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THE APPLICABILITY OF MATHEMATICS AS AN EPISTEMIC PROBLEM

Master's Thesis in Philosophy

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Number of Characters: 114 785

Tartu 2026

Table of Contents

Introduction

The success of mathematics within the natural sciences is unquestionable. Mathematics makes possible the precise formulation of our best physical theories, facilitates the drawing of inferences and making of predictions and sometimes unifies disparate physical phenomena or leads to the discovery of new ones. Examples of these abound. The physicist's description of a falling body is given by a function specifying its position over time. To predict when it will reach the ground, one sets the function to zero and solves for the time. The function derives from Newton's law of universal gravitation, which brought about the unification of terrestrial and celestial mechanics. An analogy with the mathematical form of Newton's theory led another physicist, James Clerk Maxwell, to a reformulation of the equations of electromagnetics and thus to the discovery of electromagnetic radiation.

In short, mathematics is applicable and its applications have been immensely important for physics. Philosophically, however, there is a puzzle here. Mathematics and empirical science differ along multiple dimensions. The subject matter of mathematics is taken to be abstract, acausal and not spatiotemporal, while the subject matter of empirical science is taken to be concrete, causal and spatiotemporal. Mathematics proceeds deductively and makes use of proofs; empirical science relies on experiment and observation. Mathematical knowledge has been the prime example of *a priori* knowledge, justified by proof, while scientific knowledge is justified by experience and thus *a posteriori*. There are differences in the norms and practices of each field; what makes for good mathematics is different from what makes for good physics. Given all of this, it is not immediately clear how these differences are overcome when mathematics is applied in empirical science.

This puzzle—why is mathematics applicable in empirical science, given all of their differences—has two names in the literature: the problem of the applicability of mathematics and Wigner's problem.[1] Eugene Wigner, a physicist, published an article in 1960 titled *The Unreasonable Effectiveness of Mathematics in the Natural Sciences*.[2] As the name suggests, Wigner presented the applicability of mathematics as something lacking rational explanation. Central to his discussion are the ineliminable role of aesthetic considerations in mathematics and the applicability of mathematical concepts (rather than theorems, for example). But we do not have to follow Wigner in taking on these assumptions and not all

[1] Throughout the thesis, I will use these names interchangeably.

[2] Wigner's paper is generally taken as the starting point of this debate, but questions about the relationship between mathematics and the empirical world can be found in many earlier thinkers, the earliest probably being the Pythagoreans.

authors have. Different assumptions about mathematics, empirical science and how the former is applied in the latter lead to different versions of the problem.

This is the topic of the thesis at hand. The problem of applicability is really a collection of problems[3] that rely on different assumptions and thus require different treatments. However, as I will argue, not all such assumptions hold up to scrutiny and manage to present a genuine problem. The aim of this thesis is to narrow down the playing field and to arrive at a version of the problem that is general enough to subsume at least some of this abundance. As such, the goal is largely methodological, the guiding question is *how should the problem of applicability be formulated?* The focus of the thesis will be on how the problems of applicability have been presented in the literature. I am, with one exception, not interested in proposed solutions to the problem and will not provide one of my own. The version of the problem I arrive at is this: given that the justification of mathematical statements is not defeasible by empirical evidence, why can they transmit justification to empirical conclusions?

Given the scope of the topic, some limits are necessary to keep the scope of the thesis at bay. First, I approach the topic from the side of the philosophy of mathematics, rather than the philosophy of science or physics. The problem of applicability cuts across such lines, but once I have reached a version of the problem that is clearly in the domain of the latter, I leave it there. Second, while I aim for my treatment of applicability to hold for all applications of mathematics, the emphasis here is on applicability in everyday contexts (the mathematics one does at the checkout counter) and physics.[4] Third, I am not particularly interested in the realism-antirealism debate in mathematics (or empirical science) and I aim for my discussion to be independent of it. Fourth, I take on one assumption for the thesis: we have mathematical knowledge and mathematics is primarily in the business of furthering it.[5]

One clarification before starting: the measure of the 'effectiveness' of applications of mathematics is generally taken to be empirical adequacy. Mathematics is effective when the results we get from applying it match empirical observations. The function describing a falling body is effective, when the time it predicts the body will reach the ground matches the time the body is measured to reach the ground at.

[3] By one count (Bărboianu 2020, 449), there are 48 distinct versions.

[4] See, for example, (Lesk 2000; Garte et al. 2025) for the applicability of mathematics in biology and (Velupillai 2005) for economics.

[5] This is not to be read as a commitment to the existence of mathematical objects as a prerequisite for the truth of mathematical statements. If one is so nominalistically inclined, read my claims relating to mathematical knowledge as "knowledge about what holds in the story of mathematics" or some other suitable alternative.

The thesis proceeds in three parts. Part I describes the form a coherent version of the problem ought to take—an argument that leads to a contradiction—and presents four common versions, arguing that each fails. The first emphasises that mathematics has led to physical discoveries and this, on its own, is not enough to reach a contradiction. The second relies on aesthetics playing a constitutive role in mathematics, which I argue is false. The third points to the metaphysical gap between mathematics and empirical science, relying on assumptions that oversimplify the picture of applicability. The fourth derives a contradiction from idealisations, but both its scope and its well-known solution are limited.

Part II concerns the different ways in which mathematics is applied. I consider six (semantic, descriptive and deductive applicability, non-standard applications, unification and explanation) and argue that all except semantic applicability can be understood through deductive applicability: statements of mathematics are used as premises in deductions with empirical conclusions. Descriptive applicability emphasises the peculiarity of mathematical concepts: they allow us to predict empirical phenomena. Prediction is, however, facilitated by deductive applicability. A closer look at an example of a supposedly non-standard use of mathematics—Dirac's prediction of the positron—reveals that mathematics plays only a deductive role, while the physical context determines its physical interpretation and thus the prediction of new objects or phenomena. Unification likewise results from the interplay between deductive applicability and physical interpretation. Mathematical explanations, if accepted, also rely on statements of mathematics.

Part III puts forward a problem of applicability centred on deductive applicability. When so applied, the justification of mathematical statements transmits to the empirical conclusions reached. The justification for these conclusions is empirically defeasible, and since it derives via transmission from the premises, rebutting defeaters for the former are also defeaters for the latter. Yet mathematical statements are widely considered indefeasible by empirical evidence—they do justificatory work without being open to the relevant defeaters. I end by defending the problem from possible counterarguments and examining how some influential philosophies of mathematics respond to it.

Part I

Introduction: Shape of the Problem

Before discussing and evaluating specific versions of the problem of applicability, the shape that a coherent version of the problem ought to have needs to be specified. In the literature, one of two paths is typically taken: the problem as an explanatory challenge or as an argument leading to a contradiction.

Wigner presented the problem as a mystery, the effectiveness of mathematics in empirical science is a fact for which we have no explanation, thus the applicability of mathematics is unreasonable: "the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and … there is no rational explanation for it" (Wigner 1960, 2). Later authors often present the problem as a why-question:

> Why is it that a cognitive practice that is guided by virtues like rigor, elegance, simplicity, manipulability or formal beauty proves exceptionally successful in an area [empirical science] where it is hard to see why these inner-mathematical virtues are epistemically relevant at all? (Islami and Wiltsche 2020, 160)

> The problem is epistemic: why is mathematics, which is developed primarily with aesthetic considerations in mind, so crucial in both the discovery and the statement of our best physical theories? (Colyvan 2001b, 267)

Posed like so, the problem has the form of an explanatory challenge: a combination of claims that, taken together, appear mysterious, striking and in need of an explanation (Waxman 2021). These claims include *assumptions about both mathematics and empirical science* and *facts about the applicability of mathematics*. By the former, I refer to the myriad differences between the two disciplines, e.g. the significant role of aesthetics in mathematics and not in physics. By the latter, I mean the different ways in which mathematics has been effectively applied, e.g. as leading to discovery and allowing theory statement, but also allowing predictions, unification and explanation. Emphasising particular assumptions and facts leads to different versions of the problem.

A formal treatment of the shape of the problem is given by Bărboianu (2020). Using the language of Wigner, he defines an unreasonableness:

> An unreasonableness is a conventional association of two elements I (the epistemic hypothesis) and F (the fact), (I … F), for which the following holds true:

(a) An inferential relation (causally-explanatory, abstract-explanatory or abstract-deductive) or an identity relation linking *I* and *F* has not been established and

(b) Any attempt of establishing such a relation has failed due to contradictory elements of *I* and *F* that cannot stand simultaneously.

The epistemic hypothesis and the fact[6] are composed of the same material: "empirical facts and epistemic facts such as justified beliefs, accepted theories and/or well-established local or universal theories" (Bărboianu 2020, 445). While Bărboianu gives no explicit way to differentiate which empirical and epistemic facts ought to belong to which element, from the examples he discusses it is clear that the epistemic hypothesis is meant to include the assumptions made about mathematics and empirical science, while the fact is meant to include facts about applicability. The core idea of Bărboianu's formalisation is that the epistemic hypothesis and fact are perceived as being in tension, either as leading to a full contradiction or as a strong disconnect, making it hard to see how the two could be linked together. As such, Bărboianu has in effect formalised explanatory challenges, and his account need not be treated separately. Solving a case of unreasonableness, in his scheme, involves rejecting or revising either element, or both, until the tension dissolves.

Alternatively, the problem could be presented as an argument, as in (Steiner 1998, 21):

(1) On the platonist view, physical laws and theories must express relations between mathematical and nonmathematical objects.

(2) Every relation in physics is a causal (or spatiotemporal) relation.

(3) Mathematical objects do not participate in causal (or spatiotemporal) relations.

(4) Therefore, on the platonist view, all physical laws and theories are false.

Assumptions and facts figure as premises and a conclusion that contradicts our other facts, assumptions or commitments is reached. Here, the contradiction is with the fact that physical laws and theories are generally taken to be true. Solving the problem involves rejecting or revising the premises so that the contradictory conclusion no longer follows.

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[6] Bărboianu uses the word 'fact' in an untraditional sense, given that the 'fact' element of his scheme could in principle be revised (i.e. be false). To keep the text concise, I will keep using the phrase 'facts of/about applicability' throughout to refer to the different ways in which mathematics is applied.

The explanatory challenge and argument approaches are not mutually exclusive. The claims that make up an explanatory challenge can be presented as premises and an undesirable conclusion can typically be made to follow. Similarly, the premises of an argument can be presented as claims whose conjunction cries out for explanation. I will follow the argument-style presentation of the problem, since explanatory challenges rely to some degree on intuition. If the source of the tension between the set of claims is not made explicit, one must intuit it and different intuitions are possible. Once the tension has been clearly expressed, turning the set of claims into an argument is straightforward. The distinction between the *assumptions* made about mathematics and empirical science and the *facts* about applicability will, however, remain relevant, as simply stating the facts of applicability is not enough to form a problem of applicability, this will soon be illustrated.

In the literature on Wigner's problem, there is a tendency to combine different assumptions and facts about applicability into one presentation of the problem (for example, the Colyvan quote above mentions both discovery and theory statement as facts of applicability). This is an error for two reasons. First, there is no guarantee that these combinations are exhaustive, nor is any indication typically given for why particular aspects are highlighted. It may well be that many aspects of the problem can be subsumed under others, but this requires demonstrating. Second, there is no guarantee that solutions to different aspects of the problem will coincide.

My aim in this part of the thesis is to consider some of the different assumptions and facts that lead to versions of Wigner's problem in as much isolation as possible. As a result, the versions discussed here are rarely found in exactly the same form in the literature, but I will indicate where the relevant claims appear. This part of the thesis is not meant to offer a comprehensive overview of all versions of Wigner's problem, it is meant to show the shape of the problem in action and to dismiss some common versions of the problem.

Mathematics-Led Discovery

One result of applying mathematics in empirical science is that sometimes it leads to empirical discoveries. For example, physicist James Clerk Maxwell found that the laws of electromagnetic phenomena accepted at the time (Gauss' laws for electricity and magnetism, Faraday's law, Ampère's law), taken together, contradicted electrical charge conservation. Led by mathematical analogy with Newtonian gravitational theory's conservation of mass principle—and no empirical evidence—Maxwell modified Ampère's law by adding the displacement current, so that all the laws taken together (nowadays referred to as Maxwell's equations) implied the conservation of electrical charge. The equations also implied the existence of electromagnetic waves travelling at the speed of light, the existence of which was empirically confirmed by Hertz some years later. (Steiner 1998, 77; Colyvan 2001b, 267–68)

In Maxwell's case, what was discovered was a new phenomenon. It has been argued that new objects have been discovered in a similar manner, for example the positron (Steiner 1998; Bangu 2012) and Neptune (Colyvan 2001a; Bangu 2012). Roughly, the process is this: the mathematical formalism corresponding to some physical system is manipulated to reach some end (e.g. to imply electrical charge conservation) or a new formalism is proposed; this results in parts of the formalism having no known empirical counterparts, yet scientists begin looking for them and sometimes find them. This is the sense in which I will be talking about discoveries.

The fact that mathematics can lead to physical discoveries does not make for a problem of applicability by itself. It is at most one premise of the argument. What we have is a fact about the applicability of mathematics, what is missing is some additional assumption about mathematics or empirical science that would problematise it. Candidates include the aesthetic nature of mathematics (Steiner 1998; Colyvan 2001b) and the metaphysical gap between mathematics and physics (Steiner 1998; Islami and Wiltsche 2020), which will be discussed (and rejected) in the coming sections.

Another way of problematising discovery is by combining it with other facts about applicability and assumptions relating to these. For example, the mathematical formalism involved in discoveries is sometimes developed independently of its eventual application (Bărboianu 2020). This would lead to something like:

(1) Much of mathematics is developed independently of physical applications and thus any relevant physical considerations.
(2) There is no reason why that which is developed independently of any relevant physical considerations should bear on physical findings.
(3) Thus, there is no reason why mathematics should lead to physical discoveries.

Premise (2), an assumption about empirical science, is doing most of the work here. After all, if the mathematics is developed specifically for a certain application (e.g. Fourier analysis and heat conduction), then the fact that it applies is unsurprising. Assume for the moment that the formalism Maxwell used was developed specifically to describe electromagnetic phenomena and still implied the so far unconfirmed existence of electromagnetic waves. In other words, a formalism developed to describe a specific set of phenomena implies the existence of phenomena outside of that set.[7] The empirical confirmation of electromagnetic waves in this situation would be just as surprising (or unsurprising) as it was in the actual situation. Thus, whether the mathematics was developed independently carries no weight in situations where it implies the existence of something new.

A more promising problematisation is this: the fact that mathematics can drive physicists to look for these new objects or phenomena suggests that the mathematics is trusted to know something we do not. It is assumed that since the mathematics points to something, something must be there. Less metaphorically: mathematics is being treated as having a higher epistemic status than the empirical (Bărboianu 2020). Maxwell had no empirical grounds for adding the displacement current into Ampère's law; it had already shown empirical success. Yet once electromagnetic radiation became a "mathematical possibility" (Steiner 1998, 77), it also became a physical possibility worth investigating. What is puzzling is that it is not clear what grounds there are for granting mathematics this privileged position. This assumption will be addressed in part II.

The aim of this section was to illustrate how just stating a fact about applicability—that it has led to physical discovery—is not enough to present the applicability of mathematics as problematic. What is missing is an assumption about either mathematics or empirical science that problematises the fact. I dismissed one candidate (that what has been developed independently of physical considerations should not bear on physical findings) and presented a more promising alternative (that physicists have no clear grounds for granting mathematics a higher epistemic status). The next two sections consider and dismiss two further alternatives: the role of aesthetics in mathematics and the metaphysical gap between mathematics and physics.

[7] Such a situation might result from the mathematical formalism needing to be modified in order to fit some mathematical standard (e.g. rigor) or being developed further as mathematics.

The Role of Aesthetics in Mathematics

Much of modern discussion around the applicability of mathematics takes as its starting point Wigner's *The Unreasonable Effectiveness of Mathematics in the Natural Sciences* (1960). Wigner describes mathematics in broadly formalist terms: it "is the science of skillful operations with concepts and rules invented just for this purpose" (ibid., 2). For applicability, Wigner is most interested in how the higher flights of mathematicians' fancy find their way into physics, how concepts "so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty" (ibid., 3) prove effective in application.

Although Wigner goes on to discuss at much greater length whether there is not something about modern physics that makes it amenable to mathematisation, his description of mathematics is what later authors have emphasised. Since my focus is on how Wigner's problem has been presented and discussed in the literature, I will continue with this widespread interpretation.[8] In this section, I will argue that the central assumption of this version of the problem—that aesthetics play a constitutive role in mathematics—is untenable.

Wigner's presentation rests on the claim that mathematics is based on aesthetic considerations to a significant degree, also found in (Colyvan 2001b; Islami and Wiltsche 2020; Waxman 2021). His argument was summarised and popularised by Mark Steiner (1998, 46), in slightly modified form:

(1) Mathematical concepts arise from the aesthetic impulse in humans.
(2) There is no reason why that which arises from the aesthetic impulse in humans should be effective in physics.
(3) Hence, there is no reason why mathematical concepts should be effective in physics.

What does it mean for mathematical concepts to "arise from the aesthetic impulse in humans"? For Steiner, aesthetics play a *constitutive* role in mathematics (Waxman 2021). Aesthetic criteria are used to decide whether something is or is not mathematics: "concepts are selected as mathematical because they foster beautiful theorems and beautiful theories"

[8] See Islami (2016) for an alternative interpretation.

(Steiner 1998, 64). Theorems of chess,[9] for example, fall outside of mathematics, because they lack the relevant aesthetic properties.[10]

Proponents of this version of the problem are in effect forced to give aesthetics such a significant role in mathematics. Any lesser role could reasonably be attributed to aesthetics in empirical science as well, dissolving the distinction this version of the problem relies on—mathematics is influenced by aesthetic considerations where empirical science is not, thus mathematics should not be effective in physics. Two such lesser roles are, courtesy of Waxman (2021), a normative role (aesthetic criteria distinguish good from bad mathematics[11]) and a developmental role (aesthetic criteria explain how mathematics has developed historically, why mathematicians have concentrated their efforts on some areas of mathematics and not others).[12]

Both are perfectly plausible roles for aesthetics in empirical science. Scientists readily claim that aesthetics matter in their work. Paul Dirac went as far as to say that "it is more important to have beauty in one's equations than to have them fit experiment" (1963, 47) and Henri Poincaré names beauty as the main motivation for doing science:

> The scientist does not study nature because it is useful to do so. He studies it because he takes pleasure in it, and he takes pleasure in it because it is beautiful. (Poincaré 2003, 22)

Philosophers of science have long acknowledged that aesthetic and other non-epistemic factors play a role in science (e.g. Ivanova 2017; Reiss and Sprenger 2020). Whether aesthetics could play a constitutive role in empirical science is more suspect. The idea that our scientific ontology is decided upon based on considerations of beauty and elegance would require serious convincing, given that science is typically taken to describe a mind-independent reality. Granting aesthetics a constitutive role would make the content of our physical theories dependent on human standards of beauty rather than empirical evidence. Generally, proponents of this version of the problem take the assumption that aesthetics are not constitutive in empirical science as a given, I will not contest this.

In short, to make this version of Wigner's problem stand, we must accept that aesthetic considerations play a constitutive role in mathematics. Presumably, these aesthetic

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[9] For example, that checkmate cannot be reached if one player has only a king and a knight and the other only a king.

[10] G. H. Hardy, for example, draws the distinction by pointing to the "seriousness" that mathematics has and chess lacks (Hardy and Snow 2019). Steiner appears to be content with just their difference in beauty.

[11] Where the difference is in more than just correct and incorrect mathematics.

[12] Waxman does not give this second role a shorthand name, 'developmental' is my invention.

considerations regarding what is and is not mathematics are cashed out in aesthetic judgements about particular pieces of mathematics. Whether or not mathematics is a proper object of aesthetic appreciation is a contested issue (e.g. Todd 2008; Barker 2009; Rieger 2018; Starikova 2018; Schellekens 2022), given that mathematics does not straightforwardly fit the conditions commonly taken to be necessary for aesthetic judgements (mainly, direct sensory contact and groundedness in disinterested pleasure[13]). But, granting genuine aesthetic judgements about mathematics, the claim that aesthetic judgements play a *constitutive* role in mathematics is still suspect. The considerations that constitute what is and is not mathematics are meant to serve the epistemic aims of mathematics, i.e. they are supposed to further our mathematical knowledge. Given that mathematics is primarily in the business of knowledge production, these considerations cannot be merely terminological, in that they are used to decide what is called mathematics and what is not,[14] but they are expected to guide mathematical inquiry toward truth. For aesthetic considerations to play such a role, they would need to be connected, in a significant way, to the truth or justification of mathematical claims.

There is, however, evidence that for mathematics, positive aesthetic judgements do not track its epistemic aims. Giaquinto (2016) argues that beauty and explanatoriness can come apart for mathematical proofs: there are explanatory proofs which are not beautiful and beautiful proofs which are not explanatory. Proofs by contradiction tend to fall into the first category, Giaquinto brings the example of Erdős' proof for the divergence of $\sum_n {}^1/_{p_n}$ (where p_n are primes), while elementary proofs tend to be explanatory and not beautiful. Inglis and Aberdein (2015) report experimental support for the claim that epistemic and aesthetic judgements in mathematics may sometimes overlap but are largely independent of each other.

Additionally, when aesthetic and epistemic considerations come apart, aesthetics judgements appear to come after epistemic ones. Barker shows that the elegance of proofs cannot be considered analogous to dependent beauty, because while "a dependently beautiful object can be dependently beautiful even if it wholly fails to fulfill its purpose", in the case of proofs "we cannot imagine a proof that wholly fails in its purpose but which is nonetheless

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[13] Whether these conditions are truly necessary for aesthetic judgements is also contested (e.g. Carroll 2006; Schellekens 2022).

[14] Further, if aesthetic considerations were simply terminological, then it is hard to see how a version of Wigner's problem would follow. It would need to involve a claim like "a discipline whose boundaries are decided by aesthetic criteria should not be effective in physics" and the significance of the disconnect between field-demarcation and effective applicability would need to be argued for.

elegant" (2009, 67). Todd (2008) raises a related point: we can readily think of proofs that are correct but ugly, yet a proof that is incorrect and beautiful is hard to imagine, indeed it would not strictly even be a proof. In other words, we tend to make aesthetic judgements after the correctness of a proof has been established. Todd also notes that mathematicians' aesthetic claims can often be reworded so as to be epistemic, making it seem like aesthetic language is simply masking epistemic judgements. This observation is not enough to justify the claim that aesthetic judgements are *always* secondary to epistemic ones, but it suggests that genuine aesthetic judgements may not be as widespread as Steiner and others assume.

For concepts, the case is similar. There are concepts that are considered beautiful and yet are not used in practice (in proving theorems, building theories, etc.). An example is the naive conception of a set as simply a collection of objects, which has the—by all accounts aesthetic—benefits of simplicity and intuitive appeal. The reason that the naive set is not used[15] is epistemic: it leads to contradictions. Thus, epistemic considerations outweigh aesthetic ones.

To summarise, this version of Wigner's problem fails in two steps. First, given that empirical scientists and mathematicians make very similar claims about the aesthetic character of their work, the role that aesthetics supposedly plays in mathematics needs to be constitutive in order to create a coherent problem of applicability. Second, whether aesthetics genuinely play such a role in mathematics is doubtful. There is evidence for the fact that aesthetic properties do not track epistemic ones and rather tend to be secondary to them.

The Metaphysical Gap

An obvious difference between mathematics and empirical science is their ontologies. Mathematical objects are considered the prime examples of abstract objects. They lack causal powers and spatio-temporal location. The objects of empirical science, by contrast, participate in causal relations and are spatio-temporal—they are concrete. Put another way, mathematical knowledge is knowledge about abstract objects while the knowledge we acquire through empirical science is knowledge about concrete objects. Based on this difference, a version of the applicability problem arises: "Why is it that knowledge about the abstract realm proves to be so enormously effective in generating knowledge about the world

[15] Outside of set theory, sets of mathematical objects are typically handled naively. However, given that mathematical concepts can be represented as ZFC-sets, the naive set is not truly being used.

of concrete physical phenomena?” (Islami and Wiltsche 2020, 159, emphasis removed). This is the so-called “metaphysical gap” between mathematics and physics (Steiner 2009, 642; Pincock 2011; Bangu 2012). Put differently:

(1) Mathematics describes abstract objects, which have no causal powers and are not spatiotemporal.

(2) Physics describes concrete objects, which participate in causal relations and are spatiotemporal.

(3) Since abstract objects lack causal powers and are not spatiotemporal, facts about them should have no bearing on concrete objects, which are causal and spatiotemporal.

(4) Thus, mathematics should not have any bearing on physics.

This contradicts the plain fact that mathematics is applicable: facts about its objects allow us to make predictions about physical systems. I will argue that this line of thought proceeds too fast by undermining each of the premises. The picture they present is oversimplified and by complicating it, this version of the problem dissipates.

To start, there are philosophical accounts of mathematics that deny that mathematical objects exist. This version of the problem is a problem only for platonist philosophies of mathematics. Platonists tend to circumvent it by citing the applicability (more specifically indispensability) of mathematics as the very reason for believing in the existence of abstract mathematical objects, but this lacks an explanation of why mathematics is applicable in the first place (Colyvan 2001b). Given that platonist philosophies of mathematics face this problem, we could turn to some version of nominalism. These reject the existence of mathematical objects and thus premise (1) but typically face other unsavoury consequences (see Bueno 2020). For example, fictionalism, a version of nominalism, holds that since mathematical objects do not exist, statements of mathematics are strictly speaking false (we may consider them true within the “story” of mathematics). Thus, fictionalism has to provide an account of applicability that does not require the truth of mathematics.[16]

In short, this version of the problem is not general enough. It relies on a specific understanding of mathematics that may be rejected and if it is, the question of applicability still arises, although in a different form. A version of the problem that holds regardless of

[16] Formalism, which was mentioned in connection to Wigner earlier and which states that mathematics is naught but a game of symbol manipulation, must also provide an account of applicability that does not rely on the truth of mathematics.

one's philosophy of mathematics has more force. It would indicate that something is off in our understanding of mathematics, physics or their interaction generally, not merely in one account. Most of the versions of the problem discussed throughout this thesis hold regardless of one's philosophy of mathematics (although certain philosophies of mathematics might have an answer to them at hand).

Moving on from mathematics, the claim that physics deals only with concrete objects is suspect. Forces—standard and causally relevant parts of a physicist's ontology—are abstractions: we observe the motion of or interactions between concrete objects and infer that a force acts on or between them. We use numbers to describe the magnitude and vectors to describe the direction of forces, both of which we then use to make (very often accurate) predictions about physical systems. A similar story could be told about fields and energy (perhaps even genes and species). Mathematics is applied to both abstract and concrete entities.

Lastly, it is not strictly true that when mathematics is applied, facts about abstract objects are made to bear on concrete objects. This is already illustrated by mathematics being applied to forces but additionally, many applications are accompanied by idealisations: certain properties that are known to not hold in the physical system are assumed (continuity of fluids, lack of friction on a plane, etc.).[17] False assumptions are being consciously taken on and, as a result, parts of the mathematical formalism no longer accurately represent the physical system under study. What is the mathematics then being applied to? If it is a system of concrete objects, then it is not the one under study and thus the conclusions we draw should not hold for it. If it is an abstraction of the system under study, then mathematics is no longer applied to concrete objects.[18]

In summary, the ontological version of the problem of applicability paints an oversimplified picture. The existence of abstract mathematical objects is contested. Physics does not deal only (or mainly) with concrete objects, a widespread and rather trivial example of an abstraction in physics is any type of force. Lastly, it is not true that facts about abstract objects bear on concrete objects, because the concrete system may involve abstractions like

[17] Philosophers of science sometimes distinguish between different types of idealisations (Hüttemann 2002; Elliott-Graves and Weisberg 2014). For my purposes, an idealisation is simply a false assumption made for some relevant purpose.

[18] Another objection to premise (3) is Frege's solution to the semantic problem of applicability as presented by Steiner (1998, 2009): mathematical concepts apply to concepts, which may apply to concrete objects, thus truths about abstract objects are not made to bear directly on concrete objects. However, Frege's solution, as presented by Steiner, has gaps. These will be discussed in part II.

forces and is often "prepared" for the application of mathematics through idealisations. There is more to be said about this last point, thus it will be the topic of the next section.

The Role of Idealisations in Physics

As discussed in the previous section, the application of mathematics is often accompanied by idealisations. A version of the problem arises from them as well: why can the application of mathematics yield accurate predictions, if it is applied to a false representation of an empirical situation (Bărboianu 2020, 446)? In other words:

(1) To apply mathematics in physics, one often has to make idealisations.
(2) Idealisations are false assumptions.
(3) There is no reason why applying mathematics to false assumptions should yield true conclusions.
(4) Thus, there is no reason why mathematics should be applicable (or at least effective) in physics.

In this section, I point out the limitations of both this version of the problem, also found in (Pincock 2011; Fillion 2012; Bueno and French 2018), and the proposed solution to it. While the problem it presents is genuine, it does not cover all applications of mathematics and for those it does cover, the problem belongs to the philosophy of science.

Premise (1) limits the scope of the argument to applications in which idealisations take place. But mathematics can be applied without making idealisations, this is often the case with more "trivial" examples. By adding up the number of apples and the number of pears I have, I get the total number of my fruits. Doing so does not involve taking on any idealised assumptions about the situation. One might counter by saying that even here the physical system is still prepared for the application of mathematics: the size, shape, colour, etc. of the fruits are ignored. But this is what Levy would call abstraction rather than idealisation: "an abstract description says less about its subject matter than there is to say" (2021, 5857) without thereby saying anything false. The version of the problem presented above would not go through for abstractions since lack of detail does not lead to falsehoods the way false assumptions do.

There is a fairly well-known solution for this version of the problem:

> … before any mathematical application, the physical context is stripped of its layers of complexity and made structural through idealization; hence it is prepared especially for facilitating the application; as such, there is nothing surprising that mathematics applies. (Bărboianu 2020, 432)

Physicists make idealisations precisely in order to facilitate the application of mathematics. Philosophers writing about idealisations similarly note that idealisations are often made explicitly with the aim of aiding the application of mathematics: "These procedures [idealisations] lead to simple theories, and they allow for a simple mathematical treatment of the problems in question" (Hüttemann 2002, 183).

Although correct, the solution is limited. As Bărboianu (2020) notes, it explains why mathematics is applicable but leaves unclear how physics in turn is effective empirically. How is physics able to produce accurate (true) predictions, despite beginning from idealised (false) assumptions? The problem shifts from the philosophy of mathematics to the philosophy of science. And, considering the scope of this thesis, that is where I will leave it. Successful applications in the face of idealisations are not an oddity of mathematics, but an oddity of empirical science.

In short, this version of the problem of applicability is not general enough, for it leaves out applications where idealisations do not take place. The problem can be dismissed by pointing to the fact that idealisations are done specifically so that mathematics can be applied. This explains why mathematics is applicable in physics but leaves open the question of how physics manages to relate to the world.

Conclusion

The aim of part I was to describe the shape that a coherent version of Wigner's problem ought to have and to consider some of the ways of filling it out. Such a version takes the form of an argument whose premises roughly divide into two: facts about applicability (e.g. that mathematics has led to physical discoveries) and assumptions about mathematics and empirical science (e.g. that something developed independently of physical considerations should not bear on physical findings). The conclusion contradicts something we otherwise take to be true.

I illustrated how stating facts about applicability alone is insufficient to generate a problem, using the example of mathematical discovery. I then dismissed several assumptions that might be used to problematise discovery, namely that mathematics is developed independently, aesthetics play a constitutive role in mathematics, and that there is a metaphysical gap between mathematics and physics. The latter two were dismissed generally, even if paired with a different fact about applicability, e.g. that mathematics is used to make empirical predictions, they do not succeed. Finally, I noted the limitations of both the problem based on idealisations and its proposed solution.

The versions of the problem dismissed in this part all relied on narrow or contested assumptions about mathematics and empirical science. What they did not take seriously is the variety of ways in which mathematics is actually applied, the different roles it plays and functions it serves. Part II takes up this question, not to find new assumptions to problematise applicability, but to ask whether this variety can be reduced to something more tractable. I argue that it can.

Part II

Introduction: Deductive Applicability

We now move onto facts of applicability. Mathematics is applied in empirical science in different ways and to different ends, and these have been given different names: descriptive, deductive and semantic applicability, non-standard applications, the unifying and explanatory role of mathematics. The aim of this part of the thesis is to argue that this abundance is unnecessary. Besides semantic applicability, all types and roles can be understood through one—deductive applicability. Versions of the problem of applicability that rely on anything besides deductive applicability should therefore not be taken as independent from those that do. An introduction of deductive applicability is in order.

Deductive applicability was first named and described by Steiner (1998): mathematical theorems are used as premises in deductions with empirical conclusions. I will use the broader term 'mathematical statement' to refer to mathematical propositions generally, including theorems, axioms,[19] and other accepted mathematical claims, including those whose truth was historically accepted without proof in the modern sense (the discussion on axioms below characterises examples of these). Worded like so, deductive applicability includes the computational or calculational role of mathematics, discussed by, for example, Sorin Bangu (2012, 81).

An example of deductive applicability is this:

(1) I have six apples.
(2) I have two friends.
(3) Six is an even number.
(4) Thus, I can share my apples evenly among my friends.

A mathematical statement, that six is an even number, is used to reach an empirical conclusion. There are also "mixed" statements, (1) and (2), that combine mathematical concepts ('six' and 'two') and empirical ones ('apples' and 'friends'). Mixed statements are empirical in the sense that they are justified by experience ("I have six apples" is justified by the observation that I have six apples). More abstractly then, to apply mathematics

[19] While examples of axioms being applied are probably not widespread in practice, somewhat silly ones can be constructed. If I need to build a road between two cities and am considering options for what paths the road could take, I can infer that a straight path between the two would certainly connect them, based on Euclid's first postulate. More importantly, there is no *prima facie* reason why axioms could not be used this way.

deductively is to infer a mixed statement from other mixed statements and statements of pure mathematics.[20]

The Problem and Possibility of Semantic Applicability

Mixed statements like “I have six apples” and “there is no Eulerian path through Königsberg” are truth-apt and we are capable of understanding and using them. Since they combine mathematical and empirical concepts, the question of how such statements ought to be interpreted arises. This is what some authors (Islami 2016; Bărboianu 2020) refer to as semantic applicability. In the literature on Wigner’s problem, semantic applicability is less discussed than other types of applicability, but there is a classic treatment of the topic, courtesy of Steiner (1998), which gets retold and approved without much critical engagement (Colyvan 2001b; Bangu 2012; Islami 2016; Bărboianu 2020). In this section, I present this standard account and some problems it faces. Given the limited nature of this thesis, I will not defend an alternative account but simply note that one is needed.

Consider:

(1) 7 + 5 = 12.
(2) There are seven apples on the table.
(3) There are five pears on the table.
(4) No apple is a pear.
(5) Apples and pears are the only fruits on the table.
(6) Hence, there are exactly twelve fruits on the table. (Steiner 1998, 16)

There is a problem here. In premise (1), ‘7’, ‘5’ and ‘12’ seem to be referring to objects (numbers), functioning as singular terms; in premises (2), (3) and the conclusion, ‘seven’, ‘five’ and ‘twelve’ seem to instead be describing the fruits on the table, functioning as predicates. Given this, the argument should be invalid, yet it is not. What is required is a semantics that interprets both the mathematical statement (1) and the mixed statements (2), (3) and (6) uniformly.

[20] In practice, the premises might include purely empirical statements as well, this makes no difference for my purposes. Because of the structure of deductive applicability, the conclusion will be a mixed statement, but it may be further expanded on (used to derive new consequences), especially if the conclusion itself is not directly testable or observable.

According to Steiner, such a semantics was provided by Frege. Premises (2), (3) and (6) are really of the shape "the number of Fs is n",[21] where 'F' is some suitable predicate (e.g. 'is an apple on the table') and 'n' is a numeral (e.g. '7'). Thus, numerals and number words always function as singular terms, although they may appear as a part of "the number of Fs is *n*", which is a second-order predicate (it predicates something of the concept 'F'). An extra premise must be added that states that if (2)–(5) are true, then the number of fruits on the table is 7 + 5.[22] This connects addition to disjunction (disjoint set union) and as such the mathematical statement (1) to the rest of the premises.

There are at least two problems with the Frege-Steiner solution. First, it is unclear how many applications of mathematics actually fit the scheme. Neither Steiner nor Frege give reason to think mixed statements can always be reworded so that mathematical concepts appear only as singular terms (Pincock 2011, 170). Pincock also criticises Steiner's plan for extending the solution beyond arithmetic. This involves linking Zermelo–Fraenkel set theory together with the axiom of choice (ZFC) to the physical world:

> To "apply" set theory to physics, one need only add special functions from physical to mathematical objects (such as the real numbers). Functions themselves can be sets (ordered pairs, in fact). As a result, modern—Fregean—logic shows that the only relation between a physical and a mathematical object we need recognize is that of set membership. (Steiner 1998, 23)

In other words, physical objects are added to set theory as individuals (urelements) and the rest is done by the axioms of ZFC. Pincock argues that this is not enough. All this achieves is some "elaborate set-theoretic relation" (Pincock 2011, 173) that sheds no light on why or how mathematics applies. This is in contrast with Frege's treatment of arithmetic, which was built up of the relation of one-to-one correspondence and is highly relevant if not outright explanatory for the practice of counting (see Zalta 2026). If all the relation needs to do is connect physical and mathematical objects, alternative accounts exist, e.g. Wilholt's (2004), and the burden is on Steiner to show his is preferable.

The second problem is one Steiner later acknowledges: the account presumes a platonist treatment of mathematics (Steiner 2009, 643). If numbers do not exist, numerals fail to refer and the mathematical and mixed statements alike come out false. The platonist price would be easier to accept if the solution had no other issues. As it stands, however, this is just another point against it.

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[21] Standardly shortened to $\#xFx = n$.

[22] Formally: $\forall F \forall G((\#xFx = m \land \#xGx = n \land \neg\exists x(Fx \land Gx)) \rightarrow \#x(Fx \lor Gx) = m + n)$.

A more general difficulty is that Steiner wants Frege's results without Frege's ambitions. Frege aimed at a comprehensive reduction of mathematics to logic, which required *translating* all statements involving mathematical concepts into a single logical system (see Zalta 2026). This is a demanding project that inevitably carries heavy commitments. Semantic applicability requires something more modest: a coherent *interpretation* of mathematical concepts in mixed statements. Importing the full Fregean apparatus imports unnecessary baggage.

In short, the Frege-Steiner treatment of semantic applicability has holes. Due to the limited nature of this thesis, I will not take up the task of filling them here and limit myself to pointing them out: any comprehensive account of the applicability of mathematics ought to look beyond the Frege-Steiner treatment. There is relevant literature on the semantics of mixed statements (e.g. Felka 2014; Knowles 2016; Snyder et al. 2022), though its emphasis is on the realism-antirealism debate rather than applicability and it is largely limited to number attribution. Since semantic applicability is a precondition for the applicability of mathematics in general and given that mathematics *is* applicable, semantic applicability has to be possible. I will move forward assuming that it is.

Descriptive Applicability Collapses into Semantic and Deductive Applicability

In part I, it was mentioned that Wigner's (1960) main interest lies in the applicability of mathematical concepts.[23] It is puzzling that concepts, developed based on intra-mathematical considerations (for Wigner, these are aesthetic in character), show up in our best scientific descriptions of the world (Colyvan 2001b; Bangu 2012; Islami 2016; Bărboianu 2020).[24] This was named descriptive applicability by Steiner: "the appropriateness of (specific) mathematical *concepts* in describing *and lawfully predicting* physical phenomena" (1998, 25, emphasis in original). Describing requires forming and interpreting the mixed statements we have been discussing, thus semantic applicability. Descriptive applicability is, however, meant to go further. The emphasis is no longer on the bare fact that mathematical concepts can be linked to empirical objects, but rather on the peculiarity of mathematical concepts:

[23] By 'mathematical concept' anything from 'two' and 'addition' to 'cyclical group' and 'non-Euclidean geometry' is meant.

[24] Some authors prefer to speak of the 'roles' that mathematics plays in empirical science instead of types of applicability (e.g. Bueno and French 2018). These tend to include a representational role that "involves, in general, the use of mathematics for the representation of physical phenomena" (ibid., p. 21). I will use 'description', 'representation' and their cognates as synonymous.

they allow us to do empirical science in a way that, say, the concepts of astrology or chess do not. This "allowance" manifests in mathematical concepts making it possible to draw empirically significant inferences about the physical system described by them.

Drawing such inferences is the business of deductive applicability. The claim is then that there is something about mathematical concepts that makes deductive applicability possible, rather than the other way around. In this section, I will introduce two candidates for this something—projectibility and axiomatic origins—and argue that neither succeeds in making descriptive applicability stand on its own. The section will close with a short discussion and dismissal of a structure-centred view of the problem of applicability, given that it can be handled in much the same way as descriptive applicability.

Steiner's candidate is the lawlikeness and projectibility of the description made using mathematical concepts:

> The descriptions of which I speak are thus *lawlike* or *projectible* descriptions in the sense of Goodman 1983: descriptions which could appear in natural laws and thus be used in predicting events. Only these descriptions are my concern. (Steiner 1998, 25, emphasis in original)

For Goodman (1983), both lawlikeness and projectibility are properties of statements that are confirmed by observations, i.e. inductively. The examples Steiner brings of descriptive applicability (addition, linearity, the inverse square law, analytic functions, etc.) are not of this kind. For addition, the application Steiner considers is weighing:

> If one body balances 5 unit weights, and another balances 4, then both together will usually balance 5 + 4 = 9 unit weights. The natural numbers indirectly describe, by laws of nature, not only the sets of unit weights placed on the scale, but the objects they balance. … Arithmetic is not empirical, but it predicts experience indirectly by the law: if m and n are the numbers of unit weights that balance two bodies separately, then $m + n$ units balance both. Equivalently: if one object weighs m units, and another weighs n units, then the (mereological) sum of both "weighs $m + n$ units." (Steiner 1998, 28)

Clearly, this "law" is not meant to be taken as a statement that is confirmed inductively by observations of two bodies weighing m and n units respectively weighing $m + n$ units together. Instead, the mathematical statement, 4 + 5 = 9, is being used to deduce empirical results, i.e. that two bodies weighing 4 and 5 units will together weigh 9 units.

For Goodman, the projectibility of hypotheses is also related to the projectibility of predicates (a predicate is projectible, roughly, if it can be used for induction). However, mathematical concepts do not determine projectibility, for 'has eight limbs' is projectible (the observation that “this octopus has eight limbs” supports the hypothesis “all octopi have eight limbs”), while ‘has three sons’ is not (the observation that “this man in the room has three sons” does not support the hypothesis “all men in this room have three sons”). Additionally, the projectibility of predicates is a non-issue in deduction, which is what most applications of mathematics involve. Non-projectible predicates can appear in deductions without causing any problems. Consider:

(1) This emerald is grue.
(2) Whatever is grue is either examined before time *t* and green or not so examined and blue. (Goodman 1983, 74)
(3) This emerald is not examined before *t*.
(4) Thus, this emerald is blue.

The argument goes through regardless of the non-projectibility of ‘grue’. Projectibility is relevant to inductive confirmation, not to the validity of deductive inferences. Since mathematical concepts facilitate deductive rather than inductive reasoning, their projectibility (or lack thereof) is beside the point. In short, mathematical concepts have little to do with the lawlikeness or projectibility of descriptions, since descriptions containing them are applied deductively, not inductively. They do not affect the projectibility of the predicates they appear in and even if they did, projectibility only matters in inductive contexts.

Alternatively, what makes mathematical concepts special might be the way they are defined. Most mathematical concepts are defined through other concepts—they are shorthands for longer definitions that are too unwieldy for actual use. Any application of such concepts can, in principle, be reworded in terms of more basic concepts. These basic concepts are those defined through axioms, e.g. ‘natural number’ as defined through the Peano axioms and ‘point’ as a primitive notion of geometry. Something is a natural number if it satisfies Peano’s axioms and from Peano’s axioms results can be deduced about the natural numbers. In any application of the natural numbers, we can take these axioms to figure as implicit assumptions, giving the application the shape of deductive applicability.

This works for post-axiomatisation applications, but the natural numbers were being successfully applied long before Giuseppe Peano published the axioms in 1889. The answer I offer for applications pre-axiomatisation is not a direct subsuming of them into the model of deductive applicability, but rather a view of what axioms are and what they are meant to do. Axioms, when issues like inconsistency are cleared, are decided upon based on their usefulness—what they allow us to prove (Schlimm 2013; Baker 2025). Peano's axioms were chosen to reflect what was already known about the natural numbers and to imply results that were already established about them. Thus, the concept 'natural number' is, in a sense, defined based on what statements the Peano axioms allow us to prove.

Overall, to emphasise concepts over statements is to put the cart before the horse. When we apply a concept of mathematics and deduce a result that turns out to be empirically significant, the deduction is facilitated by statements, not concepts. To predict something, in this context, *is* to deduce something using the statements of mathematics. To attribute the deduced result to the concept is to misplace the source of contribution.

In summary, descriptive applicability is presented as mathematical concepts being used to describe and predict empirical phenomena. But the description aspect is really a result of semantic applicability and prediction of deductive applicability. For descriptive applicability to stand on its own as a type of applicability, there would need to be something about mathematical concepts that makes this reduction untenable. Steiner's proposal—projectibility—fails because mathematical concepts have nothing to do with the projectibility of either the descriptions or the predicates they appear in. Pointing to the fact that mathematical concepts are, at heart, defined through axioms is not enough either, because axioms are chosen based on what they allow us to do: prove theorems.

There is another aspect of applicability that can be handled similarly. Some philosophers of mathematics hold that mathematics is properly speaking the study of structures rather than objects like numbers, sets and functions. Structuralist accounts of mathematics come in both platonist and nominalist varieties. If mathematical structures are, *à la* Shapiro (Hellman and Shapiro 2019, ch. 5), taken to exist as abstract structures independently of their instantiations, then a problem of applicability analogous to the ontological one stated and criticised in part I can be advanced. On a more nominalist interpretation, *à la* Hellman (ibid., ch. 6), this version of the problem does not arise, but others do. Hellman's modal structuralism entails rewriting physical theories in a nominalist manner, thus it arguably fails to provide an account of applicability that reflects the actual practice of applying mathematics (Bueno 2020).

A fact of applicability that arises on structuralist accounts generally is this: the empirical world appears to consistently instantiate mathematical structures. On the model of deductive applicability, instantiation amounts to the empirical system satisfying the axioms (or otherwise constraints) of the mathematical structure. In other words, the mixed statements expressing the physical system's relations in terms of those axioms are true (how such statements ought to be interpreted is the business of semantic applicability). Predictions based on structural relations are then facilitated by deductive applicability. Is there something to structural instantiation that goes beyond this?

There is if one attributes metaphysical weight to the structural fit between mathematics and the empirical world. This can be borne out as ontic structural realism (broadly, taking structures as ontologically primary over objects) or as the world having (or even being) a specifically mathematical structure. Such metaphysical theses are, however, not suggested by the fact alone that mathematical structures are effectively applied in physics nor do they have any bearing on applicability as such. A mathematical structure need only represent the empirical system at hand accurately enough where the predictions we get through deductive applicability hold true, according to the scheme detailed in the paragraph above. Given that metaphysical theses about structures make no difference to the results of applying mathematics, they are irrelevant here.[25]

What is Unstandard about Non-Standard Applications?

The three types of applicability introduced so far—descriptive, deductive and semantic—have been coined as the standard types of applicability by Bangu (2012). Besides these, he argues, there are non-standard applications of mathematics:

> The non-standard cases I have in mind are those in which it turns out that *non-computational*, *analogical (non-deductive)*, and sometimes merely *notational-symbolical* uses of the formalism allow important insights into the relevant physics. If one wonders 'how can a mathematical representation be used other than for calculations?', the answer is by way of example: puzzling cases can be documented in which the formalism was taken to offer precise suggestions as to how certain parts of it match elements of the physical world. (Bangu 2012, 82, emphasis in original)

These include Steiner's Pythagorean analogies, meaning analogies made in physics that were, at the time, inexpressible in nonmathematical language (1998, 54). Examples here include Maxwell's discovery of electromagnetic radiation (Steiner 1998), the prediction of the positron (Steiner 1998; Bangu 2012; Bueno and French 2018) and the prediction of the omega-minus particle (Bangu 2012).

[25] A proper treatment of semantic applicability might provide reasons for metaphysical theses, if the truth-conditions for mixed statements turn out to require the existence of mathematical structures as truthmakers. Such a treatment is, however, outside of the scope of this thesis.

A common thread in these examples is that non-standard applications supposedly go beyond the standard applications of mathematics and result in the prediction of new physical objects or phenomena later confirmed empirically. I will argue, however, that they can be explained through deductive applicability. The fact that physicists go looking for objects predicted by the mathematical formalism is an oddity of physical practice, rather than mathematical application. I too will proceed by example: Dirac's prediction of the positron.

The short version of the discovery of the positron is this. In 1928, Paul Dirac published a relativistic wave equation describing the behaviour of electrons and hydrogen atoms. Its solutions have two 'positive energy' parts and two 'negative energy' parts,[26] with the physical significance of the latter being initially unclear. In 1930, Dirac proposed his "hole" theory as an explanation for why we have not observed negative energy quantum states. He hypothesised that by default, all the negative energy states are filled by electrons. If a "hole" does appear, it would act like a positively charged particle, thus he identified it with the proton. After criticisms from Weyl, Oppenheimer and others, Dirac conjectured in 1931 that the negative energy solutions instead correspond to a new particle, which he called the 'anti-electron'. This particle would have the same mass as an electron but the opposite charge. In 1932, Carl Anderson reported the first empirical evidence for such a particle, later named the positron. (Steiner 1998; Bangu 2012; Bueno and French 2018)

What is puzzling about this story is that Dirac seemed to have no mathematics-independent reasons for treating the negative energy parts of the solutions as physically significant, let alone postulating the existence of new particles based on them. Sorin Bangu (2012) articulates a principle he considers to be an implicit premise in Dirac's reasoning:

> Let Γ and Γ' be elements of a mathematical formalism applied in a certain physical context. If Γ' is mathematically similar[27] to Γ, then, if Γ has a physical referent, Γ' has a physical referent as well. (Bangu 2012, 101)

Given that the positive energy solutions to Dirac's equation have physical referents, this principle licenses the inference that the negative energy parts do so too.[28]

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[26] The solutions are 4x1 matrices.

[27] "'Mathematically similar to' means that the similarity between [mathematical expressions] Y and X is of a mathematical nature. … X and Y can be mathematically alike in the sense that both are solutions to the same equation; or they can have a similar syntactic mathematical form, etc." (Bangu 2012, 114)

[28] Bangu argues that the discovery of the omega-minus particle based on the symmetry group SU(3) also involved the use of this principle. Without going into the details of this story, the following criticism of the principle holds for it as well.

Bangu's identification principle is a clarification of the "higher epistemic status" of mathematics mentioned in part I: since the formalism points to negative energy states, negative energy states must be. Clearly, this principle is too strong. It makes an unjustified jump from mathematical possibility to physical existence.[29] It is well known that physicists reserve full ontological commitment to theorised phenomena and objects until sufficient empirical evidence supporting their existence is collected. Notably, Anderson, who collected the first evidence for the positron, while aware of it, was not inspired by Dirac's theorising (Bueno and French 2018, 146). Mathematical similarity with something empirically confirmed at best lends credibility to the theorised phenomenon or object and can make physicists more inclined to look for it experimentally. The effect is psychological rather than justificatory. An improved version of the principle could thus be the following (in a simpler form as well):

> Let Γ and Γ' be elements of a mathematical formalism applied in a certain physical context. If Γ' is mathematically similar to Γ and Γ has a physical referent, then it is worthwhile to look for a physical referent for Γ'.

Like Bangu's original version, this principle is a methodological one for physics and while Bangu is somewhat reserved about delegating his principle to the realm of discovery, the improved version belongs to it much more clearly. Bangu's reservations stem from his understanding of discovery generally: scientists notice an intriguing phenomenon and look for a theoretical description of it (2012, 104–5). The situation is reversed with the positron, first a theoretical description was found and then the object it describes. However, this is a fairly limited view of discovery (Bueno and French 2018, 225) and on a broader understanding the improved principle, if not even Bangu's original, can be comfortably classified as heuristic.

Let us recap. The discovery of the positron appears puzzling if we attribute existence to it before empirical confirmation. That is, if we consider it physically real based only on the mathematical formalism. This is not how things go in practice. Having a solid mathematical footing for a postulated object or phenomenon can make physicists more inclined to look for empirical evidence, but it is by itself not enough to justify belief in them.

[29] Steiner makes a similar leap. Of Maxwell's prediction of electromagnetic radiation, he says: "This [the change to Ampère's law] made electromagnetic radiation a mathematical possibility. *The belief that it was also physically real* required a Pythagorean analogy—one that paid off." (1998, 77, emphasis mine).

There is a bit more to the story, however. Bueno and French (2018) note that the mathematical formalism underdetermines its physical interpretation. Dirac's hole theory and the anti-electron hypothesis were equally valid interpretations of the same solutions, yet only the latter made empirical confirmation possible: it "identified the *type of entity* and the *physical properties* that this entity should have for the theory to be empirically adequate" (ibid., 146, emphasis in original). Mathematics played a heuristic role, "the crucial work was done by *interpreting* the mathematical formalism" (ibid., 147, emphasis in original). Additionally, the fact that Dirac took the uninterpreted mathematics to be significant enough as to give a second interpretation for it (after the failure of the first one) can also be explained by physical considerations: it is the context of quantum mechanics that makes such oddities as negative energy states conceivable and thus worth investigating (Bueno and French 2018, 146–147).

Taking into account the role physical considerations played then, the principle can not only be classified as belonging to the context of discovery, but also as belonging to the philosophy of physics. If there is anything puzzling remaining about the discovery of the positron, it is not a result of the mathematics used, but the physical context. Thus, keeping in line with the limits of the thesis, this is where I stop.

As for the derivation of Dirac's equation, Steiner sees it as an example of the use of a formalist analogy, i.e. an analogy "based on the syntax or even orthography of the language or notation of physical theories, rather than what (if anything) it expresses" (1998, 54, emphasis removed). In deriving the equation, Dirac was guided by mathematical considerations at multiple points, mainly by an analogy with the derivation of Schrödinger's wave equation. And it was only after a formally acceptable equation was found that Dirac began thinking of how to interpret its solutions physically (ibid., 161).

Let us grant Steiner that a formalist analogy took place here, meaning that the analogy with Schrödinger's equation was purely mathematical. By all accounts then, the derivations of Schrödinger's and Dirac's equations are mathematically similar, for derivations are mathematical things just as proofs are. Schrödinger's equation was also already known to be empirically adequate. What these sum to is this: the empirical success of Schrödinger's equation lends credibility to equations that are derived in a similar manner, thus it is worthwhile to look for such equations. This is the same logic as the one behind the principle above. Once found, Dirac's equation is solved mathematically and its solutions given a physical interpretation, which generates the prediction. Deductive applicability is doing the work of drawing empirical consequences from the (suitably interpreted) formalism.

In summary, what makes cases like the discovery of the positron unstandard is that the mathematics is taken to imply the existence of something new. Mathematics by itself, however, is not enough to justify belief in the existence of such objects and physicists reserve judgement until empirical evidence is found. It is the physical context that plays a much larger role in leading to an interpretation of the mathematics.

Unification and Explanation

There are two other noteworthy functions of mathematics in empirical science. First, mathematics can unite seemingly disparate phenomena or areas of empirical science by bringing them together under one formalism (Morrison 2000; Colyvan 2001a; Pincock 2011; Bueno and French 2018). Similarly to non-standard applications, discussions of unification typically proceed through examples: Newton's unification of terrestrial and celestial mechanics (Nahin 1992), the equivalence of Heisenberg's matrix mechanics and Schrödinger's wave mechanics in quantum physics (Bueno and French 2018, ch. 6) and Maxwell's unification of electromagnetism and optics (Nahin 1992; Morrison 2000, ch. 3). Second, mathematics may play a role in explaining physical phenomena, although this is a contested topic (see Bueno and French 2018; Bangu 2021; Mancosu et al. 2025). In this section I introduce two examples of mathematics-led unification and argue that they can be adequately explained through deductive applicability. I then turn, briefly, to mathematical explanations.

The derivation of Maxwell's equations and the resulting prediction of electromagnetic radiation was introduced in part I. What was not discussed is the unification it brought about: light is an electromagnetic phenomenon. Having modified Ampère's law to imply conservation of electric charge, Maxwell found that the resulting equations also implied self-sustaining electromagnetic waves travelling at the speed of light. This made light, which was already known to behave like a wave, a possible example of electromagnetic radiation. This, of course, turned out to be just the case. (Steiner 1998, 77–79; Morrison 2000, ch. 3)

Maxwell's unification brought together both physical phenomena that were thought to be separate and the formal tools used to describe them. It was enabled by mathematical manipulations of the equations of electromagnetics. Steiner (1998) sees this as another example of a Pythagorean analogy at work. But the case reduces straightforwardly to deductive applicability: Maxwell modified Ampère's law for physical purposes—to reflect conservation of electric charge—and deduced a consequence of the new set of laws—

electromagnetic waves travelling at the speed of light. The (physically interpreted) mathematics then implied that light could be an instance of electromagnetic radiation. At this point, the formal tools for optics and electromagnetics were unified, but the phenomena themselves were not—that required Hertz's empirical confirmation that electromagnetic radiation exists. Only when the existence of electromagnetic radiation was confirmed could light be identified as an electromagnetic phenomenon.

Let us try another example: the unification of matrix and wave mechanics. Both are formulations of quantum mechanics, the situation before their unification was this:

> Matrix mechanics is expressed in terms of a system of matrices defined by algebraic equations, and the underlying space is discrete. Wave mechanics is articulated in a continuous space, which is used to describe a field-like process in a configuration space governed by a single differential equation. However, despite these differences, the two theories seemed to have the same empirical consequences. (Bueno and French 2018, 117)

This empirical agreement is what drove physicists to look for a bridge between the two theories. The connection was made by von Neumann, who reached a full isomorphism between two sets of functions, one defined on a discrete space of indexes for matrix mechanics, the other on a continuous state-space for wave mechanics (Bueno and French 2018, 121–22). Crucially, the unification was purely mathematical: "his theorem is restricted to the *mathematical structures* employed in matrix and in wave mechanics. Their *physical content*, as it were, is left untouched" (Bueno and French 2018, 112, emphasis in original). This is, however, unsurprising given that the theories were empirically equivalent from the outset, in that they led to the same predictions.

While von Neumann's effort undoubtedly required a level of genius, there is nothing new here. Applicability does not even enter the picture, since the unification happened on a purely mathematical level. In summary, if we account for the deductive applicability of mathematics and do not reify mathematical formalisms before empirical confirmation, there is nothing puzzling about mathematics unifying scientific theories. Additionally:

> [T]he fact that we can construct a common mathematical framework for dealing with a range of different phenomena does not by any means automatically insure that we have identified some set of common causal factors responsible for those phenomena—i.e., that we have produced a unified physical explanation of them. (Woodward 2019)

In cases where it does turn out that the formalism identifies common causal factors, this relies on physical interpretation and empirical confirmation.

Turning to mathematical explanation, there appear to be non-causal scientific explanations that rely on mathematical facts. Perhaps most well-known is one concerning some species of periodic cicadas, whose life-cycle lengths are prime numbers: 13 and 17 years (Baker 2005). The explanation for why these cicadas evolved such cycle lengths is that minimising overlap with other periodic organisms is advantageous (less predators, more resources) and prime periods achieve this minimisation (since primes maximise their lowest common multiple with all numbers smaller than them). Further examples include the hexagonal shape of honeycombs, soap films obeying Plateau's laws and the bridges of Königsberg (Mancosu et al. 2025).

Whether mathematical facts are genuinely doing the explaining, rather than underlying physical structures that the mathematics represents, is debated, as is the mechanism by which they would do so. I take no stance here but simply note that such explanations rely on the application of mathematical statements, i.e. deductive applicability. Any explanation can be reconstructed as an argument: the *explanans* and *explanandum* are both propositions, and the connection between them can be made explicit by adding premises that make the inference deductive. The cicada case is no different, the mathematical fact about prime periods figures as a premise from which the *explanandum* follows. Some extra conditions may be required for a deductive application of mathematics to constitute an explanation, but the explanation still falls under the broader notion of deductive applicability.

In summary, unification of the mathematical tools used to describe some phenomena does not always mean that the phenomena are therefore physically unified. When it does turn out that the phenomena share causal factors, this is based on empirical confirmation, not mathematical manipulation. If mathematical explanation is accepted, statements facilitate it, making it a (special) case of deductive applicability as well.

Conclusion

The aim of this part of the thesis was to narrow down the abundance of facts we have to take into account when discussing the applicability of mathematics. I considered six aspects: semantic, descriptive and deductive applicability, non-standard applications, unification and explanation.

The question of semantic applicability—how to interpret statements mixing mathematical and non-mathematical concepts—remains open, given the problems with the standard Frege-Steiner treatment. Since we are nonetheless capable of forming and using mixed statements, I moved forward assuming that an interpretation is in principle possible.

I then argued that the remaining types of applicability are best understood through deductive applicability. Descriptive applicability, insofar as it goes beyond semantic applicability, amounts to the use of mathematical concepts in predicting empirical phenomena—but predictions are facilitated by deductions. Two characteristics of mathematical concepts that might have tied prediction more tightly to concepts were considered and rejected. Non-standard applications, examined through the example of Dirac's prediction of the positron, turn out to involve mathematics in a deductive capacity, with physical interpretation playing the decisive role in predicting new objects. Unification likewise results from deductive applicability combined with physical interpretation. And mathematical explanation too, if accepted, relies on the application of mathematical statements.

Applicability can thus be understood as deductive applicability. A problem of applicability should centre the fact that we can use mathematics to deduce empirical conclusions. Part III puts forward such a problem, one that does not presume a specific philosophy of mathematics.

Part III

Introduction: The Applicability of Mathematics as an Epistemic Problem

We have seen that the core fact of the applicability of mathematics is deductive applicability, everything else can be understood through it. Statements of mathematics are used as premises in deductions with empirical conclusions. The rest of the premises in such deductions include mixed statements, which connect mathematical concepts to empirical things. The mathematical statement then ties these concepts together to facilitate the inference. Such deductions are at play when calculations are made, new objects are predicted or different phenomena are unified.

The question now is this: is there a problem of deductive applicability? At first glance, deductive applicability seems to leave no room for a problem. If the premises are true and the inference valid, the conclusion is true (this is guaranteed by the 'deductive' aspect of deductive applicability). Notice, however, that deductive applicability is not merely a truth-preserving procedure but an epistemic one: it is how we come to *know* empirical conclusions. Given the work done in the rest of the thesis, i.e. the criticisms advanced against other versions of the problem and the reduction of types of applicability to deductive applicability, *whatever* problem of applicability remains (if one remains) is an *epistemic* problem.

Yet even then, deductive applicability seems non-problematic: the fact that we know these conclusions is guaranteed by the principle of epistemic closure. Roughly, if one knows *p* and that from *p* follows *q*, then one knows *q*.[30] The justification for the conclusion derives from the justification for the premises. Given that deductive applicability always involves both mathematical and mixed premises, any epistemic problem must arise from some difference between how the two are justified. This is not just *a* source of tension for the problem of applicability, it is the only source, given the structure of deductive applicability. What follows is one account of precisely what that difference is and why it generates a tension. I leave open whether the difference between the justification of mathematical and mixed statements could be used to generate further tensions and thus further problems of deductive applicability.

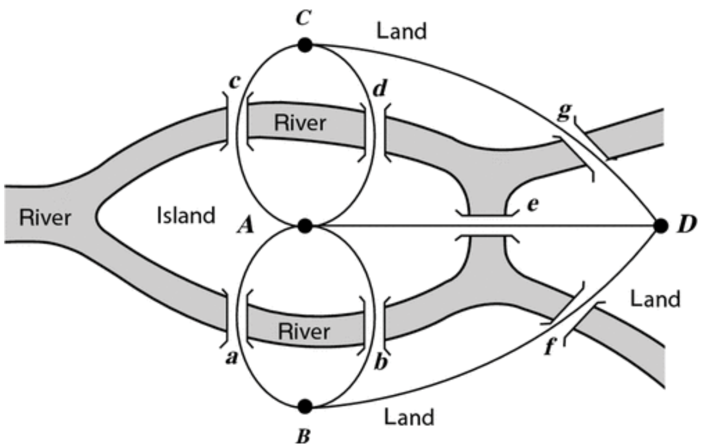
This part of the thesis will do three things. First, I will put forward a problem of deductive applicability. This problem relies on the notions of defeaters and the transmission

[30] While arguments have been advanced against closure (e.g. Dretske 2005), the idea remains generally accepted (Luper 2020).

of justification. If mathematical statements are genuine justificatory contributors to empirical conclusions, then they should be defeasible by empirical evidence, yet they are widely considered not to be so. Second, I will defend the problem from some counterarguments. This defence can be seen as a sustained introduction of the problem, given that some important clarifications arise in the course of it. Third, I will review some influential philosophical accounts of mathematics in light of whether they manage to respond to the problem.

The Problem of Deductive Applicability

I will first introduce the problem through an example and then present it rigorously. In the 18th century, the city of Königsberg had seven bridges crossing the river Pregel and its tributaries. Among mathematicians, a question spread: is it possible to take a walk through Königsberg such that every bridge is crossed exactly once? A negative answer was provided by Leonhard Euler in 1736, in modern terminology, the solution is as follows. The city of Königsberg is represented by a graph, call it *G*, where the bridges are represented by edges and the landmasses they connect are represented by vertices.



Graph *G* (Debnath 2010, 773)

The general result that is needed here is Euler's theorem: a connected graph has a Eulerian path if and only if zero or two vertices have an odd degree. A graph is connected if for every pair of vertices, a sequence of edges that starts at one and ends at the other can be found; our graph *G* is connected. The number of edges incident to a vertice is called the degree of the vertice and a sequence of edges that visits each edge of the graph once is called a Eulerian path. All four vertices of graph *G* have an odd degree, thus it has no Eulerian path.

Given that graph *G* represents the city of Königsberg accurately, there is no possible walk through the city of Königsberg that crosses each bridge exactly once. In short:

(1) The bridges of Königsberg are accurately represented by graph *G*.
(2) There is no Eulerian path in graph *G*.
(3) Thus, it is impossible to take a walk through Königsberg that crosses each bridge exactly once.

This is a well-known example in the mathematical explanation literature (e.g. Pincock 2007). Whether or not Euler's theorem genuinely explains the impossibility, it and premise (2) undoubtedly contribute to the justification of (3). Knowledge of the impossibility of taking the described walk through Königsberg is justified, at least in part, by Euler's theorem. Yet, if (3) turned out to be false, we would not go after Euler's theorem. We would most certainly reject or revise premise (1). There is a tension here: mathematics contributes to the justification of empirical claims while remaining insulated from empirical defeat. Mathematics is expected to do justificatory work, without its own justification ever coming under attack.

Let us make this tension sharper. By epistemic closure, knowing (1) and (2), from which (3) clearly follows, is sufficient for knowing (3). The justification for (3) derives from whatever justifies (1) and (2). Knowledge of the impossibility of taking the described walk through Königsberg is justified *in virtue of* the justification for (1) and (2) (and knowledge that the inference from them to (3) is valid), i.e. justification for the knowledge that graph *G* corresponds to Königsberg and that it fails to meet the criteria of Euler's theorem. In other words, the justification for (1) and (2) *transmits* to (3) (Moretti and Piazza 2023).

Here the difference in justification hinted at earlier becomes salient. Premise (1) is a mixed statement, it ties empirical objects (bridges, landmasses, a city) to mathematical concepts (edges, vertices, a graph). Although it contains mathematical concepts, its truth is ultimately established empirically: graph *G* has to accurately represent the city of Königsberg as it exists in the world. Premise (2), however, is mathematical and justified by the proof for Euler's theorem and graph *G* failing to meet its requirements. When reached through deductive applicability, the justification for (3) is thus in part empirical and in part mathematical.

Since (3) is a mixed statement like (1), its truth is also ultimately established empirically, and it is open to empirical evidence against it—it is defeasible. A belief or a proposition is defeasible if it may "lose, have it downgraded or be prevented from acquiring some positive epistemic status", most commonly the status of being justified (Moretti and Piazza 2018, 2845). Defeaters actualise this loss of status, they may be experiences, beliefs, facts, etc. My belief that it is currently 4 pm, based on a clock on the wall saying so, may be defeated by being told the clock has stopped or by seeing another clock (perhaps the one on my computer) show 5 pm.

Say we acquire evidence against (3)—someone (seemingly) manages to walk through Königsberg crossing each bridge once. This would constitute a *rebutting* defeater. These are reasons for believing the negation of a belief or a belief otherwise incompatible with it (Moretti and Piazza 2018, 2847). Rebutting defeaters undermine the justification for their defeatees by justifying incompatible beliefs. Seeing the second clock show 5 pm is a reason to believe that it is not 4 pm and to doubt the accuracy of the first clock. Since defeaters undermine justification, if a belief or a proposition is reached inferentially and its justification is—via transmission—justification for the premises, then defeaters for the former are also defeaters for the latter. In other words, premises that transmit justification to a conclusion are defeasible by evidence that tells against that conclusion. The mathematical statement (2), given that it lends justification to (3), should be defeasible by rebutting defeaters against (3). Since (3) is a mixed statement, it is open to empirical defeaters, by which I mean defeaters that are justified experientially or are themselves experiences. Thus, mathematical premises should be open to empirical defeaters.

Yet in practice, when a defeater is found for a conclusion reached through deductive applicability, it is always non-mathematical statements like (1) that get their epistemic status downgraded. Mathematical statements are at most switched out for other mathematical statements, with the mixed statements changed accordingly. The result is a whole new argument, the mathematics *itself* is not revised, only deemed inadequate for the application at hand. Additionally, although it is possible that the mathematical claim used is in fact false (e.g. "graph *G* has a Eulerian path"), its falsity is established on mathematical grounds (e.g. by showing that graph *G* cannot have a Eulerian path because of Euler's theorem), not on the basis of empirical evidence. This practice reflects, if not outright follows from, the assumption that mathematical statements are *in principle* not defeasible by empirical evidence, meaning empirical evidence cannot undermine their justification.

This is where the tension needed for a version of Wigner's problem arises from. Based on the model of deductive applicability, mathematical statements function as premises and transmit justification to the empirical conclusions thus arrived at. Rebutting defeaters for such conclusions are, because of transmission of justification, also defeaters for the premises.

Yet mathematical statements are assumed to not be defeasible by empirical evidence, their justification cannot be undermined by empirical considerations. As an argument then:

(1) When applied deductively, mathematical statements function as premises that transmit justification to empirical conclusions.

(2) Premises that transmit justification to a conclusion are defeasible by evidence against that conclusion.

(3) Mathematical statements are not defeasible by empirical evidence.

(4) Therefore, mathematical statements do not transmit justification to empirical conclusions.

This conclusion contradicts the widely held assumption that mathematised science is justified, that deducing empirical conclusions using mathematics is a way of coming to know them. Plain truth-preservation is not enough, science is concerned with knowledge. If mathematical statements fail to transmit justification, then much of mathematised science becomes unjustified and thus unknown.

This problem leaves out cases where the suitability of a mathematical formalism for a specific physical system is being tested, e.g. whether Dirac's equation accurately describes electrons. There, results deduced based on the equation are explicitly checked against empirical data so that the use of the equation itself becomes justified—it leads to accurate predictions. But once suitability is confirmed, the equation begins to lend justification to conclusions reached using it. We may add an extra premise along the lines of "mathematical formalism *M* is suitable for physical system *S*", but this does not change the fact that mathematical justification transmits to the conclusions once suitability is established.

One additional note on premise (2) of the argument. I have shown that it follows straightforwardly from the notions of transmission of justification and defeasibility, but it also reflects a standard of empirical science: it ought to be answerable to empirical evidence. Statements that lack sufficient empirical justification should not justify further claims. After all, to ignore or dismiss empirical evidence against one's scientific claims is to do bad science. Mathematics, on the picture painted by the problem of deductive applicability, occupies an odd role in the practice of empirical science. If such a role was afforded to an empirical statement, i.e. if it was used to justify other statements and yet treated as empirically indefeasible, it would quickly be labelled a dogma.

Defending the Problem

Before attempting to answer the problem, one could argue that the problem is not genuine by rejecting one or more of the premises. At first look, one might object that empirical defeaters only undermine non-mathematical premises, because the mathematical premises retain their proof-based justification regardless of empirical outcomes. Justification via proof has a degree of certainty that cannot be defeated by empirical evidence, hence we go after the non-mathematical premises. I agree. This is premise (3) of the problem in more words. Pointing to a reason why mathematical statements are empirically indefeasible does not dissolve the problem but reinforces it. Analogously, any remarks about empirical evidence being the wrong *kind* of evidence for defeating mathematical statements only serve to make the problem sharper. If mathematical statements are indefeasible by empirical evidence for whatever reason, then by premise (2), they cannot transmit justification to empirically defeasible conclusions. If we want mathematical statements to do justificatory work in empirical science, then they *should* be open to empirical defeaters.[31]

To reject premise (1) or the model of deductive applicability I have advanced in this thesis in general, one could argue that mathematical statements do not function as premises but as inference rules. There are two reasons why this does not work. First, inference rules can be converted into tautologies and added to the premises.[32] Unlike something like *modus ponens*, which cannot be fully removed from amongst the inference rules, on account of deduction itself relying on the if-then relationship it defines (see Finn 2021; Boghossian and Wright 2024), most if not all of mathematics can be so removed.

Second, transmission of justification presumes knowledge of and thus justification for the inferential link between the premises and the conclusion. One cannot justifiably infer q from p via a rule one has no justification for (Moretti and Piazza 2023). If a mathematical statement functions as an inference rule, its validity must be known for the inference to transmit justification. Yet if defeaters turn up against the conclusion, the validity of the rule is not called into question. So, justification for the mathematical inference rule is also assumed to be immune to empirical defeat, while simultaneously being required for the

[31] Appealing to proofs to explain the indefeasibility of mathematics by empirical evidence also only moves the problem further inward. Since proofs are deductive and seem to transmit justification as well, defeaters for empirical conclusions justified through deductive applicability reach back as far as the axioms that the mathematical statements in question are derived from. The question then becomes: why is the justification of mathematical axioms indefeasible by empirical evidence?

[32] *Modus ponens*, for example, is the tautology $((P \wedge (P \rightarrow Q)) \rightarrow Q)$.

justificatory link. The tension is the same: something that is indefeasible by empirical evidence is nonetheless doing justificatory work in inferences with empirical conclusions.

Premise (1) could also be undermined by noting that empirical science sometimes uses mathematics that is not *mathematically* justified, thus cannot pass on justification. The Dirac delta function, for example, was not rigorously definable until the advent of distribution theory. But the function was known to be dispensable, its appearances could be rewritten rigorously, its use was a matter of convenience (Bueno and French 2018, 134–35). In cases like early infinitesimal calculus, physicists significantly restricted when and how the unrigorous mathematics was appealed to (McCullough-Benner 2020, 2022). If anything, these cases are exceptions that prove the rule. The very fact that physicists felt the need to place restrictions and to seek rigorous reformulations suggests that the unjustified mathematical premises were understood to be deficient precisely because they could not be relied upon to transmit justification. In normal cases, mathematical premises are expected to be justified, because that justification is what justifies the conclusions drawn from them.

To reject premise (2) (premises that transmit justification to a conclusion are defeasible by evidence against that conclusion) one could claim that deductive reasoning is by definition indefeasible—the truth of the premises guarantees the truth of the conclusion. Given that deductive applicability is deductive, there are no defeaters for conclusions thus reached. This is, however, a symptom of conceptual confusion (De Almeida 2025): defeaters target the *justification* of a statement, not its *truth*. Further, while deductive reasoning is monotonic, i.e. adding information does not invalidate previous conclusions, it does not always transmit justification from the premises to the conclusion.[33] For an example of *transmission failure*, from (Wright 2003), consider two indistinguishable twins Jessica and Jocelyn. Seeing a girl who looks just like Jessica gives me grounds for

(P) That girl is Jessica,

from which I infer

(Q) That girl is not Jocelyn.

My justification for (P), seeing a girl who looks just like Jessica, is not justification for (Q), for the twins are indistinguishable. Rather, I need to know (Q) independently to have my

[33] Also, while deductive reasoning is monotonic, *scientific* reasoning as a whole is not (for example, new tools make possible new observations and thus new information that might invalidate previous conclusions) and deductive applications of mathematics take place in just this context.

perception of the girl justify (P). Transmission fails because the inference is, in a sense, circular. No such circularity is present in deductive applications of mathematics. My justification for the claim that graph *G* has no Eulerian path does not presuppose independent knowledge that it is impossible to walk through Königsberg crossing each bridge once.

Alternatively, one could point to the fact that logical truths like "it is raining or it is not raining" transmit justification to the conclusions of arguments they appear in and yet are not defeasible by empirical evidence, thus (2) is false. It is not obvious, however, that logical truths transmit any justification over and above what is already contained in knowledge of the validity of the inferential link itself (e.g. *p* or not-*p*). But even granting that they do, this generalizes rather than defeats the problem: if logical truths transmit justification to empirical conclusions, then by the same reasoning as before, they should be empirically defeasible—yet they are not. Note that it is not the applicability of *logic as such* that is thereby problematized, for logic generally does not figure as a premise in arguments, but only the narrower applicability of logical truths as genuine premise-like contributors.[34]

Against premise (3) (mathematical statements are not defeasible by empirical evidence), one can construct counterexamples in the style of:

> Even though I have carefully worked through a mathematical proof that *p*, I will not know *p* if I get empirical evidence that I am mad, or that human or mechanized experts have agreed that not-*p*, or that there is *a priori* gas[35] in the area, or that I have made lots of mistakes using a very similar proof technique in the past, or that lots of smart people are inclined to laugh when they hear my proof. (Hawthorne 2013, 209)

Mathematical statements would thus appear to be sensitive to some empirical evidence. Most of the types of evidence mentioned in the quote, however, are *undercutting* or even *higher-order* defeaters (Moretti and Piazza 2018). These attack, respectively, the link between the justification and the belief (e.g. if I have made mistakes with the proof technique in the past, then it is doubtful that *p* actually follows from the proof) and whether the justification was there in the first place (whether my first-order beliefs are the result of some flawed process, e.g. influenced by madness or *a priori* gas). These are not the kinds of defeaters at issue here.

[34] The applicability of logic as such could be problematised by the remark above about transmission of justification presuming knowledge of and thus justification for the inferential link between the premises and the conclusion. However, given that *modus ponens* and universal instantiation are constitutive of deductive inference as such (Finn 2021; Boghossian and Wright 2024), I don't see this going through all the way. To treat these logical rules as empirically defeasible would threaten the framework within which defeaters and defeasibility are diagnosed overall.

[35] *A priori* gas "induces the phenomenology of blatant obviousness for false propositions" (Hawthorne 2013, 205).

More importantly, all of Hawthorne's cases impair a particular agent's epistemic access to a proof while leaving the proof itself untouched. The fact that I cannot follow Wiles' proof for Fermat's last theorem does not make the proof invalid. The distinction is between personal justification (whether a given agent is in a position to know *p*) and mathematical justification as such (whether *p* is provable and known to be so within the mathematical community). Hawthorne's counterexamples concern the former, the problem of deductive applicability is about the latter. The kind of empirical defeasibility relevant here is whether empirical evidence could bear on the standing of a mathematical statement within the practice of mathematics itself—whether experimental results could give the mathematical community grounds to revise or reject a result.

Ways of Answering the Problem

The problem of deductive applicability places the justification of mathematical statements at the centre of the stage. What follows is a brief overview of some influential philosophical accounts of mathematics and the responses they offer.

The most straightforward answer to the problem is given by Quine-style empiricism about mathematics (Quine 1951).[36] Confirmational holism and the fall of the analytic-synthetic distinction, which offered a clear boundary between mathematics and the rest of science, imply that mathematical statements are (dis)confirmed by experience together with everything else, thus they are in principle subject to empirical defeaters. Mathematical statements only appear to be independent of empirical considerations, because they are more central in our system of knowledge: revising them would entail revising a whole lot more, which is psychologically taxing. Quine thus rejects premise (3) or the assumption that mathematics is not defeasible by empirical evidence. Kitcher (1985), while diverging from Quine on important points, arrives at a similar in-principle but rarely-in-practice empirical defeasibility. Colyvan, starting from the indispensability of mathematics to empirical science and the ontological commitment this entails to mathematical objects,[37] goes even further:

[36] Not to be confused with Lakatos-style quasi-empiricism (Lakatos 1976), which admits the fallibility of mathematics in the face of intra-mathematical considerations but does not open this up to empirical defeasibility.

[37] In argument form: we ought to have ontological commitment to all and only the entities that are indispensable to our best scientific theories, mathematical entities are indispensable to our best scientific theories, thus we ought to have ontological commitment to mathematical entities (Colyvan 2024).

> … mathematical propositions are known *a posteriori*, because the existence of mathematical objects can be established only by empirical methods—by their indispensable role in our best scientific theories. (Colyvan 2001a, 116)

Empiricism about mathematics undoubtedly succeeds in answering the problem of deductive applicability and this success derives from the indispensability debate. Confirmation and disconfirmation are symmetric: if mathematics is empirically confirmable, it is also empirically disconfirmable. Empirical defeasibility (via rebutting defeaters) amounts to the justification of mathematical statements being undermined by empirical evidence and if justification is undermined, so is the credibility of the statement itself. Thus, defeasibility falls under disconfirmation. Criticisms of the claim that mathematics is indispensable to science and the ontological commitment this entails are, however, well recorded (Colyvan 2024). Maddy's (1992, 1995) discussions about the tension between naturalism and confirmational holism in particular may provide grounds for rejecting the latter.[38]

On the nominalist side, Field's (1980) "hard road" (Colyvan 2010) involves rejecting the indispensability of mathematics for empirical science. Field's nominalisation programme entails rewriting physical theories in a nominalist manner (involving no reference to or quantification over mathematical objects, such as sets, functions, numbers) and showing the conservativeness of mathematics. A mathematical theory is conservative if any inference from nominalistic premises to a nominalistic conclusion that can be made with its help could be made without it (Field 1980, 16). If Field succeeds, then adding mathematics to a physical theory does not lead to any results that could not be derived from the physical theory alone, thus premise (1) of the problem of deductive applicability is false. Mathematics simplifies derivations but adds no justification of its own and is ultimately dispensable from physics. Whether Field's programme succeeds is, however, doubtful (Bueno 2020).

Not all nominalists fare as well. "Easy road" nominalists admit indispensability but deny that it gives grounds for believing in the existence of mathematical objects (Colyvan 2010). Azzouni, for example, takes mathematical objects to be 'ultrathin' posits, i.e. mere postulations: "A mathematical subject with its accompanying posits can be created *ex nihilo* by simply writing down a set of axioms" (2004, 127). Mathematical knowledge is grounded in proofs from such axioms and in (Azzouni 1994) he argues explicitly for a strain of

[38] The label 'platonism' is generally reserved for the metaphysical position without further epistemological commitments. Given that the indispensability argument results in this empiricist epistemology, platonists who subscribe to it have an answer to the problem of deductive applicability. Platonists who hold that mathematics is not defeasible by empirical evidence do not.

mathematical apriorism. Thus, for Azzouni, mathematics is indefeasible by empirical evidence, and the problem of deductive applicability stands.

More broadly, any account that severs ontological commitment to mathematical entities from indispensability faces the same difficulty. Since confirmation and disconfirmation are symmetric, if the empirical success of a mathematised scientific theory does not confirm the existence of mathematical entities, then its failures cannot disconfirm it either. But then existence claims like "there exists an even prime" are empirically indefeasible and if existence claims are insulated from empirical defeat, it is hard to see why other mathematical statements should be any different.

Of the historical positions on mathematics, logicism takes mathematics to be reducible to logic and, as we saw in the last section, logical truths face the problem of deductive applicability just as mathematical statements do. Formalism, which replaces mathematical truth with derivability within a consistent system, insulates mathematics from empirical defeat. Further, it remains to be explained why derivability within a consistent system can do justificatory work in empirical science at all. Intuitionism, which grounds mathematics in constructive proof, also insulates it from empirical defeat and makes its role as a genuine justificatory contributor suspect.

In summary, the problem of deductive applicability admits three types of responses, corresponding to the three premises of the argument. One can reject premise (3) by accepting that mathematics is in principle empirically defeasible, *à la* empiricism. One can reject premise (1) by showing that mathematics is ultimately dispensable from empirical science and thus never genuinely transmits justification to empirical conclusions, *à la* Field. Or one can reject premise (2) by providing an account of how justification can transmit from a premise to an empirical conclusion without that premise thereby becoming open to empirical defeat. This would require explaining what is special about the justificatory role mathematics plays that exempts it from the usual consequences of transmission. No account surveyed here takes this third path. Given that any philosophical account of mathematics needs to explain applicability, the problem serves as a test of adequacy: where an account fails to take one of these paths, there is more work to be done.

Conclusion

The aim of part III was to put forward a version of the problem of applicability centred on deductive applicability. This version of the problem relies on two notions from epistemology: transmission of justification and defeaters. Mathematical statements, appearing as premises in deductions with empirical conclusions, transmit justification to those conclusions. Empirical defeaters for the conclusions thus undermine the justification of the mathematical premises—yet mathematics is generally assumed to be indefeasible by empirical evidence. In short, why can mathematical statements transmit justification to empirical conclusions, given that their justification is not defeasible by empirical evidence?

I defended this problem against several objections: that mathematics functions as inference rules rather than premises; that empirical science has used unjustified mathematics; that the deductivity of deductive applicability guarantees the indefeasibility of its conclusions; that logical truths face the same situation; and that mathematical statements are defeasible by certain empirical evidence.

I then considered how philosophical accounts of mathematics fare against the problem. Empiricism has the most straightforward answer, since the problem bears directly on what is at stake in the indispensability debate. If successful, Field-style nominalism deflates the problem. Easy road nominalist accounts, and the historical positions of logicism, formalism and intuitionism, have work to do.

Conclusion

This thesis argued for the applicability of mathematics as an epistemic problem. It did so by criticising alternative, non-epistemic, formulations of the problem of applicability, by reducing different types of applicability to deductive applicability and by proposing a new version of the problem that centres deductive applicability. The first two moves lead to the conclusion that if there is a problem of the applicability of mathematics, then it originates from the justification of the statements of mathematised science, thus it is epistemic. The last move is one way of fleshing out this source of the problem, in terms of the transmission of justification from mathematics to empirical science and the indefeasibility of mathematics in light of empirical evidence. This problem serves as a measure of adequacy for any epistemology of mathematics.

One important limitation was set to the thesis: the problems of applicability I considered and the problem I put forward all centred mathematics, rather than empirical science. Thus, the applicability of mathematics is an epistemic problem for the philosophy of mathematics. Throughout the thesis, some versions and aspects of the problem were mentioned and deemed outside of the scope of this thesis on the grounds of this focus on mathematics. Mainly, these were the problem of how physics, which reasons from idealised, i.e. false, assumptions can lead to empirically adequate conclusions and the role of physical interpretation in the so-called non-standard applications of mathematics.

The model of deductive applicability I have put forward can help clarify these aspects to some degree. Idealisations and physical interpretations concern the formulation and interpretation of mixed statements (statements that combine mathematical and empirical concepts). Explaining how reasoning from false premises can yield empirically adequate conclusions will require a story about how the resulting mixed statements are interpreted. Physical interpretation is likewise a matter of how mathematical concepts are connected to empirical ones. The deductive model does not resolve these questions, but it locates them.

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Abstract

This thesis examines the problem of the applicability of mathematics in empirical science, commonly associated with Wigner's "unreasonable effectiveness of mathematics." The central aim is methodological: to determine how the problem of applicability ought to be formulated. The thesis argues for the applicability of mathematics as an epistemic problem. In the first part, some influential formulations are rejected because of their reliance on overly narrow or untenable assumptions about mathematics, empirical science, or their relation, specifically those focused on mathematics-led discovery, the role of aesthetics in mathematics, the metaphysical gap between mathematics and physics, and the use of idealisations. The second part looks at the variety of ways in which mathematics is applied—semantic, descriptive, deductive, non-standard, unificatory, and explanatory—and argues that all but semantic applicability can be understood through deductive applicability, namely the use of mathematical statements as premises in deductions with empirical conclusions. The final part develops an epistemic version of the problem centred on deductive applicability: mathematical statements, generally taken to be empirically indefeasible, transmit justification to conclusions that are defeasible by empirical evidence. The thesis concludes by defending this formulation of the problem and examining how several influential philosophies of mathematics respond to it.

Annotation

This thesis investigates the applicability of mathematics in empirical science and asks how the problem of applicability should be formulated. It argues that many existing versions of the problem rely on problematic assumptions about mathematics or physics. The thesis defends a deductive account of applicability, according to which mathematical statements function as premises in inferences with empirical conclusions. It ultimately formulates the problem as an epistemic one concerning the transmission of justification from empirically indefeasible mathematical statements to empirically defeasible scientific conclusions.

Keywords: applicability of mathematics; Wigner's problem; deductive applicability; philosophy of mathematics; epistemology

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