On ‘The Unreasonable Effectiveness of Mathematics in the Natural Sciences’

Chapter · January 2016

DOI: 10.1007/978-3-319-28163-6_2

1 author:

Sorin Bangu
University of Bergen

On 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences'

Sorin Bangu

Abstract I present a reconstruction of Eugene Wigner's argument for the claim that mathematics is 'unreasonable effective', together with six objections to its soundness. I show that these objections are weaker than usually thought, and I sketch a new objection.

1 Introduction

In a well-known essay published in 1960, the celebrated physicist Eugene Wigner claimed that "the appropriateness of the language of mathematics for the formulation of the laws of physics" is a "miracle" (Wigner 1960, p. 14). Despite Wigner's immense scientific reputation (he will be awarded the Nobel prize in 1963), the general sentiment is that he hasn't quite succeeded in making a case for the 'miraculousness' of the applicability of mathematics—although everyone agrees that the issue is prima facie intriguing. In fact, the issue was considered so intriguing that several of the brightest minds of theoretical physics (Dirac, Weinberg, Wilczek) found worth engaging with it; moreover, one even gets the impression, upon becoming familiar with the early literature discussing this so-called 'Wigner puzzle', that for a good while after 1960 the conundrum interested more the scientists and the mathematicians than the philosophers. This situation, it seems to me, changed significantly after the year 2000—that is, after the publication, in 1998, of Mark Steiner's landmark book *The Applicability of Mathematics as a Philosophical Problem* (Harvard Univ. Press). Thus, in the last decade or so, partly due to this book's influence, the puzzle has received significantly more attention from philosophers.

And, to be sure, there is no shortage of attempts to (dis)solve the puzzle, fact which accounts for the almost universal skeptical sentiment I mentioned above.

S. Bangu (✉)
University of Bergen, Bergen, Norway
e-mail: sorin.bangu@uib.no

E. Ippoliti et al. (eds.), *Models and Inferences in Science*,
Studies in Applied Philosophy, Epistemology and Rational Ethics 25,
DOI 10.1007/978-3-319-28163-6_2

In this paper, I will end up sharing this sentiment, but not before casting a critical eye on the proposed solutions. I shall proceed as follows. First of all, I'll spell out the puzzle—or, more precisely, a *version* of it as reconstructed from Wigner's essay. The 'unreasonableness' claim will appear as the conclusion of a valid argument, and thus the next natural step will be to inspect the premises. Then, I will identify six different (types of) solutions, each of them attacking one (or more) of these premises. Although these are cogent objections, and certainly raise doubts about the soundness of the argument, they are not decisive; a defender of Wigner's central point will surely feel their force, but will not need to concede defeat. Finally, I will sketch a different (and, as far as I can tell, novel) solution to the puzzle, drawing on what I'll call 'ecological' considerations affecting scientific research.[1]

2 Wigner's Argument

Before we get to discuss Wigner's argument per se, it is important to clarify two aspects of it. First, Wigner talks about the unreasonable effectiveness of 'mathematics' in physics, but what he has in mind is something slightly more specific: the effectiveness of the mathematical *language*—and by this it is pretty clear that he means the effectiveness of a fair amount of *mathematical concepts* (and *structures*), such as complex number, group, Hilbert space, etc. (these are some of his own examples). The second and related question is 'what are these concepts effective *for*?' Wigner's answer is that these concepts are effective for "the formulation of the laws of physics" (1960, p. 6), that is, in *describing* natural phenomena, or, more exactly, certain law-like regularities holding in nature.

This clarification of *what* is effective (mathematical concepts), and what they are effective *for* (describing nature), is necessary in order to distinguish Wigner's concern from a recent proposal of a somewhat similar problem by Steiner (1998). For Steiner, what is primarily (and ultimately mysteriously) effective are mathematical *analogies*, and what they are effective for is the formulation of *novel laws* of physics —that is, laws formulated by analogy with the existent mathematically formulated laws.[2] (The new laws are needed in domains of reality not covered by the existing laws, such as the quantum domain.) Unlike Wigner's, Steiner's main concern is thus the *heuristic* role of mathematics, or its ability to mediate the development of new laws. Here, however, I will put this issue aside, and focus on Wigner alone.[3]

---

[1]I deal with the puzzle in my (2009) and, more thoroughly, in my (2012, Chap. 7). Although there is some overlap between this paper and my treatment of the issue in my book, the current paper offers a different reconstruction of the puzzle. My conclusion, however, is the same—that Wigner's riddle can be (dis)solved.

[2]Grattan-Guiness (2008) seems to me an example, among others, of conflating these separate issues: Wigner's, who focused on the role of mathematics in describing nature, and others' concerns with its role in theory-building.

[3]For my take on Steiner's own argument, see my (2006) and (2012, Chap. 8).

Now, what is Wigner's argument in his 1960 paper? As it happens, this is not immediately clear, since his points are open to a couple of reconstructions. Steiner (1998, pp. 45–6) offered one of the first such careful renderings. He identified two versions of the argument; one is as follows:

> Concepts c_1, c_2, c_3,…, c_n (some listed in the paper; see above for a sample) are unreasonably effective in physics, and these concepts are mathematical.
>
> Hence, mathematical concepts ('mathematics') are (is) unreasonably effective in physics.

This argument is invalid, and if this version is what the critics had in mind then their discontent is understandable. Steiner points out that the conclusion doesn't follow; what follows is a weaker claim, that *some* mathematical concepts are unreasonably effective—and this invites the query as to how this unreasonable effectiveness is related to their being mathematical. Yet, with Steiner, I also believe that a more charitable reconstruction is possible and, taking my cue from his analysis (and also departing from it), I will put one forward below—and call it 'W_A'.

My W_A is meant to be the version of Wigner's concern that fascinated those most brilliant theoretical physicists I named above, and its specificity is that it is a *diachronic*, or *historically-based* reconstruction of his point. I favor this specifically diachronic version since it reflects faithfully the oddity of a certain "situation" (Dirac's word; see below) noticed not only by Wigner, but also by other people, both before and after the publication of his article. Here is what Paul Dirac said in 1939 in his note on 'The relation between mathematics and physics':

> One may describe this situation by saying that the mathematician plays a game in which he himself invents the rules while the physicist plays a game in which the rules are provided by Nature, but as time goes on it becomes increasingly evident that the rules which the mathematician finds interesting are the same as those which Nature has chosen. (1939, p. 124)

This quote is very suggestive, as it encapsulates all the elements I will include in the W_A: the idea that mathematicians 'invent the rules', that what drives this invention is what they find 'interesting' (hence the aesthetic aspects of W_A), and finally the overt reference to the temporal succession. Similar to Dirac's point above, Steven Weinberg writes:

> It is positively spooky how the physicist finds the mathematician has been there before him or her. (1986, p. 725),

where, importantly, what led the mathematicians 'there' was their aesthetical sense:

> [M]athematicians are led by their sense of mathematical beauty to develop formal structures that physicists only later find useful, even where the mathematician had no such goal in mind. […]. Physicists generally find the ability of mathematicians to anticipate the mathematics needed in the theories of physics quite uncanny. It is as if Neil Armstrong in 1969 when he first set foot on the surface of the moon had found in the lunar dust the footsteps of Jules Verne. (1993, p. 125)

Wigner himself talks explicitly in diachronic terms, when referring to physical concepts as discovered

> independently by the physicist and recognized then as having been conceived *before* by the mathematician. (1960, p. 7; my emphasis)

Finally, against this background, this is the W_A:

1. Modern mathematical concepts originate in our (mathematicians') aesthetic preferences.
2. It is unreasonable that these concepts, originating in the subjective aesthetic domain, are effective in the objective domain of physics.
3. And yet this is the case: several physical theories proposed at a later time t' turned out to benefit significantly from the application of mathematical concepts developed at an earlier time t.
4. Therefore, it is unreasonable that modern mathematical concepts (developed up to an earlier time t) are effective in the physics introduced at a later time t'.

Curious as it may seem, such explicit reconstructions of the problem are not common in the literature. It is not always recognized what the Wigner puzzle in fact is, namely a *pre-established harmony* type of mystery[4]: how can it be that such a temporal anticipation of physics by mathematics exists throughout the history of science?

Since the validity of argument W_A is not an issue anymore, the objections have to focus on the truth of the premises. And, as I said, all proposed solutions so far are formulated as attacks on one, or several, of these three premises. I will examine the premises in the next section (and, after that, the solutions).

3 A Closer Look at the Premises

Let us put the premises under a magnifying glass. I will take them in turn. To begin with the first, what does it mean to say that modern mathematics and, more specifically, modern mathematical concepts and structures, have aesthetic origins? That is, what can one make of the claim that modern mathematics is "the science of skillful operations with concepts and rules invented just for this purpose", where the purpose is for mathematicians to "demonstrate [their] ingenuity and sense of formal beauty."? (Wigner 1960, p. 2)[5]

---

[4]In fact, Bourbaki, when referring to this issue, uses the word 'preadaption'. Here is the entire quote: "Mathematics appears […] as a storehouse of abstract forms—the mathematical structures; and it so happens—without out knowing why—that certain aspects of empirical reality fit themselves into these forms, as if through a kind or pre-adaption." (1950, p. 231) I found this quote in Ginammi (2014, p. 27).

[5]Wigner also writes that mathematical concepts "are defined with a view of permitting ingenious logical operations which appeal to our aesthetic sense … [they are chosen] for their amenability to clever manipulations and to striking, brilliant arguments." (1960, p. 7).

This first premise makes two claims. First, that (i) mathematics is a human *invention*, i.e., the concepts are free inventions of the mind, and also that (ii) among these many free creations, some of them strike the mathematicians as particularly beautiful, interesting, etc.—and thus they are selected, on the basis of these *aesthetic criteria*, to be studied and developed (typically by formulating and proving theorems about them.)

It is important to clarify what 'aesthetic' means in this context. The central idea of an aesthetic preference has to be construed as a rather broad notion. It is an umbrella-term, standing of course for what Wigner himself called above "formal beauty", but also covering a wider gamut of related sentiments such as certain concepts being 'interesting', 'elegant', 'simple', 'deep', 'unifying', 'fruitful', 'stimulating', 'intriguing', etc. Like other important physicists (his friend John von Neumann included; see below), Wigner believes that mathematicians are free to choose what concepts to work with, and they select what they find—in these various guises—'beautiful'.

Thus, to say that the primary creative impulse of a (modern) mathematician is aesthetic is to stress that the concepts and structures she selects to study are

(a) *neither* descriptions of some natural phenomenon,
(b) *nor* tools to help the development of an existing (perhaps incipient) physical theory.

Two examples may clarify the matter here. The invention of *real* analysis (or 'calculus'), by Leibniz and Newton, provides one particularly clear illustration of a mathematical achievement that does *not* have aesthetic origins. On the other hand, the concept *complex number* (and, consequently, *complex* analysis) does qualify as having aesthetical ancestry, since the introduction of complex numbers satisfied clauses (a) and (b) above.[6] The same relation holds in other subfields of mathematics, for instance between Euclidean geometry and its various multi-dimensional generalizations or alternatives. It is also important to understand that this 'mathematical aestheticism' is perfectly compatible with some of the aesthetically-driven mathematicians' *hope* or *desire* that maybe in the future the physicists will find the concepts she studied useful. This kind of attitude (sometimes transpiring in their writings) doesn't make the initial impulse to focus on these concepts and structures less 'pure', i.e., less aesthetical. (We'll get back to this point when we'll discuss the Riemann episode below.)

Returning to the first premise, its two parts have different statuses. Component (i) expresses adherence to a metaphysical view of the nature of mathematics (anti-Platonism), while (ii) sounds more like a factual statement about certain historical/psychological events, or processes: the circumstances of origination, or

[6]Jerome Cardan, who is credited with introducing them in the 16th century, remarked that "So progresses arithmetic subtlety the end of which, as is said, is as refined as is useless." (Cited in Kline 1972, p. 253). According to Kline, neither did Newton regard complex numbers as significant, "most likely because in his day they lacked physical meaning." (1972, p. 254).

invention, of certain concepts. I will leave (i) aside for the moment (I will get back to it in Sect. 4), as it is notoriously difficult to search for justifications for such basic metaphysical commitments—here I'll only focus on (ii). This is a claim that can be vindicated by research into the history of mathematics. This kind of research is available and, as it happens, seems to confirm Wigner. The historian Kline (1972, pp. 1029–31) summarizes the situation as following:

> [G]radually and unwittingly mathematicians began to introduce concepts that had little or no direct physical meaning (…) [M]athematics was progressing beyond concepts suggested by experience (…) [M]athematicians had yet to grasp that their subject … was no longer, if it ever had been, a reading of nature. (…) [A]fter about 1850, the view that mathematics can introduce and deal with rather arbitrary concepts and theories that do not have immediate physical interpretation but may nevertheless be useful, as in the case of quaternions, or satisfy a desire for generality, as in the case of n-dimensional geometry, gained acceptance.[7]

Another way to go about this first premise is to simply ask the (great) mathematicians themselves: do *they* think that a view like Wigner's has any credibility?[8] If the practitioners' avowals are to be given any weight, then aestheticism is supported by quite a few, and prominent mathematicians. Among the most cited such confessions is the one belonging to Richard Hamming (of the 'Hamming code' fame), that "*artistic taste* plays a large role in modern mathematics" (1980, p. 83; author's emphasis)[9]; another belongs to no less a figure than John von Neumann. He makes the point at the end of the paragraph below, worth quoting in full because it also canvasses some important insights into the relation between physics and mathematics. Like Dirac, he talks about a certain "situation":

> The situation in mathematics is entirely different [from physics]. (…) 'Objectively' given, 'important' problems may arise after a subdivision of mathematics has evolved relatively far and if it has bogged down seriously before a difficulty. But even then the mathematician is essentially free to take it or leave it and turn to something else, while an important problem in theoretical physics is usually a conflict, a contradiction, which 'must' be resolved. (…) The mathematician has a wide variety of fields to which he may turn, and he enjoys a very considerable freedom in what he does with them. *To come to the decisive point: I think that it is correct to say that his criteria of selection, and also those of success, are mainly aesthetical*. (1961, p. 2062; emphasis added)

More pronouncements like these can be found, but I will now move on to the second premise. It states that the modern mathematical concepts, originating in the subjective domain of our aesthetic sense, should not be effective in the objective domain of physics—and hence it is 'unreasonable' if they are. What does Wigner

[7]The selection of quotes is from Maddy (2007, p. 330).

[8]But, should one take into consideration their views on the matter, when they bothered to express them? My answer (for which I don't have space to argue here) is 'yes', but not everybody agrees; see Azzouni (2000, p. 224).

[9]Hamming continues by saying that "we have tried to make mathematics a consistent, beautiful thing, and by doing so we have had an amazing number of successful applications to the physical world" (1980, p. 83)—yet another expression of the Wigner problem.

claim here? The short answer is that what he says amounts, in essence, to voicing the generally accepted idea that there is no obligation for the world to conform to our human, parochial aesthetic preferences, in the sense that there is no obligation for the laws governing the world to be expressible in mathematical concepts. He expresses the same sentiment as Freeman Dyson who once asked, "Why should nature care about our feelings of beauty?" (1986, p. 103)

A more complete answer has to bring up the most intriguing element of the entire Wigner issue: yes, the Universe and the laws of nature are under no such 'obligation'—unless they, together with the human race, have somehow been *designed* to match. That is, unless a certain form of *anthropocentrism* is true. This means that we inhabit a 'user-friendly Universe',[10] that the human species has a privileged place in the grand scheme of things, that our subjective aesthetic inclinations (expressed in favoring certain concepts) have a correlate in objective physical reality,[11] i.e., are truth-conducive. This (intelligent) *design* suggestion has of course been long questioned, opposed, and considered 'unreasonable' by many. Thus, in doubting it in the second premise, Wigner doesn't in fact make any novel or controversial claim, but simply joins this rather influential line of thought. In the end then, although the premise may initially sound problematic, it turns out that it reflects the general naturalistic, agnostic (even atheistic) contemporary scientific zeitgeist.

We have now reached the third and last premise, which completes the argument. Under the (generally shared) assumption that modern mathematical concepts stem from the mathematicians' aesthetic inclinations, Wigner also pointed out that such concepts should not be effective in physics—and thus it is 'unreasonable' if they are. The third premise closes the circle by stating that they are so indeed: several physical theories proposed at certain points in time turned out to benefit significantly from the already developed mathematical theories and concepts. Let's ask, once again, what is claimed here. Similar to the second component (ii) of the first premise, one can't help but notice that this third premise also sounds like a factual claim; hence, such 'situations' can be documented historically. Then, the relevant question to ask here is whether Wigner (or anyone else) has done any *quantitative* assessment of the historical record, counting (α) the number of physical theories in this situation (the 'successes'), as well as (β) the ratio of successes to 'failures'.

I will now go over all available solutions, beginning with the one that takes issue precisely with the third premise. As it turns out, out of the six solutions I'll be discussing, only one attacks the first premise—although, as I'll argue in the last section of the paper, this premise is in fact the most vulnerable one.

---

[10]This reconstruction is heavily influenced by Steiner (1998), and I warn the reader that I may have been reading too much into Wigner's (1960) paper.

[11]Wigner makes the point about the independence (hence objectivity) of the laws of a huge variety of particular circumstances on pp. 4–5 in his (1960).

4 Revisiting the Available Solutions

The literature on Wigner's problem spans more than half a century, but (I dare saying) any attempt to deal with his argument is summarized by one (or several) of the six characterizations below (I present them here in abridged form, but more details follow):

It is *not* unreasonable to find mathematical concepts available to be applied in physical theories because …

> the situations when this happens are *not* numerous, and thus these 'successes' can be attributed to chance (Solution S1)
>
> Wigner's starting point, that mathematics is invented, is just *false* (S2)
>
> mathematical concepts have *empirical origins* (as opposed to aesthetical ones) (S3)
>
> applicability presupposes modeling, i.e., the 'preparation' of physical systems *in order to* apply mathematics (S4)
>
> there is *over-determination* in the relation between mathematics and physics, i.e., there is a lot of mathematics to choose from when we need to embed a physical insight (S5)
>
> our aesthetical mathematical sense is shaped by evolution, hence it is *sensitive to environment* (S6)

The first solution S1 attacks premise (3)—that several physical theories proposed at a later time turned out to benefit significantly from the application of mathematical concepts developed at an earlier time—by pointing out that its advocates make an exaggerated claim. Quantitatively speaking, the situation can be the result of pure chance.

Fair enough, the premise *is* rejectable in this way. However, before a counting of successes is done, the premise does not strike one as *clearly* false. One has to admit that the counting (once we settle upon *what* and *how* to count) *may* confirm Wigner. But this is perhaps too defensive; a sympathizer of Wigner's argument may also counterattack by proposing that this third premise should be read along *qualitative* lines too. Or, more precisely, that one should balance the strict quantitative reading against a more qualitative one. She could say that although there may be many natural phenomena that fail to receive a mathematical description, we should also judge the relative *relevance* of these 'failures' (and 'successes') in the larger context. From this perspective then, what the third premise says is that we should focus on the rather few *major*, truly *important* episodes in modern theoretical physics; and, if we do this, we'll see that they support Wigner's claim in premise (3). These major episodes are not very numerous (in absolute number) to begin with, hence the number of mathematical concepts and theories which were 'waiting', available for physicists to use them, should not be expected to be numerous either.

On this reading, the premise says that one can list *relatively* many major achievements in modern physics which fit Wigner's W_A scheme perfectly. It is, for instance, widely accepted that Einstein's General Theory of Relativity (developed, roughly, between 1905 and 1916) drew massively on Riemannian geometry (developed before 1900), that quantum mechanics (in essence a product of the first quarter of the 20th century) makes essential mathematical use of complex numbers (among other concepts well-established before 1900). Moreover, groundbreaking work (within quantum field theory) on the classification of elementary particles by Wigner himself, Gell-Mann and others between (roughly) 1930s and 1970s employed group theory concepts (such as group 'representation') introduced much earlier (beginning with Frobenius, Lie, Shur, E. Cartan, and others.)

The list can be continued with several other well-known examples; thus, if the qualitative reading of this third premise is allowed to counteract the blind quantitative aspect, then one reads premise (3) as follows: 'restricting judgment to the *few* major breakthroughs in modern physics, many *of them* were anticipated by mathematical concepts and structures'. Read this way, one may begin to see that the support for this premise is actually not weak at all, to say the least.

The second solution S2 above rejects the first component (i) of the first premise. Such an objector denies that mathematics is 'invented' and, in particular, that modern mathematical concepts were invented to satisfy the mathematicians' aesthetic preferences. Thus, the picture Wigner proposes—that mathematicians invent concepts and decide to study those that foster beautiful theorems—is wrong. The correct metaphysical picture is something one may call Theistic Keplerian Platonism: there is a Creator of the Universe (God), and He made the world using a mathematical blueprint. For instance, when God created the solar system, He implemented in it the mathematical properties of the five 'perfect' solids—as the numerical values of the radii of the planets moving around the Sun; this is what Kepler, and others, genuinely believed.[12]

On this picture, a mathematician doesn't choose what concepts to study, but rather 'sees', with his 'mind's eye', what is 'there' (in the realm of mathematical forms) to be investigated; these concepts more or less 'force upon' him (to allude to the recent Platonist, Kurt Goedel). His job as a mathematician is thus not to invent anything, but rather to describe (as theorems), the eternal, true relations among these concepts; a mathematician's responsibility is therefore to discover the ways to connect these concepts, i.e., to discover proofs of the theorems. Moreover, once one does this properly, one may expect that one will *also* discover truths about the physical (material) world! This is so since, by assumption, these concepts and relations served as God's blueprints in designing the world.

Although some of the details of the story are still to be filled in, it should now be clear that there can be no problem as to why the Wigner-type coincidences arise. In fact, S2 is so radical that it almost generates an anti-puzzle: to the extent that one is a genuine mathematician (i.e., able to peek at God's blueprints), one *must* find such coincidences! Moreover, one should stop calling them 'coincidences', since they are the result of intentional acts of Divinity.

[12] A well-known passage from Kepler reads: "Thus God himself was too kind to remain idle, and began to play the game of signatures, signing his likeness into the world; therefore I chance to think that all nature and the graceful sky are symbolized in the art of geometry." Quoted in Dyson (1969, p. 9).

What about Theistic Platonism then? While it may have sounded credible a few centuries ago,[13] there are very few people who believe it today.[14] The general metaphysical view underlying it strikes us as creating more problems than it solves. (To mention an obvious one: what kind of evidence does one have, or *can* one have, for the postulations of this doctrine? What kind of epistemology do we have to develop to make sense of this picture of the world and of mathematics?) In the end, this seems a rather clear case in which the cure is worse than the disease, so to speak, as the amount of controversial metaphysical baggage one has to assume in order to make S2 work is *too* large. On balance, this way out seems then implausible; hence, one would be better off ignoring it, and looking for alternative ideas.

And it is not unusual that many find a more plausible alternative in the third solution S3. Thus, one can also object to the first premise, but not so much to component (i), as to component (ii). One rejects the view that mathematical concepts have aesthetical origins because, on this view, mathematical concepts have *empirical* origins. Philosopher Ernst Nagel's pronouncement is usually invoked here, as it nicely summarizes this position:

> It is no mystery, therefore, that pure mathematics can so often be applied (…) because the symbolic structures it studies are all suggested by the natural structures discovered in the flux of things. (1979, p. 194)

To this, a defender of Wigner's argument has two replies. First of all, this objector forgets about an important aspect of premise (1), namely that it is about *modern* mathematics, not about the basic, traditional arithmetical and geometrical concepts. They of course may well have empirical origins, and Wigner himself grants this in his paper.[15] But his point is not about these types of concepts; it's about the modern/advanced ones. These, as we saw above, are usually recognized as belonging to the corpus of mathematics in so far as the mathematicians find them interesting and intriguing, and not because they are 'suggested' by nature.

The second reply takes issue with the ambiguity of the idea that a certain structure is 'suggested' by nature. What this meant was clarified above in the form of conditions (a) and (b). Now, as is evident, many modern mathematical concepts and structures are the results of various kinds of generalizations and modifications of more basic concepts and structures, and this is just the normal course of mathematical development. If one grants that these basic concepts are directly reflected in nature (and thus one agrees that they were 'suggested' to the mathematicians in

---

[13] As Kline (172, p. 1028) describes: "the Greeks, Descartes, Newton, Euler, and many others believed mathematics to be the accurate description of real phenomena (…) [T]hey regarded their work as the uncovering of the mathematical design of the universe."

[14] A recent author seemingly embracing this idea is Plantinga (2011, pp. 284–91).

[15] See Wigner (1960, p. 2): "Furthermore, whereas it is unquestionably true that the concepts of elementary mathematics and particularly elementary geometry were formulated to describe entities which are directly suggested by the actual world, the same does not seem to be true of the more advanced concepts, in particular the concepts which play such an important role in physics."

this way), does it follow that the later modified/generalized concepts are *also* suggested by nature, via some kind of transitivity? Does it follow that if concept C* is a generalization/modification of concept C, and C is suggested by nature, then C* is also suggested by nature?

This reasoning is dubious; it is clear that one can modify and generalize a basic concept or structure in a multitude of ways, and yet, in perfect accordance with Wigner's position, the only generalizations/modifications that survive as mathematically viable are the ones which are regarded as 'interesting' enough to fascinate the mathematicians to further study them. So, although one can perhaps trace *all* modern mathematical structures to some 'natural structure', it is simply incorrect to maintain that this kind of transitivity supports the idea that modern mathematical concepts and structures are also suggested by nature.[16]

This fallacy is worth discussing in some detail, as I find it committed by the historian Kline and, following him, by the philosopher Maddy; see below[17]. In commenting on the modern developments of Group Theory, Kline points out (correctly) that the origin of this theory is in the attempts to solve polynomial equations, which he takes to be (correctly, again) "so basic a problem" (1980, p. 294), in the sense that solving equations which directly represent physical situations is an activity directly linked to ('suggested' by) the physical world. But one can accept this idea, and yet object to Kline's point (1980, pp. 293–4) that the more advanced concepts introduced in this theory much later (continuous symmetries, Lie algebras, group representations, etc.) *also* share this feature. In fact, they have nothing to do with 'nature' anymore. Unlike their ancestors, these concepts have been introduced for their aesthetical properties, as part of a mature and sophisticated mathematical theory.

Before making this point about group theory, Kline claims the same about Riemann's work on geometry. He says the following:

> The pure mathematicians often cite the work of Riemann, who generalized on the non-Euclidean geometry known in his time and introduced a large variety of non-Euclidean geometries, now known as Riemannian geometries. Here, too, the pure mathematicians contend that Riemann created his geometries merely to see what could be done. Their account is false. The efforts of mathematicians to put beyond question the physical soundness of Euclidean geometry culminated, as we have just noted, in the creation of non-Euclidean geometry which proved as useful in representing the properties of physical space as Euclidean geometry was. This unexpected fact gave raise to the question, since these two geometries differ, what are we really sure is true about physical space? This question was Riemann's explicit point of departure and in answering it in his paper of 1854 (Chapter IV) he created more general geometries. In view of our limited physical knowledge, these could be as useful in representing physical space as Euclidean geometry. In fact, Riemann foresaw that space and matter would have to be considered together. Is it to be wondered then that Einstein found Riemannian geometry useful? Riemann's foresight

[16] I discuss a different kind of transitivity in my (2012, Chap. 7).

[17] Ivor Grattan-Guiness reasons along the same fallacious line: "Much mathematics, *at all levels*, was brought into being by worldly demands, so that its frequent effectiveness there is not so surprising." (2008, p. 8; my emphasis).

> concerning the relevance of this geometry does not detract from the ingenious use which Einstein made of it; its suitability was the consequence of work on the most fundamental physical problem which mathematicians have ever tackled, the nature of physical space. (1980, p. 293)

But in a different work Kline himself contradicts this view:

> Bolyai, Lobatchevsky, and Riemann. It is true that in undertaking their research these audacious intellects had in mind only the logical problem of investigating the consequences of a new parallel axiom. (1964, p. 429)

So, it is after all unclear what Riemann "had in mind" when working on his new geometries: "only" the attempt to play with the mathematical possibilities,[18] or, as we were told above, his intention was in essence to solve "a fundamental physical problem", to find out "the nature of physical space".

I find the 'Riemann-qua-physicist' picture much less convincing than the 'Riemann-qua-pure-mathematician' picture. On reflection, bringing the former in discussion is perhaps the result of confusing two aspects of his work. One aspect has to do with understanding what he actually *did*. The question to ask here is: did Riemann's work consist in taking an element of physical reality (or an aspect of a physical theory of his time) and trying to describe it mathematically? Were his innovations 'suggested by the natural structures discovered in the flux of things'? Recalling clauses (a) and (b) above, the answer has to be 'no': there were no such things (i.e., differentiable manifolds) to describe in the physics of his time, let alone identified in nature, hence he couldn't have received any 'suggestion' from these two sources. (Slightly more precisely, what he was doing was to work out a mathematically profound generalization of the very idea of space.[19] This led to the notion of a differentiable manifold, and further, as part of the package, to a generalized notion of distance, together with a 'Pythagorean' theorem for such manifolds.) Physical-perceptual, tri-dimensional space provided of course the initial inspiration, and the object of description, for traditional geometry; nevertheless, as we saw, it just doesn't follow that devising ways to generalize it are also inspired or suggested by 'nature'.

The second aspect relevant here is what Riemann perhaps *hoped*, or desired, to achieve in his work—and this is an entirely different matter from what he actually did. It is perhaps true that Riemann hoped, even expected, that maybe one of the alternatives he was thinking up will be proven, as a matter of empirical fact, to be a description of the real, physical space (which, as we know, did happen in Einstein's work on General Relativity in 1916).

[18]In the same passage quoted above from (1964, p. 429), Kline calls this kind of work "an ingenious bit of mathematical hocus-pocus".

[19]In fact, the second passage in his 1854 masterpiece 'On the Hypotheses Which Lie At The Bases Of Geometry' contains the generalization point: "It will follow from this that a multiply extended magnitude is capable of different measure-relations, and consequently that space is only a particular case of a triply extended magnitude." (Riemann 1854; reprinted in Hawking 2007, pp. 1031–2; translated by W.K. Clifford).

With this distinction in place, talking about Riemann's "motivation" [as Maddy does (2007, p. 337)][20] is prone to perpetuate the conflation of the two aspects mentioned above. On one hand, we can of course assume that Riemann's 'motivation'—understood as *hope*—was to contribute to the progress of science by making available models of possible physical spaces. On the other, if 'motivation' refers to what actually led him to the manifold concept, we can be sure that he was *not* following some suggestion from 'nature'. He certainly couldn't have been in the business of taking cues from an extra-mathematical source ('nature', or physics) and writing down a mathematical formalism encoding them. To stress, what led him to introduce a new battery of concepts was the attempt to generalize, unify, etc.—and these are exactly aesthetical elements (recall, in a broad understanding of 'aesthetics').

Before we move on to S4, it is important to address a type of reaction very similar in spirit to the S3. Many are tempted to embrace a line of thinking of the *common cause* type, where the common cause is not 'nature' per se (as above), but the structural similarities, or symmetries holding both in nature and in the mathematical domain. Simplifying, what one often hears is this: 'Scientists study symmetries (structures, patterns) occurring in nature, and similarly, mathematicians are often fascinated by symmetries (structures, patterns) at an abstract level; thus, given this common basis, there is no surprise that a (temporal) harmonious correlation exist.'

To reply, one must repeat the idea that aside from some very basic symmetries (patterns and structures) studied in mathematics because they indeed pop up everywhere in the physical domain, the kinds of symmetries and structures making up modern mathematics are *invented* by the mathematicians. They are not imposed on them by 'nature'; they are selected (among the many available structures they invent) to be studied and developed because they are found aesthetically pleasing. A paraphrase of Dirac's point above illustrates the gist of this reply: the problem doesn't go away just because there is this common ground (symmetries, structures) between mathematics and physics:

> One may describe this situation by saying that the mathematician plays a game in which he himself invents the <u>symmetries / structures</u> while the physicist plays a game in which the <u>symmetries / structures</u> are provided by Nature, but as time goes on it becomes increasingly evident that the <u>symmetries / structures</u> which the mathematician finds interesting are the same as those <u>symmetries / structures</u> which Nature has chosen. (1939, p. 124)

Let us investigate the fourth solution S4 now. With it, we enter the region of less discussed objections, in part because these have been articulated rather recently. The essence of this solution is captured by the memorable words of Wilczek (2006a, p. 8): "One way to succeed at archery is to draw a bull's-eye around the place your arrow lands." The premise of W_A under attack here is the second one.

[20]Maddy (2007, p. 337) accepts the (problematic) Kline picture, backing it up with a quote from Kline himself (1968, p. 234): "So Riemann's motivation was not 'purely aesthetic' or in any sense 'purely mathematical'; he was concerned, rather, with the needs of physical geometry and his efforts were successful."

The critic points out that it is not unreasonable that modern mathematical concepts find uses in physics, since the applicability of mathematics relies heavily on *modeling*. That is, scientists don't apply mathematical concepts directly to nature, but they 'prepare' the physical systems first—they idealize, abstract, simplify them, etc.—precisely *in order to apply* the concepts they have available: mathematics typically applies to a *model* of the system. Thus, to adapt a well-known proverb: 'if all you have is a hammer, then turn everything into a nail'.[21]

This idea has been advanced by several authors, both philosophers and scientists. Among philosophers, Maddy (2007) and French (2000) argue for a similar view, admittedly troublesome for the Wignerian perspective. And yet one may insist that not all worries have been removed. For, on this picture, what we do is take a collection of aesthetically-generated concepts and model the physical reality to fit them. In doing so, we are successful quite often and—here is the crucial point in the rebuttal—one wonders, how could this happen? In this context it is instructive to reflect on what Jakob T. Schwartz (of the 'Ultracomputer' fame) writes:

> Mathematics (…) is able to deal successfully only with the simplest of situations, more precisely, with a complex situation only to the extent that *rare good fortune* makes this complex situation hinge upon a few dominant simple factors. (1986, pp. 21–2; emphasis added)

Is this 'good fortune' rare? The Wignerian demurs, and replies that in fact *many*[22] natural processes and phenomena "hinge upon a few dominant simple factors ", and thus remain meaningful after all the simplifications, idealizations, omissions are operated on them. So, why isn't it the case, one insists, that the opposite happens on a regular basis, namely that once we model and make these adjustments in order to apply mathematics, what we get in the end is so rigid and empty that a mathematical description, even correct, would be useless or meaningless? Looked at it from this angle, the Universe does seem 'user friendly' after all. So, to return to the proverb above, at the important junctures in science the scientists *often* tried to 'make' nails—and they succeeded. And, if they did, this means (the Wignerian insists) that this very possibility was somehow present there, and has to be accounted for.

In closing the discussion of S4, I should mention a variation on this theme by Wilczek (2006, p. 8), who writes:

> Part of the explanation for the success of mathematics in natural science is that we select what we regard as the scientifically interesting aspects of nature largely for their ability to allow mathematical treatment.

---

[21]Note that such an objector has no troubles to accept the first premise of W_A, that these concepts are in the mathematical corpus because they are interesting and intriguing; if this objection from preparation and modeling is viable, the origin of concepts is just irrelevant.

[22]Recall that 'many' is relative; it means, 'many *among the truly important ones*', since these are the ones that matter, as we remember from the discussion of premise 3 above.

That such a selection strategy[23] is in use in science sounds like a factual claim, and thus it is open to investigation (we can perhaps run a survey among scientists?). However, even before the results are in, the proposal strikes me as an exaggeration. To one who says that "[scientists] select what [they] regard as the scientifically interesting aspects of nature largely for their ability to allow mathematical treatment", a Wignerian is tempted to reply that although there may be cases like these on record (and Wilczek mentions one, the behavior of ultrapure semiconductor hetero-junctions subject to ultra-strong magnetic fields at ultralow temperatures), it is extremely hard to believe that this is a fundamental, and widely accepted, rule of the game in science. Moreover, when one recalls the standard examples of 'aspects of nature' to motivate the puzzle—as mentioned above: General Relativity, Quantum Mechanics, the characterization and classification of elementary particles in Quantum Field Theory—one remarks that *none* of these fit the selection strategy idea. Gravitation or the observed invariances holding among elementary particles were surely *not* "regard[ed] as scientifically interesting aspects of nature largely for their ability to allow mathematical treatment." On the contrary, it seems pretty clear that they were considered scientifically interesting *independently* of the existence of such a treatment.

The fifth solution S5 is built around the idea of what can be called *over-determination*. Now one rejects premise 2: there is nothing 'unreasonable' about the fact that the aesthetically-generated concepts and structures find a home in physics simply because there is a lot of them. There is a lot of mathematics to choose from when one looks for embedding a physical idea, and this quantitative fact alone solves the problem generated by the existence of the anticipatory coincidences.[24] To adapt Wilczek's archery metaphor, there is no mystery in the fact that when *many* arrows are shot, they'll eventually hit even a very small target.

The over-determination idea is admittedly very powerful, and yet can be doubted. To exploit the archery metaphor further, it is true that when many arrows are shot, they'll eventually hit even a very small target—but only if they are shot in the right direction, that is, in the direction of the target! How would that translate in less metaphorical terms: if the concepts are aesthetically-driven indeed, the fact that there are so many available to choose from doesn't really affect Wigner's overall point. That the 'arrows' (the subjective, aesthetically-selected concepts) are all shot in a direction other than the 'target' (the objective, careless world) surely doesn't make it more likely that the 'target' will eventually get hit *even if* there are very many of them.

---

[23]See also Maddy (2007, p. 339): "As a mathematical analog, I suggest that we tend to notice those phenomena we have the tools to describe. There's a saying: when all you've got is a hammer, everything looks like a nail. I propose a variant: if all you've got is a hammer, you tend to notice the nails."

[24]Maddy (2007, p. 341) puts it as follows: "With the vast warehouses of mathematics so generously stocked, it's perhaps less surprising that a bit of ready-made theory can sometimes be pulled down from the shelf and effectively applied."

Finally, we should now examine closely the sixth solution S6. Its key-insight is somewhat similar to S3, but its proponents develop the argument in a different manner. Supposing that one accepts that the modern mathematical concepts are generated and selected on the basis of our aesthetic sense (i.e., premise 1), Wigner's conclusion still doesn't follow since our aesthetic sense itself is a result of evolution, and thus shaped by, and sensitive to, our environment—i.e., to the 'natural structures' around us. Hence it enjoys some sort of objectivity, due to its origin; therefore the contrast subjective v. objective underlying the puzzle doesn't hold.[25] As I said, this solution re-iterates S3, but in a subtler way: it accepts the first premise at a superficial level, but rejects it at a deeper level.

Still, the Wignerian remains unconvinced. There is no doubt that evolutionary pressure plays an immensely important explanatory role in a variety of areas. Yet the explanation proposed here is hopelessly sketchy. On one hand, it's perhaps not hard to see how preferences for certain types of mating partners (the muscular and faster specimens, the more vividly colored ones, etc.) may be interpreted as reflecting what living creatures (humans included) take to be aesthetically pleasing. But, on the other hand, even if such a reduction of the aesthetical to the evolutionary advantageous is accepted (and it's by no means clear that it should be!), one still has a long way to go until one demonstrates that the aesthetical criteria *involved in shaping modern mathematics* are also subject to the same kind of reduction. When mathematicians talk in terms of beauty, they have in mind a highly formal, and abstract, type of beauty—not the 'corporeal', or mundane beauty (supposedly) efficacious in natural selection. The relevant question then becomes how exactly can the environment, and evolutionary pressure more generally, shape this formal/abstract beauty; this, the Wignerian urges, is a yet unanswered question. Moreover, given how counterintuitive the suggestion is after all, it is fair to say that the burden of proof (rather: answer) is on the proponent of this kind of solution.

5 A Sketch of yet Another Solution

At the end of the examination of the first solution S1 (the only one disputing premise 3), I concluded that there are ways to read this premise that would make it plausible. If all that matters is how we count the scientific episodes that vindicate Wigner, then we can also count in his favor; this is the 'qualitative' perspective I introduced above. The solution I'll be sketching now assumes this qualitative perspective, and still attacks this third premise, but in a more radical fashion. What I'll discuss here is not the number of successes of applicability [aspect (α) above], but the ratio of successes to failures [aspect (β)].

[25] I take Pincock (2012, pp. 184–5) to advance this line of thought: "Even an argument based on natural selection seems imaginable according to which our tendency to make aesthetic judgments is an adaptation precisely because these judgments track objective features of our environment."

The insight behind this solution is of an 'ecological' nature, i.e., it has to do with the way ideas 'survive' in the scientific 'environment'. The scenario I envisage is quite simple. Imagine a physicist—call him *Neinstein*—thinking up a novel physical theory at time *t*. Assume further that his idea is bold, and a candidate to belong among the several (yet, recall, not that many in absolute number) major scientific achievements one typically mentions in connection to the qualitative approach to the Wigner's problem. Now, it is a fact that such a scientist *has to* embed his insight into a mathematical formalism. I take it to be beyond doubt (again, as a matter of sociological/professional scientific fact) that without such an embedding, i.e., without the ability to write down the theory's central mathematical equations, the theory is extremely unlikely to draw any interest from the scientific community. Neinstein's theory—again, left in the form of a vision, or deep insight into the nature of things—might of course float around for a while, in the heads of other fellow scientists, but until it comes packed in a mathematical formalism, very few (if any) will be ready to take it seriously as more than mere speculation. In other words, the un-mathematized insight will not survive, just like organisms and species don't survive in uninhabitable environments. So, on this picture, what would have happened with a Neinstein proposing General Relativity before Riemann? Bluntly put, we would have never perhaps heard of him and his theory (or, as one might note, we shouldn't even call it a 'theory' in the first place, but stick with the initial label and call it a 'vision', 'revelation', 'insight', 'speculation', etc.)

In fact, one doesn't even need to make the assumption that our physicist's idea is a major one: *any* idea in physics, no matter how trivial, needs mathematical embedding. Yet, just to stay within the confines of the above qualitative interpretation of Wigner's point, let's assume we focus only on 'major' insights here. Thus, the thought behind this seventh solution is that *it is guaranteed* that for any case of a major idea in physics (developed at a certain time *t*), the scientist(s) proposing it *will have found* a mathematical formalism available to embed it, and thus express it as a proper scientific theory (where the formalism was of course developed, at least in part, at an earlier time *t′*). As intimated above, the reason for this is immediate: were this *not* the case, that idea would not have been recognized (as a valid scientific contribution); it would not have survived, and there would have been no theory to talk about today in the first place. Thus, there is no anticipation, no pre-adaption, no pre-established harmony, no miracle—only a filtering ('ecological') effect operating in the scientific environment. To stress the point above, if the mathematics had *not* been available when needed, such an idea/theory would most likely have been lost, perhaps forever—and thus the Wignerians could not have counted it (nor Neinstein) among the examples of (major) achievements/achievers.[26]

---

[26]Sometimes the physicists themselves try to develop the mathematics they need, but usually aren't successful. Here is the story of Gell-Mann in Steiner (1998, p. 93), relying on Doncel et al. (1987, p. 489): "[In trying to generalize the Yang-Mills equations] [w]hat Gell-Mann did without knowing was to characterize isospin rotations as a 'Lie Algebra', a concept reinvented for the occasion, but known to mathematicians since the nineteenth century. He then (by trial and error) began looking for Lie Algebras extending isospin—unaware that the problem had already been

To return to Weinberg's rendering of the Wigner problem, we now see that there *can't* be any cases of (major) achievements in physics in which the mathematician hasn't been 'there' before. The very fact that there is an achievement to talk about (i.e., recognized as such) is already a guarantee that there was a mathematician 'there' first. To begin by presenting a number of examples of achievements and then wonder how could it be that a mathematician was 'there' first is like wondering how could it be that all the people we find in a hospital are sick.

This new criticism[27] against the third premise amounts to maintaining that the quantitative comparison implicit in the third premise (even when read along the qualitative lines I proposed above) may actually be *unintelligible*. What we should be able to estimate is the number of the cases in which important physical ideas were advanced but *no* mathematical embedding for them was available—and then compare it to the number of successful cases (which, again, we assume we can list). Then, the argument goes, we have something to worry about only if the later number is much larger than the former (given premise 1, the aesthetical origins of mathematical concepts, and 2, the assumption of anti-anthropocentrism.) However, as I hope it is now clear, when it comes to this relevant ratio, we are able to (roughly) estimate only one number (the successes), but no way (even in principle) to estimate the other (the failures).

Acknowledgements Thanks are due to the stimulating audience of the *Models and Inferences in Science* conference, in particular to the editors of this volume, Emiliano Ippoliti, Fabio Sterpetti, and Thomas Nickles. The responsibility for the final form of the text is entirely mine.

References

Azzouni, J.: Applying mathematics: an attempt to design a philosophical problem. Monist **83**(2), 209–227 (2000)

Bangu, S.: Steiner on the applicability of mathematics and naturalism. Philosophia Math. (3) **14**(1), 26–43 (2006)

Bangu, S.: Wigner's puzzle for mathematical naturalism. Int. Stud. Philos. Sci. **23**(3), 245–263 (2009)

Bangu, S.: The Applicability of Mathematics in Science: Indispensability and Ontology. Palgrave Macmillan, London (2012)

Bourbaki, N.: The architecture of mathematics. Am. Math. Monthly **57**, 221–232 (1950)

Dirac, P.A.M.: The relation between mathematics and physics. In: Proceedings of the Royal Society (Edinburgh) (James Scott Prize Lecture), vol. 59, pp. 122–129 (1939)

(Footnote 26 continued)

solved by the mathematicians—but failed, not realizing that the first solution required eight components."

[27] A point distantly related to the present one is that there are major scientific achievements (the theory of evolution, and other work in biology) in which mathematics doesn't play any role (Wilczek 2007; Sarukkai 2005). However, Wigner's problem centers on physics (despite the general title of his paper).

Doncel, M., et al.: Symmetries in Physics (1600–1980). Servei de Publicacions, Universitat Autonoma de Barcelona, Barcelona (1987)
Dyson, F.J.: Mathematics in the physical sciences. In: The Mathematical Sciences (ed.) Committee on Support of Research in the Mathematical Sciences (COSRIMS) of the National Research Council, pp. 97–115. MIT Press, Cambridge (1969)
Dyson, F.: 'Paul A. M. Dirac' *American Philosophical Society* Year Book 1986 (1986)
French, S.: The reasonable effectiveness of mathematics: partial structures and the application of group theory to physics. Synthese **125**, 103–120 (2000)
Ginammi, M.: Structure and Applicability. An Analysis of the Problem of the Applicability of Mathematics. PhD Dissertation, Scuola Normale Superiore, Pisa (2014)
Grattan-Guiness, I.: Solving Wigner's mystery: the reasonable (though perhaps limited) effectiveness of' mathematics in the natural sciences. Math. Intelligencer **30**(3), 7–17 (2008)
Hamming, R.: The unreasonable effectiveness of mathematics. Am. Math. Monthly **87**(2), 81–90 (1980)
Hawking, S. (ed.): God Created the Integers. Running Press, Philadelphia & London, The Mathematical Breakthroughs that Changed History (2007)
Kline, M.: Mathematics in Western Culture. Oxford University Press, New York (1964)
Kline, M.: Mathematics in the Modern World. W. H. Freeman and Company, San Francisco (1968)
Kline, M.: Mathematical Thought from Ancient to Modern Times. Oxford University Press, New York (1972). (Vol. 3)
Kline, M.: Mathematics: The Loss of Certainty. Oxford University Press, New York (1980)
Maddy, P.: Second Philosophy. Oxford University Press, New York (2007)
Nagel, E.: 'Impossible Numbers' in Teleology Revisited. Columbia University Press, New York (1979)
Pincock, C.: Mathematics and Scientific Representation. Oxford University Press, New York (2012)
Plantinga, A.: Where the Conflict Really Lies: Science, Religion, and Naturalism. Oxford University Press, New York (2011)
Riemann, B.: On the Hypotheses Which Lie At The Bases Of Geometry. Reprinted in Hawking (2007) (1854)
Sarukkai, S.: Revisiting the 'unreasonable effectiveness' of mathematics. Curr. Sci. **88**(3), 415–423 (2005)
Schwartz, J.T.: The pernicious influence of mathematics on science. In: Kac, M., Rota, G.C., Schwartz, J.T. (eds.) Discrete Thoughts. Essays on Mathematics, Science and Philosophy. Birkhauser, Boston (1986)
Steiner, M.: The Applicability of Mathematics as a Philosophical Problem. Harvard University Press, Cambridge (1998)
Von Neumann, J.: The Mathematician. In: Newman, J.R. (ed.) The World of Mathematics, vol. 4, pp. 2053–2063. George Allen and Unwin, London (1961)
Weinberg, S.: Lecture on the applicability of mathematics. Not. Am. Math. Soc. **33**, 725–733 (1986)
Weinberg, S.: Dreams of a Final Theory. Vintage, London (1993)
Wigner, E.: The unreasonable effectiveness of mathematics in the natural sciences. Commun. Pure Appl. Math. **13**(1), 1–14 (1960)
Wilczek, F.: 'Reasonably effective: I. Deconstructing a Miracle', Physics Today, pp. 8–9 (2006)
Wilczek, F.: 'Reasonably effective: II. Devil's advocate', Physics Today, pp. 8–9 (2007)