Philosophy of Mathematical Physics

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

Abstract

A philosophy of mathematical physics ought to balance a philosophy of physics with a philosophy of mathematics, such that the early modern 'mathematization of the world picture' as well as the theory-laden and ambiguous character of the targets of mathematical models of modern physics fall into place. Wigner's 'unreasonable effectiveness of mathematics in the natural sciences' also falls within its scope. Guided by a historical survey, we propose that theories of mathematical physics (such as general relativity, quantum mechanics, and statistical mechanics) should be seen as meaning-constitutive *a priori* constructions, hypothetical but far from arbitrary, whose models mediate between theory and nature, that is, between the *a priori* and the *a posteriori*. Models do so by playing the role of Wittgensteinian yardsticks or objects of comparison to be held against data models, where the comparison is done via *surrogative inference*. This balancing act involves a loss of realism on both sides.

Contents

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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 paper hopes to contribute to these attempts from the perspective of a mathematical physicist. Some of the most famous philosophers in history including Plato, Aristotle, Kant, and Wittgenstein were troubled already by "everyday" mathematical objects like triangles and numbers, asking for example how the irrationality of $\sqrt{2}$ (which could hardly be inferred empirically from a drawn triangle) or the equality $7+5=12$ (whose apparent reference to actual "numbers" has puzzled philosophers down to Frege) are related to the changing and unreliable world of human experience, which seems separated from the timeless truth, certainty, and perhaps even "necessity" of mathematical theorems.

These problems are difficult enough and they arguably remain unresolved.[1] The perspective of mathematical physics adds three further complications to this already highly nontrivial general applicability problem. The first of these was stated by 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.[2] (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 (and most if not all earlier) mathematics could still reasonably be argued to be instantiated within nature in a straightforward way (bracketing the issue of idealization, which we will discuss in detail). Newton's mathematics could not.[3] 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' (to paraphrase Dijksterhuis's title) in the work of Galilei (as well as his predecessors like Plato, Ptolemy, Copernicus, and Kepler in so far as astronomy and cosmology were concerned). From 1687, we see a direct historical and conceptual path to the potentials introduced by Legendre, Laplace, and Poisson around 1800, to the (Lorentzian) metrics in Einstein's theory of general relativity from 1915 (which assembles 10 potentials for the gravitational field into a single geometric object). Similarly, we see a path from *Principia* via Euler's (1736) rewriting of Newtonian mechanics in terms of the calculus, to Lagrange's abstract analytical mechanics including his high-dimensional configuration spaces, to the infinite-dimensional complex Hilbert spaces of quantum mechanics.[4]

[1] Basic familiarity with the philosophy of mathematics at the level of textbooks like Shapiro (2000), Bostock (2009), Linnebo (2017), etc. would be helpful but is not strictly necessary for reading this paper.

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

[3] This is not necessarily Dijksterhuis's own view, since he *defines* force as mass times acceleration.

[4] A history of mathematical physics remains to be written; for some of these parts see e.g. Lützen (1995, 2010, 2011), Pulte (2005), Rédei and Stöltzner (2001), Janssen and Renn (2020), and Landsman (2021).

In all these cases, the relevant mathematics goes well beyond what might reasonably be expected to be instantiated by natural objects. This also has repercussions for the status of mathematical models, which obviously play a central role in mathematical physics. The precise meaning of mathematical models will be discussed in §4; for the moment one may think of space-times solving Einstein's equations as models of general relativity; the Standard Model of high-energy physics as a model of QFT; specific Hamiltonians and Hilbert spaces as models of non-relativistic QM; and 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 Alberta (van Fraassen, 2008). In such cases models only have a practical purpose for making predictions and giving insight.

But mathematical models of modern physics often do not have given and unambiguous targets: their would-be "targets" are (at least partly if not entirely!) model- or theory-dependent constructions and, reverting the situation in general science (or even daily life) just discussed, models in mathematical physics are typically much clearer than their alleged targets. 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 of high-energy physics (Galison, 1983, 1987; Pickering, 1984ab) and the black hole in M87 imaged by the Event Horizon Telescope (Muhr, 2023; Ochigame, Skulberg, and van Dongen, 2025). In both cases the idea of simply observing some natural phenomenon (the alleged "target"), which may then be described by some theory, has been blasted to such an extent that the logical positivists must turn in their graves.[5] And here is the reason:[6]

> 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 well beyond the cliché that (almost) all observations are "theory-laden". The point is rather that beyond 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 ambiguous and 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. As we shall see, in our view weak neutral currents are part of some model (like the Standard Model), whose purpose it is to make inferences about a data model. Similarly, in our view black holes do not "exist", they aren't "phenomena".

[5]Even more recently, in a famous paper in the philosophy of science Bogen and Woodward (1998) talk about weak neutral currents as 'examples of phenomena', as if these were *given*. In a similar context, Ngyuen (2016) speaks about 'the interactions between neutrinos and bosons' as *given* 'phenomena'.

[6]To some extent 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'.

Black holes are theoretical constructions within general relativity, which may be used to explain or understand specific data models extracted from astronomical observations.[7]

This peculiar lack of a clear connection between mathematical models of modern physics and targets is closely connected to the previous complication for the philosophy of mathematical physics we discussed, i.e., the lack of instantiation in the natural world of many mathematical concepts; if there had been clear referents for those, there might have been clear targets. But this lack also reflects a more general feature of modern science. Andersen (2023) notes that students of science typically learn their trade by doing textbook problems whose answers may often be found at the back of the book.

> This presupposition, the expectation of there being a single right answer we either got or failed to get, is ill-fitting in the context where science is genuinely expanding the boundaries of human knowledge. In these cases, what can be called the liminal cases of research, there just is no equivalent to the answer at the back of the book. There is no chance to check our answers against the "right" one to see we are solving problems correctly. And importantly, this is more fundamental than there simply being a lack of access to the answers, determinate though the answers are. It is not merely that such answers exist but we can't get to read them. They simply don't exist. There isn't an equivalent of the instructor's manual with all the right answers in it. There will never be a point where we get to peek behind the veil of the universe and see if we got it "right." There is no guarantee of a path to a guaranteed, unique solution via the recommended resources for the problems that face us. There is no designated set of tools to solve it that the chapter points us toward. And even as we progress in physics, these answers don't appear: in principle, they simply are not there. (Andersen, 2023, p. 72)

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,[8] viz.:

1. 'Mathematical concepts turn up in entirely unexpected connections.'[9]
2. '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.[10] Together, his points add up to the claim in his title: *The unreasonable effectiveness of mathematics in the natural sciences*. We return to this in §6.

[7]This does not make the author a strong social constructionist, see the end of the Discussion.

[8]The secondary literature on Wigner (1960) is quite large; we benefited from Maddy (2007), Bangu (2012), Brush (2015), Islami (2017), Islami and Wiltsche (2020), Ferreirós (2017), Bueno and French (2018), which includes convincing criticism of the earlier analysis by Steiner (1998), and Rédei (2020).

[9]Here the contrast is with *expected* connections, such as the use of geometry in land measurement; of arithmetic in trade, taxation, and accountancy; of calculus in mechanics; of probability theory in gambling and games of chance; and of partial differential equations in the theory of waves and vibrations. In all such applications the mathematics in question was developed for that purpose.

[10]One may also think of the appearance of conical sections in Newtonian gravity, which 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). Other examples are

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*, because the required balancing act between philosophy of mathematics and philosophy of science significantly reduces the embarrassment of riches one finds on both sides. 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 be discarded.

As a first step towards its philosophy, let us try to *define* mathematical physics. As in "the philosophy of mathematics" (scare quotes!), there is some circularity involved here, since (unlike for example astronomy, biology, or arguably even physics) neither mathematics nor mathematical physics are uncontroversially delineated concepts or activities, and hence defining what these mean is already part of their philosophy (perhaps this problem is shared with the philosophy of time). To cut this circle we agree with Netz that:

> What unites a scientific community need not be a set of beliefs. Shared beliefs are much less common than shared practices. This must be the case in general, because shared beliefs require shared practices, but not vice versa. (...) Whatever is an object of belief, whatever is verbalisable, will become visible to the practitioners. What you believe, you will sooner or later discuss; and what you discuss (...) you will sooner or later debate (Netz, 1999, p. 2)

We take this to mean that the philosophy of a specific scientific field is the (descriptive or normative) study of the *various* beliefs that are or should be held by the pertinent scientific community concerning their *shared* practices. This reduces the problem to defining the latter. Although our scope will be much wider, here are two 21st century benchmarks:

> *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 problems in physics, the development of mathematical methods suitable for such applications, and the formulation of physical theories.[11]
>
> 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.[12]

Adapting these ideas to a definition that makes sense through the ages, we propose that:
Mathematical physics is the use of rigorous and sophisticated mathematics (by contemporary standards) for the study (i.e. explanation and understanding) of inanimate nature.

Boltzmann's use of probability theory to explain the thermodynamic concept of entropy; the use of Riemannian geometry and tensor calculus in Einstein's general relativity; and the use of functional analysis and group theory in quantum theory (which incorporates complex Hilbert space and hence Wigner's example). The unexpected application may also lie in pure mathematics; just think of the crucial use of partial differential equations (and even entropy) in Perelman's proof of the Poincaré conjecture in topology.

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

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

Although mathematical physics is often said to have started with Newton, our definition includes the surviving contributions by Euclid, Archimedes, and Ptolemy to the 'mixed sciences' of Antiquity, i.e., astronomy, harmonics, optics, and mechanics.[13] Since these were already discussed by Aristotle, who was a philosopher of both mathematics and science (arguably even the first philosopher of science), our 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 §2 we present a survey of (implicit or explicit) philosophies of mathematical physics from Plato and Aristotle via Kant to Wittgenstein,[14] also 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 itself. As we shall see, this history, sufficiently coarse-grained, involved 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.

In §3 we combine what we learnt from this history with the philosophy of mathematics proposed by Landsman and Singh (2023), according to which mathematics is a 'rhododendron' of language games à la Wittgenstein whose rules are inferential, and whose "pure" side is inferentialist. Though *conventional* in the sense of a description of a game, except for degenerate cases that typically soon disappear the rules of mathematics are by no means *arbitrary*, since they were (and still are) inspired by both empirical science and the autonomous development of mathematics itself (here we largely follow Hilbert).

To avoid confusion between various kinds of conventionalism on the market and to do justice to the historical transition described in the previous paragraph we actually replace 'conventional' by 'hypothetical' whenever possible, if only to stress that we do not endorse concepts of "truth by convention". Inspired by various authors on the history and philosophy of mathematical physics as well as on philosophy itself,[15] we arrive at the view that *theories* of mathematical physics (such as general relativity and quantum mechanics) should be seen as meaning-constitutive *a priori* constructions, upon which their *models* mediate between theory and nature, i.e., between the *a priori* and the *a posteriori*.[16]

As discussed in §4, models play this role as mediators through their capacity as *objects of comparison* or *yardsticks* to be held against nature, where the comparison–not to be confused with a *description*–is done via *surrogative inference*.[17] At a more abstract level,

[13] See Roux (2010) for the claim that the mathematization of nature by no means started with Galilei.

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

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

[16] The specific role we assign to theories and models resonates with a 'models as mediators' approach (Morrison and Morgan, 1999), in which the mediation in question is between theory and observation, too. However, this approach assumes that models are rather more independent of theories than we would allow.

[17] See Suárez (2004, 2024), Bueno and French (2018), Warren (2022), and Povich (2024). According to what is called *predictive processing theory* (or *predictive coding*), something similar happens in the brain. This theory replaces traditional views, according to which the brain just *records* sense data, by the idea that he brain actively produces *predictions* about the world, which are compared with incoming sense data, and then updated if necessary. See e.g. Hohwy (2013) and Millidge, Seth, & Buckley (2022).

specific *models* may (for example) be determinstic, where the underlying theories *define* what it means to be deterministic. So we follow Kant in putting such notions on the *a priori* side, but we depart from Kant in that they are by no means taken to be necessary or intuitive:[18] properties like determinism are merely held against nature in order to figure out to what extent they align. But nature (let alone Nature) lacks such properties.[19]

But "comparison" with what? Following van Fraassen (2008), we take it that models are not held against nature (or natural phenomena) itself, but against some *data model*. We will discuss some examples of this scheme from actual mathematical physics (involving black holes and elementary particle physics), and then give a brief philosophical conclusion in which we explain how we managed to combine "best practices" from Aristotle onwards, empasizing our loss of realism on both the mathematical and the physical side. Finally, since (as textual evidence shows) Wigner's problem of 'the unreasonable effectiveness of mathematics in the natural sciences', was posed from a formalist philosophy of mathematics and an empiricist philosophy science, which is close to our own position, we cannot but return to this problem (without, alas, fully resolving it).

2 Historical survey

Returning to our opening quotation of Steiner, let us start this historical part with the 'apparently good reasons to believe why mathematics should not be applicable to Nature.' In so far as we still have a record, these reasons go back 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.[20] 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.[21] As summarised by Burnyeat (1987), p. 221, the Platonic argument, though 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.

[18]This departing has a long tradition in neo-Kantianism (Gardiner, 1981; Köhnke, 1991) and, beginning with Reichenbach (1920), was also picked up by the logical positvists, cf. Friedman (2001) and §3 below.

[19]Nature with upper case N is like Kant's *das Ding an sich*, which, following Kant, we cannot know; we use the term 'nature' with lower case n as *das Ding für mich*, which is the subject of empirical science.

[20]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*.

[21]In developing his views he was arguably also worried by the tumultuous political times in which he lived (Atack, 2024).

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)

This view resurfaced in the twentieth century,[22] but it hardly played a role during the period we are going to describe, except as a foil for both Aristotle and Wittgenstein. Moreover, it is unclear whether Plato himself actually meant the realm he just described to be the home of mathematical objects.[23] 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 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) that the universe itself has a mathematical structure, as in the *Timaeus*.[24] 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). 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.[25]

[22]Most notably: '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), and similarly: '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).

[23]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.

[24]See Gregory (2022) for a recent accounts. Gregory's warning that Plato did not use equations, but employed geometry and harmony, so that we should not too glibly read mathematics in the *Timaeus*, is odd since equations did not occur in ancient Greek mathematics at all, including Euclid. Plato's cosmos here is basically a geometric model of the heavenly bodies based on certain arrangements of circles, at least in this respect predating Ptolemy's infinitely more sophisticated model in the *Almagest*.

[25]In this light, it is remarkable that two of the top mathematical physicists of the second half of the twentieth century, Roger Penrose and Alain Connes, are outspoken platonists. See e.g. Penrose (2004) and Connes, Lichnerowicz, and Schützenberger, (2001). Their technical arguments for platonism, based on Gödel's incompleteness theorems are arguably dubious or circular (and were not used by Gödel himself), but in addition a strong emotional commitment is clearly visible, shared by many mathematicians.

2.1 Aristotle

We proceed with Aristotle, whose inclusion may seem surprising at first sight:

> 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, the relationship between Aristotle's natural philosophy and the mixed sciences is complicated, and taking this, as well as his general philosophy of mathematics, into account leaves room for a considerably more positive and lasting contribution to the philosophy of mathematical physics. Let us condense a famous passage from *Physics*:[26]

> We now consider in what way the mathematician differs from the physicist. For natural bodies have planes and solids and lengths and points, and the mathematician investigates these things (...) but he does not treat them as limits of a natural body; nor does he study their properties as the properties of such bodies. [Instead] he makes a separation: for they are separate from motion in thought, and it makes no difference, nor does anything false result when they make the separation. (...) Whereas geometry investigates a physical line but not as physical, optics investigates a mathematical line but not as mathematical; i.e., as physical. (*Physics* B2, 193–194)

He repeats this point in usefully different phrasing in his *Metaphysics* M, as follows:

> The best way of studying each thing would be this: to separate and posit what is not separate, as the arithmetician does and the geometer. A man is one and indivisible as a man, and the arithmetician posits him as one indivisible, then studies what is incidental to the man as indivisible; the geometer, on the other hand, studies him neither as a man nor as indivisible, but as a solid object. For clearly properties he would have had even if he had not been indivisible can belong to him irrespective of his being indivisible or a man. That is why the geometers speak correctly: they talk about existing things (...). (*Metaphysics* M3, 1078a).

In other words, 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,[27] to be explained via causes, definitions, and essences. In Aristotelian parlance, a mathematician looks at a

[26] This is an excerpt. See Lear (1982) and Mueller (2006) for full text and analysis.

[27] One even has a similar distinction within mathematics: a geometer separates (or abstracts) different mathematical properties than a number theorist, much as a physicist and a biologist separate different aspects if they look at some living organism.

bronze sphere *qua* (mathematical) sphere, whereas a physicist looks at it *qua bronze*.[28] 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',[29] in other words, mathematical objects are instantiated by physical objects and hence form part of the natural world, which obviates the need for a would-be "platonic" realm supposed to be home to mathematical objects. 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 triangle (having sharp rather than round angles, composed of such lines); which they should in order to sideline Plato.[30]

The Aristotlelian 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),[31] and a *physical* part concerning both the constitution of the heavenly bodies and the causes of their motion. Duhem (1908) famously proposed that it 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 criticized from the early 1960s onwards.[32] 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 did not match the (astro)physics.[33]

On the positive side, Aristotle's theory of demonstrative sciences in the *Posterior Analytics*,[34] which is an axiomatic-deductive call for arms, cast a long and beneficial shadow:

[28]More generally "*X qua Y*" is short for "*X* in the respect that *X* is *Y*" (Mendell, 2019).

[29]McMullin, 1985, p. 249, footnote 5.

[30]For example, Lear (1982) and Distelzweig (2013) disagree about this: the former argues, via selected quotations, that Aristotle does believe this, whereas the latter denies it on the basis of Aristotle's rejection of both the platonic *and* the pythagorean philosophy of mathematics. The quotations by Lear are challenged by Bostock (2012), who declares the matter undecidable. See also Mendell (2019) for a balanced view.

[31]As described e.g. by Netz (2022), Chapter 5, eclipses and other remarkable planetary configuration were seen as signs of the gods, with possible (political) impact on earth. Hence it was crucial for rulers to predict (and retrodict!) these divine messages. This started with Babylonian astronomy around the 7th century BCE and reached an all-time high point in Ptolemy's *Almagest*, written around 150 CE, at which time the Latin word *mathematicus*, with which Ptolemy would identify himself, simply meant "astrologer" (Netz, 2022, p. 355)! Indeed, he also wrote a work specifically on astrology, called *Tetrabiblos*. See below.

[32]See especially Mittelstraß (1962), Lloyd (1978), and Jardine (1979).

[33]Aristotle's *De Caelo* may be read in a realist spirit, cf. Judson (2015), but then it isn't a work in mathematical astronomy. The other famous astronomical text of Antiquity, the *Almagest* by Ptolemy, was certainly a mathematical work, but its underlying philosophy is also quite complex, see Feke (2020).

[34]Aristotle mainly applied this theory outside mathematics, in which Euclid would become the dominant influence. Both Euclid's method of axiomatization and his approach to logical deduction are different from Aristotle's. See Heath (1956) for the relationship between Euclid and Aristotle in so far as their axioms and definitions etc. are concerned. Unlike Aristotle, Euclid's rules of inference are not explicitly stated (except for his Common Notions, which are not nearly sufficient for this purpose) and have to be guessed from his proofs, but in any case they are clearly not based on Aristotle's syllogisms. This became increasingly clear in the middle ages and led to some sort of a crisis about the reliability of mathematics (the so-called *Quaestio de Certitudine Mathematicarum*), see Mancosu and Mugnai (2023). Avigad, Dean, and Mumma (2009) present a formal proof system for Euclid, but broadly speaking all his proofs can be (re)done using

> 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 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 encountered by Newton, in connection with whom we will return to this. But before getting to Newton, we merely jump about four centuries after Aristotle and discuss what is arguably the most famous and elaborate text in mathematical physics (as we construe it) before Newton's *Principia*, namely Ptolemy's *Almagest*.[35]

2.2 From Ptolemy to Galilei

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). 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). It therefore seems that the *Almagest* was the first book in history that constructed a serious mathematical model of some part of the natural world and compared the two (and even did so via a data model à la van Fraassen, see below). Duhem's claim of pure instrumentalism in ancient astronomy may now be put in a more modern perspective:[36]

> 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)
>
> And still, Duhem's intuition was not silly. (...) 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)

Jumping another 1400 years we arrive at Ptolemy's heir Copernicus, whose primarily computational style was hardly any different from Ptolemy's; they also shared not only a

first-order logic. See also §2.3 for Lakatos's (ahistorical) concept of a 'Euclidean system'.

[35]The standard translation of the *Almagest* is Toomer (1984). Netz (2022), pp. 378–379, gives a very helpful conceptual comparison between the *Almagest* and the *Elements*. We 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.

[36]See Feke (2020) for a book-length account of Ptolemy's mathematical philosophy.

heavy use of, but also an outspoken admiration for mathematics.[37] Unfortuately, except for his outspoken realism, as far as we know Copernicus did not explicitly analyze the relationship between pure and applied mathematics in philosophical terms. Perhaps his successor Kepler did entertain a "philosophy of mathematical physics", as follows:

> For Kepler it is because the Creator reproduced a part of His 'essence' both in the human mind and in the created world that the human mind is possessed of an 'illumination' through which knowledge of the hidden form of the cosmos is attainable. It is because God created both the human mind and the world as a geometer that the mind's illumination 'most especially thrives on geometrical figures', and hence can hope to resolve theoretical disputes through apprehension of the geometrical harmony of the dispositions and motions of the cosmos through which the Creator partially reveals His essence. (Jardine, 1979, p. 170)

The Platonic echo is again obvious: God is a geometer. An additional Judeo-Christian voice is heard in his insistence that this God reveals himself through his creation.

As already noted, Ptolemy, Copernicus, and Kepler were all mathematical physicists according to our definition. Nonetheless, their mathematics was only applied to astronomy, physics on earth still being understood via Aristotle's natural philosophy, which blocked the use of mathematics is favour of a search for causes and essences. This is what Galilei challenged and changed. Because of his introduction of mathematics into natural philosophy, mathematical physics is often said to have begun with Galilei.[38] But as we have seen, nothing was new in his claim that 'this grand book, the universe (...) is written in the language of mathematics': the theme that God reveals himself through a second book, the book of nature, was a familiar one at the time, and even the point that this revelation used the mathematical language of geometry was not new, having its roots in Plato, and also occurring in Kepler's philosophy. Galilei's originality as a philosopher of mathematical physics is better displayed in a less familiar passage from the *Dialogue*:

> SALV. Then whenever you apply a material sphere to a material plane in the concrete, you apply a sphere which is not perfect to a plane which is not perfect, and you say that these do not touch each other in one point. But I tell you that even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens the same way in the abstract. It would be novel indeed if computations and ratios made in abstract numbers should not thereafter correspond to concrete gold and silver coins and merchandise. Do you know what does happen, Simplicio? Just as the computer 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

[37] 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*!

[38] In comparison with our definition of mathematical physics given above, it should be noted that Galilei's mathematics was far from rigorous or sophisticated even by the standards of his time (Blåsjö, 2023), but his historical and philosophical role is so huge that we cannot skip him in this account.

> 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)

This passage (from the second day) is analyzed by both McMullin and Palmerino:[39]

> The *point* of idealization (but here is the great divide between the Platonic and Aristotelian traditions) is not simply to escape from the intractable irregularity of the real world into the intelligible order of Form, but to make *use* of this order in an attempt to grasp the real world from which the idealization takes its origin. (McMullin, 1985, p. 248)

> 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 coherent 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)

Thus Galilei (heard above through his spokesman Salvatio) was not a platonist but a pythagorean (in the sense we defined these terms in §2), in believing that the *physical* world has a mathematical structure. But he followed Aristotle rather than Plato in his interest being in this physical world–grasping it though through mathematical idealizations enforced by the limited capabilities of the human mind. Hence his distinction between what is real and what we are able to describe via idealizations was an epistemological one. In other passages, 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. Unfortunately, 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.'

[39] See also Finocchiaro (2010).

2.3 Newton

In the footsteps of Galilei, Kepler, Descartes, and Huygens,[40] mathematical physics soon reached its all-time high (or, some would say, was created) in Newton's *Principia* from 1687. Newton starts his book with a Preface to the reader,[41] which should be read as the enunciation of his philosophy of mathematical physics:

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

Thus Newton overcomes the traditional discrepancy between the perfection of mathematics and its alleged location either in some platonic realm or in the mind and the imperfection of its realization in natural objects (combined with the unreliability of observation) by a direct *identification* of geometric objects with natural ones. From parts of the Preface not quoted here, and other texts,[42] it is clear that Newton attributes any kind of imperfection to the 'practitioner'; as opposed to 'the most perfect mechanic of all' (i.e. God).[43]

Having said this, if anyone realized that (for example) 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 idealized nature of his exact descriptions of geometric figures generated by motion; hence many of the deepest results in *Principia* are attempts to overcome such idealizations. 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.

[40] We will not analyse the philosophy of mathematical physics of Descartes and Huygens here; as explained by Dijksterhuis (1961) the former was sidelined by Newton and hence did not connect to (even early) modern physics, whereas the latter held no outspoken philosophical views (as far as we know).

[41] 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). We 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.

[42] Notably *De Gravitatione*, see Domski (2017).

[43] The latter is clear from the General Scholium, viz. 'This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent being.'

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 essentially pythagorean, even close to Galilei's: at least in so far as they are related to motion, his mathematical objects are *in* nature. In other words, 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,[44] 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 generalized;[45] the difference is that for Newton the underlying "motion" was natural rather than man-made.

Moreover, 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. To explain this, we quote from Dear (1995), who himself starts by quoting the Jesuit Franciscus Aguilonius (1613) who elaborates on Aristotle:

> 'For a single [sensory] act does not greatly aid in the establishment of sciences and the settlement of common notions, since error can exist which lies hidden for a single act. But if [the act] is repeated time and again, it strengthens the judgment of truth until finally [that judgment] passes into common assent; whence afterwards [the resulting common notions] are put together, through reasoning, as the first principles of a science.'
>
> The conception that Aguilonius here presents can, perhaps, best be explicated by a contrast with the modern hypothetico-deductive view of scientific procedure. Some version of the latter, whether confirmationist or falsificationist, would place experience, at least as regards its formal justificatory role, at the end of a logical structure of deduction from an initial hypothesis: the hypothesis yields conclusions regarding observable behavior in the world, and experiment or observation steps in to confirm or falsify these predictions and hence, in a logically mediated way, to confirm or falsify the original hypothesis itself. A methodological Aristotelian, however, approached these issues in quite different fashion. Since the point of Aristotelian scientific demonstration was to derive conclusions deductively from premises that we already accepted as certain–as with those of Euclidean geometry–there was no question of testing the conclusions against experience. The proper role for experience was to ground the assertions contained in the original premises, as Aguilonius assumed. Once they had been established, so too, from an empirical standpoint, had the conclusions potentially deducible from them. (Dear, 1995, p. 44–45)

[44]See Sepkoski (2007), Chapter 4, and Guicciardini (2009), §8.1.
[45]See e.g. Bos (1993, 2001).

As analyzed in detail by Pulte (2001, 2005), this also applies to Newton.[46] Using terminology borrowed from Lakatos (1978), Pulte calls the *Principia* a “Euclidean system” in the sense of 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.’ (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) ‘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).[47]

Book I of the *Principia* indeed starts with a list of definitions, so as to emphasize 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 section 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 summarized above.[48] 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, which was also the key to the success of *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 greatly differ from the motions they generate. Such motions are geometric objects 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. Although in the *General Scholium* Newton leaves no doubt about the reality of gravity,[49] in Book I forces in general and centripetal forces in particular are introduced via *definitions*. This created a completely new situation, not only in (mathematical) physics but also in its philosophy:

[46]See also Smith (2002) and Ducheyne (2005), and references therein, for Newton’s methodology including his rejection of the hypothetico-deductive method.

[47]See Paseau and Wrigley (2024) for a recent survey of the ‘Euclidean Programme’. As they emphasize, this programme or system is what is what taken to mean in early modern science. Since Euclid’s philosophical views are unknown (and even the *Elements* as we now have it seems to be full of later interpolations, even on the first few pages that develop his general framework), philosophical attributions to him are bound to be historically questionable. On top of this, the precise mathematical and philosophical relationship between Aristotle and Euclid (and his sources) remains contentious (von Fritz, 1955; Mueller, 1991).

[48]This much is clear from his fourth rule for the study of natural philosophy, quoted above. It is here, in our view, that Wittgenstein’s idea that mathematical propositions are ‘empirical regularities hardened into a rule’ comes into its own, at least at the level of axioms. See Landsman and Singh (2023), §3.

[49]‘And to us it is enough, that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.’

> 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)

For our purposes, Newton's novel concept of force had two closely related consequences:

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 objects 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).

2. It undermined the Euclidean, or, *qua* philosophy of (demonstrative) science, even Aristotelian structure of *Principia*. Aristotle's premises and Euclid's axioms were supposed to be obviously and indubitably true. But Newtonian forces (especially the gravitational one) can hardly claimed to be "true" in this sense, neither as mathematical ingredients of nature (like motions) nor as more abstract objects.[50]

The second point was to some extent acknowledged by Newton himself, since his fourth rule for the study of natural philosophy quoted above admits that the 'propositions collected by general induction from phaenomena' (i.e., his premises) may only be 'very nearly true', or even 'liable to exceptions'. This admission creates a tension within the *Principia* that Newton himself never overcame: taking his own remark seriously he should have admitted that his premises were hypotheses, but this possibility was blocked by his general ideology described earlier, culminating in his famous words *'hypotheses non fingo'* in the *General Scholium*, through which he expressed simultaneous his animosity towards Descartes and his allegiance to the Ancients. It took about 200 years to overcome this tension. 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.

2.4 The eighteenth century

The mathematical physics of the eighteenth century largely consisted of rewritings and generalizations of Newton's *Principia*. First, its continental European reformulation in

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

the style of Leibniz's calculus by Euler (1736) replaced Newton's choice to cast the mathematics of his mechanics into 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 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.[51] These fields were actually far less abstract and conceptually innovative than Newton's *Principia*, and being easily visualizable and (by construction) applicable, apparently 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 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 a way Pulte (2001) summarized as follows:

> From this shift results a growing independence of mathematical physics from the philosophical foundations of its principles, be these foundations 'empirical' or 'rational': It is the deductive power of principles rather than their empirical contents, their axiomatic status rather than their status as 'laws of nature', their formal truth rather than their material truth, which become important. To borrow again from Lakatos'

[51] See Darrigol (2005), Truesdell (2012), and Gray (2021).

> picture: If the deductive channels are filled with truth, and the truth flow down to the phenomena can be guaranteed, the source of truth becomes less important. (...)
>
> 'Deductivity' wins over conceptual foundation – or at least makes conceptual foundation a problem which no longer requires discussion. (...)
>
> the whole system is held together by logical coherence rather than by material truth. This is what happened in eighteenth-century rational mechanics, this is what later happened in geometry. (Pulte, 2001, p. 75, 78, 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. We here restrict ourselves to his most relevant work in this direction, namely *Metaphysischen Anfangsgründe der Naturwissenschaft* from 1786,[52] translated as *Metaphysical Foundations of Natural Science* (Friedman, 2004).[53] As noted by Michael 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)

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:[54]

> 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

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

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

[54] 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.

> 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 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)

We follow Pulte (2005) in his interpretation of Kant's edifice as an effort to salvage Newton's "Euclidean system" underwriting *Principia* (see §2.3) 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 (as well as his predecessors, cf. §2.2), 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 human intuition as a synthetic *a priori* was revolutionary.

2.5 The nineteenth century

From our point of view of the philosophy of mathematical physics under construction, 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 preceding section, 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 self-evidence in the work of C. Neumann, Jacobi, Riemann, and Hamilton completed the process.[55]

- Besides mechanics (which in this respect co-evolved with mathematics, see also below), physics as a whole underwent a dramatic innovation in the nineteenth century that introduced degrees of abstraction in physics itself (rather than in mathematics), which would have looked quite foreign to the previous centuries and arguably caused a further breakdown of the "Euclidean system". First, in the context of thermodynamics the concept of *energy* was introduced,[56] initially in the forms of heat and work, but gradually more generally and pervasively, as it has remained ever since. Though in mechanics energy is closely related to Newtonian force, we already diagnosed the latter as the first departure from Newton's own 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 formalized 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) totally lacked self-evident axioms (even Maxwell's *equations* were hardly obvious).

- 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 within a corpuscular basis of physics of the kind that increasingly found favour in the nineteenth century, such mathematical descriptions were merely approximate.[57] 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". Despite being cut off from physical "truth" in this way, the mathematics of partial differential equations still made sense, and was found to be extremely interesting and worthy of study by itself. This decoupling of mathematics from physical "truth" and even from physics altogether is one of the routes via which mathematics became "pure" (Maddy, 2008).

- Mathematics as a whole underwent a transition from a "Euclidean" to a hypothetico-deductive view—the one in mechanics just described may be seen as part of this. Mathematics gradually decoupled from physics, intuition, and self-evident truth to became an independent discipline characterized by abstraction, formal validity, and

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

[56] Or perhaps: re-introduced, since there were some precursors. But its absolutely central role in physics dates back to the nineteenth century and originated in thermodynamics. See Harman (1982), Smith (1998), and Coppersmith (2015).

[57] See Lützen (2010, 2011), Darrigol (2005), Gray (2021), and references therein.

progress that largely came from reflections on mathematics itself.[58]

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.[59]

- The discovery of non-Euclidean geometries challenged Kant's edifice of the *a priori*, at least in so far as Euclidean geometry was supposed to be part of it.[60] Here Helmholtz was a pivotal figure, who proposed some sort of a compromise in the form of an empiricist philosophy of geometry backed up by an analysis of measurements and rigid bodies that still left room for an *a priori class* of geometries in the form of three-dimensional Riemannian manifolds of constant curvature.

2.6 The twentieth century

All this led to what is sometimes called the "modernist transformation" of mathematics centered around 1900 (Mehrtens, 1990; Gray, 2008), after which mathematics had become autonomous: in our previous parlance, its former "Euclidean" style had been replaced by a hypothetical-deductive style, just as in (mathematical) physics. 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). Perhaps even more remarkably, Hilbert, who championed the modernist transformation, played a pivotal role in both these applications. See also §6 in connection with Wigner (1960).

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.[62] 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

[58]The Humboldtian 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 Schelsky (1963) and Jahnke (1990).

[59]See e.g. Jahnke (2003), Gray (2004, 2007, 2008), Gabbay and Woods (2004), etc.

[60]See Coffa (1991), Friedman (2001), Pulte (2005), DiSalle (2006), Gray (2008), Torretti (2012, 2019), and Stump (2015), as well as Gardiner (1981) and Köhnke (1991) for the wider neo-Kantian context.

[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.

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

program led to Russell's paradox and other signs of a foundational crisis like the need for Zermelo's 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:

> 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)

For example, following his initial goal of finding a logical basis for mathematics that was absolutely certain, Russell eventually came to believe that logic:

> 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)

In this context, it cannot be overemphasised what the outcome of this development was: what replaced sets (or classes) as real objects was a set of rules for working with them.

> To replace this [naive] notion [of a set] the axiomatic method is employed; that is, one formulates a number of postulates in which, to be sure, the word "set" occurs but without any meaning. Here (in the spirit of the axiomatic method) one understands by "set" nothing but an object of which one knows no more and wants to know no more than what follows about it from the postulates. The postulates are to be formulated in such a way that all the desired theorems of Cantor's set theory follow from them, but not the antinomies. (von Neumann, 1925/1967, p. 395).

The background to this view is the idea of *implicit definition*, according to which such objects are defined by the axiom systems in which they occur, as opposed to the traditional order according to which axioms are about objects whose meaning was already clear, as maintained from Euclid to Frege. Hilbert's take lies at the conceptual basis of modern mathematics.[63] The best introduction remains the Hilbert–Frege correspondence, especially Hilbert's letters from 29 December, 1899 and 22 September 1900.[64]

Enriques (1906, 1907, 1922) and Schlick (1918/2009) extended the idea of implicit definition from mathematics to physics, though their actual development of this extension is vague and unsatisfactory.[65] Yet Schlick makes an interesting point in noting that:

> the only way of generating exact concepts is to completely disconnect them from reality, [which] happened via implicit definitions, which defines concepts through concepts, as opposed to empirical properties. (Schlick, 2009, p. 717)

[63] See e.g. Torretti (2012), Giovannini and Schiemer (2021), Biagioli (2024), and Sereni (2024).

[64] The correspondence is reprinted with commentary in Gabriel *et al.* (1980). See aso Shapiro (2005), Hallett (2010), Blanchette (2018), and Rohr (2023).

[65] See also Coffa (1991), chapter 9, for the muddle Schlick got into by his efforts to combine some form of neo-Kantianism with psychologism, Hilbert-style implicit definition, and Einstein's theory of relativity.

This complete disconnection of exact concepts from reality predated Wittgenstein's famous description of what mathematics (in his view) does:[66]

> It is as if we had hardened the empirical [regularities] 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.[67] (*Remarks on the Foundations of Mathematics*, §VI.22b)

Wittgenstein's reference to 'empirical proposition[s]' accords with his view that mathematics is necessarily applied.[68] This describes just a small fragment of modern mathematics and even of modern mathematical physics, but this is easily remedied by extending the idea of the 'empirical' so as to incorporate informal theories of mathematics (such as Euclidean geometry seen from Hilbert's perspective) as well as practices involving some kind of mathematics, as for example in economics. In any case, the general picture that mathematics emerges from a process of 'hardening into rules' is a powerful summary of Hilbert's program of axiomatization,[69] which has remained both descriptive and normative of all serious mathematical physics and mathematics since he started it around 1900.

The second part of Wittgenstein's quote moves us into the territory of the *a priori*.[70] 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 could no longer be counted on, 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 definitions just mentioned, led not only Schlick but in his wake also Reichenbach, Carnap, Cassirer, Ayer, (C.I.) Lewis, Pap,[71] and Wittgenstein (who was not motivated by physics, though), to various attempts to rescue at least one aspect of the *a priori*, namely its constitutive role in shaping knowledge and meaning.

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. In particular,[72] 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,

[66] Pap (1946) contained a similar idea to the effect that 'fundamental principles of sciences' are 'hardened into definitions'. See Stump (2015), chapter 5.

[67] Here we follow Steiner (2009) in replacing Wittgenstein's 'empirical propositions' by 'empirical regularities', which is what he meant and which avoids confusion with mathematical propositions. See also Pérez-Escobar (2023).

[68] See also *Remarks on the Foundations of Mathematics*, §V.2: 'I want to say: it is essential to mathematics that its signs are also employed in *mufti* [German: im *Zivil*]. It is the use outside mathematics, and so the meaning of the signs, that makes the sign-game into mathematics.'

[69] Hilbert's Sixth Problem from 1900 also calls for exactly that, although he talked about axioms rather than definitions: 'Mathematical Treatment of the Axioms of Physics. The investigations on the foundations of geometry suggest the problem: *To treat in the same manner, by means of axioms, those physical sciences in which already today mathematics plays an important part; in the first rank are the theory of probabilities and mechanics.*' (Hilbert, 1902).

[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.

[71] See Stump (2015) for all of these, and Coffa (1991) for the first four, as well as Wittgenstein.

[72] Kant's narrow syllogistic Aristotelian logic on the analytic side of the *a priori* did not help either!

respectively. This pushed back at least some mathematics and physics to the empirical side. In Reichenbach's wake, Friedman (2001, 2002) revived and extended the idea of *relativizing* the Kantian *a priori* to a particular historical stage in the development of science. For example, Einstein's theory of general relativity does not specify the geometry of space-time (which would have been flat Minkowski geometry in the absence of gravity), allowing any solution to his field equations for the metric. But it does rely on a general theory of manifolds with Lorentzian metrics, which theory, then, would be *a priori*.

The problem remains how this (relativized) *a priori* relates to the *a posteriori*. To understand this, we return to Wittgenstein's *Philosophical Investigations* (PI).[73]

In the argument building up to §108 of the PI, which starts at §97, Wittgenstein proposes a specific relationship between language games and natural language, which in the next section we will mirror via what we see as a similar relationship between theories and models of mathematical physics in the role of Wittgenstein's logic or language games, and natural phenomena in the role of natural language. First, Wittgenstein 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). Conversely, logic should not be bent so as to accord with natural language (in which case logic would become empirical), since its rigorous and clear rules are like glasses on our nose; we cannot even *contemplate* taking them off. Wittgenstein's solution lies in 'turning the enquiry around' via a *relocation* of the 'crystalline purity' of logic:

> §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 (...)
>
> §108. 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. (One might say: the inquiry must be turned around, but on the pivot of our real need.) (...)

Hence, analogously to Kant's relocation of logic and mathematics from the phenomenal world to the *a priori*, Wittgenstein relocates rigour and crystalline purity from language to language games, which as already mentioned play the role of logic (vastly generalized).

Moreover, unknowingly echoing Galilei as quoted and discussed in §2.2, in which comparison Wittgenstein implicitly replaces physics and mathematics by language and language games, respectively, he adds an important clarification to the effect that language

[73] Secondary sources for what follows are Railton (2000) and Kuusela (2019), §4.4. As Kuusela explains, language games are late Wittgenstein's version of logic and is sometimes also denoted as such.

games (or at least the 'clear and simple ones', words reflecting his earlier 'crystalline purity') are not to be seen as parts of language but as tools of *examination* thereof–they provide benchmarks or yardsticks with which some linguistic practice can be *compared*:

> §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.)

These paragraphs should be compared with a much more famous one in the PI, viz.

> §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.

Elsewhere Wittgenstein explains that such use is governed by rules, for example:

> §108 (. . .) . But we talk about [language] 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?"
>
> §197 (. . .) Where is the connection effected between the sense of the words "Let's play a game of chess" and all the rules of the game? – Well, in the list of rules of the game, in the teaching of it, in the everyday practice of playing.
>
> §567. But, after all, the game is supposed to be determined by the rules! (. . .)

Hence it would be natural to conclude that language games are meaning-constitutive. But although this conclusion is drawn more clearly in his middle period.[74], we endorse the clear suggestion that through the rules governing them, at least some language games define the meaning of the words and sentences appearing therein. Following these rules then comes down to using words and sentences correctly, that is, as intended by the community that made up the rules (which could be in any form, e.g., by example, as in some spoken natural language, or as written down, as in schoolbooks about that language, etc.).[75] Even

[74] See for example Coffa (1991), Chapter 14, and Kienzler (1997).

[75] Thus Wittgenstein says that 'following a rule is a practice', cf. §202.

in that case language games may be seen as yardsticks, but this time they are normative ones, as opposed to exploratory (or empirical) ones. A splendid example, due to Wittgenstein in the early 1930s, covers both:[76]

> 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.

Characterizing and distinguishing "grammatical" and "empirical" rules in general is a challenge that also Carnap and the like faced;[77] in the context of mathematical physics vis-a-vis natural phenomena we will address this problem in the next section.

To close this survey of the twentieth century, we mention the *inferentialism* of Brandom (1994, 2001). Among various other influences it absorbs, inferentialism is also a sharpening of Wittgenstein's late philosophy of language from the PI, from which it inherits its non-representational character where meaning (via reference) does not determine use, but rule-governed use determines meaning. 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.[78] The shortest characterization of inferentialism is arguably that Brandom extends the attribution of meaning through use in making inferences from the logical to the non-logical vocabulary,[79] extending the idea of introduction and elimination rules to the effect that some sentence is either the premise or the conclusion of some inference:

> The idea is to understand propositional contents as what can both serve as and stand in need of reasons, where the notion of a reason is understood in terms of inference. So propositional contentfulness is taken to be a matter of being able to play the role both of premise and of conclusion in inferences. Once the notion of introduction and elimination rules as exhaustively constitutive of the content of logical concepts has been generalized to take in the circumstances and consequences of application of nonlogical concepts, the step to inferentialism is taken when one understands their

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

[77] 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*.

[78] For example, the introduction rule for the implication sign $\rightarrow$ states that $A \rightarrow B$ follows if from A *as a hypothesis* one can infer B, whereas its elimination rule is the *modus ponens* (i.e., A and $A \rightarrow B$ imply B. Since inferring B from A involves many other rules, this also shows that rules of inference form a network that can only be used as a whole. See also Peregrin (2014, 2020), Warren (2020), Povich (2024), and Incurvati and Schlöder (2023).

[79] 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).

content as exhaustively constituted by the material, nonlogical inferential connection between those circumstances and consequences. The content of a concept such as temperature is, on this view, captured by the constellation of inferential commitments one undertakes in applying it: commitment, namely, to the propriety of all the inferences from any of its circumstances of appropriate application to any of its appropriate consequences of application. (Brandom, 2010, p. 161)

> Another approach, which we may identify with Dewey and the later Wittgenstein,[80] 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)

We are quite serious about the relevance of his last point to mathematical physics (and also to mathematics and physics). For it explains three aspects of scientific practice which–in the different context of responsible agency–McGeer (2018), 303–304, denoted by:

(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,* which is the indubitable phenomenon 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'.

Wittgenstein's famous analysis of rule following in the *Philosophical Investigations*, which forms one of the major sources of inspiration behind our paper (though not cited by McGeer) could hardly have been paraphrased more clearly.[81]

All of this can be seen throughout the history of mathematics and mathematical physics. 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, for example–but as recalled by von Neumann (1947), it 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 summarizes the situation as follows:

> I have told the story of this controversy in such, detail, because I think that it constitutes the best caution against taking the immovable rigour of mathematics too much

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

[81] See especially §§185–242 of the PI. Baker and Hacker (2009) remains the authoritative exegesis. For a briefer discussion see also Mühlhölzer, (2010), §I.5. McGeer's norm-guided property recalibration also resonates with Wittgenstein's last work, *On Certainty*, notably §97: '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.'

> for granted. This happened in our own lifetime, and 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, 1961, p. 6)

Even within accepted standards, mathematical books and articles are full of mistakes, made even by the greatest mathematicians of all times like Euclid and Hilbert. But it is exactly as McGeer has it: *individual fallibility can be identified right because of collective infallibility*, which on the other hand is a dynamical notion, being subject to recalibration. Even the very concept of physics (and hence mathematical physics) changed dramatically from Aristotle to Newton (after which it seemed relatively stable). *Et cetera.*

3 Conventionalism and the *a priori* in mathematical physics

We now try to use the previous insights from history to formulate a philosophy of mathematical physics. As far as the *a priori* is concerned, we use what Warren (2022) calls a 'meaning-based theory of the *a priori*', which in its post-Kantian construal is intimately connected to conventionalism.[82] 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), according to which the axioms (or, in ordinary language, certain sentences containing the *definienda*) constrain (or determine) the meaning of the *definienda* by the 'free stipulation of the truth' of these sentences,[83] we endorse the version that does not attribute any kind of truth to the conventions that form the *a priori*. In this, we follow Poincaré as paraphrased by Ben-Menahem:[84]

> On this construal, implicit definition is indeed a matter of stipulation, but does not purport to generate truth. (...) Rather than conceiving of the axioms as freely postulated truths, we should think of them as hypothetical conditions, somewhat analogous to a set of equations that determines the values of a set of variables. If (and only if) we convince ourselves that these hypothetical conditions are satisfied by a particular set of entities, we consider the axioms to be true (of these entities). The axioms are

[82] See e.g. Ben-Menahem (2006), Stump (2015), Warren (2022), and Povich (2024).

[83] This is shared by the neo-logicists, see e.g. Hale and Wright (2000), also cf. footnote 108.

[84] And earlier in her book: 'The literature is replete with ambiguity as to what the meaning of 'convention' is, misunderstandings about the aims of conventionalism, and conflation of conventionalism with other philosophical positions, such as instrumentalism and relativism. The most serious confusion pertains to the notion of *truth by convention* typically associated with conventionalism. A central theme of this book is that conventionalism does not purport to base truth on convention, but rather, seeks to forestall the conflation of truth and convention.' (Ben-Menahem, 2006, p. 1).

> 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)

Our use of theories of mathematical physics as meaning-constitutive devices, partly through implicit definitions (see §3, continued below) follows this. Meaning is then defined by use as in late Wittgenstein, which use in turn is governed by rules of inference à la Brandom. Though driven by a different agenda, Jacobs (2024) puts this very nicely:

> Mathematical models acquire content by the way they are used in practice.

The rules behind the theories of mathematical physics (which, as we shall see below, also fix their models), then, are *conventional*, like choosing a particular language (game). And this is the case for all mathematical theories.[85] But at least in mathematical physics (and even in most or all of healthy mathematics) these conventions are by no means *arbitrary*:[86] since (and largely because of) Hilbert, theories of mathematical physics, here seen as language games with inferential rules, arise via the axiomatization of sufficiently mature informal theories of either (theoretical) physics (or some other applied field) or mathematics itself, which domains overlap for example in theories of space and quantity.[87] To avoid confusion between the various kinds of conventionalism on the market (and especially their different concepts of truth, of which as just said we reject "truth by convention"), and more importantly to relate our terminology to the decisive transition from the 'Euclidean' to the 'hypothetico-deductive' model reviewed in the previous chapter, we henceforth replace 'conventional' by 'hypothetical' whenever possible.

Thus 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 still relates via a complicated feedback process described in the next section, in which models play a crucial role. Also recalling Wittgenstein's insights in the *Philosophical Investigations* as quoted and used in the previous chapter, we reach the following conclusions:

1. Mathematics (though man-made!) should not compromise. Its exactness and rigour were crucial for the succes of Newton 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 was in fact only approximate;

    (b) all attempts to give exact mathematical descriptions of nature are eventually replaced by others, often using very different principles and theories.

[85] This wasn't Poincaré's view, or even Hilbert's. We argue for it in Landsman and Singh (2023), and up to this point also Warren (2022) is on board.

[86] Here we once again follow Poincaré and Ben-Menahem (2006), e.g., 'In the more circumscribed context of the philosophy of science, Poincaré too had warned his readers against the view that conventions are arbitrary. Well aware of the complexity of the fact–convention distinction, he had stressed that convenience may itself be responsive to fact. Preference for a particular convention over putative alternatives may therefore be more reasonable in one factual situation than in others.' (p. 33). This differs from Warren (2020, 2022), who accepts the arbitrariness of conventions in defending what he calls 'unrestricted inferentialism' and 'radical pluralism'. But then he is not constrained by physics. See also Landsman and Singh (2023).

[87] As we saw in §2.6, Wittgenstein captured this procedure in a memorable way via his credo that mathematics 'hardened the empirical proposition into a rule'.

3. This makes it hard to believe that nature (as opposed to mathematics) is governed by (exact) rules. 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 meaning-constitutive for their associated models, which in turn are the Wittgensteinian yardsticks to be held against nature. Hence 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) the apparent enigma that mathematics should not be applicable to nature, although in fact it is. This problem may be summarized by the original view that created it: Plato found fault with the natural world against the perfection of mathematics and hence believed 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 noted by Plato by inventing a hypothetical ineffable realm, since the mismatch is just what one would expect if mathematics does not describe but merely measures.

The question remains what kind of *a priori* we are now talking about.[88] It is clearly not the Kantian one, grounded as this was in both intuition and necessity–a hypothetical *a priori* is neither. Yet it is both *independent of experience* and *meaning-constitutive*. The first property is subtle, at least in our context of mathematical physics, since as we explained the *a priori* is often actually grounded in experience. But the (Wittgensteinian) point was that such experience has been 'hardened into rules', much as Schlick had earlier concluded that 'the only way of generating exact concepts is to completely disconnect them from reality': a line has to be crossed. The second property is trivial assuming that meaning is identified with use, which in turn is determined by (inferential) rules; which in turn form the grammar of language games deemed to *define* the *a priori*.

Our discussion of implicit definitions in §2.6 may be continued here: just as mathematical theories in general implicitly define the mathematical concepts appearing therein (such as space-time) through their axioms, we follow Enriques and Schlick in claiming that theories of mathematical physics (such as general relativity) implicitly define the physical concepts appearing therein (such as space-time) through their stipulations (assuming the mathematical concepts, such as a manifold and a metric, are understood via the axioms of the underlying mathematical framework). Returning to the Frege–Hilbert opposition, 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 (subsequently) appear, whereas Hilbert (whom we obviously side with) defines these concepts *by* general relativity.[89] In this regard, we should distinguish between theories and rules that:

[88]The distinction between the analytic and the synthetic *a priori* seems fruitless; it strongly depends on one's concept of logic and is only well defined in case it is trivial. See also Coffa (1991) and Stump (2015).

[89]This seems similar to what is called *spacetime functionalism*, especially in what Knox and Wallace

- *Determine* the meaning of basic logical and set-theoretical concepts (assuming we use standard twentieth-century mathematics in our theories of mathematical physics–this may change in the future!). As in the von Neumann (1925) quote, this is partly achieved through implicit definition, whilst most of the *notation* of set theory is introduced via explicit (i.e., stipulative) definitions. Moreover, it was understood in the 20th century that the rules of natural deduction determine the meaning of logical symbols (Gentzen); this was the basis of inferentialism.

- *Define* a field of mathematics (such as Euclidean geometry or group theory) or mathematical physics (such as general relativity or quantum mechanics), using the concepts in the previous points and, especially in mathematical physics, typically also other fields of mathematics introduced previously in a similar way. Let us henceforth call such fields *theories*. Such theories also 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. Theories may also include what are often regarded as *laws of nature*, such as Einstein's equations or the Schrödinger equation, or consequences thereof (like laws of stellar evolution).

- *Construct* solutions, examples, or models of such theories. Here the meaning of everything should have been determined already. It is these *models*, rather than the underlying *theories*, that are the objects of comparison or "yardsticks" to be held against nature. Thus models are not empirical, but they are what makes some theory empirically relevant. At the same time, it is quite clear from an example like general relativity, where whole books have been written about exact solutions to Einstein's equations, that such models (etc.) are normally studied without ever looking through a telescope. Nonetheless, we see models as mediators between theory and nature, in which capacity they float between the *a priori* and the *a posteriori*.

4 Models

We now focus on the latter, i.e., on models. Historically, the rise of models in, and since, the (late) nineteenth century has been well documented, both in general,[90] and in the case of *mathematical* models to which we naturally restrict ourselves.[91] Compared to the general case, we therefore do not need to discuss mechanical models, scale models, maps, paintings, etc. Neither will we discuss topics like the 'syntactic view' (also called the 'received view'), or the 'semantic view'. Among the general objections to these views,[92] we 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

(2023) call its 'constitutive' form. The origins of this idea in the logical positivist tradition (as opposed to the philosophy of mind usually emphasized) are worth exploring; see also Butterfield and Gomes (2023) for some first steps in this direction. See also Landsman (2025) for further details and other examples.

[90]Cf. van Fraassen (2008), Morrison (2015a), de Regt (2017), Elgin (2017), Frigg (2022), Suárez (2024).

[91]Cf. Blanchette (2017), Ferreirós (2022), Friedman and Krauthausen (2022).

[92]See for example Morrison (2007, 2015ab), Halvorson (2012, 2015), Frigg (2022), Wallace (2022, 2024), and Suárez (2024).

rarely discussed as a logical theory, except by philosophers; in mathematical physics this theory is stated in terms of standard differential geometry, partial differential equations, and topology. Perhaps the semantic view comes closer, in focusing on space-times 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: it is the Einstein equations themselves, rather than their solutions, that 'define' general relativity. Likewise for quantum mechanics and quantum field theory, seen as mathematical frameworks within which specific physical systems can be formulated as models.[93]

As explained in the previous section, we need both theories and models: the former are constitutive of meaning, whereas–given such meaning–the latter are the yardsticks to be held against reality (see below for how this is done in practice). In a historical context, the last aspect couldn't have been expressed better than by the following quote:

> [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)

The 'something else' here is a model; the 'Scottish' reference here is most famously to Thomson (Kelvin) and Maxwell, who used *mechanical* models for all kinds of things including the electromagnetic field; but the point applies just as well to mathematical models, which in our approach will precisely play the role of objects of comparison against which nature (or specific natural phenomena) may be held. As explained in most of the historical literature on models,[94] there is a close link between this Scottish commonsense tradition and the German *Bildtheorie* of Helmholtz, Hertz, and Boltzmann, from which, in turn, there is a direct route to Wittgenstein.[95] Of the *Bildtheorie*, we follow Suárez (2024), p. 38, in highlighting its inferential aspects, as described by the following quote:

> We form for ourselves images or symbols of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured. (Hertz, 1899, p. 1)

This idea returns in what for us is a more appropriate mathematical form in the *Tractatus*:

> In life it is never a mathematical proposition which we need, but we use mathematical propositions only in order to infer from propositions which do not belong to mathematics to others which equally do not belong to mathematics. (§6.211)

[93]The categorical formulation of physical theories and models advocated by Halvorson (2015, 2019) and Weatherall (2019ab) is under development in mathematical physics (perhaps with a different emphasis), see e.g. Brunetti, Fredenhagen, and Verch (2003), Baez and Lauda (2011), and Heunen and Vicary (2019).

[94]See especially Suárez (2024).

[95]See for example Preston (2016), Staley (2024), and Tomashpolskaia (2024). This literature is mostly concerned with the influence of the *Bildtheorie* on the *Tractatus*, but as we all know the *Philosophical Investigations* from which we quoted in the previous section was in many ways Wittgenstein's reconceptualization of the themes in his earlier book. In particular, his 'turning our whole enquiry around' in §108 of the PI is one his decisive breaks with the *Tractatus*, in which the 'crystalline purity' is sought in a logically perfect language itself in its role of a perfect image of the world, e.g. TLP, Prop. 2.12; 'The image is a model of reality' and Prop. 6.12: 'Logic is a mirror image of the world.' See Kuusela (2019), chapter 4.

Combining these, we replace Hertz's 'images in thought' with mathematical models, which are 'images' of external objects *only* in the sense that, paraphrasing Hertz, 'the necessary consequents of the model are always the images of the necessary consequents in nature of the things pictured.' Our 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 solutions), or of exact solutions to approximations to these equations, or even of approximate solutions to approximate equations adapted from the theory. In general,

a model uses the conceptual framework of the theory it is supposed to be a model of.

This means that its concepts are *defined* by the theory in question. Models take definitions and meaning as given and hence their study belongs to what Kuhn (originally) called 'normal science'. We then follow Friedman (2002) and Stump (2015), and with hindsight even Reichenbach (1920), in observing (or claiming) that paradigm shifts–or, if these are 'incommensurable', even scientific revolutions–correspond to changes in the *a priori*–which is only possible if the latter is relativized. A textbook example is the shift from Newtonian space-time to relativistic space-time: since in their capacity of *theories* of mathematical physics (as opposed to models thereof) we put Newtonian mechanics and gravity as well as general (or even special) relativity on the *a priori* side,[96] the transition from the former to the latter certainly marks a pretty revolutionary paradigm shift.[97]

Our approach may be clarified by using the five 'distinguishing characteristics of a theoretical model' listed by Redhead (1980), p. 146, as a benchmark:

(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.

[96]Friedman only puts the mathematical framework for general relativity on the *a priori* side, whereas we also include the Einstein equations there–but not the specification of particular energy-momentum tensors and corresponding solutions, which are on the model side. See the next section. Our approach arguably gives a better fit with Kuhn's identification of normal science with adopting some *a priori* framework.

[97]From the enormous literature on this topic we highlight Saunders (1993), Galison (1997), §9.3, and Wray (2018), chapter 7, all of whom emphasize elements of continuity in radical theory change. For Saunders this occurs particularly at the level of certain mathematical features that despite their allegedly radical reinterpretation are merely "deformed" in an appropriate sense; here one may think of Lie groups in passing e.g. from the Galilei group to the Poincaré group, or of equations of motion in passing from classical to quantum mechanics; indeed, in the title of his revolutionary 1925 paper Heisenberg famously mentioned that all he was doing was give a 'quantum-theoretical reinterpretation of kinematical and mechanical relationships'! Wray similarly notes that equations (even those called 'laws of nature') often survive–approximately and with due stipulations of their domain of validity–theory changes. Galison highlights continuities in experimental practices (which are of course easily missed when one focuses on theory).

Of these, only (3) and (4) are essential to us, whereas the others may or may not apply. However, we look at (3) in a very different way: Redhead (following Achinstein) says that (3) reveals the difference between theories and theoretical models, in that 'we believe our theories to be true', whereas 'a theoretical models is definitely believed to be false.' In our view theories *have no truth value*, whereas models, being merely objects of comparison, cannot be true or false. At the same time, models are all there is concerning our ability to compare some theory with the natural phenomena that inspired it. The history of physics convincingly shows that even all the great theories of mathematical physics, from *Principia* to general relativity and quantum (field) theory, are in fact merely mathematical constructions whose models are, in the words of Catherine Elgin, '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. (...) 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). They are (typically) representations of phenomena; that is, they typically have targets. But they purposefully misrepresent those targets. Although some, being non-propositional, are not strictly false, I label them falsehoods because they misrepresent. I consider them felicitous because their inaccuracies are epistemically fruitful; they are not defects. The falsehoods are inaccurate in ways that enable them to non-accidentally provide epistemic access to obscure or occluded aspects of their targets. Not despite, but because of their inaccuracy, they afford the access that they do. (Elgin, 2022, pp. 7–12)

How, then, is the comparison between models and nature actually done? How does some mathematical theory relate to (*a priori* non-mathematical) natural phenomena?

Our (*grosso modo* inferentialist) philosophy of mathematics (Landsman and Singh, 2023) matches a combination of *surrogative inferentialism* and *constructive empiricism*:[98]

> The two poles of scientific understanding, for the empiricist, are the observable phenomena on the one hand and the theoretical models on the other. The former are the target of scientific representation and the latter its vehicle. (...) How can we answer the question of how a theory or model relates to the phenomena by pointing to a relation between theoretical and data models, both of them abstract entities? The

[98] For inferentialism in the present context see Suárez (2004, 2024), Bueno and French (2018), Warren (2020), and Povich (2024). Constructive empiricism as we use it is closely associated with van Fraassen (2008); see also Morton & Mohler (2021). Note that van Fraassen did not endorse inferentialism!

> answer has to be that the data model represent the phenomena; but why does that not just push the problem [namely: *what is the relation between the theoretical model and the phenomena it models*] one step back? 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, pp. 238, 253)

On this view the link between mathematical models and nature has three ingredients:[99]

1. A mathematical representation of natural phenomena by some ("surface") *data model*; think of a numerical table in which the position of a certain planet in the sky is recorded on a daily basis, or of the terabytes of data produced by the Event Horizon Telescope in their black hole imaging, suitably organized. Even in the most advanced sciences, the mathematical structure of data models is elementary: they consist of numerical tables or more 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.

2. A *theoretical* model of this *data* model, henceforth called a *mathematical* model, as in our context it is some model of a theory of mathematical physics (within the variety described above). In the planetary case this theory could be Newton's theory of gravity, or Einstein's. The associated mathematical model would be some Newtonian space-time 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 are estimated from the data models they are supposed to measure, confirming the endless ping-pong match between theory and data inherent to physics. Unlike data models, mathematical models (like the masses and orbital parameters) can be as mathematically advanced as one likes.

3. A "user" of the mathematical model and the data model (which may be a team of scientists!) acting as a "middle man" between the two. The user also carries out the data analysis, which as recalled in the Introduction is often theory dependent.

Comparison of a mathematical model with a data model by the user is then done by surrogative inference, as described before; the details depend on the case at hand. 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 paper we claim one cannot reasonably believe after Newton, or introduce some circularity.[100] 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.

[99]The critique of this approach initiated by Bogen and Woodward (1988) is off target for us. Briefly, their claim is that theories explain phenomena rather than data. But this takes 'phenomena' as given, which at least in modern physics a highly questionable assumption. See the Introduction.

[100]See Wallace (2022, 2024). It should be obvious from this paper that we wholeheartedly sympathize with Wallace's 'math-first' approach to physical theories, but reject his associated structural realism.

For example, the theories of Ptolemy, Copernicus, Kepler, Newton, and Einstein all allow logical inferences about future positions of the planets from current or past ones, which may be checked against data models in the form of tables; here, Hertz's 'necessary consequents in nature of the things pictured' are simply successions of positions in time. For the first three astronomers such inferences were their only goal, whereas for the last two these were arguably a by-product of a loftier endeavour; but even towards those also Newton and Einstein ultimately made inferences (though different ones).

In a more complicated example, the mathematical model is the Kerr black hole spacetime of general relativity (which is even an exact solution), 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 (with far better accuracy for the former than for the latter) estimated from a data model, which in case of the famous EHT images of M87 and Sagittarius A* consists of data from various runs of an array of radio telescopes all over the world, appropriately synchronized and organized. 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). In our view, properties typically attributed to the target (a "supermassive black hole") on the basis of data actually belong to the mathematical model only, and form the basis of comparing inferences within the mathematical model to the those within the data model. This example also shows the role of the (relativized) *a priori*: the *theory* of general relativity *constitutes* the meaning of "spacetime" and "black holes", since it is within this theory that one can *define* these concepts in a model-independent way,[101] in order to *apply* them to specific models, which, then, may or may not have a black hole in them.

Our next example highlights the role of theories as yardsticks. The *Standard Model of elementary particle physics* (SM) was developed in the 1960s and 1970s, and turned out to be astonishingly accurate, culminating in the discovery of the Higgs boson in 2012 at CERN.[102] The *theory* underlying this *model* is quantum field theory (QFT),[103] 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.[104] 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). But today, physicists *hope* that experimental results from colliders or cosmic rays *violate* the predictions (inferences) of the model! For that would be the only way forward: the model is a yardstick against which empirical information (and the associated data models) is held, as opposed to a true description thereof. This may seem an extreme case; but we

[101] 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.

[102] See Galison (1983), Pickering (1984b), Pais (1986), and Brown et al. (1997) for various aspects of the history of the Standard Model. Cottingham and Greenwood (2007) and Schwartz (2014) are textbooks.

[103] To be precise, quantum field theory has not yet reached the state of mathematical rigour that contemporary mathematical physics asks for, but renormalized perturbation theory has; which so far has been all that is needed for a comparison of the Standard Model with nature.

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

claim that *every* physical theory has this status. *Every* theory is eventually overtaken, often accompanied by profound conceptual changes and redefinitions of basic terms. The emergence of non-Euclidean geometry in the 19th century may also be (re)interpreted in this light: the original view of Euclidean geometry as a true description of the geometry of the world gave way to a number of alternative possibilities but remained viable as a yardstick.[105] Similarly for the exact solutions of Einstein's equations for general relativity:[106] Minkowski space-time is an accurate yardstick in most 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. These are all models in our sense, and as Elgin (2022) said in the text just quoted: no theory or model or law of nature is ever true.

Truth with capital T is reserved for claims at a meta-level, of the following kind:[107]

- 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).[108]
- For models this implies that it isn't properties p of models M as such that are true (where M having property p should correspond to a theorem φ of the theory T of which M is a model), but claims to the effect *that* M has property p.

The first bullet on Truth in pure mathematics is defended in Landsman and Singh (2023). The second bullet is a corollary thereof: M *having* a certain property is a matter of fact. Floating between the *a priori* and the *a posteriori*, properties themselves lack truth values.

5 Discussion

Our philosophy of mathematical physics took the 'crystalline purity' of exact theories like general relativity and quantum mechanics out of nature and relocated it to the *a priori*, construed in a relativized way and decoupled from both intuition and necessity, so that its meaning-constitutive part remains: 'normal' research (in a Kuhnian sense) in both experimental and theoretical physics is enabled by adopting relevant theories of mathematical physics, and paradigm shifts correspond to such theories being overthrown and replaced.

[105] See for example Gray (2007) and Torretti (2019).

[106] Here Stephani et al. (2003) and Griffiths & Podolský (2009) are standard references. See also Landsman (2022) for a recent mathematical introduction to general relativity.

[107] Gödel (1995) made a similar point against the claim that truth is conventional, in pointing out that even if numbers like 5, 7, and 12 are conventions and the use of + and = is conventional, too (all of which he doubted), the statement *that* $5+7=12$ follows from these conventions is by itself not conventional but is a (combinatorial) truth. Moreover, since in mathematical physics (as well as in Gödel's example from arithmetic) the underlying theories arose from 'hardening empirical regularities into rules', properties of mathematical models ultimately reflect on reality even if these models are not used as yardsticks.

[108] This differs from Hale and Wright (2000), §5, who regard the claims of T *itself* as true, then do not see how this truth could accommodate empirical falsification thereof, and hence propose a certain modification of T as the vehicle of *a priori* definitions. This is not necessary in our approach. See also Landsman (2025).

Their associated models (in the fairly general sense detailed above) float between the *a priori* and the *a posteriori*: they serve as yardsticks or objects of comparison held against nature via some sort of surrogative inference (whose details depend on the example).

In this way, we have tried to combine "best (philosophical) practices" from Aristotle onwards into a coherent framework. To start, without in any way claiming that Aristotle himself would favour the following move (which foreshadows the later Wittgenstein), we propose that although his "*qua*" act of separation as such rarely if ever leads to *ideal* mathematical objects or properties, it still gives rise to some kind of 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. This gives Aristotle's mathematics the 'if ... then' or fictional character often attributed to it: *If* this hoop were a perfect or ideal circle, and *if* this stick were a (sufficiently long) perfect line, *then* the hoop would intersect the stick at either zero, one, or two points. At the very least, these perfect mathematical objects would then be "located" in the mind of the individual carrying out the "separation". But such a location would be unnecessarily limited: summarizing the relevant part of Husserl's philosophy of mathematics, Rota made the following point:[109]

> The constitutive property of mathematical items is not existence, but identity. (...) It is painful to abandon the age-old prejudice that identity must presuppose existence. The permanence of the identity of a mathematical item through space and history, and across civilizations, is an extraordinary phenomenon for which there is no easy explanation, and which is shared by few objects of the world. (Rota, 2000, p. 93)

The connection to Aristotle and Wittgenstein is that it is the act of idealization that introduces the 'crystalline purity' of pure mathematics, which, although it initially takes place in the mind, makes the ensuing perfect mathematical object mind-independent, in giving it the 'permanence of the identity' attributed to it by Rota (on behalf of Husserl). Thus mathematical purity resides neither in the ineffable platonic realm (whose existence we, following Aristotle and Wittgenstein, deny) nor is it just a psychological construction: it is a shareable resource, both synchronically and diachronically, which consists of rules.

This way of locating the 'crystalline purity' of exact theories also resolves 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; the former exists in the way just stated, but this existence is not in nature. In mathematical physics, then, the role of the mathematically simple is to serve as an object of comparison to be held against the complex phenomena in nature.

On the other hand, little of Newton's philosophical legacy is saved by our proposal. As we saw, the tension he encountered was between his traditional commitment to seeing mathematical objects actually realized in nature (notably mechanical trajectories) and his extremely innovative and successful concept of force, especially the gravitational one, which blasts this traditional ("Euclidean") framework. All of this has now been moved to the *a priori* side, and Newton's mathematical realism (or naturalism) is completely lost.

[109]See also Landsman & Singh (2023) for further context and references to Husserl himself.

In fact, all realism has disappeared from mathematical physics: in our philosophy it resides neither in pure mathematics, which is essentially formalist, nor in science, of which our philosophy is empiricist. The following quotes state our view very well:

> 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, pp. 327)

> One clear moral for our understanding of mathematics in application is that we 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)

This loss of realism also has consequences for models. As we already noted in the Introduction, in the pertinent literature the key theme is the relationship between a model and its "target". We use scare quotes here, since we wish to draw attention to the tacit assumption that the target is known, clear, and unambiguous. If this is the case, a model is at best a tool of some practical interest, rather than a deep move in epistemology; but as noted in the Introduction, in modern physics–including even Newtonian physics, as we have hopefully made clear–the target is often not very clear at all (cf. the Introduction).

Van Fraassen's use of data models (rather than phenomena) as targets of (mathematical) models seems a good way of overcoming this difficulty, except for the fact that it adds a further complication to the ping-pong match between the *a priori* and the *a posteriori*. Recall that our picture of mathematical physics already involves an interplay between theory and observation (and/or experiment) that is quite subtle: carefully selected 'empirical regularities' inferred from natural phenomena are 'hardened into rules' (Wittgenstein) that form mathematical theories, in which capacity these regularities have become 'exact concepts completely disconnected from reality' (Schlick). The use of data models complicates this process, since these are usually theory-laden in the sense that extracting data even from structured natural phenomena is far from unambiguous: it is informed by some theory (the ultimate example in high-energy physics is the successful search for the Higgs boson, but there are hardly any *exceptions* to the theory-ladenness of data models).

Another challenge to our philosophy of mathematical physics lies in the recovery (from inferentialism) of some kind of referential semantics in case that it applies.[110] This

[110]See Bueno and Colyvan (2011), Bueno and French (2018), Soto and Bueno (2019), Khalifa, Millson, and Risjord (2022), and Povich (2024) in connection with applied mathematics, and Nefdt (2020) and Brandom (1994, 2001) and especially Hlobil and Brandom (2024) in the philosophy of language.

is basically a reconstruction problem, since if some mathematical term actually refers, then what it refers to typically originated in the empirical phenomena which the ambient mathematical theory has 'hardened' into rules. The meaning of the term, given through its (inferential) use, should then identify its referent. See Landsman (2025) for examples. Reference is logically secondary to inference and in principle we could live without it.

This is not to say that the author is a 'strong social constructivist' (in believing that all knowledge is constructed and originates in power structures, as opposed to being answerable to some external reality). As convincingly explained by Galison (1987, 1997), even in modern high-energy physics, with its intricate interplay between theory and data, and even for the EHT, at the end of the day (or of the experiment) there is matter of fact about what we would describe as a comparison between a yardstick (given by some mathematical model) and reality (represented though by some data model). And similarly, on the theory side there is a matter of fact about mathematical models having some property.

6 Epilogue: Wigner's 'unreasonable effectiveness'

Finally, we return to Wigner (1960). As shown by Ferreirós (2017) on the basis of both the 1960 paper and other sources, Wigner was a formalist.[111] Furthermore, as a philosopher of science he was an empiricist, or at least he had a very pragmatic attitude.[112] Since this is close to our position, and because the three great theories of mathematical physics (viz. GR, QM, and statistical mechanics) are both perfect illustrations of his first point (i.e., 'that mathematical concepts turn up in entirely unexpected connections'),[113] as well as of the aside that follows it ('Moreover, they often permit an unexpectedly close and accurate description of the phenomena in these connections'), we feel a special responsibility to analyse the 'unreasonable effectiveness of mathematics in the natural sciences'.

Let us start with the famous ending of the paper:

> The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future

[111] For example, Wigner (1960), p. 2, answers the question *What is Mathematics?* by: 'mathematics is the science of skillful operations with concepts and rules invented just for this purpose. The principal emphasis is on the invention of concepts.' This is followed by a paragraph in which the word 'rules' occurs five times.

[112] Wigner's answer to the question *What is Physics?* is that 'The physicist is interested in discovering the laws of inanimate nature', said to be concerned with regularities and invariance principles, which however 'contain, in even their remotest consequences, only a small part of our knowledge of the inanimate world. All the laws of nature are conditional statements which permit a prediction of some future events on the basis of the knowledge of the present, except that some aspects of the present state of the world, in practice the overwhelming majority of the determinants of the present state of the world, are irrelevant from the point of view of the prediction.' (p. 5). The word 'reality' occurs just once, in a negative context (p. 14).

[113] Riemannian geometry was not invented in support of some theory of gravity, and complex numbers and Hilbert spaces were conceived well before the advent of (mature) quantum mechanics.Admittedly, Hilbert spaces were only defined by von Neumann in 1927 with the specific purpose of a mathematical formulation of quantum mechanics. Yet many examples of this structure, quite unrelated to quantum theory, preceded this move; the corresponding eigenvalue problems were in fact partly inspired by *classical* physics. Wigner (1960) emphasizes the unexpected use of complex numbers in this context, whose introduction by Cardano and others around 1545 took place in the context of cubic equations.

> research and that it will extend, for better or for worse, to our pleasure even though perhaps also to our bafflement, to wide branches of learning. (Wigner, 1960, p. 14)

Different from his 'first point', this concerns the applicability of mathematics to physics *per se*. This already looks threatening for a formalist, especially if phrased as follows:[114]

> Pure mathematics and physics are becoming ever more closely connected, though their methods remain different. One may describe the situation by saying that the mathematician plays a game in which he himself invents the rules while the physicist plays a game in which the rules are provided by Nature, but as time goes on it becomes increasingly evident that the rules which the mathematician finds interesting are the same as those which Nature has chosen. (Dirac 1940, p. 124)

In the relatively short paper from which this passage is quoted, Dirac mentions 'beauty' 12 times, typically as the driving force behind mathematics. More generally, as long as one emphasizes the autonomy of mathematics and its guiding principles, its applicability is miraculous. But we have seen that this autonomy is a phenomenon of the late nineteenth and twentieth centuries, before which mathematics and physics went hand in hand or were even inseparable. In particular, the development of mathematical ideas greatly relied on physics. Since Hilbert, we may say that mature mathematical theories arise from the axiomatization of either physical theories or heuristic mathematical theories, or some combination thereof. This takes the sting out of the general applicability problem.[115]

Wigner's first point remains; the relevance of mathematics to modern mathematical physics may at first sight appear especially surprising, or even miraculous. However, the amount of surprise is sometimes softened by studying the actual historical record: for example, Maddy (2007), §IV.2, points out that Einstein's use of Riemannian geometry in his theory of general relativity 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).[116] More generally:

> We forget how much of even the purest mathematics has its roots in physical sources and how many structural similarities hold between diverse physical situations; we forget how many phenomena can't be described in mathematical terms and how much pure mathematics has no application; we forget what a wide range of pure mathematics there is to choose from in our efforts to describe the world; and we don't take into account the widespread fudging that is involved in successful applications. (Maddy, 2007, p. 343)

Furthermore, 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. Moreover, the relocation of the 'crystalline purity' of mathematics from the natural realm to the *a priori* and the accompanying role of models as objects

[114] We owe this reference to Islami and Wiltsche, (2020), p. 158. Dirac was Wigner's brother in law.

[115] This comment is far from new; see the references in footnote 8.

[116] Einstein's innovation consisted of passing firstly from space to spacetime (whilst changing the signature of the metric) and secondly from the electromagnetic to the gravitational field.

of comparison rather than descriptions of nature proposed here 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 surprising. Finally, in van Fraassen's approach–which we adopt–mathematical physics is applied to data models rather than natural phenomena; already the latter are *selected* by physicists from all that happens; and the extraction of suitable data involves a further selection procedure in which, as already remarked, proto-theories already guide the process and mature theories even give meaning to physical concepts to be studied. Wigner himself acknowledged as much:

> [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)

In our view, the unreasonable effectiveness of mathematics therefore mainly lies in Wigner's aside (i.e., 'Moreover, they often permit an unexpectedly close and accurate description of the phenomena in these connection'). And this observation is further reinforced by the historical fact that it is not just the *accuracy* but also the *generality* of the main theories of mathematical physics that was truly unexpected, or indeed miraculous.

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