y_m$.

(D) It is unreasonable that something elaborated on the basis of $x_1, \dots, x_n$ can satisfy *in such an effective and precise manner* the critieria $y_1, \dots, y_m$.

(E) Conclusion: the employment of mathematical concepts in phisics is unreasonable.

So, we can focus now on the single premises (A)-(D), and try to understand whether they should be accepted or not.[11] Wigner doesn't give many convincing arguments for us to accpet his premises, and his analysis is surely unsatisfactory, especially from the philosophical point of view. However, to throw it away without considering his words worthy of a deeper philosophical analysis would be undoubtedly hasty.

3 Applicability problem and ontological considerations

The first point of Wigner's account concerns, as we saw, the fact that mathematical concepts are invented by mathematicians according to aesthetical criteria. The claim is indeed double: he is saying firstly that mathematical concepts are *invented*, and secondly that what guides mathematicians in this invention is beauty. Both these claims deserve our attention.

To say that mathematical concepts are invented could be seen as an adherence to an anti-realistic view on mathematics: mathematical concepts does not have an ontological reference in (what realists call) mathematical entities, since they are just an invention of human intellect. Actually, if we interpret Wigner's claim in this stronger, ontological sense, one could reply by saying that the problem of applicability is therefore a problem for anti-realists only. Thus, a realist philosopher would not have such a problem, since according to her mathematical concepts are not invented but has a precise (although abstract) reference in reality.

We can surely take Wigner's claim in this stronger sense, but I think it is not what Wigner had in mind. I think that what he means by "invention" is just that we select a class of mathematical properties or manipulation rules without any consideration of their physical interpretation. It is an invention, in the sense in which this word is opposed to "discovery", because it is the result of an act of free composition; but notwithstanding it is completely detached from the ontological level, since we are not caring about that. Let's take the case of complex numbers: they was "invented" simply by allowing for the solution of the operation $\sqrt{-1}$ and giving new definitions for the operations with the new numbers thus obtained. The sense of "invention" in mathematics is as the same as in painting: we invent a mathematical concept as well as we "invent" a picture. In both the cases, we are simply speaking on a level

[11]Obviously, I will omit to discuss premise (C), since it is just what we should explain if we don't accept Wigner's conclusion.

different from the ontological one. In a similar vein, Steiner (1998) says that mathematician is «closer to the artist than to the explorer» (p. 47). The emphasis is on the epistemological difference between the mathematician-artist and the physicist-explorer. Their epistemological strategies are different and that is what motivate the wonder for mathematical applicability.

However, the stronger, ontological interpretation raises a question: is it really true that the problem of applicability of mathematics is a problem for anti-realists only? Is it really true that for realists everything goes well with the applications of mathematical concepts? If it were so, then we could take the fact that realism weeds the applicability problem out as an evidence for accepting realism in mathematics. Actually, there are philosophers who assert this claim, for example Davies (1992, pp. 140-60) and Penrose (1990, pp. 556-7).

In this connection, Colyvan (2001*b*) tryes to face this opinion by showing that the applicability of mathematics is a problem for realist as well as for anti-realist philosophers. His argument moves from the presupposition that mathematics is useful not only in *describing* empirical reality, but also in *discovering* empirical laws.[12] Colyvan takes into consideration two important philosophies of mathematics, the first (realist) due to Quine (1948) and Putnam (1979), and the second (anti-realist) due to Field (1980). «Both of these philosophical positions – Colyvan says – are motivated by, and pay careful attention to, the role mathematics plays in physical theories. It is rather telling, then, that each suffers similar problems accounting for Wigner's puzzle».[13]

The first account (Quine & Putnam) starts from the general ontological principle according to which we are ontologically committed to all that we cannot dispense with. In more precise terms: if talk of some entity ξ is indispensable to a theory T, and T is our best scientific theory of some phenomena, then we are committed to the existence of ξ.[14] Since, according to Quine & Putnam, mathematical entities are indispensable in this precise sense, their conclusion is that we are committed in mathematical entities.[15]

The argument is evidently shaped in parallel with the analogous argument for scientific realism:

> It's no miracle, claim scientific realists, that electron theory is remarkably effective in describing all sorts of physical phenomena such as lightning, electromagnetism, the generation of X-rays in Roentgen tubes and so on. Why is it no miracle? Beacuse electrons exist and are at least partially causally responsible for the phenomena in question. Furthemore, it's no surprise that electron theory is able to play an active role in novel discoveries such as superconductors. Again this is explained by the existence of electrons and their causal powers. There is, however, a puzzle here for the anti-realist.[16]

In the case of scientific realism, there is indeed a pressure on anti-realists, since they seem not to be able to explain the efficacy of the electron theory in describing reality. But the argument can hardly be exported in the field of mathematical realism. The reason, according to Colyvan, is that

> There is an important disanalogy [...] between the case of electrons and the case of sets. Electrons have causal powers – they can bring about changes in the world.

[12] Also Steiner 1989, 1998 moves this point – a point that Wigner seems not to take into consideration.
[13] (Colyvan 2001*b*, p. 269).
[14] See (Colyvan 2001*b*, p. 270).
[15] The reader interested in a deeper survey on indispensability argument can refer to Colyvan (2001*a*).
[16] (Colyvan 2001*b*, pp. 270-1). As an example of such an argument for scientific realism, see Smart (1963).

> Mathematical entities such as sets are usually taken to be causally idle – they are platonic in the sense that they do not exist in space-time nor do they have causal powers. So how is that the positing of such platonic entities reduces mystery?[17]

So, it seems that the realist philosopher has two alternatives: (1) to say that mathematical entities are causally active; or (2) to accept that for her, as well as for the anti-realist, the applicability of mathematics causes a problem. Cheyne & Pigden (1996) have argued, against Platonism, that Quine's account is indeed compelled to (1). Someone else has tried to challenge the common sense according to which mathematical entities are causally inactive, by just substantivating alternative (1), but this road seems to be pretty much marginal within the contemporary debate.[18]

On the other side, Colyvan takes into consideration Field's nominalistic proposal. It can be summed up in the following three moves:

(1) *Fictionalism*, according to which all the mathematical sentences are literally false, but true in the fiction of the mathematical practice. This fictionalism is intended to remove the commitment to mathematical entities implied by our quantifications within mathematical statements.

(2) Enunciation of the so called *Principle of Conservativeness*, according to which if a mathematical theory is added to a nominalist set of propositions N (e.g., a scientific theory) no nominalistic consequences follow that wouldn't follow from N alone. This principle is intended to account for the possibility to use mathematical theories in science: we can do it beacuse this application is conservative. Move (2) also originates the Field's nominalistic project expounded at the next point.

(3) Nominalization of all our best physical theories. The complete realization of this last point assures that mathematics is indeed *dispensable*, contrary to Quinean belief. Field, for his part, gives a nominalization of a consistent fragment of Newtonian gravitational theory.

Field's project has undoubtedly many merits, but also many complaints,[19] including the fact that, as Colyvan underlines,

> [...] despite Field's careful attention to the applications of mathematics, he leaves himself open to Wigner's puzzle. [...] What he fails to provide is an account of why mathematics leads to simpler calculations. Moreover, Field gives us no reason to expect that mathematics will play an active role in the prediction of novel phenomena.[20]

Field's Principle of Conservativeness gives us good reasons to understand why mathematics *can* be used in physics, but he does not give us reasons to understand why mathematics is indeed used in physics. Since the application of mathematics is conservative (in the above

[17](Colyvan 2001*b*, p. 271).

[18]See, for example, Maddy (1990). However, in her next works (See (Maddy 1997, 2007)) she seems to have abandoned such a view on mathematical entities.

[19]One of its merits consists in giving a possible solution to the so called "Benacerraf's dilemma" (See (Benacerraf 1973)). The complaints concern both the effective possibility to expand the nominalization to certain branch of the physics (See (Malament 1982)) and whether all the concrete entities that he accepts are nominalistically acceptable (See (Resnik 1983, 1985)).

[20](Colyvan 2001*b*, p. 272-3).

sense), we can use it *if we have some good reasons to do it*. But which are these good reasons? That's what Field omits to say.

So, we can conclude, both realist and anti-realist have to face the same problem raised by Wigner. One could objects that Colyvan focused only on two particular realist and anti-realist philosphies of mathematics, and that other realist accounts of mathematics could "explain away" the problem of applicability. I think that this objection has no ground, for a very simple reason: any *ontological* choice we make about mathematical objects cannot fill the *epistemological* gap between physics and mathematics. By epistemological gap I mean the fact that mathematics and physics are devised wtihin two different epistemological framework. Physics, just to state the obvious, is much more empirical than mathematics and the comparision with empirical world has a bigger importance in physics rather than in mathematics. Mathematicians have a freedom in creating their own concepts that physicists don't have, and the way in which the former arrive to the knowledge of mathematical "facts" is very different from the way in which the latter arrive to the knowldge of physical phenomena. So, if we admit that in both the cases (in physics and in mathematics) we gain knowledge, we must also admit that the difference concerns first of all the way in which this knoledge is gained by us, and only secondarily the "what" that we are trying to knowledge. I think that this is just the way in which we should interpret Wigner when in premise (A) he speaks of mathematical "invention" of concepts: just in an epistemological – and not ontological – anthitesis with physical discovery. In other words, mathematical knowledge and physical knowledge are epistemologically different, and this epistemological difference cannot be accounted by ontological decisions.

4 Beauty and anthropocentrism

This gap between mathematics and physics is particularly marked in Wigner's formulation, because of the high importance he attaches to aesthetical criteria in the development of mathematics.[21] According to him, as we saw, mathematical concepts arise from the aesthetic impulse in humans. In order to better understand this claim, we would like to know what he means by "aesthetical criteria", but he is quite reticent about that. The only clarification he gives is when he esplicitly denies that simplicity is one of these aesthetical criteria. One could note that only a small number of mathematical concepts are used by physicists in formulating their laws of nature and that sometimes physicists do not *chose* these mathematical concepts, but they develop them independently and then recongnize them as having been conceived before by mathematicians.

> It is not true, however, as is so often stated, that this had to happen because mathematics uses the simplest possible concepts and these were bound to occur in any formalism. As we saw before, the concepts of mathematics are not chosen for their conceptual simplicity (even sequences of pairs of numbers are far from being the simplest concepts) but for their amenability to clever manipulations and to striking, brilliant arguments.[22]

However, we could reply that this argument is not forcefull at all. Indeed, even if simplicity is not among the aesthetical criteria adopted by mathematicians, it could be that the

[21] There is a wide shared opinion among working mathematicians about the tight connection between mathematics and beauty. See, for example, Rota (1977) and Hardy (1992).

[22] (Wigner 1960, p. 7).

resulting mathematical theories are indeed the simplest tool for pursuing a certain aim. For example, it could be that, after all, some mathematical structure are the simplest way to represent a certain physical system – even if that structure was not developed by mathematicians for its simplicity but for its aesthetical properties. The crucial point is that the fact that a theory has been selected on the basis of aesthetical criteria does not exclude that that theory could have some other interesting properties, and that those properties could make the theory desirable by the physicists for their own purposes. We could also admit that matrix theory were developed for aesthetical purposes, but the resulting theory is not only beauty: it has also other interesting properties that prompted physicists to use that for representing and manipulating – as an instance – the rotation of a body in the space. Besides, there are branches of mathematics that were developed under the push of physical questions. That's the case of the analysis, developed by Newton and Leibniz during XVII century mainly to give an answer to concrete applied problems. We can also think that also in this case mathematicians selected the prettier theory among a number of possible theories, but the fact remains that all the possible theory among which they selected the prettier must be, as a necessary requisite, apt to solve the applied problem for which they were devised. So, if we conceive aesthetical criteria as only acting a selection within mathematics (as Wigner seems to do), there is no reason to think that there could not be another more foundamental criterion acting on the formulation of mathematical concepts such that it could explain why mathematics is so effective in physics, so bridging the epistemological gap aforementioned.

A stronger version of Wigner's puzzle is given in Steiner (1998), where is also given a different puzzle valid not only for descriptive applicability but also for the nondeductive role of mathematics in discoverying the laws of nature. Especially, Steiner stresses the *costitutive character* of the aesthetical criteria in mathematics: by providing several quotations from renowed mathematicians as von Neumann and Hardy, he arrives even to conclude: «That the aesthetic factor in mathematics is *constitutive* has actually become a truism in the mathematical community».[23] Such aesthetical criteria are *species-specific*,[24] so that mathematics turns out to be – according to Steiner – an eminently *anthropological* production, to which we cannot attach any character of objectivity. Moreover, by appealing on this anthropocentric character and by showing the wide employment of pythagorean and formalists analogies in the discovery of new theories by the 20th century physicists, Steiner arise a critic to naturalism's pretension to properly account for the process of scietific inquiry. A "Pythagorean" analogy is a mathematical analogy between physical laws not paraphrasable at time t into nonmathematical language. A "formalist" analogy is an analogy based on the syntax or even orthography of the language or notation of physical theories, rather than what it expresses [25] As an example of the former, Steiner instances Maxwell's prediction of electromagnetic radiation and Schroedinger's discovery of wave mechanics. As an example of the latter, he instances the extension of the quantum mechanical formalism to configuration spaces with "deviant" topologies and the strategy of "quantization" of quantum systems.[26]

[23](Steiner 1998, p. 65). Italics mine.

[24]See (Steiner 1998, p. 6). By the way, it is interesting to note that the strong stress he puts on costitutive role of beauty in mathematics also gives him a reason to refuse structuralist philosophies of mathematics: for there is no objective criterion for a structure to be mathematics, and not every structure counts as mathematics. Chess, for example, has structure, but it does not count as mathematics since it – according to Steiner – does not embody mathematical beauty (See (Steiner 1998, p. 7 and p. 66).

[25]See (Steiner 1998, p. 54).

[26]See (Steiner 1998, chapters 4-6).

I agree with Pincock (forthcoming) when he objects to Steiner that we have to keep the question "What makes this concept mathematical or nonmathematical?" separate from the question "What makes this concept a good or a bad mathematical concept?" and that Steiner's arguments in answering the former can answer at the most the latter.[27] So, we can accept that beauty has a *selective* role in mathematics, since it selects beautiful theorems or theories and promote the development of this or that branch; but that it has also a *costitutive* role is not certain at all.

Now, the anthropocentric character of mathematics claimed by Steiner is a direct consequence of the constitutive role played by beauty in mathematics. But if we deny this constitutive role of beauty in mathematics, we can still say that beauty has a selective role in mathematics without being compelled to say that mathematics is anthropocentric. For if beauty is not a *constitutive* criterion for accepting a mathematical concept (or theory), then there could be another constitutive criterion – and this constitutive criterion might also be non-anthropocentric and leave open the possibility of a link between mathematics and physics.

This line of inquiry is instantiated, for example, by structuralist philosophers of mathematics, according to which the constitutive criterion for X to be mathematics is that X is a structure. Thus, structuralists can solve the Wigner's puzzle by saying that mathematics is the general science of structures, and that these structures are just those displayed in nature and studied by physicists.[28]

Actually, also Steiner thinks that the Wigner's puzzle about the descriptive applicability of mathematics can be *partially* solved by giving up the pretension of solving the problem in general and by focusing on the effectiveness of single mathematical concepts. For each of these concepts, we can try to detect a physical concept (or property) matching it. As an example, Steiner mentions the concept of fiber boundle and its employment in gauge field theory, where «the remarkable applicability of fiber boundle theory to physics rests on the translatability of the concepts of fiber boundle theory into the concepts of gauge field theory».[29] However, this strategy cannot be generalized, since there are cases in which we are not able (at least at the moment) to found a complete matching between mathematical and physical concepts.[30]

Moreover, Steiner's remarks about the employment of Pythagorean and formalist analogies in discovering new laws of nature extend the extent of Wigner's puzzle, so that Steiner's puzzle is wider and deeper than Wigner's one. Even if we accept structuralist solution to the descriptive applicability problem, we still have a problem, according to Steiner, concerning his puzzle about the heuristic role of some purely mathematical analogies in contemporary physics.

5 Application and mathematization

In order to make this puzzle a bit less obscure, we can note what follows. There is an important aspect, concerning the applicability of mathematics, that we have still to take into consideration. Premise (B) in Wigner's argument underlines the fact that physicists and

[27] See (Pincock forthcoming, chapter 8) for a more detailed analysis of Steiner (1998).

[28] For more details on structuralism and its account of applicability, see Shapiro (1983) and Shapiro (1997). See also Bueno & French (1999) and French (2000) for an interesting case study.

[29] So that, as an instance, the global gauge corresponds to the principal coordinate boundle, the gauge type to the principal fiber boundle, the gauge potential b^k_μ to the conncection on a principal fiber boundle, and so on. See (Steiner 1998, pp. 32-4).

[30] Steiner mentions the case of the application of complex analysis in physics.

mathematicians have different criteria in mind when they work with mathematical concepts. Premise (D) says that what is unreasonable is that this difference could be in some sense fruitful. Both these premisses focuses on the criteria adopted by physicists and mathematicians in order to develop, employ and apply mathematical concepts. But there is another aspect that we should take into consideration, that is to say, the amount of work that physicist has to do in order to make a physical concept *compatible* with the mathematical apparatus that she is going to apply.

There is a general tendency for philosophers to speak of the "use" or "the employment" of mathematical concepts by physicists. The resulting picture seems to be the following: mathematicians devise their mathematical concepts for their own purposes and following their own methods; then the physicists select some of these concepts and simply "employ" them in their theories. It seems as if it were mathematics that does all the work. Sometimes, as Wigner said, physicists elaborate mathematical concepts and then discover that those concepts were already devised by mathematicians; but also in these cases, the mathematical concepts were already available. But what exactly means that physicists employ mathematical concepts in their theories? What we have not still considered is that this employment is not simple at all, but requires a number of "adaptation moves" that are sometimes very complex.

Let me take a very trivial example. When Kepler, speaking with the vulgar, "applied" the conic theory (developed eighteen centuries before by Apollonius of Perga) to the planets' movement, he had to previously idealize the planets themselves, making them nothing more than mathematical points in a bidimensional space. Namely, he had to abstract from their corporeal nature.

A more complex example can be found in the application of group theory to particle physics.[31] In ordere to make it possible, physicists had to make a number of theoretical moves. A preliminar move was Heisenberg and Dirac's work on the quantum indistinguishable particles, that showed an interesting symmetry characteristic, that is, that quantum states are invariant under permutation. This permutational symmetry is an abstraction from particles' identity and is what permitted the application of symmetric group. Then, by ignoring inter-electronic interactions, they could describe rotational symmetry by means of the proper group representation. And again, by ignoring the differences between the masses of protons and neutrons and abstracting from their charge, physicists conceived the atomic nucleus as consisting of a single kind of particle (the nucleon). By means of this last move, physicist introduced (by analogy with the situation in the atom) the notion of isospin.[32]

What we must note is that what permitted to arrive at this mathematical description is a list of idealisations that disclosed the underlying symmetry principles. As French notes,

> the physics is manipulated in order to *allow it to enter into a relationship with the appropriate mathematics*, where what is appropriate depends on the underlying analogy. At the most basic level, what motivates this manipulation and therefore underpins the effectiveness of mathematics in this case are the empirical results concerning intra-nuclear forces and the near equivalence of masses.[33]

In applying a mathematical theory to physics, we have often to import structure from the mathematical level to the physical one, and we can do that only by means of some theoretical

[31] See Bonolis (2004) for a detailed historical reconstruction.

[32] The analogy is here Pythagorean, according to Steiner's definition, since the analogy has no physical justification, being completely mathematical. As Steiner (1998) points out: «even today, physicists see no *physical* analogy between the quantities "spin" and "isospin"» (p. 90).

[33] (French 2000, p. 114). Italics mine.

moves. In very general terms, we could say that the aim of these moves consists in gaining a more abstract level. This abstraction can be obtained by means of idealization, or generalization, or approximation, or carelessness of some details, or definitions, or by means of more of these strategies together. A very interesting case is that of definition. Let consider the following quotation from a textbook on knot theory:

> Almost everyone is familiar with the simplest of the common knots, e.g., the overhand knot [...] and the figure-eight knot. [...]. A little experimenting with a piece of rope will convince anyone that these two knots are different: one cannot be transformed into the other without [...] "tying" or "untying". Nevertheless, failure to change the figure-eight into the overhand by hours of patient twisting is no proof that it can't be done. The problem that we shall consider is the problem of showing mathematically that these two knots [...] are distinct from one another.
>
> *Mathematics never prove anything about anything except mathematics*, and a piece of rope is a physical object and not a mathematical one. So before worrying about proofs, we must have a *mathematical definition of what a knot is*. [...]. This problem [...] arises whenever one applies mathematics to physical object that approximate the physical object under consideration as closely as possible.[34]

What this quotation shows is that the application of mathematics to physics is less a dipping mathematics into physics than a raising physics to the abstract level of mathematics. It is just by focusing on this abstract character of mathematics that maybe we can – I guess – find a way out from Steiner's puzzle.

We can reformulate the premise (B) in a more suitable way:

(B') In order to apply mathematics in physics, physicist has to shape their concepts on the model of mathematical ones.

So formulated, (B′) defuses the premise (D), since it is no more a question of differing criteria. Whatever the mathematical criteria $x_1, \ldots, x_n$ be, there is something in the resulting outcome that interests physicists. Thus, the question is not "Why does anything elaborated on the basis of $x_1, \ldots, x_n$ can satisfy *in such an effective and precise manner* the different critieria $y_1, \ldots, y_m$?", as suggested by premise (D); but rather: "Which characteristic(s), owned by mathematical concepts, is (are) so interesting in the eyes of the physicist? And why does the possession of such a characteristic by physical concepts make them so effective and fruitful in describing and discover the laws of nature?". We can also think, as Wigner and Steiner do, that the criteria determining the development of mathematics are completely different from the criteria determining the development of physics, but this does not mean that these criteria cannot produce a resulting characteristic that is of some interest for the physicist. A good candidate for this characteristic is probably *abstraction*, but it could be not the only. However, if the physical concepts must undergo a process of progressive abstraction in order to "accomodate" mathematical structure, then it is not a surprise that, in highly abstract contexts, there could be cases in which purely formal analogies give important heurisitc inupt.

Of course, there is still much to say about the reasons why abstraction (or whatsoever characteristic) is so effective in describing and discover the laws of nature. For example, how is it possible that abstract concepts could give so precise predictions of *concrete* phenomena? However, in conclusion, the reformulation of Wigner's puzzle we has come to, far from being less problematic, seems at least to suggest us a possible way out from the Steiner's puzzle and

[34] (Crowell & Fox 1963, p. 3). Italics mine.

to bridge, in some sense, the epistemic gap that separate physics and mathematics. For the two epistemological frameworks in which mathematics and physics are developed, all things considered, could have a point of contact just in this (or these) characteristic(s).

6 Conclusion

The considerations stated up to now don't allow us to prove, once and for all, that Wigner is wrong in considering the applicability of mathematics as unreasonable. Maybe it's really true that mathematical effectiveness is «a wonderful gift which we neither understand not deserve». Maybe it is really unreasonable and we'll never understand it. However, I hope that the previous analyses have at least scratched the aura of mistery coming from Wigner's formulation, suggesting that, difficult as it could be, philosophers should not give up the challenge. After all, we should never forget the words of Peirce:

> The second bar which philosophers often set up across the roadway of inquiry lies in maintaining that this, that, and the other never can be known. [...]. But to avert that that answer will not be known tomorrow is somewhat risky; for oftentimes it is precisely the least expected truth which is turned up under the ploughshare of research».[35]

[35](Peirce 1931, I.138).

References

Benacerraf, P. (1973), 'Mathematical truth', *Journal of Philosophy* 70, 661–80. Now in Benacerraf & Putnam (1983). 35

Benacerraf, P. & Putnam, H., eds (1983), *Philosophy of Mathematics*, 2nd edn, Cambridge University Press, Cambridge (Mass.). 42

Bonolis, L. (2004), 'From the rise of the group concept to the stormy onset of group theory in the new quantum mechanics. A saga of the invariant characterization of physical objects, events and theories', *Rivista del Nuovo Cimento* 27(4-5). 39

Bueno, O. & French, S. (1999), 'Infestation or pest control: the introduction of group theory into quantum mechanics', *Manuscrito* XXII(2), 37–68. 38

Cheyne, C. & Pigden, C. R. (1996), 'Pythagorean powers or a challenge to platonism', *Australasian Journal of Philosophy* 74(4), 639–645. 35

Colyvan, M. (2001*a*), *The Indispensability of Mathematics*, Oxford University Press, Oxford. 34

Colyvan, M. (2001*b*), 'The miracle of applied mathematics', *Synthese* 127, 265–277. 34, 35

Crowell, R. H. & Fox, R. H. (1963), *Introduction to Knot Theory*, Ginn and Co., Boston. 40

Davies, P. C. W. (1992), *The Mind of God*, Penguin Book, London. Trad. it. *La mente di Dio*, Mondadori, Milano 1996. 34

Field, H. (1980), *Science Without Numbers*, Princeton University Press, Princeton. 34

French, S. (2000), 'The reasonable effectiveness of mathematics: partial structures and the application of group theory to physics', *Synthese* 125, 103–120. 38, 39

Hardy, G. H. (1992), *A Mathematician's Apology*, Cambridge University Press, Cambridge. First edition: 1940. 36

Maddy, P. (1990), *Realism in Mathematics*, Clarendon Press, Oxford. 35

Maddy, P. (1997), *Naturalism in Mathematics*, Oxford University Press, Oxford. 35

Maddy, P. (2007), *Second Philosophy*, Oxford University Press, Oxford. 35

Malament, D. (1982), 'Review of Field's *Science Without Numbers*', *Journal of Philosophy* 79, 523–534. 35

Peirce, C. S. (1931), *Collected Papers of Charles Sanders Peirce*, Vol. 1, Harvard University Press, Cambridge (Mass.). 41

Penrose, R. (1990), *The Emperor's New Mind: Concerning Computers, Minds and the Laws of Physics*, Vintage, London. Trad. it. *La nuova mente dell'imperatore*, Rizzoli, Milano 1992. 34

Pincock, C. (forthcoming), *Mathematics and Scientific Representation*, Oxford University Press, Oxford. 38

Putnam, H. (1979), Philosophy of logic, *in* 'Mathematics Matter and Method: Philosophical Papers Vol. 1', 2nd edn, Cambridge University Press, Cambridge. 34

Quine, W. V. O. (1948), 'On what there is', *The Review of Metaphysics* 2(5). Reprinted in *From a Logical Point of View*, 2nd edition, Harvard University Press, Cambridge (Mass.) 1980. 34

Resnik, M. D. (1983), 'Review of Hartry Field's *Science Without Numbers*', *Noûs* 17, 514–519. 35

Resnik, M. D. (1985), 'How nominalist is Hartry Field's nominalism?', *Philosophical Studies* 47, 163–181. 35

Rota, G. C. (1977), 'The phenomenology of mathematical beauty', *Synthese* 111(2), 171–182. 36

Russell, B. (1993), *Introduction to Mathematical Philosophy*, reprint edn, Routledge, London and New York. Firs edition: 1919. 31

Shapiro, S. (1983), 'Mathematics and reality', *Philosophy of Science* 50, 523–548. 38

Shapiro, S. (1997), *Philosophy of Mathematics. Structure and Ontology*, Oxford University Press, Oxford. 38

Smart, J. J. C. (1963), *Philosophy and Scientific Realism*, Routledge and Kegan Paul, New York. 34

Steiner, M. (1989), 'The application to mathematics to natural science', *Journal of Philosophy* 86, 449–480. 34

Steiner, M. (1998), *The Applicability of Mathematics as a Philosophical Problem*, Harvard University Press, Cambridge, Mass. 30, 34, 37, 38, 39

Steiner, M. (2005), Mathematics — application and applicability, *in* S. Shapiro, ed., 'The Oxford Handbook of Philosophy of Mathematics and Logic', Oxford University Press, Oxford, pp. 625–650. 30

Wigner, E. (1960), 'The unreasonable effectiveness of mathematics in the natural sciences', *Communications in Pure and Applied Mathematics* 13(1), 1–14. Reprinted in *Symmetries and Reflections*, Indiana University Press, Bloomington 1967. 31, 32, 36

Wilholt, T. (2006), 'Lost in the way from Frege to Carnap: How the philosophy of science forgot the applicability problem', *Grazer Philosophischen Studien* 73, 69–82. 30