Philosophy of Mathematical Physics

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Dedicated to the memory of Jan Hilgevoord (1927–2025)

Abstract

A sound philosophy of mathematical physics balances a philosophy of mathematics with a philosophy of physics, sharpening the general applicability problem of mathematics by also taking care of: (i) the early modern 'mathematization of the world picture'; (ii) the remote and theory-laden character of the targets of mathematical models of modern physics; and (iii): Wigner's 'unreasonable effectiveness of mathematics in the natural sciences'. Guided by a historical survey, I propose that theories of mathematical physics (such as general relativity, quantum mechanics, and statistical mechanics) are meaning-constitutive *a priori* constructions, conventional but far from arbitrary and best described as hypothetical. Their models subsequently mediate between theory and nature, that is, between the *a priori* and the *a posteriori*. Models mediate by playing the role of Wittgensteinian yardsticks or objects of comparison to be held against nature as represented by data models, where the comparison is made via *surrogative inference*. This balancing act compromises realism.

Contents

1 Introduction

> To an unappreciated degree, the history of Western Philosophy is the history of attempts to understand why mathematics is applicable to Nature, despite apparently good reasons to believe that it should not be. (Steiner, 2005, p. 625)

This *Element* hopes to contribute to these attempts from the perspective of a mathematical physicist. But before getting to this specific perspective, why is the applicability problem so difficult in the first place? Plato and Aristotle were troubled already by "everyday" mathematical objects like triangles, noting for example that the irrationality of $\sqrt{2}$ could hardly be inferred empirically from a drawn right-angled triangle with short sides of unit length. Kant and Wittgenstein even stumbled over $7+5=12$ and $25\times 25=625$, whose apparent reference to "numbers" is puzzling: despite a lifetime of effort Frege did not even manage to define the number one. These problems are difficult enough and they remain unresolved. Mathematical physics (whose scope and nature I shall address shortly) adds three further complications to this already nontrivial general applicability problem.

1. The first of these was enunciated by the Dutch historian of science Dijksterhuis:

> classical mechanics is mathematical not only in the sense that it makes use of mathematical terms and methods for abbreviating arguments which might, if necessary, also be expressed in the language of everyday speech; it is also mathematical in the much more stringent sense that its basic concepts are mathematical concepts, that mechanics itself is a mathematics.[1] (Dijksterhuis, 1961 p. 499)

This goes well beyond Galilei's famous claim that 'this grand book, the universe (...) is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures.' Galilei's mathematics could still reasonably be argued to be instantiated within nature in a straightforward way (bracketing the issue of idealization, which I shall discuss below). But Newton's mathematics could not. His introduction, in *Principa*, of the gravitational force as a mathematical entity blasted the revolutionary but, until then, still naive 'mathematization of the world picture' by Galilei (as well as by his predecessors like Plato, Ptolemy, Copernicus, and Kepler in so far as astronomy and cosmology were concerned).[2] Take the following two paths initiated by Newton in 1687:

- From his forces to the potentials introduced by Legendre, Laplace, and Poisson around 1800, via the metric tensor in Riemannian geometry, to the Lorentzian metrics in Einstein's theory of general relativity from 1915 (which neatly assembles 10 potentials for the gravitational field into a single geometric object).
- From his idiosyncratic mechanics to Euler's rewriting thereof in terms of the calculus, to Lagrange's analytical mechanics in high-dimensional configuration spaces, to the infinite-dimensional complex Hilbert spaces of quantum mechanics.

[1]The curious form 'is a mathematics' at the end also occurs in the Dutch original ('is een wiskunde').

[2]At the end of his book (and after a lifetime of study) Dijksterhuis (1961) concludes that the *mechanization* of the world picture (which is his title) consisted in the *mathematization* of the world picture.

Except on some extreme form of mathematical realism, these theories describe properties that go well beyond those that may reasonably be held to be instantiated by natural objects.

2. The second complication is closely related the first, namely the *status of models*.

The precise meaning of mathematical models of physics will be discussed in §5; for now, one may think of spacetimes solving Einstein's equations as models of general relativity; of the Standard Model of quantum field theory (defined rigorously via renormalised perturbation theory); or of Hamiltonians defined on Hilbert spaces in quantum mechanics; or of specific stochastic processes as models of classical statistical mechanics. Outside mathematical physics, in the general philosophy of science models typically have *given*, unambiguous, and conceptually clear *targets*, such as ships (Frigg, 2022), cities, the London Tube, people modeled by portraits (Suárez, 2024), or the deer population of Princeton (van Fraassen, 2008). But mathematical models of modern theoretical physics often do not have given and unambiguous targets: their would-be "targets" are model- or theory-dependent constructions. Opposite to the situation in general science just discussed, models in mathematical physics are typically much clearer than their alleged targets. For example, two iconic cases that have been studied in detail in the history and philosophy of modern physics are the weak neutral currents from the Standard Model and the black hole in M87 imaged by the Event Horizon Telescope. In both cases (and many others), the idea of simply "observing" some natural phenomenon (the alleged "target"), which may then be modeled by some theory, or extracting "raw data" from it, has been compromised to such an extent that the logical positivists must turn in their graves.[3] Why?

> We will see in detail how much of the burden of experimental demonstration has shifted to data analysis. For it is in this stage, in the sorting of signal from background, that twentieth-century experimental physics has deviated most sharply from the earlier concept of a demonstration. (...) In the longest term this may be the sea change of twentieth-century experimental physics. (Galison, 1987, p. 151)

This goes beyond the cliché that (almost) all observations are "theory-laden".[4] The point is rather that apart from the role of theory in setting up an experiment, it is also the result (i.e., the 'signal') that is at least partly created by theory, as its separation from the background is typically accomplished by letting the very theory that is supposed to predict the result simulate the background (so that a different theory used for this purpose may well annul the intended result). Thus it is not very clear at all what a "phenomenon" is, and hence what the "target" of a mathematical model relevant to modern physics should be. This also punctures the possibility of a referential interpretation of such models.

On my view, weak neutral currents in particle physics are part of some model (like the Standard Model), whose purpose it is to make inferences about a data model. Similarly, black holes are theoretical constructions within general relativity, which may be used to explain or understand specific data models extracted from astronomical observations.

[3] See Pickering (1984) and Galison (1987) about weak neutral currents and Doboszewski & Elder (2025) and Ochigame, Skulberg, & van Dongen (2025) about the EHT.

[4] See also Bokulich and Parker (2021). This is not only a twentieth-century phenomenon: for example, all attempts to see "pure" planetary motion as uniform and circular from Plato onwards had to deal with retrograde motion (Kuhn, 1957), which also involves 'the sorting of signal from background'.

This typical lack of a clear connection between mathematical models of physics at very small or very large scales and targets is closely connected to the previous complication for the philosophy of mathematical physics, i.e., the lack of instantiation in the natural world of its mathematical objects; if there had been clear referents for those, there might have been clear physical targets. In some cases there are; but these are exceptions.

3. A third point that makes a philosophy of mathematical physics not just a special case of a philosophy of applied mathematics was famously raised by Wigner,[5] viz.:

    (a) 'Mathematical concepts turn up in entirely unexpected connections.'

    (b) 'Moreover, they often permit an unexpectedly close and accurate description of the phenomena in these connections.' (Wigner, 1960, p. 2)

Wigner's most convincing example of his first point is the use of complex numbers in quantum mechanics, but there are many others, starting with the appearance of conical sections in Newtonian gravity.[6] Wigner's points add up to his title, a claim: *The unreasonable effectiveness of mathematics in the natural sciences*. I return to this in §6.

Understanding why mathematics is applicable to Nature therefore seems to be made more *difficult* by focusing on mathematical physics. But this focus also makes it *easier* by reducing the embarrassment of riches in the philosophies of mathematics and science. For example, the points of Dijksterhuis and Wigner make empirically oriented philosophies of mathematics like Aristotle's or Mill's unattractive, whilst the applicability of mathematics altogether is difficult to explain for both Platonism (as already pointed out by Aristotle in *Metaphysics M* and *N*) and for purely psychological philosophies like Brouwer's intuitionism. Likewise, all non-mathematical philosophies of science can safely be ignored.

As a first step towards its philosophy, let us try to *define* mathematical physics.[7] As in "the philosophy of mathematics" (scare quotes!), there is some circularity involved here, since (unlike for example paleontology or arguably even physics) defining what mathematics means is already part of its philosophy. To cut this circle I agree with Netz (1999), p. 2, that what unites a scientific community is not shared *beliefs* but shared *practices*. Thus I propose that mathematical physics is *defined* by its shared practices, and that its *philosophy* consist of the (descriptive or normative) study of the various beliefs that are or should be held about these practices. So what are these shared practices?

> *Journal of Mathematical Physics* features content in all areas of mathematical physics. Articles focus on areas of research that illustrate the application of mathematics to

[5]The secondary literature on Wigner (1960) is quite large, including e.g. Maddy (2007), Bangu (2012), Islami (2017), Ferreirós (2017), Rédei (2020), and Bueno and French (2018), which also criticises the earlier analysis by Steiner (1998). All of these works are also relevant to the general topic of this paper.

[6]This example was already given by Dijksterhuis (1961), footnote 5 to his last chapter—whose Dutch original appeared in 1950, well before Wigner. His context was similar, namely the long delay between the invention of some mathematical concept and its use in the natural sciences (which he finds unremarkable). Frank (1937), §V, also anticipated much of Wigner's analysis as well as its possible resolution.

[7]An integral history of mathematical physics remains to be written. There is of course a substantial body of literature on Newton and his predecessors (who will be identified below). His greatest successors, such as Euler, Laplace, Riemann, Poincaré, Hilbert, Weyl, von Neumann, and Kolmogorov, are typically studied by historians of mathematics. One may also turn to case studies like Lützen (1995, 2010, 2011), Pulte (2005), Rédei and Stöltzner (2001), Darrigol (2005), and Landsman (2021).

> problems in physics, the development of mathematical methods suitable for such applications, and the formulation of physical theories.[8]
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> The mission of *Communications in Mathematical Physics* is to offer a high forum for works which are motivated by the vision and the challenges of modern physics and which at the same time meet the highest mathematical standards.[9]

These modern century benchmarks plus general history suggest a timeless answer:

Mathematical physics consists of the use of rigorous and sophisticated mathematics (by contemporary *standards) for the study of inanimate nature, in which mathematics is not just instrumental but also plays a role in conceptualizing and understanding physics.*

Although mathematical physics is often said to have started with Newton, my definition ranges from the 'mixed sciences' of Antiquity, i.e., astronomy, harmonics, optics, and mechanics, to the classics of 20th century mathematical physics.[10] Since the former were already discussed by Aristotle, who was a philosopher of both mathematics and science (probably even the first philosopher of science), my definition also makes him the first philosopher of mathematical physics–although, as we shall see, he denied its very possibility! This makes a historical approach both unavoidable and inspiring, so that in sections 2 and 3 I present a survey of (implicit or explicit) philosophies of mathematical physics from Plato and Aristotle to Wittgenstein and Hilbert,[11] including the philosophical views of Galilei and Newton (and more briefly Ptolemy, Copernicus, and Kepler), as well as some relevant parts of the history of mathematical physics. This history consisted of a gradual transition from what Lakatos called the *Euclidean model*, in which premisses of a deductive enterprise are considered self-evident and true à la Aristotle (which model Newton still used but simultaneously undermined), to the current hypothetico-deductive model, in which premisses are merely assumptions. This transition was driven by:

- the fall of what I call "pythagoreanism", i.e., the idea (having roots in Pythagoras, Plato, as as well as Aristotle) that mathematical objects are instantiated in nature;
- the increase of rigour (in mathematics) and abstraction (in both mathematics and physics) that arose in the 19th century for the needs of both research and teaching;
- the rise of models since the 19th century and ensuing return to the time-honoured practice of surrogative reasoning (which may be traced back to Ptolemy at least).

These developments clearly obscure the relationship between (mathematical) theory and (empirical) reality. Hence, then and now, they require a reconceptualisation of this relationship. One such approach, originally taken up for different philosophical reasons, was initiated by Kant and his notion of the *a priori*. This will be developed in §4, in which,

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[8] See the journal homepage at `https://pubs.aip.org/aip/jmp`.

[9] See the journal homepage at `https://link.springer.com/journal/220`.

[10] Such as Weyl (1919, 1928), von Neumann (1932), Streater and Wightman (1964), Ruelle (1969, 1978), Reed and Simon (1972–1978), Hawking and Ellis (1973), Haag (1992), and Choquet-Bruhat (2009).

[11] Wittgenstein never wrote about mathematical physics as such, but I will use some key insights from his late philosophy of language, which *was* influenced by his philosophy of mathematics (Kienzler, 1997).

perhaps surprisingly, after Kant my main inspiration has been Wittgenstein.[12] Poincaré, discussed only briefly in this *Element*, could have been an alternative and indeed far more obvious bridge between some neo-Kantian *a priori* and mathematical physics,[13] but I prefer (late) Wittgenstein not only because in my view the *a priori* in mathematical physics consists of mathematical theories construed as language games based on rules, but also because these rules relate to reality via Wittgensteinian *objects of comparison* or *yardsticks* to be held against nature: as developed in detail in §5, the *models* of such theories (broadly conceived, ranging from exact solutions of exact equations provided by the theory to approximate solutions of approximate equations) play the role of these yardsticks, and hence mediate between theory and nature, i.e., between the *a priori* and the *a posteriori*.[14] Here, models are not held against natural phenomena themselves (which would involve the difficulty of comparing a mathematical structure with a non-mathematical one), but against *data models* thereof, and the actual comparison is done via *surrogative inference*. This methodology also resolves (or at least alleviates) the "target" problem mentioned earlier, in that even ambiguous targets induce data models.

The *a priori* theories are supposed to be meaning-constitutive for the concepts and terms appearing in their models. Though *conventional* both in a strict Hilbert-style sense of a description of a game,[15] and in a looser Poincaré-style analysis of the relationship between physics and mathematics, the rules defining mathematical theories of physics are by no means *arbitrary*, since they are inspired not only by empirical science, but also by such things as the autonomous development of mathematics, aesthetic criteria, and the desire to overcome internal contradictions and/or clashes with new experiments in earlier theories, etc. Hence the word 'hypothetical' describes the situation better than the usual 'conventional', if only to stress that I do not endorse concepts of "truth by convention".[16]

Thus §4 and §5 contain a proposal for a philosophy of mathematical physics that intends to clarify the relationship between mathematics, physics, and empirical phenomena that was compromised by the historical development just summarised. In §5 I also discuss some examples from actual mathematical physics (involving black holes and elementary particle physics). In §6 I give a short philosophical conclusion in which I explain how I tried to combine "best practices" from Aristotle onwards into what I see as an inescapable attempt to formulate a historically informed philosophy of mathematical physics. I also briefly discuss the challenge the philosophy proposed here poses to realism; again, at least for me–but I may be a victim of confirmation bias–the history of the subject points in such a clear direction that some serious loss of realism seems unavoidable (and unregretable). This turns out to lead back to Wigner's problem, which I address, and hopefully bring forward a little, alas without resolving it: some "miracle" remains (also for realists!).

[12] Coffa (1991) traces the corresponding development of the *a priori* from an epistemic to a semantic concept, which is the one relevant here.

[13] The primary sources are Poincaré (1902, 1905). For secondary literature see Friedman (1999), DiSalle (2002), Ben-Menahem (2006), Ivanova (2015), Stump (2015), Warren (2022), and Povich (2024).

[14] This resonates and sympathises with a 'models as mediators' approach (Morrison and Morgan, 1999), but that approach assumes that models are much more independent of theories than I would allow.

[15] See Landsman & Singh (2025) for a detailed discussion of this point.

[16] See Pulte (2005), Friedman (2001, 2002), Ben-Menahem, (2006), Kuusela (2019), Warren (2020, 2022), and Povich (2024).

2 Historical survey: Plato to Galilei

Returning to my opening quotation of Steiner, I start with the ‘apparently good reasons to believe why mathematics should not be applicable to Nature.’ In so far as we still have a record (I restrict myself to the Western philosophical tradition) these reasons go back to Plato (or, more generally, to Antiquity). Plato noticed for example that the irrationality of $\sqrt{2}$ (which was well known at the time) follows from proof but could never be inferred empirically from a drawn triangle.[17] More generally, Plato noted a discrepancy between the certainty and timelessness of the theorems of mathematics and the uncertainty of empirical knowledge, mirrored by the difference between the ideal objects these theorems are supposed to be about, such as the “breadthless” lines soon to be defined by Euclid (precursors of whom were surely known to Plato) and the perfect figures (like circles and triangles) made thereof, and what one actually sees or can construct in the natural world.[18]

As summarised by the great classicist Burnyeat (1987), p. 221, the Platonic argument, though perhaps never stated literally in this form by Plato himself, may be taken to be:

(1) The theorems of mathematics (geometry, astronomy, etc.) are true;

(2) They are not true of physical objects in the sensible world;

Therefore,

(3) They are true of ideal objects distinct from sensible things.

The question, however, is which conclusions Plato drew from this. The traditional answer is that he postulated an ineffable realm in which perfect mathematical object reside:

> But none of the poets of this world has ever yet sung the praises of the region beyond the heaven, nor will they ever sing them in a worthy manner. But we must dare to speak the truth anyway, especially when truth is our theme. So this is how it is: this region contains the colourless, utterly formless, intangible being that actually is, with which the realm of true knowledge is concerned, seen only by reason, the pilot of the soul. (*Phaedrus*, 247cd, translation by Robin Waterfield, *Oxford World Classics*)

Yet it is unclear whether Plato himself actually meant the realm he just described to be the home of mathematical objects.[19] In any case, the idea of an (alleged) “Platonic” realm outside space, time, and the causal order of the natural world, which mathematicians supposedly discover and describe though some special faculty of the mind, but which exists

[17] See e.g. von Fritz (1945) and Novak (1982, 1983) for Plato’s familiarity with the irrationality of $\sqrt{2}$, and also $\sqrt{3}$ all the way up to $\sqrt{17}$. Relevant dialogues include *Menon*, *Theaetetus*, and *Republic*.

[18] In developing his views Plato was influenced by his tumultuous political environment (Atack, 2024).

[19] Landry (2023) is quite outspoken that he did not. See however Burnyeat (1987, 2000), and Panza & Sereni (2013), §1.1. Apparently, much depends on one’s selection of texts and even of translations. A relevant passage is *Republic* 510e–511a, in which Socrates tells Glaucon: ‘The very things which they [i.e., the mathematicians] mould and draw, which have shadows and images of themselves in water, these things they treat in their turn as only images, but what they really seek is to get sight of those realities which can be seen only by the mind’ (translation: Paul Shorey, Loeb Classical Library). Landry (2023, pp. 11–12), who uses a different translation, takes this passage to mean that mathematical objects are hypothetical, existing only in an “if–then” sense. But one can hardly blame earlier interpretations in which the mathematical ‘realities’ are put in Plato’s ‘region beyond the heaven’, which only the mind can see.

independently of these mathematicians (or indeed of any natural being altogether), should be distinguished from the related Platonic idea (often claimed to go back to Pythagoras,[20] a dubious attribution) to the effect that the universe itself has a mathematical structure, as in the *Timaeus*.[21] To disambiguate the term "Platonism" (and similarly "realism" in the philosophy of mathematics), we call the alleged ineffable version "platonism" and its naturalistic version "pythagoreanism" (both deliberately with lower case p).

As far as I know, until the second half of the twentieth century not a single important mathematical physicist was a platonist, but many, including Galilei and Newton, endorsed some version of pythagoreanism. Platonism with small p became popular only in the twentieth century,[22] and hence it played practically no role during the period I am going to describe, except—and this is important—as a foil for both Aristotle and Wittgenstein.

I proceed with Aristotle, whose inclusion may seem surprising at first sight, since in *Metaphysics E* he denied the applicability of mathematics to what we now call physics!

> Given the definitions of natural philosophy as concerning those things that change and have an independent existence, and the definition of mathematics as concerning those things that do not change and have no independent existence, and given that the task of explanation is to account for physical and mathematical questions in terms of the essential properties of the physical and mathematical domains respectively, there can be no role for mathematics in physical explanation. Natural philosophy is an autonomous discipline, reliant only on its own principles. (Gaukroger, 2020, p. 61)

Denying its very possibility, Aristotle may be claimed to have held up progress in mathematical physics (and hence in physics as a whole) for almost two millennia! On the other hand, his philosophy of mathematics and the mixed sciences leaves room for a more positive and lasting contribution to the philosophy of mathematical physics.[23] To see this, let me quote two famous passages from his *Physics* and his *Metaphysics*, respectively:

> We have next to consider how the mathematician differs from the physicist or natural philosopher; for natural bodies have surfaces and occupy spaces, have lengths and present points, all which are subjects of mathematical study. (...) Physicists, astronomers, and mathematicians, then, all have to deal with lines, figures and the

[20]See Burkert (1972) for a debunking of Pyhthagoras as a historical figure and founder of mathematics.

[21]See Gregory (2022) for a recent account. Plato's cosmos is a geometric model of the heavenly bodies based on arrangements of circles, predating Ptolemy's infinitely more sophisticated model in the *Almagest*.

[22]Top pure mathematicians from the twentieth century who strongly endorsed platonism include Hardy and Gödel, for example: 'I believe that mathematical reality lies outside us, that our function is to discover or *observe* it, and that the theorems which we prove, and which we describe grandiloquently as our 'creations', are simply our notes of our observations.' (Hardy, 1940); 'The objects and theorems of mathematics are as objective and independent of our free choice and our creative acts as is the physical world. (...) Thereby I mean the view that mathematics describes a non-sensual reality, which exists independently both of the acts and [of] the dispositions of the human mind and is only perceived, and probably perceived very incompletely, by the human mind.' (Gödel, 1951). In the second half of the century also leading mathematical physicists like Penrose and Connes, were outspoken platonists; see for example Penrose (2004) and Connes, Lichnerowicz, and Schützenberger (2001). The technical arguments for platonism forwarded by Connes and Penrose, based on Gödel's incompleteness theorems, are in my view circular (Landsman & Singh, 2025, §4), and in any case were not used by Gödel himself. But their strongest commitment to platonism seems emotional, and as such is shared by many if not most modern mathematicians.

[23]See e.g. *Physics* B2, 193–194, and *Metaphysics* M3, 1078a, and Bostock (2012) and Mendell (2019).

> rest. But the mathematician is not concerned with these concepts *qua* boundaries of natural bodies, nor with their properties as manifested in such bodies. Therefore he abstracts them from physical conditions; for they are capable of being considered in the mind in separation from the motions of the bodies to which they pertain. (*Physics* B2, 193–194)[24]

> The best way to conduct an investigation in every case is to take that which does not exist in separation and consider it separately; which is just what the arithmetician or the geometrician does. For man, *qua* man, is one indivisible thing; and the arithmetician assumes man to be one indivisible thing, and then considers whether there is any attribute of man *qua* indivisible. And the geometrician considers man neither *qua* man nor *qua* indivisible, but *qua* something solid. (*Metaphysics* M3, 1078a)[25]

Thus mathematicians and physicists look at the very same natural object X; but the former limit their attention to its mathematical (i.e., *quantitative*) properties, whereas the latter only consider its physical (that is, *qualitative*) properties, to be explained via causes, definitions, and essences. In (Latinised) Aristotelian parlance, a mathematician looks at a bronze sphere *qua* sphere, whereas a physicist looks at it *qua* bronze.[26] This act of separation (or abstraction) led Aristotle to conclude that 'mathematics deals not with separable Forms but with quantitative characteristics which exist only as embodied in matter';[27] in other words, mathematical objects are instantiated by physical objects and hence form part of the natural world. This, then, obviates the need for a "platonic" realm, although it is not clear whether Aristotle believed that physical objects at least in some cases *perfectly* instantiate mathematical properties like a straight, breadthless line or a sharp triangle made thereof; which they should in order to sideline Plato.[28]

Aristotle's barrier between mathematics and physics comes to a head in ancient astronomy. This consisted of a *mathematical* part concerning the motions of the Sun, the Moon, the planets, and the stars in the sky, whose goal it was to make *predictions* (e.g. in the interest of astrology), and a *physical* part concerning the constitution of the heavenly bodies and the causes of their motion. Duhem (1908) proposed that astronomy was purely instrumentalist in being intended to just "save the phenomena", in supposed contrast with the realism of Copernicus, Kepler, and Galilei that heralded the scientific revolution.

But this view has been criticised from the early 1960s onwards.[29] To the extent that Greek mathematical astronomy was indeed instrumentalist, it was probably the first example of what we now call *(inferential) surrogative reasoning*, in that the only role of the mathematical models was to infer the future positions of the heavenly bodies from the current and past ones. In any case, the mathematics was separate from the (astro)physics.

On the positive side, Aristotle's theory of demonstrative sciences in the *Posterior Analytics*, which is an axiomatic-deductive call to arms, cast a long and beneficial shadow:

> The details of Aristotle's theory are obscure, but its outline is clear: a demonstrative science is an axiomatised deductive system comprising a finite set of connected

[24]Transl. F. M. Cornford and P. H. Wicksteed, *Loeb Classical Library* (Harvard University Press, 1957).
[25]Transl. H. Tredennick, *Loeb Classical Library* (Harvard University Press, 1961).
[26]More generally "X *qua* Y" is short for "X in the respect that X is Y" (Mendell, 2019).
[27]McMullin (1985), p. 249, footnote 5.
[28]See Bostock (2012) and Mendell (2019) for balanced views and references.
[29]See especially Mittelstraß (1962), Lloyd (1978), and Jardine (1979).

> demonstrations. (...) the premisses of a demonstration must be (a) true; (b) necessary and universal; (c) immediate; and (d) causally related to the conclusion, which must itself be true, necessary, and universal. (Barnes 1969, p. 123)

Such was the situation still encountered by Newton, to whom I will return. But before getting there, I merely jump about four centuries after Aristotle and discuss the greatest text in mathematical physics (as I construe it) before *Principia*, namely Ptolemy's *Almagest*.[30] Consisting of thirteen books, like Euclid's *Elements*, and written in a similar deductive mathematical style, Ptolemy's surely saw the *Elements* as a benchmark (as did Newton, see below). The *Almagest* was a device for calculation of positions of the heavenly bodies from earlier ones and mathematical principles. But what makes it unprecedented is that 'the parameters for his planetary models are derived explicitly by geometric techniques from specific dated observations', as well as the fact that Ptolemy 'reduces his models to tables, and the means by which the tables are constructed are explicitly given–the appeal is to geometry, not the arithmetical schemes of the Babylonians' (Goldstein, 1997, p. 1). As far as we know, the *Almagest* was the first book that constructed a serious mathematical model of some part of the natural world and compared model and world; as such it was the first major work of mathematical physics. Ptolemy's philosophy was as follows:[31]

> For Ptolemy, it seems that the mathematical agreement of the models (here called "hypotheses") with the observational data is evidence for the physical reality of the models, even if they seem unduly complicated. (...) According to Ptolemy the phenomena are 'real' and not illusions, for they are the criteria by which the models are judged, not the other way around. (Goldstein, 1997, p. 8)

As Copernicans, we now realise the mistake Ptolemy made: his (alleged) inference from the (empirical) success of a model to its "reality" is unsound. Yet, in his defense:

> We need to understand Ptolemy as under the influence of two unequal forces: the pull of a Pythagoreanizing metaphysics of the universe as a harmonious, mathematical structure and the pull of the mathematical technique of the construction and computation of astronomical tables. In the practice of Ptolemy's astronomy, the pull of technique is the more powerful, and so technique becomes nearly its own independent pursuit, the metaphysics revealing itself merely intermittently. (Netz, 2022, p. 381–384)

These unequal forces are still with us, continuing to pull mathematical physics—note the inherent tension already in the very name of the field!—in opposite directions. The beauty of our models, like Ptolemy's, remains breathtaking, their power beyond expectation. Hence the lure of inferring physical reality from successful models, which Ptolemy succumbed to, remains as compelling to us as it was for him, despite the eventual fall of the *Almagest* and indeed all later, even more beautiful and successful models of the sky.[32]

[30] The standard translation of the *Almagest* is Toomer (1984). Feke (2020) discusses Ptolemy's mathematical philosophy. Netz (2022), pp. 378–379, gives a helpful comparison between the *Almagest* and the *Elements*. I skip Archimedes, undoubtedly one of the great mathematical physicists in history, since at least in his surviving manuscripts he did not really reflect philosophically, cf. Netz (2022), pp. 206–207.

[31] This may be more evident from his *Planetary Hypotheses* (Hullmeine, 2024) than from the *Almagest*.

[32] See Kuhn (1957) for the decline of Ptolemy and the rise of Copernicus.

In this *Element* I hope to show that the realist lure should be resisted on both historical and philosophical grounds, letting the 'pull of the mathematical technique' reign instead.[33]

Jumping another 1400 years, we arrive at Ptolemy's heir Copernicus, whose computational style was similar to Ptolemy's.[34] Alas, except for his outspoken realism, as far as I know Copernicus did not explicitly analyze the relationship between pure and applied mathematics in philosophical terms. In his successor Kepler one finds a pythagorean echo (with a Judeo-Christian twist) to the effect that God is a geometer, who reveals himself through his creation, and endowed the human mind with an 'illumination' through which knowledge of the hidden geometric form of the cosmos is attainable.[35]

Though hardly a mathematical physicist in the sense I defined (if only because of his quite elementary use of mathematics),[36] Galilei was a pivotal figure in the history of mathematical physics: while Ptolemy, Copernicus, and Kepler, though real mathematical physicists in my sense, only applied mathematics to astronomy, Galilei's innovation was to apply it to physics on earth, and as such he outspokenly broke with Aristotle's natural philosophy. But even so, by itself his famous claim that 'this grand book, the universe (. . .) is written in the language of mathematics' was far from revolutionary at the time: the theme that God reveals himself through a second book, the book of nature, was a familiar one, as was God's use of geometry (cf. Plato's *Timaeus* and Kepler). Galilei's originality as a philosopher of mathematical physics (now in the sense I did define: that is, reflecting on its practice) is better displayed in the following passage from the *Dialogue*:

> SALV. Do you know what does happen, Simplicio? Just as the calculator who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales, and other packings, so the mathematical scientist (*filosofo geometra*), when he wants to recognize in the concrete the effects which he has proved in the abstract, must deduct the material hindrances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations. The errors, then, lie not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting. Hence if you had a perfect sphere and a perfect plane, even though they were material, you would have no doubt that they touched in one point; and if it is impossible to have these, then it was quite beside the purpose to say *sphaera aenea non tangit in puncto*. (Galilei, 1953, p. 241)

Galilei's point, expressed as usual by his spokesman Salvatio, is that mathematical laws are attempts to grasp the real world via *idealizations* thereof, necessitated by the idea that the actual mathematical structure of the world is too complex to be grasped by our finite intellect. Thus Galilei was not a platonist but a pythagorean (in the sense I defined):[37]

> Galilei's claim that nature is written in the language of mathematics, far from being a rhetorical statement or an unwarranted metaphysical conviction, is grounded in co-

[33]See the discussion in §6 for the possible relevance of this work for the realism debate.

[34]They also shared an outspoken admiration for mathematics. For Ptolemy see Feke (2020). Copernicus–or perhaps his publisher Johannes Petreius–even put Plato's famous warning (in Greek) that no one who is ignorant of geometry may enter (i.e., the Academy) on the title page of *De Revolutionibus*!

[35]See Jardine (1979), p. 170.

[36]See Blåsjö (2023) for a devastating view of not just Galilei's mathematical level but also his rigour.

[37]See also McMullin (1985) and Finocchiaro (2010).

> herent ontological and epistemological arguments. In his works Galilei repeatedly argues that mathematical entities are ontologically independent from us and that the physical world has a mathematical structure. This structure is, however, too complex to be fully grasped by our finite intellect, which is why we need to simplify physical phenomena in order to be able to deal with them mathematically. What scholars have regarded as an opposition between the abstract and the concrete, the mathematical and the physical, was intended by Galilei as a distinction between what is mathematically simple, and hence easy for our intellect to grasp, and what is mathematically complex and hence unknowable. (Palmerino, 2016, pp. 31–32)

In particular, Galilei's distinction between what is real and what we are able to describe via idealizations was an epistemological one. In response to the Aristotelian dilemma if the physical world may instantiate *perfect* mathematical objects, Galilei seems to have allowed this possibility, somewhat ambiguously arguing that on the one hand irregular figures are more likely to occur (because there are far more of these), whereas on the other hand the simplest figures (like perfect spheres), though fewer in number, are the easiest to obtain. Alas, this issue remains unresolved, since Salvatio ends the topic by the following comment: 'let us waste no more time on frivolous and quite trivial altercations.'

3 Historical survey: Huygens to Hilbert

The 'mathematization of the world picture',[38] which one might take to be an alternative definition of mathematical physics, got into full swing after Galilei:

> In the era bounded by Galileo's *Dialogo* of 1632 and Newton's *Principia* of 1687, science changed. Observation, even when performed with enough care to be called experimentation, gave way to rigorous mathematical analysis as the primary approach to physical phenomena. Whereas Galileo aimed to instruct laymen about his view of the world order by means of plausible arguments and analogies, only an experienced mathematician could hope to understand the world picture envisioned by the *Principia*. This mathematization of physics was a defining element of that intellectual upheaval we call the Scientific Revolution, and the requirement, still imposed today, that a theoretical physicist be an able mathematician stems from a tradition that flowered in the seventeenth century. (Yoder, 1988, p. 1)

Horologium Oscillatorium by Huygens from 1673, which is the subject of Yoder's book just quoted from, is one of the masterpieces in the mathematization of the world picture published between *Dialogo* and *Principia*. But Huygens's clearest statement on his methodology comes from the preface of another major work, his *Treatise on Light*:

> One finds in this subject a kind of demonstration which does not carry with it so high a degree of certainty as that employed in geometry; and which differs distinctly from

[38]In the wake of Dijksterhuis (1961), still magisterial, more recent accounts include Mahoney (2008), Guicciardini (2013), and Van Dyck (2021). Roux (2010) discusses the mathematization of nature before Galilei. The pivotal role of Stevin also deserves more attention (Dijksterhuis, 1970; Davids et al., 2020).

> the method employed by geometers in that they prove their propositions by well-established and incontrovertible principles, while here principles are tested by the inferences which are derivable from them. The nature of the subject permits of no other treatment. It is possible, however, in this way to establish a probability which is little short of certainty. This is the case when the consequences of the assumed principles are in perfect accord with the observed phenomena, and especially when these verifications are numerous; but above all when one employs the hypothesis to predict new phenomena and finds his expectations realized. (Huygens, 1690)

Thus Huygens (against Newton, whom he had met in 1688) considers scientific theories never certain but at best probable. He also stresses (this time against Descartes, and agreeing with Newton) the importance of matching mathematical physics with experiment and observation. Finally, we see a hint at surrogative inferential reasoning in a context of modeling, which will occupy us in §5. On the other hand, typical philosophical questions concerning the ontology of mathematical objects seem not to have occupied Huygens (at least not in his published writings), whilst he seems to have taken the certainty of mathematical derivations from clearly stated assumptions for granted. Nonetheless:

> Huygens' description of his method has a surprisingly modern ring. The insightful formulation and ensuing application of the hypothetico-deductive method represents the culmination of the I7th century quest to develop a justification for causal explanations that do not obey the classical demonstrative mode. He has abandoned certainty in scientific explanations in favour of a high degree of probability–something Newton would only grudgingly concede in his old age–and recognized how one may validly demonstrate causes from their effects, or hypotheses from their consequences (Shapiro, 1989, p. 225)

I will return to the transition from the classical demonstrative mode to the hypothetico-deductive method in detail, after having discussed Newton's *Principia* from 1687, in which the former mode, along with mathematical physics itself, reached its all-time high.

Principia starts with a Preface to the reader,[39] in which Newton states his philosophy of mathematical physics, probably with a nod to Aristotle's *Physics* quote above:

> The description of right lines and circles, upon which geometry is founded, belongs to mechanics. Geometry does not teach us to draw these lines, but requires them to be drawn; for it requires that the learner should first be taught to describe these accurately, before he enters upon geometry; then it shows how by these operations problems may be solved. To describe right lines and circles are problems, but not geometrical problems. The solution of these problems is required from mechanics; and by geometry the use of them, when so solved, is shown; and it is the glory of geometry that from those few principles, brought from without, it is able to produce

[39]This Preface is discussed for example by Garrison (1987), Dear (1995), chapter 8; Domski (2003), Pulte (2005), chapter III, Guicciardini (2009), Chapter 13, and Smeenk (2016). The standard modern translation of *Principia* (Cohen & Whitman, 1999) merely notes that 'Newton's comparison and contrast between the subject of rational or theoretical mechanics and practical mechanics was a common one at the time' (p. 381). I do not discuss the originality of Newton's views; for example, his idea that geometric figures arise from mechanical motion already occurs in the work of his teacher and predecessor Barrow.

> so many things. Therefore geometry is founded in mechanical practice. (*Principia*, 1687, Author's Preface)

Here, Newton overcomes the discrepancy between on the one hand the perfection of mathematics via its location either in some platonic realm or in the mind, and on the other, the imperfection of its realization in natural objects (combined with the unreliability of observation) by a direct *identification* of geometric figures with natural ones. From this Preface and other texts,[40] it is clear that Newton attributes any kind of imperfection to the 'practitioner'; as opposed to 'the most perfect mechanic of all' (that is, God).

Having said this, if anyone realised that planets did not follow exact elliptical trajectories it was Newton; first, the other planets disturb such exact solutions to the gravitational two-body problem, and second, planets aren't point particles. Newton was fully aware of the idealised nature of his exact descriptions of geometric figures generated by motion; hence some of the deepest results in *Principia* try to overcome such idealizations. In this context, the fourth in the list of 'rules for the study of natural philosophy' (*regulae philosophandi*) that (after a brief introduction) open Book III of *Principia* reads:

> In experimental philosophy we are to look upon propositions collected by general induction from phaenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phaenomena occur, by which they may either be made more accurate, or liable to exceptions.

Sometimes 'more accurate' can even mean 'exact', as in Newton's results (proved in Section 12 of Book I) to the effect that spherical bodies may be replaced by point particles in so far as their gravitational force is concerned. All of this is part of a very delicate interplay between inference from observations, mathematical models taken to be literal and exact, and ensuing predictions (or retrodictions) of other observations (Smith, 2002).

So far, Newton's philosophy of mathematics remains pythagorean, close to Galilei's: at least in so far as they are related to motion, his mathematical objects reside *in* nature. Newton doesn't "apply" mathematics *to* nature but studies the mathematics *of* nature, perhaps with the unusual feature that geometric figures occurring in nature are seen dynamically rather than statically–though even that wasn't completely new: apart from its origin in Barrow,[41] one may argue that even Euclid entertained the view that such figures are "dynamically" generated by constructions with ruler and compass, which constructions in early modern times prior to Newton had already become increasingly generalised;[42] the difference is that for Newton the underlying "motion" was natural rather than man-made.

Newton's philosophy of science in the *Principia* was also quite conservative: although he followed (and trumped) Galilei and Huygens in their decisive use of mathematics in physics or natural philosophy, and hence departed from Aristotle in that respect, otherwise he structured and interpreted his book in accordance with the latter's theory of demonstrative sciences.[43] Pulte (2001, 2005) calls the *Principia* a "Euclidean system" in the sense

[40] Notably *De Gravitatione*, see Domski (2017), and the General Scholium in *Principia*.

[41] See Sepkoski (2013), Chapter 4, and Guicciardini (2009), §8.1.

[42] See e.g. Bos (1993, 2001).

[43] See Dear (1995), Pulte (2001, 2005), Smith (2002), and Ducheyne (2005), and references therein, for Newton's methodology including his rejection of the hypothetico-deductive method.

defined by Lakatos, i.e., a 'deductive system with an indubitable truth-injection at the top (a finite conjunction of axioms)–so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system.'[44]

Indeed, Book I of the *Principia* starts with a list of definitions, so as to emphasise its mathematical character by analogy with Euclid's *Elements* (which begins likewise). The 'top' in the sense of Lakatos is then given by Newton's *axiomata sive leges motus* (axioms or laws of motion) that follow the Definitions and the Scholium in Book I, that is, the famous three laws. The title of the chapter already shows that Newton conflates axioms and laws of nature, and, indeed writing in the style of Euclid, regarded these axioms or laws (and hence all their logical consequences) as true, albeit based on empirical observations in Aristotelian fashion, as summarised above.[45] Thus Newton identifies basic laws of nature with the axioms of a mathematical theory of nature, which is justified by the earlier quote from his Preface to the effect that mathematics (or at least geometry) is *in* nature:

> The material truth of axioms, inundating the whole system of propositions, stems from *mathematics* itself. (Pulte, 2001, p. 70).

Newton's real innovation in *Principia* was his introduction of *forces* into mechanics in a way that far exceeded the everyday use of the word (although that was included), culminating in the gravitational force. Forces differ from the motions they generate. Such motions are geometric figures which Newton, as we have seen, places in nature (or rather: in space and time) itself, without an independent platonic mathematical "existence" elsewhere; they can be observed and measured. The "existence" of the gravitational force, on the other hand, can only be *inferred* from such motions, for which almost the entire apparatus of *Principia* is required. Newton's mathematical forces therefore created a completely new situation, not only in (mathematical) physics but also in its philosophy:

> Force, which Newton unquestionably conceived as an ontologically real entity, is not an 'object' in the same sense as either the metaphysical essence, universal category, or Platonic form of a triangle is. Neither, however, is it an artificial construction of the mind, a 'concept' without a physical referent. Force is a distinctly new class of ontological being: it is real without being tangible, and its effects are mathematically quantifiable but its physical properties are unknown and perhaps unknowable. Although Newton uses geometry (and analysis) to demonstrate the existence of gravity, the force itself cannot be conceptualized as a simple geometrical object or construction, but rather requires the complicated series of geometrical demonstrations in the *Principia*. (Sepkoski, 2013, p. 107–108)

Newton's novel concept of force had two closely related consequences:

[44]Quoted from Lakatos (1978), p. 28. Here (at least for Lakatos) the contrast is with "quasi-empirical theories", whose axioms or assumptions are not supposed to be true but conjectural, and in which (in the spirit of Popper still endorsed by Lakatos at the time) 'the logical flow is not the transmission of truth but rather the retransmission of *falsity*–from special theorems at the bottom up towards the set of axioms' (p. 28). See Paseau and Wrigley (2024) for a recent survey of the 'Euclidean Programme'.

[45]This much is clear from his fourth rule for the study of natural philosophy, quoted above. It is here, in my view, that Wittgenstein's idea that mathematical propositions are 'empirical regularities hardened into a rule' works particularly well, at least at the level of axioms. See §4 below.

1. It paved the way for one of the main features of modern mathematical physics already mentioned, namely the use of mathematical objects that (unlike for example geometric figures and numbers) are not instantiated in the natural world, at least not in the immediate way geometric figures or numbers are. As already noted, in so far as the philosophy of mathematical physics is concerned there is hardly any conceptual difference between Newton's gravitational force and Einstein's (Lorentzian) metric in his theory of general relativity: each is just a mathematical construction.[46]

2. It undermined the Euclidean, or, *qua* philosophy of (demonstrative) science, even Aristotelian structure of *Principia*. Aristotle's premisses and Euclid's axioms were supposed to be obviously and indubitably true. But Newtonian forces (especially the gravitational one) could hardly claimed to be "true" in this sense, neither as mathematical ingredients of nature (like motions) nor as more abstract objects.[47]

The second point was acknowledged by Newton himself, since his fourth rule quoted above admits that the 'propositions collected by general induction from phaenomena' (i.e., his premisses) may only be 'very nearly true', or even 'liable to exceptions'. This admission creates a tension within the *Principia* that Newton never overcame: he should have admitted that his premisses were hypotheses, but this was blocked by his general ideology described earlier, culminating in his famous words *'hypotheses non fingo'* in the *General Scholium*, through which he expressed simultaneously his animosity towards Descartes and his allegiance to the Ancients. It took about 200 years to overcome this tension.[48] As we shall see, the philosophy of mathematical physics since Newton's time may even be analysed as the interplay and eventual convergence of these two points. Of course, it is a pleasure for me to repeat how far Huygens was ahead of Newton and his time at least in this respect, as we saw in the quotation from his *Treatise on Light*.

Grosso modo, the mathematical physics of the eighteenth century centered around (highly nontrivial and innovative!) rewritings and generalizations of Newton's *Principia*. First, its continental European reformulation in the style of Leibniz's calculus by Euler replaced Newton's choice to cast the mathematics of his mechanics in a Euclidean mould. But despite the strong philosophical flavour of Newton's own justifications for his mathematical physics (and Leibniz's primary status as a philosopher), the style of Euler and his successors was quite pragmatic. Indeed, this style was neither too philosophical nor even mathematically rigorous, so that the mathematical foundation of the calculus (be it Newton's or Leibniz's or Euler's) remained shaky throughout the eighteenth century.

Second, the invention of partial differential equations by D'Alembert and Euler enormously expanded the scope of mathematical physics, which could now describe the physics of vibrations and waves, culminating in the new mathematical discipline of hydrodynamics.[49] These fields were actually far less abstract and conceptually innovative than Newton's *Principia*, and, being easily visualizable and (by construction) applicable, ap-

[46]Frank (1937) also–but in different terms–analyses the mistaken claim that it is only twentieth century physics that brought the idea that 'The world is no longer a machine but a mathematical formula' (p. 41).

[47]Although Newton himself would emphatically deny this, the (perceived) lack of rigour in the mathematical deductions in *Principia* also weakened its "Euclidean" structure.

[48]This point is developed by Pulte (2005), which I endorse. See also Stan (2017).

[49]See Darrigol (2005) and and Gray (2021).

parently did not lead to much philosophical reflection worth recalling (indeed, despite introducing his critical philosophy almost fifty years *after* the invention of partial differential equations and hydrodynamics, Kant still reflected on Newton entirely, see below).

Third, parallel to this development but somewhat at odds with it, mechanics itself was further elaborated, using the new theory of partial differential equations (which as just stated had been invented for different purposes). This culminated in the work of Lagrange (1788), who returned to Newton's axiomatic setting, but now with a totally non-obvious starting point consisting of a variational principle for virtual velocities. Consequently,

> Axioms become formal principles of organization rather than principles with empirical content, and the whole system is held together by logical coherence rather than by "material" truth. In Lagrange's concept of mechanics, the higher calculus serves as the uniting element in the deductive chains. (...)
>
> Lagrange shaped the image of analytical mechanics as a model science for more than half a century. His understanding of rational mechanics as a "self-sufficient" and formal mathematical science, however, inevitably leads to a smouldering conflict with the tradition meaning of axiom as a self-evident first proposition, which is neither provable nor in need of a proof. (Pulte, 2009, p. 82)

In other words, compared to Newton, Lagrange's axioms and mathematical language were so abstract and remote from empirical observations that the axioms could hardly be called obviously or indubitably true, and the language only served as means of formal deduction. As described and explained in great detail by Pulte (2005), this put the nail in the coffin of Newton's "Euclidean" or even "Aristotelian" philosophy of mathematical physics as described in the previous section: it brought the first of the two numbered points made at the end of that section to a head, whilst resolving the second in such a way that:

> 'Deductivity' wins over conceptual foundation – or at least makes conceptual foundation a problem which no longer requires discussion. (...) [T]he whole system is held together by logical coherence rather than by material truth. (Pulte, 2001, p. 79)

Unlike the seventeenth century (in which admittedly mathematics, physics, and philosophy were hardly separate activities and were often practiced together by the same person), the dominant philosopher of mathematical physics in the eighteenth century was not a mathematician but a philosopher, viz. Kant. I here restrict myself to his most relevant work in this direction, namely *Metaphysischen Anfangsgründe der Naturwissenschaft* from 1786,[50] translated as *Metaphysical Foundations of Natural Science* (Friedman, 2004).[51] As noted by Friedman in his introduction to his translation,

> [F]or the eighteenth century as a whole, the age of Enlightenment and the triumph of Newtonianism, the recent culmination of the scientific revolution of the sixteenth and seventeenth centuries in the work of Newton had elevated natural science to previously undreamt of heights within the intellectual firmament. Thinkers as diverse as Voltaire, Hume, and Kant himself all took the Newtonian achievement in natural science as a model of the human intellect at its best, and as a model, more specifically, for their own philosophical activity. (Friedman, 2004, p. vii)

[50] Mostly untranslated secondary literature in German may be traced back from Pulte (2005).

[51] See also Harman (1983), Friedman (1992), Watkins and Stan (2023), and references therein.

Kant's book contains a detailed (and controversial) technical development of Newtonian physics, but for us his main contribution lies in its Preface, in which Kant specialized his own critical philosophy (which dates from the same decade) to the case at hand:[52]

> A rational doctrine of nature thus deserves the name of a natural science, only in case the fundamental natural laws therein are cognized a priori, and are not mere laws of experience. (...) All *proper* natural science therefore requires a *pure* part, on which the apodictic certainty that reason seeks therein can be based. And because this pure part is wholly different, in regard to its principles, from those that are merely empirical, it is also of the greatest utility to expound this part as far as possible in its entirety, separated and wholly unmixed with the other part; indeed, in accordance with the nature of the case it is an unavoidable duty with respect to method. This is necessary in order that one may precisely determine what reason can accomplish for itself, and where its power begins to require the assistance of principles of experience. Pure rational cognition from mere *concepts* is called pure philosophy or metaphysics; by contrast, that which grounds its cognition only on the *construction* of concepts, by means of the presentation of the object in an a priori intuition, is called mathematics. (...) I assert (...) that in any special doctrine of nature there can be only as much proper science as there is *mathematics* therein. (...) Now rational cognition through construction of concepts is mathematical. Hence, although a pure philosophy of nature in general, that is, that which investigates only what constitutes the concept of a nature in general, may indeed be possible even without mathematics, a pure doctrine of nature concerning *determinate* natural things (doctrine of body or doctrine of soul) is only possible by means of mathematics. (Kant, 1786/Friedman, 2004, pp. 4–6)

Thus mathematics is the *a priori* tool that enables "proper" natural science (which culminates in Newtonian mathematical physics); as such mathematics is 'wholly different' from the empirical' and is something that 'reason can accomplish for itself'. Elsewhere in the book, Kant puts the concepts of (Euclidean) space and (Newtonian) force into this *a priori* mathematical realm, although specific forces are empirical. Here is a summary:

> In the eighteenth century, then, Newton's physics was an unqualified success in both mathematical and empirical terms, but there remained serious conceptual problems concerning whether and how this brilliantly successful theory actually made rational sense. Kant's problem, accordingly, was not to sketch a program for a new mathematical physics, but rather to explain how our actual mathematical physics, the mathematical physics of Newton, was itself possible in the first place. And his answer, in the briefest possible terms, is that the concepts of space, time, motion, action, and force do not function to describe a metaphysical realm of entities or "true causes" lying behind the phenomena. Nor are they simply abstractions from our experience, which we can then apply to the phenomena because we have already found them there. Rather, such concepts as space, time, motion, action, and force are a priori forms or constructions of our own, on the basis of which alone we can coherently order the phenomena of nature into a unified and law governed spatiotemporal totality. (Friedman, 2001, p. 10)

[52]The *Critique of Pure Reason* is from 1781, the *Critique of Practical Reason* from 1788, and the *Critique of Judgement* from 1790. The *Metaphysical Foundations* therefore lies in the middle of these.

I follow Pulte (2005) in his interpretation of Kant's edifice as an effort to salvage Newton's "Euclidean system" underwriting *Principia* by relocating the 'apodictic certainty' of its mathematical laws (and their rigorously derived consequences) *from* the empirical realm from which Newton extracted them *to* the allegedly unassailable (synthetic) *a priori*. Compared to the pythagorean views held by Newton and his predecessors, according to which mathematical objects reside in nature and mathematical reasoning simply tracks the flow of truth, removing mathematics from the natural realm and putting it into the human mind was revolutionary. As I will explain in the course of this *Element*, with due reinterpretation of the Kantian synthetic *a priori*, this move remains correct, as I see it.

The nineteenth century marked a transition *from* what Pulte (2009) calls the 'classical mathematical philosophy of nature', i.e., Newton's view that 'natural philosophy can be established on the basis of certain unshakable "axioms" of mechanics', which was '*de facto* regarded as epistemologically equivalent to Euclidean geometry by nearly all scientists and philosophers of science' (pp. 77, 95), *to* a hypothetico-deductive view. Like many major transitions, also this one had multiple roots, which partly intersected:

- As we saw, in the context of mechanics the transition in question was initiated especially by the work of Lagrange in the late eighteenth century; further increase of abstraction and formality at the cost of self-evidence in the work of C. Neumann, Jacobi, Riemann, Hamilton, and others completed the process.[53]

- Besides mechanics (which, if not seen as mathematics, co-evolved with it), physics as a whole underwent spectacular innovations in the nineteenth century compared to its Newtonian and Eulerian heritage. This introduced new degrees of abstraction beyond Newton's concept of force as I already discussed, which, for different reasons than Huygens's, caused a further breakdown of the "Euclidean system".

  In particular, in thermodynamics, the concept of *energy* was introduced,[54] initially as heat or work, but gradually in all kinds of other forms, too. Though in mechanics energy is closely related to Newtonian force, I already diagnosed the latter as the first departure from the old Aristotelian (and indeed Newtonian) idea that mathematics is part of nature; and potentials are more abstract, paving the way for all kinds of further abstractions like the metric in Einstein's theory of general relativity. Elsewhere in physics, energy is also simply a mathematical concept (eventually formalised as the Hamiltonian). Second, electrodynamics and its underlying field concept became a central part of physics, which despite Faraday's visualizations and Maxwell's later mechanical models of the ether (see below) lacked both obvious reality and self-evident axioms–Maxwell's equations were far from that.

- In the eighteenth century the description of natural phenomena like the flow of fluids or (a bit later, in 1822) of heat by partial differential equations (the latter due to Fourier) was apparently seen as quite literal and truthful. But (looking at the idealizations that were made in the derivation of these equations) it is obvious that

[53] See Lützen (1995) for mathematical history and Pulte (2001, 2005, 2009) for philosophical analysis.

[54] Or re-introduced, since there were some precursors. But its central role in physics dates back to the nineteenth century and originated in thermodynamics. See e.g. Harman (1982) and Coppersmith (2015).

within a corpuscular basis of physics of the kind that increasingly found favour in the nineteenth century, such mathematical descriptions were merely approximate.[55] At the same time, atoms and molecules were still invisible, mysterious, and hypothetical, so that neither mathematics nor physics seemed to contain any kind of obvious "truth". But despite being cut off from physical "truth", the mathematics of partial differential equations still made sense *by itself*, and was deemed worthy of study. This decoupling of mathematics from physical "truth" and even from physics altogether is one of the routes via which mathematics became "pure".

- Driven by various factors, including making calculus and analysis rigorous for reasons of both research and teaching (at the new universities), mathematics as a whole (which included mechanics) underwent a transition from a "Euclidean" to a hypothetico-deductive view. It gradually decoupled from physics, intuition, and self-evident truth to became an independent discipline characterised by abstraction, formal validity, and progress that largely came from reflections on mathematics itself.[56] The most famous ingredient of this development is undoubtedly the construction of non-Euclidean geometries by Gauss, Bolyai, and Lobachevsky and the closely related (but historically probably independent) invention of (metric) differential geometry by Riemann (1854), whose title was even: *Über die Hypothesen, welche der Geometrie zu Grunde liegen* (*On the hypotheses which lie at te basis of geometry*, underlining added); but in fact fields like projective geometry, logic, algebra, and analysis (co-) evolved and decoupled similarly from physics.[57]

- The discovery of non-Euclidean geometries challenged Kant's *a priori*, at least in so far as Euclidean geometry was supposed to be part of it.[58]

All this led to what is sometimes called the "modernist transformation" of mathematics centered around 1900,[59] after which mathematics had become autonomous: its former "Euclidean" mode had been replaced by a hypothetical-deductive style, just as in physics. The development of set theory and logic is a good illustration of this transformation, deepened by its extensive interaction with philosophy through people like Frege and Russell, but simultaneously embedded into mainstream mathematics by Hilbert and his school.[60] As is well known, around 1900 a combination of Cantor's set theory, Frege's program of deriving arithmetic from logic, and Russell's even more ambitious logicist program led to Russell's paradox and other signs of a foundational crisis like the need for Zermelo's controversial axiom of choice. This crisis was eventually resolved by the axiomatization of both set theory and logic in the style of Hilbert. The result of this was that:

[55] See e.g. Darrigol (2005), Gray (2021), and references therein. The next point is due to Maddy (2008).

[56] The reforms of the German education system around 1800, culminating in the foundation of the University of Berlin in 1809 (now called the Humboldt University), reinforced this development through its idea that mathematics should be cultivated as an intellectual activity, free, like all academic discourse, from applications, and carried out in *Einsamkeit und Freiheit* (i.e., *solitude and freedom*). See Jahnke (1990).

[57] See e.g. Jahnke (2003), Gray (2007, 2008), etc.

[58] See Coffa (1991), Friedman (2001), Pulte (2005), DiSalle (2006), Torretti (2012), and Stump (2015).

[59] As far as I know this idea was introduced by Mehrtens (1990) and further developed by Gray (2008).

[60] See Coffa (1991), Grattan-Guinness (2000), Giaquinto (2002), and Ferreirós (2008).

> By the end of the first decade of our century, set theory had become a discipline that had no recognizable link with its tradition logical neighbors: concepts, intensions, and meanings. It had also be come a hypothetico-deductive discipline whose main purpose was not find a priori or even merely true assumptions but to save the mathematical phenomena. (Coffa, 1991, p. 114)

The importance of the outcome of this development can hardly be overemphasised: *what replaced sets (or classes) as real objects was a set of rules for working with them.* The same applied to logic: following his initial goal of finding a logical basis for mathematics that was absolutely certain, even Russell eventually came to believe that its reliability:

> is fundamentally the same as that of every other science. There is the same fallibility, the same uncertainty, the same mixture of induction and deduction, and the same necessity of appealing, in confirmation of principles, to the diffused agreement of calculated results with observation. (Quoted by Coffa, 1991, p. 121)

For mathematical physics, the most remarkable feature of the modernist transformation is that this turn to abstraction *preceded* the most spectacular applications of mathematics to physics, viz. the formulation of Einstein's theory of general relativity in terms of Riemannian geometry,[61] and the formulation of quantum mechanics in terms of complex Hilbert spaces (and more generally in terms of functional analysis). Indeed, one might naively expect that the decoupling of mathematics from physics that took shape in the nineteenth century, as just described, would be the end of mathematical physics. Adding to this apparent paradox, Hilbert, who championed the modernist transformation like no other mathematician, played a pivotal role in both of these spectacular applications.[62]

I defer a philosophical discussion of this problem to §6 below, but historically this seems the right place to draw attention to the crucial 'modernist' concept of an *implicit definition*, which was developed by Hilbert, Poincaré, Peano, Enriques, and others, and was also taken up by Wittgenstein in his philosophy of language and by Schlick in the philosophy of science (see below).[63] Here are two complementary formulations:

> In my opinion, a concept can be fixed logically only by its relations to other concepts. These relations, formulated in certain statements, I call axioms, thus arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the definitions of the concepts. (Hilbert to Frege, 22 September 1900; Gabriel *et al.*, 1980, p. 51)

> It is, incidentally, very important that by merely looking at the little pieces of wood I cannot see whether they are pawns, bishops, castles, etc. I cannot say, "This is a pawn and such-and-such rules hold for this piece." Rather, it is only the rules of the game that define this piece. A pawn is the sum of the rules according to which it moves (a square is a piece too), just as in language the rules of syntax define the logical element of a word. (Wittgenstein, 1967, p. 134)

[61] More precisely: its formulation in terms of what is called *Lorentzian* geometry, which involves a change in the signature of the metric of Riemannian geometry. But this change was in itself contained in Minkowski's mathematical reformulation of Einstein's earlier theory of *special* relativity. See also §6.

[62] See Renn (2007) for Hilbert's role in general relativity and Duncan & Janssen (2023) and Landsman (2021) for his (and von Neumann's) contributions to the mathematical development quantum theory.

[63] For more information see Giovannini and Schiemer (2021), Sereni (2024), and Landsman (2025) .

• The traditional idea was that the terms (or words) connected to each other by some system of axioms in mathematics (or some set of rules for language games) are familiar already before stating the axioms or rules, as they refer to things previously defined or known, along with the Aristotelian claim that axioms are self-evident and true.

• The new idea is that in the crucial case of *indefinables* these terms are non-referential but instead are given meaning *by the axioms*, which (like the rules of chess), thereby also lose their self-evident truth (and indeed their truth altogether).[64] It should be clear from the foregoing historical account of the transition from the Euclidean model to the hypothetico-deductive model that implicit definitions are indispensible to this transition.

In particular, if the objects of the theories of mathematical physics are no longer supposed to be "there" in the natural world, then they must be purely theoretical entities whose use is determined by the theories in which they appear and whose connection to the world remains to be developed. Likewise, these theories cannot reasonably held to be "true".

The logical positivist, who picked this up initially through Schlick (who knew Hilbert's work and also talked to Wittgenstein around the time the latter returned to philosophy from 1929 onwards),[65] tried to define this connection via so-called coordination procedures, but their efforts are widely taken to have been in vain.[66] Nonetheless, I will return to the logical positivists and especially to Wittgenstein for inspiration, and after a reconceptualization of the *a priori* will try to make this connections via surrogative inferences.

In any case, with hindsight we see that mathematics could be successfully applied to modern physics not *despite* but *because* of the increased abstraction of mathematics and its apparent decoupling from physics as described above. The identification of mathematical concepts with natural kinds may initially have launched the idea and the possibility of mathematical physics, but such identifications simultaneously narrowed the scope of the mathematics in question to the specific application that involved it (and for which is was often invented). This made it difficult to recognise different applications of the same piece of mathematics, and also made it even more tricky to apply areas of mathematics that were developed for different purposes, especially if these were internal to mathematics. Overcoming the connection between mathematics and reality or natural kinds made room for entirely new applications, such as general relativity and quantum theory.

4 The *a priori* in mathematical physics

I now try to use the previous insights from history to formulate a philosophy of mathematical physics. I first update the *a priori*. It is of course well known that both special and general relativity–predated by the non-Euclidean geometries of the nineteenth century, as we already saw–suggested that not only Euclidean geometry but also Newtonian physics

[64]See also Landsman (2025) and references therein.

[65]See Wittgenstein (1967) for his discussions with Schlick, whose analysis of implicit definitions may be found in his *Allgemeine Erkenntnislehre* (Schlick, 1918/2009). See also Coffa (1991) and Stump (2015).

[66]In a delightful televised discussion with Bryan Magee in 1977 on 'Logical Positivism and its Legacy', Alfred Ayer replied to Magee's question what he regarded as the main shortcomings of logical positivism: 'I suppose the greatest defect is that nearly all of it was false.' Quoted by Edmonds (2020), p. 259. See `https://www.youtube.com/watch?v=x2SrXueVAos` at 34:19.

was no longer the last word, so that nothing seemed to be left of Kant's *a priori* (at least in so far as physics was concerned). This, as well as Hilbert's work on axiomatization and implicit definition, led the logical positivists like Reichenbach, Schlick, Carnap, Cassirer, Ayer, (C.I.) Lewis, and Pap, to various attempts to rescue at least one aspect of the *a priori*, namely its *constitutive role* in shaping knowledge and meaning.[67]

In particular, Reichenbach (1920) argued that Kant's relocation of mathematics was essentially correct, but was executed far too rigidly because of his insistence on the very specific form this *a priori* mathematics and mathematical physics was supposed to take: Kant's adoption of Euclidean geometry as well as his restriction to Newtonian forces and laws of motion have clearly not stood the test of time in both mathematics and physics, respectively. This led Reichenbach, and in his wake Friedman (2001, 2002), to propose and revive the idea of *relativizing* the Kantian *a priori* to a particular historical stage in the development of science.[68] This neo-Kantian development pushed back at least some mathematics and physics to the empirical side. What, then, is the right balance between the *a priori* and the *a posteriori*? Though he was motivated by mathematics rather than physics (and wasn't a logical positivist!), a key point was made by Wittgenstein in 1939:

> It is as if we had hardened the empirical propositions into a rule. And now we have, not an hypothesis that gets tested by experience, but a paradigm with which experience is compared and judged. (Wittgenstein, 1968, §VI.22b)

In other words, heuristic regularities or other quantifiable features of the natural world cross the line from the *a posteriori* into the formal *a priori* and thus become rules.

To do justice to modern mathematical physics one should interpret Wittgenstein's 'empirical propositions' much more widely than what he probably had in mind himself, more or less along the line of Hilbert's program of axiomatization (initially of mathematics, and then extended to physics via his sixth problem).[69] See also below. Its scope then ranges from the simplest ("Humean") case of empirical regularities turned into "laws of nature" (such as Boyle's ideal gas law or the law of Buys-Ballot) to the multi-step process leading from the empirical study of free fall and planetary motion via the equivalence principle to Einstein's field equations for general relativity; but the point is always that at some stage 'soft' heuristic procedures and/or empirical conclusions are 'hardened' into rules.

Similarly, not just in his (earlier) philosophy of mathematics but also in the *Philosophical Investigations* (Wittgenstein, 2009) one of the multiple roles of language games also lies in their being "hardened" linguistic practices. My strategy will be to turn this aspect of Wittgenstein's philosophy of language into a similar relationship between mathematical theories or models of physics and natural phenomena, respectively.[70] This analogy

[67] See e.g. Coffa (1991), Friedman (1999, 2001), Ryckman (2005), DiSalle (2006), and Stump (2015).

[68] Van Strien (2021) argues that Boltzmann (1905), pp. 321, 354, 398–399, on the one hand defended the Kantian *a priori*, especially in the case of causality (which he calls the 'unique determination of natural processes'), but that on the other hand such laws of thought have an evolutionary origin and hence are not absolute; indeed, because they have become such strong habits, they can in certain cases 'overshoot the mark.' With some goodwill this may be seen as a precursor of Reichenbach's relative *a priori*.

[69] See Corry (2004, 2018); also cf. Landsman and Singh (2025) for the relationship between the ideas of Hilbert and Wittgenstein on rule following, games, and axiomatization.

[70] The focus on the *a priori* is not obvious from the *Philosophical Investigations*, but, as explained by Coffa (1991), chapter 14, as well as by Steiner (2009), it is quite prominent in middle Wittgenstein.

seems in the spirit of Wittgenstein himself, who in his middle period started applying ideas from his philosophy of mathematics to the philosophy of language,[71] for example,

> But if we had to name anything which is the life of the sign, we should have to say that it was its *use*. (Wittgenstein, 1958, p. 4)

This led to (late) Wittgenstein's famous "meaning is use" philosophy of language, which is a central theme of his *Philosophical Investigations*. It is highlighted in a paragraph like:

> §43. For a *large* class of cases of the employment of the word "meaning" –though not for *all* –this word can be explained in this way: the meaning of a word is its use in the language.

For our understanding of the *a priori* in mathematical physics, the argument starting with the following paragraph and leading up to §108 (quoted below) is of central importance:

> §97. Thinking is surrounded by a nimbus. – Its essence, logic, presents an order: namely, the a priori order of the world; that is, the order of *possibilities*, which the world and thinking must have in common. But this order, it seems, must be *utterly simple*. It is *prior* to all experience, must run through all experience; no empirical cloudiness or uncertainty may attach to it. – It must rather be of the purest crystal. But this crystal does not appear as an abstraction, but as something concrete, indeed, as the most concrete, as it were the *hardest* thing there is (...)

As we shall see, theories of mathematical physics like general relativity or quantum mechanics play a similar *a priori* role. Wittgenstein then warns against a misconception we are 'prone to', namely that the 'ideal' of what he aptly calls the 'crystalline purity' of logic is part of natural language and hence is to be found in reality; but in fact, natural language is too complicated for that. He also warns against bending natural language so that it approaches this ideal, for language has to be taken as it is (like mathematics). Neither should logic be bent so as to accord with natural language, since in that case logic would become empirical and we would compromise its rigorous and clear rules. Wittgenstein's remarkable solution of this dilemma consist of two steps. First, he 'turn[s] the enquiry around' via a *relocation* of the 'crystalline purity' of logic:

> §108a. We see that what we call "proposition", "language", has not the formal unity that I imagined, but is a family of structures more or less akin to one another. – But what becomes of logic now? Its rigour seems to be giving way here. But in that case doesn't logic altogether disappear? For how can logic lose its rigour? Of course not by our bargaining any of its rigour out of it. The *preconception* of crystalline purity can only be removed by turning our whole inquiry around. (...)

Hence, analogously to Kant's relocation of logic and mathematics from the phenomenal world to the *a priori*, Wittgenstein relocates the crystalline purity of logic from language, where it doesn't belong, to language games, which in his late philosophy arguably play the role that logic (vastly generalized) played in the *Tractatus*.[72] It is also worth quoting the

[71] See primarily Kienzler (1997), and also Gerrard (1996), Mühlhölzer (2010), and Kuusela (2019).

[72] See Railton (2000) and Kuusela (2019), §4.4 for this highly non-trivial observation.

end of this paragraph, since, as I see it, applying its argument to the relationship between physical theories and natural phenomena instead of Wittgenstein's relationship between (rule-governed) languages games and natural languages, it makes *the* anti-realist point:

> §108cd. We're talking about the spatial and temporal phenomenon of language, not about some non-spatial, atemporal non-entity. (...) But we talk about it as we do about the pieces in chess when we are stating the rules for their moves, not describing their physical properties. The question "What is a word really?" is analogous to "What is a piece in chess?"

Second, Wittgenstein adds an important clarification to the effect that language games (or at least the 'clear and simple ones', words reflecting his earlier 'crystalline purity') provide benchmarks or yardsticks with which some linguistic practice can be *compared*:[73]

> §81. F. P. Ramsey once emphasized in conversation with me that logic was a 'normative science'. I do not know exactly what idea he had in mind, but it was doubtless closely related to one that dawned on me only later: namely, that in philosophy we often *compare* the use of words with games, calculi with fixed rules, but cannot say that someone who is using language *must* be playing such a game. – But if someone says that our languages only *approximate* to such calculi, he is standing on the very brink of a misunderstanding. (...)
>
> §130. Our clear and simple language-games are not preliminary studies for a future regimentation of language as it were, first approximations, ignoring friction and air resistance. Rather, the language games stand there as *objects of comparison* which, through similarities and dissimilarities, are meant to throw light on features of our language.
>
> §131. For we can avoid unfairness or vacuity in our assertions only by presenting the model as what it is, as an object of comparison, so to speak as a yardstick; not as a preconception to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.)

Thus, consistent with Wittgenstein's earlier remarks that we 'harden empirical regularities into rules', the *a priori* is so to speak fed by the *a posteriori*, eats it, and finally, in its digested ('hardened') state, has become independent of the empirical realm, to which it nonetheless still relates by providing yardsticks or objects of comparison through which this realm may be studied (more details on this feedback process are in the next section).

As I see it, this procedure resonates with, and provides ample philosophical background for, Hilbert's views on the role of mathematics in the physical sciences:[74]

> Mathematics has a two-fold task: On the one hand, it is necessary to develop the systems of relations and examine their logical consequences, as happens in purely mathematical disciplines. This is the *progressive task* of mathematics. On the other

[73] The German original 'Maßstab' may be superior to the English translation 'yardstick' in that the original better expresses the dual character of the word. The most famous section in the *Philosophical Investigations* about yardsticks is §50 about the standard metre in Paris, but I find it pretty confusing.

[74] See also Muller (2004), Mühlhölzer (2008, 2010, 2012), Friederich (2011), and Landsman & Singh (2025) for the intellectual relationship between Hilbert and Wittgenstein in the relevant context.

hand, it is important to give the theories formed on the basis of experience a firmer structure and a basis that is as simple as possible. For this it is necessary to clearly work out the prerequisites and to differentiate exactly what is an assumption and what is a logical conclusion. In this way, one gains clarity about all unconsciously made assumptions, and one recognizes the significance of the various assumptions, so that one can overlook what modifications will arise if one or the other of these assumptions has to be eliminated. This is the *regressive task* of mathematics.[75] (Hilbert, 1919/1992, pp. 17–18, translation by the author).

Hilbert also stressed that axioms are by no means cast in stone, but instead are liable to change, and especially in physics should be modified time and again:[76]

> [In] physical theories the elimination of contradictions that arise will always have to be done by changing the choice of axioms and the difficulty lies in making the selection in such a way that all observed physical laws are logical consequences of the selected axioms. (Hilbert, 1918, p. 411)

This again coheres with a Wittgensteinian theme, namely the *dual nature of yardsticks* (provided by language games in his philosophy of language; provided by models of theories of mathematical physics in my philosophy of mathematical physics, as will be explained in §5 below). Namely, yardsticks may be either *normative* or *empirical*.[77] A splendid example, due to Wittgenstein himself in the early 1930s, covers both:[78]

> Can one read off the geometry of a cube from a wooden cube or from a picture of a cube? (...) Does geometry talk about cubes? Does it say that the shape 'cube' has certain properties? What could be called a property of the shape 'cube'? Surely what a true proposition says of it, hence, say, that a house is cube-shaped. (...) In this way geometry says nothing about cubes, but rather constitutes the meaning of the word 'cube', etc. Geometry tells us, e.g., that the edges of a cube are equal in length, and *nothing seems more tempting* than a confusion of the grammar of this proposition with that of the proposition 'The sides of this wooden cube are equal in length'. And yet the one is an arbitrary grammatical rule, the other an empirical proposition.[79]

[75] As pointed out to me by Wesley Wrigley, what is still missing here (and elsewhere) is an account of informal mathematics, shadowed in my own approach by the lack of a 'philosophy of theoretical physics'.

[76] Indeed, Hilbert published no fewer than seven different editions of his *Grundlagen der Geometrie*!

[77] Similar terminology would be *grammatical* versus *exploratory*. Characterizing and distinguishing these two categories of rules in general is a challenge that also Carnap and the other logical positivists faced; in representational theories of language this would be the distinction between *non-descriptive* and *descriptive* sentences. Price (2013) calls the existence of this distinction the *bifurcation thesis*. I like Stump's (2015) terminology of 'settled science' for the normative side, including the stipulation that–notwithstanding the fallibility of science–the propositions of settled science are used *as if they were certain*.

[78] Source: Waismann and Baker (2003), p. 55. Quoted in part by Coffa (1991), p. 265.

[79] Similarly, my earlier quotation from Wittgenstein (1968), i.e., his *Remarks on the Foundations of Mathematics*, continues as follows: 'For one judgment is: "He worked out 25×25, was attentive and conscientious in doing so and made it 615"; and another: "He worked out 25×25 and got 615 out instead of 625." But don't the two judgments come to the same thing in the end? The arithmetical proposition is not the empirical proposition: "When I do *this*, I get *this*"–where the criterion for my doing this is not supposed to be what results from it.' (Wittgenstein, 1968, §VI.22cd)

Likewise, in the context of mathematical physics, for example the Standard Model of high-energy physics (to which I will return) has a dual use: a new particle collider or experiment may by calibrated via uncontroversial fragments of the model, which is then in its normative mode. But the goal of the subsequent experiments would be to use the model in its empirical mode, ready to be tested and hopefully falsified. Wittgenstein's last work, *On Certainty*, discusses the balance between these two aspects of rules, e.g.:

> The mythology may change back into a state of flux, the river-bed of thoughts may shift. But I distinguish between the movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other. (Wittgenstein, 1969, §97)

Wittgensteins (implicit) concept of normativity was developed by Brandom (1994, 2001), who inherits the (generally) non-representational character of language from the *Philosophical Investigations*, so that meaning (via reference) does not determine use, but rule-governed use determines meaning. But Brandom insists that such rules be *inferential*, effectively generalizing Gentzen's proof system of *Natural Deduction* in logic, where each logical symbol has an introduction rule and an elimination rule. These are seen as rules of inference for its use, from which its 'usual' meaning is supposed to follow.[80]

Logic is also an excellent example of the dual nature of yardsticks and the idea of 'hardening of empirical regularities into rules': the idea is that for various reasons styles of argumentation and pursuation arose in political, philosophical, and mathematical discourse,[81] became widely adopted, and eventually crossed the line from the *a posteriori* into the *a priori* and hence became rules of inference. These were on the one hand normative, as exemplified by the millennia-long dominance of both Aristotle's explicitly stated syllogistic rules of deduction and Euclid's largely implicit rules of mathematical proof. For example, in normative mode the use of *modus ponens* would simply mean that the user has correctly understood the logical symbol $\rightarrow$ for implication.[82] But on the other hand the very tension between these the Aristotelian and the Euclidean systems,[83] as well as later developments in mathematics calling for new logical systems (culminating in first-order logic as developed by Frege, Russell, and Hilbert, followed by modal logic, etc.) showed that the allegedly *a priori* rules of logical can actually be tested against (mathematical) experience and be changed accordingly.[84] In a nutshell, Brandom then extends the attribution of meaning through inferential use from logic to language.[85]

But what is status of those rules, and hence of the *a priori*?

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[80]For example, the introduction rule for the implication sign $\rightarrow$ states that $A \rightarrow B$ follows if from A one can infer B; its elimination rule is the *modus ponens* (i.e., A and $A \rightarrow B$ imply B. Since inferring B from A involves other rules, rules of inference form a network that can only be used as a whole. See also Garson (2013), Peregrin (2014, 2020), Warren (2020), Povich (2024), and Incurvati and Schlöder (2023). Admittedly, a fully inferentialist interpretation of mathematics remains a project under development.

[81]Szabó (1978) and Dutilh Novaes (2020) convincingly speculate about the origins of these rules.

[82]Wittgenstein's famous 'hardness of the logical must' just reflects this *a priori* status of logic. See Wittgenstein (1969), Part I, §121 and Part VI, §49, as well as Kuusela (2019) and Bangu (2021, 2026).

[83]This tension led to the so-called *Quaestio de Certitudine Mathematicarum* (Mancosu, 1999).

[84]See for example Ferreirós (2001) and Haaparanta (2009).

[85]Brandom eventually even makes purely logical implications (such as $A \rightarrow A$, which are valid for any A) derivative of material implications (such as $A \rightarrow B$ provided the actual content of A and B validates this).

> Another approach, which we may identify with Dewey and the later Wittgenstein,[86] begins with inference conceived as a social practice, whose component performances must answer originally not to an objective reality but to communal norms. Here the appropriateness of an inference consists entirely in what the community whose inferential practices are in question is willing to approve, that is to treat or respond to as in accord with their practices. (Brandom, 2019, p. 17)

This surely applies to mathematics, where it explains three key aspects of its practice:[87]

(1) *Individual fallibility*, to the effect that 'individuals can have perfectly good knowledge about the objective properties of things and yet be wrong in their judgements';

(2) *Collective infallibility*, in the sense that 'it is not the case that everyone could be wrong in this way (...) because the collective generates the norms that determine exactly what, at any given time, these properties actually are.'

(3) *Norm-guided property recalibration,* in that 'the constituting norms of the collective evolve or change as the constituting norms of the collective evolve or change' where 'different factors will affect the stability of such norms, including whether stability itself is considered an asset or a liability'. (McGeer, 2018, 303–304)

In sum, *individual fallibility can be identified right because of collective infallibility*, which on the other hand is a dynamical notion, being subject to recalibration. For example, standards of proof have not only changed enormously over the 2500 years that (axiomatic-deductive) mathematics has existed so far–compare Euclid with Euler or Fourier, and then with Hilbert; as recalled by von Neumann (1947), they even changed three times in the first half of the twentieth century! Having recalled the different ideas of proof held by Brouwer, Weyl, and Hilbert, he summarises the situation as follows:

> I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode, and how they changed three times in succession! I hope that the above three examples illustrate one-half of my thesis sufficiently well–that much of the best mathematical inspiration comes from experience and that it is hardly possible to believe in the existence of an absolute, immutable concept of mathematical rigour, dissociated from all human experience. I am trying to take a very low-brow attitude on this matter. Whatever philosophical or epistemological preferences anyone may have in this respect, the mathematical fraternities' actual experiences with its subject give little support to the assumption of the existence of an a priori concept of mathematical rigour. (von Neumann, 1947/1961, p. 6)

Likewise, as we saw in the historical survey above, even the very concept of physics, and hence of mathematical physics, changed dramatically from Aristotle to Galilei, then from Galilei to Newton, and then again, during the 19th-century transition to modern physics.

From this history, I draw some lessons (see also the discussion on realism in §6):

[86] Brandom's attribution of these ideas to Wittgenstein is disputed by McDowell (2019).

[87] McGeer (2018) wrote this in the different context of responsible agency, but in my view the same points apply to the mathematical sciences, even her points (not quoted here) about the role of fashion.

1. Mathematics (though man-made!) should not compromise. Its exactness and rigour were crucial for the succes of Newton's and all subsequent mathematical physics.

2. But we cannot make nature more exact than it is. The history of science shows that:

    (a) what was once believed to be exact in nature (such as Newton's laws–many make a similar misjudgment of quantum theory) was in fact only approximate;

    (b) Replacements of flawed attempts to give exact mathematical descriptions of nature often use very different principles (which fail, too, now or later).

3. This makes it hard to believe that nature (as opposed to mathematics) is governed by (exact) rules; and even if it is, that we will ever find them. Thus leaving both mathematics and nature as they are, one 'turns the whole enquiry around': successful theories of mathematical physics like Newton's mechanics, Einstein's general relativity, or von Neumann's quantum mechanics are not located in nature but are *a priori* rules. These are meaning-constitutive for their associated models, which in turn are the Wittgensteinian yardsticks to be held against nature. Hence (*pace* Newton) their crystalline purity in the form of exactness and rigour is not a property of nature but of our mode of examination. It is this relocation of mathematics in the study of physics that resolves the apparent tension between mathematics as a man-made practice and physics as a description of some man-independent reality.

4. Hence Wittgenstein's yardstick idea solves (or rather obviates) our opening enigma that mathematics cannot be applicable to nature, although in fact it is. This problem may be summarised by the original view that created it: Plato found fault with the natural world against the perfection of mathematics, believing in the ultimate reality of the latter–which creates a problem for mathematical physics. But a mathematical model of nature need not (and cannot) match nature: it is not a description thereof, and hence there is no need to overcome the mismatch–correctly diagnosed by Plato–by inventing a hypothetical ineffable realm, since the mismatch is just what one would expect if mathematics does not describe but merely compares and measures.

To close this section I now describe the *a priori* I am talking about in some more detail. It is surely not the Kantian one, if only because his distinct analytic *a priori* and synthetic *a priori* has now been replaced by the single concept of the *hypothetical a priori*. I use what Warren (2022) calls a 'meaning-based concept of the *a priori*', which is both *independent of experience* and *meaning-constitutive*. The first property is subtle, since as we have seen in mathematical physics the *a priori* is grounded in experience. But the Wittgensteinian move was to 'harden' such experience into rules. The second property of being meaning-constitutive follows if meaning is identified with use as determined by rules; which in turn form the grammar of a specific language game *defining* the *a priori*.

This kind of *a priori* is often connected to conventionalism. But instead of the version of conventionalism in which 'certain truths are merely reflections of what we mean–or which concepts we employ' (Warren, 2024, p. 28), where axioms constrain (or determine) the meaning of the sentences by the 'free stipulation of the truth' of these sentences, I endorse the version introduced by Poincaré,[88] which has been summarised as follows:

[88] See footnote 13 for references.

> Rather than conceiving of the axioms as freely postulated truths, we should think of them as hypothetical conditions (...) The axioms are considered to bestow meaning on the implicitly defined terms in the sense that they fix a range of possible interpretations. On this construal, there is no postulation of truth by convention. (Ben-Menahem, 2006, p. 140).

Apart from the fact that Poincaré apparently lacked the idea of relativizing the *a priori*, it seems hard to me to improve on his concept of the latter in so far as mathematical physics is concerned (of which he was a master). It is all there, from the idea of implicit definition (which right from the start in *Science and Hypothesis* he called 'disguised definitions or conventions') both in mathematics (especially in geometry) and in physics (where he interpreted Newton's laws as such), to a full appreciation of the subtlety that specific theories and concepts cross the line (as I called it) from the *a posteriori* to the *a priori*, to the conclusion that even if the rules behind the theories of mathematical physics are *conventional*, these conventions are far from *arbitrary*. Let me give just one quote:[89]

> The principles are conventions and disguised definitions. Yet they are drawn from experimental laws; these laws have, so to speak, been exalted into principles to which our mind attributes an absolute value. Some philosophers have generalised too far; they believed the principles were the whole science and consequently that the whole science was conventional. This paradoxical doctrine, called nominalism, will not bear examination. How can a law become a principle? It expressed a relation between two real terms A and B. But it was not rigorously true, it was only approximate. We introduce arbitrarily an intermediary term C more or less fictitious, and C is *by definition* that which has with A *exactly* the relation expressed by the law. Then our law is separated into an absolute and rigorous principle which expresses the relation of A to C and an experimental law, approximate and subject to revision, which expresses the relation of C to B. (Poincaré, 1913, pp. 125–126)

Hilbert, more than Poincaré, in addition emphasised that theories of mathematical physics arise via the axiomatization of sufficiently mature informal theories of either (theoretical) physics or mathematics itself. Both to avoid confusion between the various kinds of conventionalism on the market, and to relate the terminology on the transition from the 'Euclidean' to the 'hypothetico-deductive' model reviewed in the previous section, I therefore prefer to call *a priori* rules or theories 'hypothetical' rather than 'conventional'.

Just as mathematical theories in general implicitly define the mathematical concepts appearing therein (such as spacetime) through their axioms, theories of mathematical physics (such as general relativity) implicitly define the physical concepts appearing therein (such as spacetime) through their stipulations (assuming the mathematical concepts, such as a manifold and a metric, are understood via the axioms of the underlying mathematical framework). Likewise, in quantum theory concept like observables, states, spectra, wave-particle duality, and probability are *defined* by the mathematical formalism. Take the following highlight of the Frege–Hilbert correspondence, whose opposition marked the transition from nineteenth to twentieth century mathematics:[90]

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[89] Ivanova (2015) contains a tasteful choice of some others quotations relevant to this discussion.

[90] This point pervades their entire correspondence (Gabriel et al., 1980) and e.g. Blanchette (2018).

> You [Frege] say further: 'The explanations in sect. 1 are apparently of a very different kind, for here the meanings of the words "point", "line", ... are not given, but are assumed to be known in advance.' This is apparently where the cardinal point of the misunderstanding lies. I do not want to assume anything as known in advance; I regard [all the axioms of groups I to V] in sect. 1 as the definition of the concepts point, line, plane. (Hilbert to Frege, 29 December, 1899; Gabriel *et al.*, 1980, p. 39)

Hence it is as if Frege would insist that concepts like space and time are understood *prior to* the establishment of a theory like general relativity in which they appear, whereas Hilbert (whom I obviously side with) would define these concepts *by* general relativity. Likewise for the case of quantum theory as just discussed.[91]

5 Models

> [The] Scottish commonsense tradition emphasized the relativity of knowledge: the idea that knowledge of an object always emerges from a comparison of that object with something else. (Suárez, 2024, p. 22)

I now discuss the nature of models, which in my approach are the yardsticks to be held against nature (see below how this is done).[92] Historically, the rise of models since the (late) nineteenth century has been well documented, both in general,[93] and in the case of *mathematical* models to which I can restrict myself.[94] Compared to the general case, I therefore do not need to discuss mechanical models, scale models, maps, paintings, etc.

Neither will I discuss topics like the 'syntactic view' (also called the 'received view'), or the 'semantic view'. Among the general objections to these views,[95] I especially note that in mathematical physics practice no theory or model of mathematical physics tends to be formulated in such terms. For example, general relativity is rarely discussed as a logical theory, except perhaps by philosophers; in standard mathematical physics this theory is stated in terms of differential geometry, partial differential equations, and topology. Perhaps the semantic view comes closer to this practice, in focusing on spacetimes with a Lorentzian metric satisfying the Einstein equations for given matter content, but even so, few mathematical physicists would say that this class of models *defines* the theory: if anything it is the Einstein equations themselves that 'define' general relativity.[96] Likewise for quantum mechanics and quantum field theory, seen as mathematical frameworks within which physical systems can be formulated as models.

The different roles of theories and models are a corollary of the previous section:

[91] This seems similar to what is called *spacetime functionalism*, especially in what Knox and Wallace (2023) call its 'constitutive' form. The origins of this idea in the logical positivist tradition (as opposed to the philosophy of mind usually emphasised) are worth exploring; see also Butterfield and Gomes (2023).

[92] It may be a lucky coincidence in my favour, but the word 'model' derives from the Latin via *modellus*, *modulus*, and *modus*, the latter denoting a small measuring deivice (Gerlee and Lundh, 2016, p. 1).

[93] See for example van Fraassen (2008), Morrison (2015), Gerlee and Lundh (2016), de Regt (2017), Elgin (2017), Frigg and Hartmann (2018), Frigg (2022), and Suárez (2024).

[94] See for example Blanchette (2017), Ferreirós (2022), Friedman & Krauthausen (2022).

[95] Cf. Morrison (2007, 2015), Halvorson (2012), Frigg (2022), Wallace (2022, 2024), and Suárez (2024).

[96] I doubt that the class of solutions to the Einstein equations would even determine these equations–this is not model theory in logic. But even if they do, my conception of a model would make this irrelevant.

- *Theories* define an area of mathematics (such as Euclidean geometry or group theory) or mathematical physics (such as general relativity, quantum mechanics, or statistical mechanics), the latter often combining some of the former. Using the idea of implicit definition, theories define the meaning of their own concepts, such as space and time, evolution, determinism, randomness, etc., as well as for example black holes or elementary particles. Thus theories are meaning-constitutive.
- *Models* are solutions or examples of such theories, construed broadly–my concept of a mathematical model is quite pragmatic: it may (ideally) consist of exact solutions of the fundamental equations of the theory, or, if this is impossible in practice, of approximate solutions thereof (including numerical ones that may even be shown graphically), or of exact solutions of approximations to these equations, or even of approximate solutions of approximate equations suggested by the theory. But in all these cases the meaning of everything should have been determined already:

  A model uses the conceptual framework of the theory it is a model of.

Thus models are neither referential nor empirical, yet they are what makes some theory empirically relevant. This approach may be benchmarked against the five 'distinguishing characteristics of a theoretical model' listed by Redhead (1980):

> (1) It is a set of assumptions about some object or system.
> (2) These assumptions attribute an inner structure, composition or mechanism, which manifests itself in other properties exhibited by the object or system.
> (3) These assumptions are treated as a simplified approximation useful for certain purposes.
> (4) The model is proposed in the framework of some more basic theory or theories.
> (5) The model may display an analogy between the object or system described and some other object or system.

Of these, only (4) is essential in my approach; the others may or may not apply.[97] But if it applies I look at (3) in a very different way from Redhead, who says that (3) reveals the difference between theories and theoretical models, in that 'we believe our theories to be true', whereas 'a theoretical model is definitely believed to be false.' I concur with the latter: as eloquently explained by Elgin, models are nothing but 'felicitous falsehoods':

> Modern science is one of humanity's greatest epistemic achievements. It constitutes a rich, variegated understanding of the natural world. Epistemology could readily accommodate its extraordinary success if that understanding were expressed in accurate representations of the phenomena. But it is not. Science couches its deliverances in models that are purposefully inaccurate –models that simplify, augment, exaggerate and/or distort. (...) [the] point is not just that contemporary science contains idealized models that are strictly false; rather, science *consists* of them. (...)

[97]Expanding point (3), Gerlee and Lundh (2016), p. 13, quite rightly insist that *isolation* (i.e., the restriction of a phenomenon to something that is graspable) must precede *simplification*, and that their conjunction comprises the essence of models. As we saw, Galilei was a model builder *par excellence* in that respect.

> models enable us to understand reality in ways that we would be unable to if we restricted ourselves to the unvarnished truth. The point is not just that the features that a model skirts can permissibly be neglected. They *ought* to be neglected. Too much information occludes patterns that figure in an understanding of the phenomena. The regularities a model reveals are real and informative. But many of them show up only under idealizing assumptions. (...) Effective models are what I have called *felicitous falsehoods* (Elgin, 2017).

But–and here lies my difference with Redhead and other realists– the same applies to all theories! The history of mathematical physics as reviewed earlier in this *Element* suggests that all its great theories, from *Principia* to general relativity and quantum (field) theory, are merely mathematical constructions, whose models are 'felicitous falsehoods'; although the term 'falsehood' is misleading in the sense that in my approach neither theories nor their models even have a truth value. As to theories, this is a corollary of my specific Wittgensteinian interpretation of theories (i.e., as empirical regularities hardened into rules), as well as of the *a priori*, as explained in the previous section.[98] Models, on the other hand, float between the *a priori* and the *a posteriori* and as such lack truth values *themselves*. However, there is some room for truth: *that* a model *has* a certain property (such as the possibility of superluminal communication) is a matter of fact and hence may be deemed true, although neither the model nor this property by itself have truth values.

This relative concept of truth may be put in a more general context:[99]

- In pure mathematics, where each specific theory T carries its own (implicit) definitions and proof system, it is not theorems φ of T but metamathematical claims of the sort $T \vdash \varphi$ (i,e., 'T proves φ') that are true (or false). This is quite anti-platonic.
- Since a model M having property p corresponds to a theorem φ of the theory T of which M is a model, it follows that properties p of models M have no truth value; but claims to the effect *that* M has property p do. Even realists might accept this!

A further relative kind of truth (or at least accuracy) may also arise from the comparison between a model and nature. *How is this comparison made?* First, recall from the Introduction that in modern physics the notion of a "target" is problematic, so that traditional ideas about the relationship between a model and its target do not apply. To obviate this problem I follow van Fraassen (2008) in letting a *data model* stand between the natural phenomena some mathematical theory and ensuing model are supposed to be about, and that mathematical model. Think of a numerical tables in which the positions of planets are recorded on a daily basis.[100] Even in the most advanced sciences, the mathematical structure of data models is elementary: they consist of numerical tables or more

[98]Different interpretations of conventions would make the rules stating these true by definition.

[99]The first bullet on truth in pure mathematics is defended in Landsman and Singh (2025). The second bullet is a corollary thereof, although it may be defensible also by itself, without endorsing the first one.

[100]The associated theory could be Newton's theory of gravity, or Einstein's, whose pertinent models would then be some Newtonian spacetime with specific heavenly bodies moving according to Newton's laws, or a solution to Einstein's equations for some energy-momentum tensor. The parameters in these mathematical models (like the masses and orbital parameters) are estimated from the data models they are supposed to measure, exemplifying the endless ping-pong match between theory and data inherent to physics. Unlike data models, mathematical models can be as mathematically advanced as one likes.

complicated arrays of numbers, or graphs, or, like in the old days of bubble chambers, of photographs displaying simple geometric figures or patterns. As van Fraassen states, data models do not stand on their own or come out of the blue; they are typically constructed on the basis of theories. Van Fraassen then asks and answers the million dollar question:

> *what is the relation between the theoretical model and the phenomena it models*?
>
> The short answer is this: construction of a data model is precisely the selective relevant depiction of the phenomena *by the user of the theory* required for the possibility of representation of the phenomenon. (van Fraassen, 2008, p. 253)

With due flexibility in the interpretation of what is meant by a 'user' (which may be a synchronic or even diachronic team of scientists!),[101] I take this to be not only a valid description of the practice of (astro)physics, but also an answer to the applicability question: how mathematics can possibly relate to nature, given that, as I have argued, the latter lacks a mathematical structure (much as it lacks a linguistic structure). On a Wittgensteinian view mathematics is not discovered but invented, so that any kind of mathematical structure can only reside in our theories of nature, not in nature itself (even granting the realists that nature is not a product of the mind). Indeed, it is the 'user' who extracts mathematical structure from natural phenomena, and who presumably selects these phenomena right because of the possibility of doing so.[102] In this repect we side with Wigner and Zimmer:

> [T]he point which is most significant in the present context is that all these laws of nature contain, in even their remotest consequences, only a small part of our knowledge of the inanimate world. (Wigner, 1960, p. 5)
>
> The beams of scientific flashlights are bright, but only because they are narrow. (Zimmer, 2021, p. 10)

Thus the role of models as yardsticks or objects of comparison has been shifted from a naive place between theories and natural phenomena to their proper place between mathematical theories and data models (which as said are also mathematical, but low-grade).

The question how the comparison is done remains. Once the move from phenomena to data models has been made this becomes a pragmatic question. The history of mathematical physics as reviewed above, in particular the stage set in this respect by Ptolemy and subsequently Huygens, whose methodological views (though not his theories) eventually trumped Newton's, suggests that this is done by *surrogative inferentialism*. This originally meant that inferences about a model enable inferences about its target, based on some kind of structural similarity between model and target.[103] Since in modern mathematical physics targets as usually conceived are questionable for the reasons stated in the Introduction, the inferences about a mathematical model (of some underlying theory) should lead to inferences about a *data* model for some range of natural phenomena.[104]

[101] The user also carries out the data analysis, which as recalled in the Introduction is theory dependent.

[102] See e.g. Bogen & Woodward (1998) and Bokulich & Parker (2021) for critical discussions of data models. Since both accept unambiguous targets, their arguments need not apply to mathematical physics.

[103] See e.g. Swoyer (1991), Bueno & French (2018), Warren (2020), Frigg (2022), and Povich (2024).

[104] The vast differences in mathematical structure and sophistication between mathematical models and data models make referential or representationalist practices less appealing than inferential ones, although

Even so, mathematical models keep their tradition role of explaining, understanding, predicting and retrodicting various aspects of the phenomena as captured by the data model.

The use of data models is a crucial part of the answer to the question how mathematical models relate to nature. In different approaches, one either has to assume that nature itself is mathematical, which as explained in the first half of this *Element* I claim one cannot reasonably believe after Newton, or introduce some circularity.[105] Data models break the latter threat in having a mathematical structure, low-grade as it may be, so that the comparison between mathematical models and data models is done *within* mathematics.

Since mathematical models of modern theories of physics use high-level mathematics whereas data models are low grade, there is little hope for structural similarities between the two: think of the Schwarzschild (or Kerr) spacetime define by general relativity versus a table of dated planetary positions (or VLBI radio data coming from M87 or Sag A*). For this reason I side with Suárez (2024) in asking very little of surrogative inferences, except that the (mathematical) model has what he calls *representational force* and *inferential capacity*, i.e., the capacity to enable 'a suitably competent and informed inquirer in the appropriate normative context of use' to actually make inferences about the target (in his case), or the data model (in van Fraassen's approach). Efforts to make this more precise are, in my view, doomed to fail, and in any case are less useful than concrete examples.

First, the theories of Ptolemy, Copernicus, Kepler, Newton, and Einstein all allow inferences about future positions of the planets from current or past ones, which may be checked against data models in the form of tables. Or, in a more complicated example: the mathematical model is the Kerr black hole spacetime of general relativity, perturbed by surrounding matter that produces radio waves of which some reach the Earth. The relevant parameters (like the mass and angular momentum of the black hole) are estimated from a data model, which in case of the famous EHT images of M87 and Sagittarius A* already alluded to consists of data from various runs of an array of radio telescopes all over the world, appropriately synchronised and organised. Here the mutual interdependence of mathematical model and data model is so intricate that the traditional idea that the former has some target becomes chimerical (cf. the Introduction).[106] Indeed, properties typically attributed to a target ("there is a supermassive black hole in the center of our galaxy") on the basis of data belong to the mathematical model. This example also shows the role of the *a priori*: the *theory* of general relativity *constitutes* the meaning of "spacetime", "causal structure", and "black holes", since it is within this theory that one can *define* these concepts in a model-independent way,[107] in order to *apply* them to specific models.

My second example highlights the role of theories as yardsticks. The *Standard Model of high-energy physics* (SM) was developed in the 1960s and 1970s, and turned out to

in some simple cases the former may do similar work as the latter (especially if a data model is dressed up to as to resemble a mathematical model). Even in that case one could reformulate referential semantics inferentially. My preference for the latter even when one has a choice comes from my more general Wittgensteinian view of mathematics and language, cf. §4 above, as well as Landsman & Singh (2025).

[105]I obviously sympathise with Wallace's (2022, 2024) 'math-first' approach to physical theories, but reject his associated structural realism; see also Psillos (2001). See Frank (1937) for a penetrating early analysis of the idea that 'nature can only be described through abstract formulas'. (p. 41).

[106]Again, see Doboszewski & Elder (2025) and Ochigame, Skulberg, & van Dongen (2025) for details.

[107]See e.g. Landsman (2022), chapter 10, largely based on work of Penrose. Many textbooks on general relativity do not cleanly separate the development of the conceptual apparatus from its use in models.

be astonishingly accurate, culminating in the discovery of the Higgs boson in 2012 at CERN.[108] The *theory* underlying this *model* is quantum field theory (QFT),[109] which for example *defines* elementary particles, including concepts of mass, charge, and symmetry (of which the SM has various sorts). Similarly, QFT *defines* what is meant by a vacuum, by energy, etc.[110] Somewhat depending of what one exactly means by a QFT, the SM is an interpretation or realization of its mathematical and conceptual framework.

Upon its completion in 1975, the SM was triumphantly hailed as the truth about (fundamental) physics, incorporating everything that was known about atoms, light, electrodynamics, radio-activity, nuclei, elementary particle physics, etc. (except gravity). Part of this triumph remains: in its normative role the SM defines what we *mean* by electrons, photons, quarks, weak neutral currents, etc. But its second role as an *empirical* yardstick resonates with the fact that today, physicists *hope* that new experimental results from colliders or cosmic rays will *violate* the predictions (inferences) of the model! Thus the SM is held against new data models perhaps without expecting agreement.

The Standard Model may seem an extreme case; but I claim that *every* physical theory has this status, albeit with different balances between the normative and the empirical interpretations of its yardsticks (i.e., its models)–this balance is fluid in any case. *Every* theory is eventually overtaken, often accompanied by profound conceptual changes and redefinitions of basic terms (see the discussion below). The emergence of non-Euclidean geometry in the 19th century may also be (re)interpreted in this light: following the 2215 years (300 BCE–1915) in which Euclidean geometry was seen as a true description of the geometry of the world, it remains viable as a yardstick.[111] Similarly for the exact solutions of Einstein's equations for general relativity:[112] Minkowski spacetime is an accurate yardstick in empty places in the universe; for the solar system we use the Schwarzschild solution; near a rotating black hole one uses the Kerr solution; the expanding universe is approximately matched by the Friedman (Lemaître–Robertson–Walker) solution, etc.

Are such models, then, approximately "true"? They are, but only in the rather weak sense that inferences made from them enable approximately valid inferences about data models extracted from phenomena in the corresponding natural realm. Though developed for differente purposes (made clear in the second part of the quote below), the pragmatist concept of truth expounded by Andersen (2023) captures this process quite well:

[108] See Galison (1983), Pickering (1984b), Pais (1986), and Brown et al. (1997) for various aspects of the history of the Standard Model. Cottingham & Greenwood (2007) is a nice textbook.

[109] This case differs from general relativity in that quantum field theory, which I would like to take the SM to be a model of along the lines explained in this section, has not yet reached (and perhaps will never reach) the state of rigour that contemporary mathematical physics asks for. In contrast, in general relativity both the theory and the models are mathematically rigorous. However, given a set of Feynman diagrams and Feynman rules suggested by some Lagrangian, renormalised perturbation theory is completely rigorous (even for gauge theories), including its truncations and approximations. These diagrams and rules could be taken to be the *definition* of the model. The unfortunate fact that physics textbooks derive Feynman rules and renormalised perturbation theory in a heuristic way from a non-rigorous theory (whilst mathematical physicists are unable to do much better so far) admittedly changes the relationship between theory and model, but it does not compromise the possible use of this model as a yardstick against data models.

[110] See, from somewhat different perspectives, Haag (1992) and Duncan (2012).

[111] See for example Gray (2007) and Torretti (2012).

[112] Here Stephani et al. (2003) is a standard reference.

> Thinking of truth as trueing, like trueing a bicycle wheel, facilitates thinking of models and products like diagrams or maps in terms of how well-trued they are to their target. It captures the Duhemian holism that is also central to pragmatism. It emphasizes the *process* by which models are brought into true with their target systems, and the fact that this process is not accomplished once and then is done forever, but instead requires upkeep and ongoing fine-tuning even in the best of cases. True need not be thought of as part of a binary state ascribed to propositions corresponding to individual bits of the world, true forever or never. True should instead be thought of in terms of being *in true*. It is a directed relation between a knowledge product and some feature(s) of the world. It is not ascribed only to individual propositions, or even to well-defined collections of propositions. It admits of degrees and can be applied to a wide variety of formats, including maps, diagrams, highly idealized models, and more. There is no single Right Way to true a wheel, though there are clearly norms by which we can judge if a wheel is in true. (...)
>
> Many of us are concerned with a characteristic anti-scientific attitude, one which situates the activities of non-specialists reading misleading pop articles as "research," one which rejects scientific results with challenges of the form, "But do we really *know* that?" This challenge is one we lose when we cede ground by acknowledging that we do not know, for instance, that human-caused climate change is driving increasing ecological disasters and climate instability. The current strategy is often to say, "No, we don't *know* that, but ..." and then add caveats such as: science can't really know anything; here is our extensive justification for this claim; here is how knowledge generation in science works; etc. I think these are all wrong, from the start. We *do* know these things, full stop, on a pragmatist account of truth and knowledge. (Andersen, 2023, pp. 68–70)

6 Summary and discussion

> Just as sea and sky blur together at the horizon, so, too, dream and reality could easily become confused when viewed from a distance. (Yukio Mishima, *Runaway Horses*)

The philosophy of mathematical physics proposed in this *Element* takes the 'crystalline purity' of exact theories like general relativity or quantum mechanics out of nature by relocating such theories to the *a priori*, construed in a relativized way and decoupled from Kantian intuition and necessity, so that only its meaning-constitutive part remains. In this way, I have tried to integrate the history of mathematical physics (and to some extent mathematics) with "best philosophical practices" from Aristotle to Wittgenstein.

To start, although Aristotle's "*qua*" act of separation as such rarely if ever leads to *ideal* mathematical objects or properties, it still gives rise to mathematical objects, albeit "imperfect" ones (such as a line with breadth or a triangle with if ever so slightly rounded corners). The mind may then so to speak complete the act of separation by replacing these imperfect objects with perfect ones, which may then act as *objects of comparison* to be held against the imperfect mathematical objects, perhaps first without, and subsequently with their physical properties.[113] These perfect mathematical objects can then be

[113]This gives Aristotle's mathematics the 'if ... then' or fictional character often attributed to it.

shared so as to become "memes": mathematical purity resides neither in the ineffable platonic realm (whose existence, following Aristotle and Wittgenstein, I deny) nor is it just an individual psychological construction à la Brouwer: it is a *shareable resource*, both synchronically and diachronically,[114] which consists of *rules*–ideally, crystal clear ones.

This way of (re)locating the 'crystalline purity' of exact theories also incorporates Galilei's epistemological distinction between what is mathematically simple and hence within reach of human intellect and what is actually happening in nature, being too complex and therefore unknowable. In mathematical physics, then, the role of the mathematically simple is to serve as an object of comparison held against the complex phenomena in nature. On the other hand, although Newton was the main founder of mathematical physics, little of his philosophical legacy is saved on my proposal: the tension he encountered between his traditional commitment to seeing mathematical objects actually realised in nature (notably mechanical trajectories) and his innovative and successful concept of force, especially the gravitational one, increasingly undermined and eventually blasted his traditional ("Euclidean") framework. Indeed, the principal historical development in mathematical physics (and also in mathematics and theoretical physics) has been the replacement of this framework by the hypothetical-deductive one pioneered by Huygens.

Finally, the inextricable tangle of theory and observation in modern physics (incorporating in particulart collider physics and relativistic astrophysics) has severely compromised the idea that models have targets. This suggests the replacement of natural phenomena by data models à la van Fraassen as well as the rejection of the idea of structural similarity between (mathematical or theoretical) models and their targets in favour of surrogative inference as the pertinent relationship between mathematical models and data models. This practice in fact goes back to Ptolemy at least; what we should reject is his accompanying realism, much as we should reject the realism of Copernicus.

The end result of this entire history and philosophy of mathematical physics (as I see it) is that external reference to real targets in both nature and mathematics has disappeared:

> [W]e are not in fact uncovering the underlying mathematical structures realized in the world; rather, we are constructing abstract mathematical models and trying our best to make true assertions about the ways in which they do and do not correspond to the physical facts. (Maddy, 2008, p. 33)

> Many scientists, in particular theoretical physicists, have a strong tendency to think that their theories are even more real than phenomena themselves. This tendency stems in the last analysis from a somewhat primitive epistemology, from a lack of acknowledgment of the subtle relations between the theories (that they handle so well in practice), the models subsequently established, and the data obtained from their experimental colleagues. It is the theoretician's *hubris* and a form of residual Platonism (ideas being more real than appearances), paradoxically the outcome of a too pragmatic, hence, naïve orientation. The subtle equilibrium that scientists like Hertz or Maxwell managed to attain, probably thanks to their mastery of both theoretical and experimental practices, and their acquaintance with conceptual and philosophical difficulties, seems to have been lost in our age of hyper-specialized scientists. (Ferreirós, 2022, pp. 327)

[114] See the discussion on Husserl in the Introduction of Landsman and Singh (2025).

The philosophy of mathematical physics I propose also resonates with the idea, perhaps already present in embryonic form in Reichenbach (1920) but certainly made famous by Kuhn (1962),[115] to the effect that 'normal' science takes place within a generally accepted *a priori* framework, whereas paradigm shifts (or 'lexical changes' as Kuhn came to call them later) correspond to such frameworks beginning to shake for all kinds of reasons (ranging from new experiments or observations to the discovery of internal contradictions, or problematic embeddings in larger fields), be overthrown, and finally replaced,[116] even in such a way that theories may be 'incommensurable' with previous ones.

This Kuhnian picture, in turn, at least in part inspired the "pessimistic (meta) induction" threat to realism (associated with Putnam and especially Laudan). Of the various forms this argument may take,[117] I just take the following simple and exemplary one. If:

1. in the past most if not all scientific theories have undergone such radical changes that none of them was ever correct except perhaps empirically and approximately;

2. the present is not privileged, so that these changes may be expected to continue,

then no theory can be said to describe reality in the sense that the entities theory posit are real and the descriptions of the world they describe are true or approximately true (where truth is defined by correspondence with reality, which in turn requires that the theory in question is at least in part referential).[118] Except for denying the second premiss (which indeed is hard to refute but too Whiggish to my taste), an apparently promising realist answer to this argument, at least in the context of physics and astronomy, is to admit that although the conceptual structure of physical theories has changed dramatically, there are decisive elements of continuity through theory changes, especially at the level of mathematical structures and/or equations.[119] These may survive theory change either approximately, within some domain of validity (like Newtonian mechanics for small velocities and small masses) or even entirely if they are duly "deformed" (which is one way of looking at quantum mechanics).[120] In particular, as an early example, Poincaré 'claimed that science is concerned not with the things by themselves but with the relations between things', that 'the discontinuities that occur in the history of science concern the "nature" of theoretical entities' (rather than the structure of the theory), and that therefore 'there are no radical discontinuities at the level of mathematical structure'.[121]

[115]Later revised in various ways (Horwich, 1993; Kuhn, 2000; Friedman, 2001, 2002).

[116]This is what makes the *a priori* 'relativized'. In general relativity (*sic*) Friedman (2001) puts the mathematical framework for general relativity on the *a priori* side, but not Einstein's equations. I prefer to add these–but not the specification of particular energy-momentum tensors and corresponding solutions, which are on the model side. Either choice has (dis)advantages but mine aligns with my view on models.

[117]See Wray (2018), chapter 5, for a detailed discussion of four different versions of the argument.

[118]See Psillos (1999) for various forms of scientific realism, which I condensed into the version above.

[119]This may be seen as a special case of Psillos' (1999) *divide et impera* argument to the effect that one may cherry-pick certain parts of theories that survive theory change. Even apart from the argument that follows I am skeptical about this way of taking "holistic" theories apart; see also Vickers (2013).

[120]See e.g. Saunders (1993) and Wray (2018), chapter 7. Hacking (1983) and Galison (1997), §9.3, highlight continuities in experimental practices.

[121]See Ivanova (2015), §2, who also documents the structural realists claiming Poincaré as one of them. Mathematical structures are themselves mathematical objects and hence I have never understood the alleged difference in ontology between the two that is supposed to save some form of mathematical realism.

Though correct in principle, this move may only save those forms of realism which declare that what is real is what is preserved through theory changes. This can only be done with hindsight, and is also somewhat circular. But on top of these objections, if what is preserved and hence is considered real is mathematical, then (except on certain extreme forms of platonism or other kinds of mathematical realism wholly at odds with the view of mathematics as a human practice I advocate) this argument runs against one of the main points I have made: that mathematical objects and structures used in mathematical physics since Newton's introduction of forces in *Principia*, all the way down to Einstein's Lorentzian metric in general relativity and von Neumann's complex Hilbert spaces and operator algebras in quantum (field) theory, go well beyond those that may reasonably be held to be instantiated by natural objects; and that one should therefore deny at least pythagorean reality to all but the most basic ones of these objects and structures, hardly any of which play a role across radical theory changes. And if it is not pythagorean or platonic realism (which has its own problems), it is obscure what mathematical realism even means. The only acceptable solution seems to me to attribute idealised mathematical objects the reality of *memes* (like rules of a game), as alluded to above, shared both synchronically and diachronically. But that seems remote from the ambitions of realists.

This reply may seem countered by the Quine–Putnam *indispensability argument*:

> According to this line of argument, reference to (or quantification over) mathematical entities such as sets, numbers, functions and such is indispensable to our best scientific theories, and so we ought to be committed to the existence of these mathematical entities. (Colyvan, 2024)

This may be used with or without the above continuity argument, but either way, further to the objections reviewed by Colyvan (2024),[122] in the spirit of this *Element* one would like to add that the indispensability argument confuses object (i.e., natural phenomena) with measuring device (i.e., mathematical theories and their associated models used as yardsticks), projecting properties of the latter onto the former. See points 3 and 4 in §4.

Last but not least, realists have forwarded the 'no miracles' argument, often seen as the strongest argument for scientific realism, according to which the success of scientific theories is only explicable if they are at least approximately true. Again there are various versions of this argument,[123] of which the strongest form seems to me to be closely related to a point made by Wigner (1960).[124] Towards his conclusion, ending his paper, that

> 'The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve' (p. 14),

he argues, juxtaposing two entirely different points in a single paragraph, that

> Mathematical concepts turn up in entirely unexpected connections. Moreover, they often permit an unexpectedly close and accurate description of the phenomena in these connections. (p. 2)

[122] Of these, the one due to Field claiming that mathematics is dispensable in science is remote from me.

[123] See Psillos (1999) and Wray (2018) for these versions, from opposing views.

[124] Strangely enough, this famous paper is rarely if ever cited in the context of the no miracles argument.

The first point made here is actually not difficult to answer.[125] The greater the abstraction of some piece of mathematics, the greater its generality, and hence the greater its applicability, whatever its origin in physics or mathematics or elsewhere. In addition, the relocation of the 'crystalline purity' of mathematics from the natural realm to the *a priori*, and the accompanying role of models as objects of comparison rather than descriptions of nature, makes theories and models into *tools* rather than *truths*, whose wider applicability than their original purpose is not really miraculous, although it may still be impressive.

The underdetermination thesis strengthens this reply.[126] This thesis even makes the use of mathematics that was originally invented for different purposes quite natural: people like Einstein, in his development of general relativity, or von Neumann, in his mathematical development of quantum mechanics, first looked for mathematics that was available. Their genius enabled them to develop this mathematics according to their needs.[127]

Combined with a flavour of conventionalism (cf. footnote 16), the underdetermination thesis also produces an argument against at least pythagorean mathematical realism. Namely, specific theories (of mathematical physics) may be rewritten in mathematically equivalent ways that however significantly change the physical picture. For example:

- Although Newtonian gravity was originally based on Euclidean geometry, and is still generally seen that way, it has been known since the 1920s that it also admits a (natural) formulation in curved spacetime, like general relativity.[128]

- Although general relativity is usually formulated in terms of a Lorentzian metric g on some manifold M as the primary mathematical structure,[129] the alleged reality of this geometric rendition is compromised in multiple ways:

    1. Einstein himself did not regard general relativity as a geometric theory.[130]
    2. One may formulate general relativity as a theory of interacting massless helicity-2 particles (gravitons) in Minkowski spacetime.[131]
    3. The metric may be merely a *façon de parler* that encodes dynamical behaviour of the matter fields, rather than the cause or origin thereof.[132]

[125] See also the references in footnote 5.

[126] This thesis claims that there could be various incompatible theories that all describe or explain certain natural phenomena or data sets, so that it would be hard to consider one and hence not the others to be true. Wigner (1960) himself repeatedly highlights the possible lack of uniqueness of the theories of physics.

[127] Einstein did this by combining Riemann's metric geometry in positive signature for curved spaces with Minkowski's metric geometry in indefinite signature for flat spacetimes. Maddy (2007), §IV.2, points out that Einstein's use of Riemannian geometry becomes less surprising if one realizes that during his development of differential geometry Riemann himself was looking both at the physical structure of space and at field theory (in the physics sense of the word). Einstein's use of Minkowski spacetime was of course very natural, as it was developed as a mathematical framework for his special theory of relativity. Von Neumann knew or found many examples of what we now call Hilbert spaces, which he then abstracted and defined axiomatically, along with his theory of unbounded operators. See Landsman (2021).

[128] The original argument was due to É. Cartan. See Malament (2012) for an excellent discussion.

[129] This view was pushed especially in the famous textbook by Misner, Thorne, and Wheeler (1973).

[130] Weinberg (1972) also criticised the geometric view, although he thought that Einstein held it–who in fact felt that (using tensor calculus) his achievement was to unify gravity and inertia (Lehmkuhl, 2014).

[131] This idea goes back to Pauli and Fierz. See Salimkhani (2020) for discussion and references.

[132] This idea originated with Brown (2005). See Brown & Read (2021) for a recent review.

- Quantum theory admits many reformulations, of which my favourite one is in terms of pure state spaces with a transition probability, where all linearity and complex numbers have been removed in favour of a dance of interlocked two-spheres.[133]

Returning to Wigner's second quote, it is the second point, almost an aside, that supports the 'no miracles' argument, especially if it is strengthened by adding the historical point (not made by Wigner) that it is not just the *accuracy* but also the striking *generality* of the main theories of mathematical physics that was truly unexpected. In particular, quantum mechanics, which was invented as a theory of atoms and light, turned out to apply to condensed matter physics, nuclear physics, etc.; similarly, quantum field theory, which was originally developed merely as a framework for quantum electrodynamics, came to encompass all of particle physics, and also there was astonishingly accurate. The fact that in principle all of condensed matter physics follows from quantum electrodynamics deserves to be more widely known. Likewise, general relativity started as a theory of gravity and inertia, but it gave us black holes and the big bang. Another spectacular example is thermodynamics, which initially was just a theory of heat and work inspired by the industrial revolution, but over 150 years evolved into a framework for energy and entropy that turned out to be universally applicable, from steam engines to black holes.

This does remain miraculous; but it is a miracle to the realist and the empiricist alike. Neither has a good explanation for the unbelievable luck that physicists found such unexpectedly powerful theories from very limited data in the first place, and this puzzle is independent of the question whether these theories are true or merely empirically successful; or believed or just accepted. Perhaps the least unconvincing explanation is the 'selectionist' one originated with van Fraassen (1980), pp. 39–40, to the effect that theories that fail to be more accurate and/or general than originally expected tend to be discarded.[134]

Let me end on another sobering note. The loss of realism in mathematical physics I advocate faces an additional challenge beyond Wigner's, namely answering the question:

'*what data are taken to provide evidence about*.'[135]

My answer would be that data provide evidence about those aspects of the natural phenomena from which these data are extracted that the 'user' of these data is interested in. This answer must be unbearable to a realist, but for an empiricist it is the point of science.

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[133]See von Neumann (1937/1981) and Landsman (1998), and references therein.

[134]See Wray (2018), chapters 9 and 10, for a more recent (and supportive) discussion of this argument.

[135]See Bokulich & Parker (2021), p. 17, who note that '[in] van Fraassen's scientific structuralism (...) the relation between data models and the world is not just unaccounted for, but in effect erased.' (p. 6).

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