# Rigour from rules: Deduction and definition in mathematical physics

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**Abstract** We ask how and why mathematical physics may be seen as a rigorous discipline. Starting with Newton but drawing on a philosophical tradition ranging from Aristotle to (late) Wittgenstein, we argue that, as in mathematics, rigour ultimately comes from rules. These include logical rules of inference as well as definitions that give a precise meaning to physical concepts such as space and time by providing rules governing their use in models of the theories in which they are defined. In particular, so-called implicit definitions characterize "indefinables" whose traditionally assumed familiarity through "intuition" or "acquaintance" from Euclid down to Russell blasts any hope of both rigour and innovation. In terms of the basic physics concepts, one may subsequently define derived concepts (like black holes or determinism). Definitions are seen as *a priori* meaning-constitutive conventions that are neither necessary à la Kant nor arbitrary à la Carnap, as they originate in empirical science as well as in the autonomous development of mathematics and physics. As such they are best seen as *hypothetical*.

## 1 Introduction

The two most influential books in the history of the exact sciences are unquestionably Euclid's *Elements* and Newton's *Principia*. The former marked the beginning of mathematics as a rigorous discipline, whereas the latter performed the same job for mathematical physics. Newton apparently modeled his *opus magnum* on Euclid: both start with definitions and axioms,[1] and proceed to derive a large number of

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[1] Euclid splits his axioms into postulates and common notions (the latter being principles of proof common to all 13 books of the *Elements* or even mathematics as a whole. Newton does not do this but conflates axiomas with laws: *axiomata sive leges motus* i.e. 'axioms or laws of motion'. Newton

mathematical propositions from these.[2] Pulte (2001, 2005) even calls *Principia* a 'Euclidean system' in a sense described by Lakatos (1978), viz. 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). On the other hand, although this is the way Euclid was mostly read until at least the early modern era, it is unknown how Euclid himself (who is silent about all this) interpreted his postulates; it has even been argued that he did so rather more hypothetically than truthfully, in the style of both traditional Greek dialectics before Plato and modern mathematics since Hilbert.[3] Historically speaking, the term 'Aristotelian system' may therefore be more appropriate for *Principia* than 'Euclidean system'. Indeed, although the (alleged) Scientific Revolution—of which *Principia* was undoubtedly the culmination—at least in the hands of Galilei and Descartes incorporated an explicit break with Aristotle, *Principia* seamlessly fits into the mould of Aristotle's theory of demonstrative sciences as expounded in the *Posterior Analytics*:

> 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)

Once again, also since the causal connection between Aristotle and Euclid is unclear,[4] it is a difficult question to what extent also the *Elements* actually fits into this Aristotelian mould,[5] but for our purposes it is enough to note that for at least 2000 years it was widely taken to do so and as such was seen as the pinnacle of rigorous reasoning, copied in style not only by Newton in *Principia* but also in a completely different context by Spinoza in his *Ethics*. This practice defined, more or less by example, what is actually meant by "rigour", at least in a mathematical context. The key ingredients of this example have usefully been summarized by Burgess (2015), pp. 6–7:

> We can read in—or read into—Aristotle a statement of what remains today the requirement of rigor by which the individual mathematical author hoping to achieve publication in a recognized journal is confronted:

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also differs from Euclid, who writes as tersely as possible, in going out of his way to explain his definitions, axioms, and propositions via comments and entire scholia.

[2] Even the presence of two kinds of propositions, namely theorems and problems or constructions, is shared.

[3] See Szabó (1960, 1978), Jahnke (2009), Paseau and Wrigley (2024), and surely others.

[4] Apart from the interpretation of the premisses, this is also a technical question about deduction: there is a mismatch between Aristotle's syllogistic reasoning (which is based on clearly stated rules) and Euclid's reasoning, which is not only informal, but, if formalized, cannot be based on syllogisms (Mancosu and Mugnai, 2023). 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* (Mancosu, 1999). Avigad, Dean, and Mumma (2009) present a formal minimal proof system for the *Elements* Euclid, and of course Hilbert .

[5] See for example Heath (1956) and Mancosu and Mugnai (2023).

(1) Mathematical rigor requires that every new proposition must be deduced from previously established propositions (. . . )

(2) On pain of circularity or infinite regress, if later propositions must be proved from earlier ones, then we must start from some unproved propositions or postulates.

Such unproved propositions are also called *axioms*.

(3) Mathematical rigor requires that every new notion must be defined in terms of previously explained notions (. . . )

(4) On pain of circularity or infinite regress, if later notions must be explained in terms of earlier ones, then we must start from some unexplained notions or primitives.

The first two points concern *deduction*, the last two are about *definition*; though as we shall see, there is no sharp boundary between axioms and definitions. Superficially, the idea of rigour has therefore hardly changed since Aristotle: it is grounded in an axiomatic-deductive style of reasoning with due attention to definition. What has changed, though, is the interpretation of concepts like deduction and definition. At least since Newton's *Principia*, mathematical physics has aspired to the same standards of rigour as mathematics itself (for better or worse), and as far as deduction is concerned there seems to be little difference between mathematical physics (as opposed to theoretical physics!) and mathematics. We will therefore be brief about deduction and focus on definition, whose role in mathematical physics does stand apart from (pure) mathematics.

Thus in §2 we review the status of deduction in mathematics (and hence mathematical physics) vis-à-vis rigour. The more substantial §§3–5 are concerned with definition in historical context, starting with Aristotle and moving on to Frege and Hilbert, thus initially in mathematics but moving to mathematical physics, highlighting different uses in these different contexts, and paying special attention to the problem of indefinables. We illustrate our approach with the examples of general relativity, focusing on the definition of a black hole (which inspired this investigation).

## 2 Deduction

Deduction is of course no longer based on either Aristotle's syllogisms or Euclid's informal reasoning. Since Frege, Russell, and Hilbert, at least mathematical deduction has been formalized via *explicitly stated rules of inference appropriate to mathematics*, of which there are many valid and even inequivalent versions.[6] Barring some rare exceptions, in practice mathematical physicists simply adopt the style of mainstream mathematicians. In the 17th century Newton was in fact one of these exceptions,[7]

[6] In our view Gentzen's system of Natural Deduction, which exists in both classical and intuitionistic versions, is the crowning achievement of the quest for such rules (Pelletier and Hazen, 2012, 2024; von Plato, 2013; Peregrin, 2020). The existence of competing (and from a modern perspective inequivalent) logical systems is ancient, starting with Stoic logic as an alternative to syllogisms, or even earlier (Kneale and Kneale, 1962).

[7] *Principia* is a mix of 'ancient' techniques and Newton's own innovations (Whiteside, 1974; Guicciardini, 2009).

but this style never became mainstream (since no one was able to copy it) and his exception is so singular that for our purposes we may as well ignore it in so far as mathematical inference is concerned. In the 18th and early 19th centuries there was hardly any difference between (mainstream) mathematics and mathematical physics: top mathematicians like Euler, Lagrange, and Laplace co-created both.[8] In the 19th century mathematics and (mathematical) physics did start to decouple,[9] but some of the greatest and most influential mathematicians involved in this process, like Riemann and later Hilbert and Poincaré, continued to work in both mathematical physics and mathematics, and in their latter capacity adopted or were even instrumental in defining the corresponding standards of rigour, so that these (changing) standards of mathematical rigour continued to be used in mathematical physics, too. As the classics of 20th century mathematical physics exemplify,[10] this resulted in the fact that mathematical physicists tend to present their mathematical formalism in the same way as mainstream mathematicians.[11]

Thus the question what rigour of deductions (or proofs) in mathematical physics amounts to is prcatically the same as for mathematics as a whole. This does not mean that this question is easy; but it is not our main theme, so we will be brief. [12] *In principle* a proof is rigorous if it has been formalized in such a way that each step follows from the previous one(s) according to some explicitly indicated rule(s) of inference of the proof system in use. Moreover, anyone trained in the use of such rules should be able to verify and validate the proof (like a chess umpire).[13]

> For [this] concept of rigor we make a historical claim: That rigor is absolute and here to stay. The future may see additional axioms for sets or alternatives to set theory or perhaps new new more efficient ways of recording (or discovering) proofs., but the notion of a rigorous proof as a series of formal steps in accordance with prescribed rules of inference will remain. (Mac Lane, 1986, p. 378)

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[8] See e.g. Darrigol (2005), Lützen (2010), Truesdell (2012), Gray (2021), and Stan (2025).

[9] See e.g. Jungnickel and McCormmach (1986), Gray (2008), and Maddy (2008).

[10] Here one may think for example of von Neumann (1932), Streater and Wightman (1964), Ruelle (1969, 1978), Reed and Simon (1972–1978), Hawking and Ellis (1973), Thirring (1977–1980), Arnold (1978), Bratteli and Robinson (1979, 1981), Haag (1992), and Choquet-Bruhat (2009), which despite its publication date belongs to the 20th century.

[11] Today this includes a tacit reliance on ZFC set theory and first-order logic to which higher structures are added in Bourbaki-style. Davies (2005) and Landsman (2017, chapter 12) are exceptions, calling for constructive mathematics.

[12] See Thurston (1994), Dutilh Novaes (2011), Weir (2014), Hamami (2018), Ording (2019), Avigad (2021), Burgess & De Toffoli (2022), and Stillwell (2022) for perspectives on the wide variety of styles of proof in mathematics.

[13] This analogy is based on the following correspondence between a formal proof and a game of chess, proposed by Weyl (1926): the *axioms* of some theory are analogous to the starting position of a game of chess; the *deduction rules* are analogous to the possible moves; a *sentence* (as defined in logic) is analogous to *some* position on a chess board; a *theorem* is like a *legal* position in a correctly played chess game; and finally a *proof* is like a game leading to that position, played according to the rules. In mathematics, the ultimate umpire would be a proof assistant that verifies a completely formalized proof, see e.g. Geuvers (2009) and Wiedijk (2023) for introductions and examples. See also Sing (2023), who discusses this possibility in more detail as a language game, and Landsman and Singh (2025).

*In practice*, except for exams and *Principia Mathematica*, no proof is ever stated in this way:[14]

> Moreover, there are good reasons why Mathematicians do not usually present their proofs in fully formal style. It is because proofs are not only a means to certainty, but also a means to understanding. Behind each substantial proof there lies an idea, or perhaps several ideas. They idea, initially perhaps tenuous, explains why the result holds. The idea becomes Mathematics only when it can be formally expressed, but that expression must be so couched as to reveal the idea; it will not do to bury the idea under the formalism. (. . . ) Proofs serve both to convince and to explain—and they should be so presented. (Mac Lane, 1986, p. 378–379)

Alternatively, according to Wittgenstein,[15] 'The proof must be surveyable' in the sense that 'it should be visible that a proof is a proof.' The key tension is that a short informal proof is intended to be clear at the cost of rigour, whereas a formal proof is intended to be rigorous by sacrificing clarity. Both are error-prone, in different ways: informal inferences may be hard to verify by an 'umpire' and may even be wrong,[16] whereas formal proofs may introduce errors because of their gigantic length. Moreover, errors in formal proofs are more severe than those in informal ones:

> an attempt at a formal derivation with even a single error is not a derivation, [whereas] an informal proof can have small mistakes and yet still reasonably lead us to believe in the correctness of its conclusion. (Avigad, 2021, pp. 7384–7385)

One may add the conceptual complications added by computer-assisted proofs, as in the four-colour theorem, where the reduction of the proof to an astronomical number of case distinctions is clear, whilst the actual verification thereof is as remote from 'surveyable' as one can have it.[17]

In conclusion, the gap between the Scylla of formal rigour (with its associated fragility) and the Charybdis of informal clarity (with its intrinsic human frailty) remains wide. As importantly, what also remains undecided is the *origin* of the rules of inference (i.e., of logic).[18] We merely state our own view (Landsman and Singh, 2025; Landsman, 2025), inspired by Wittgenstein:[19]

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[14] Indeed, 'most mathematicians cannot even state the axioms of a particular formal system' (Avigad, 2021, p. 7378).

[15] Quoted from MS 122: p. 105, see Mühlhölzer, 2010, p. 574, and Floyd (2023).

[16] On top of this even informal proofs may have thousands of pages: the classification of finite simple groups comes to mind, (Wikipedia, undated). In mathematical physics the proof of the stability of Schwarzschild space-time is a worrying example (Dafermos *et al.*, 2019, 2021), as are many other proofs in the field called mathematical relativity.

[17] See Haken (2006) and Gonthier (2008) for the theorem, and Tymoczko (1979) and Shanker (1987), pp. 143–160, for the ensuing philosophical debate.

[18] See e.g. Jacquette (2002, 2007), Shapiro (2007), Haaparanta (2009), and Blanchette (2012) for various opinions.

[19] Wittgenstein admittedly says this about $25 \times 25 = 625$ rather than about the rules of logic. However, as explained in Landsman (2025), various passages of the *Philosophical Investigations*, such as §§130–131, suggest that he held similar views about language games, which are his ("late") forms of logic (Kuusela, 2019).

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

Just read 'mathematical practice' for 'the empirical proposition' and 'experience': the idea is that for various reasons rules of inference arose in this practice (presumably well before Euclid),[20] became widely adopted, and eventually—even before they were formalized and explicitly stated—became normative. This normativity is an example of Friedman's 'relativized *a priori*' in the sense that a given system of rules of inference is meaning-constitutive for some formalization of mathematics but nonetheless is not immune to possible change.[21] Wittgenstein's famous 'hardness of the logical must' just reflects this *a priori* status of logic: once some practice has hardened into rules—once it has crossed the line from the *a posteriori* to the *a priori*, so to speak—it has become immune to empirical verification and instead has become a paradigm or yardstick.[22] See also §5.

## 3 Definition: Aristotle

> Socrates, disregarding the physical universe and confining his study to moral questions, sought in this sphere for the universal and was the first to concentrate upon definition. (Aristotle, *Metaphysics* 987b1)

We now analyze the second ingredient of rigour in mathematics and mathematical physics (and even in philosophy and reasoning in general), side by side with deduction: *definition*.[23] Many of Plato's dialogues are, or include, searches for definitions,[24] but ironically, most such attempts fail. For both Plato and Aristotle, defining

[20] See e.g. Szabó (1960, 1978) and Dutilh Novaes (2020) for various speculations about the origins of these rules.

[21] See Friedman (2001, 2002) for the relativized *a priori*, which Friedman applies to theories of space and time etc. rather than to logic; putting the latter into this context was inspired by Peregrin (2020). Landsman and Singh (2025) argue that mathematics may be seen as a 'rhododendron of language games' in which various proof systems for mathematics (or logical foundations thereof) peacefully coexist, corresponding to different branches of the 'rhododendron'.

[22] See especially *Philosophical Investigations*, §§130–131. The German original 'Maßstab' is superior to the English translation 'yardstick' in that the original much better expresses the intended normative character of the word.

[23] The literature on this topic is immense. Our summary of Aristotle below is based on Charles (2010) and Bronstein (2025). See also Charles, ed. (2010) and Smith (2022). General historical and philosophical surveys and studies of definitions are Robinson (1954), Abelson (1967), Gupta & Mackereth (2023). In connection with mathematics see also Werndl (2009), Cellucci (2018), Coumans (2023), Sereni (2024), and Anstey and Bronstein (2025).

[24] For example, *Meno*, *Eutyphro, Laches, Sophist, Theaetetus, Charmides, Philebus*, and last but not least the *Republic* are, roughly speaking, searches for the definition of virtue, piety, courage, sophistry, knowledge, temperance, pleasure, and justice, respectively, whilst *Cratylus* is a quest for the nature of names and signs, asking questions about conventions and reference quite similar to those analyzed about 2300 years later in analytic philosophy.

was a search for essence, which Plato sought in the lofty realm of forms or ideas, whereas Aristotle approached it on earth through his theory of the demonstrative sciences in the *Posterior Analytics*. As we have seen, Aristotle's demonstrations start from premisses. These include *axioms* (which are common to all sciences) as well as *definitions* and *hypotheses* (both of which belong to some given science): whereas the former are neutral as to the possible existence of some *definiendum*, the latter assures existence. All premisses, and hence in particular all definitions, must be 'true, primary, immediate better known or more familiar than the conclusion, prior to the conclusion, and causes of the conclusion.'[25]

The purpose of a definition of $X$ is to reveal the essence of $X$, where $X$ is some concept,[26] so that its definition is an answer to the question *what* $X$ is by explaining *why* it is; in other words, the questions what $X$ is and why $X$ is have the same answer, namely the definition of $X$. This, in turn, involves identifying one or more of the four possible causes of $X$, which for Aristotle famously could be efficient (i.e., preceding), final, formal, or material.[27] Thus a definition of $X$ could be the conclusion of a demonstration that provides such causes of $X$ (and ideally also shows that $X$ is "necessary"), following which $X$ might serve as a premiss of some new demonstration. This gives the dual nature of definitions: scientific demonstrations or explanations rely on definitions, which in turn may be the conclusions of such demonstrations. But they need not be, in which case some definition is said to be *indemonstrable*, also called a *principle* or *primary premiss*.

Indemonstrable definitions have haunted philosophy from Plato (who clearly saw their need and problems, too, albeit perhaps in a less formal way) to the present day. Aristotle's own way out follows a dismissal of Plato's suggestion that such principles are innate, concluding that:

> It is clear that we must get to know the primary premisses by induction; for the method by which even sense-perception implants the universal is inductive. Now of the thinking states by which we grasp truth, some are unfailingly true, others admit of error— opinion, for instance, and calculation, whereas scientific knowing and intuition are always true: further, no other kind of thought except intuition is more accurate than scientific knowledge, whereas primary premisses are more knowable than demonstrations, and all scientific knowledge is discursive. From these considerations it follows that there will be no scientific knowledge of the primary premisses, and since except intuition nothing can be truer than scientific knowledge, it will be intuition that apprehends the primary premisses – a result which also follows from the fact that demonstration cannot be the originative source of demonstration, nor, consequently, scientific knowledge of scientific knowledge. If, therefore, it is the only other kind of true thinking except scientific knowing, intuition will be the originative source

[25] See Smith (2022), here quoted *verbatim*, for the meaning of these criteria (which is not uncontroversial).

[26] Hence Plato and Aristotle are often said to seek *real* definitions. Any concept $X$ is of course conventionally approached via some word, but the idea is that both the word(s) for $X$ and its 'real' definition are linguistic formulae for some language-independent part of the world. In the footsteps of Aristotle, Galen more explicitly split definitions into two parts: a nominal part pinpointing $X$ by initial naming (accepted by all competent speakers of the given language) and an essential one found out by enquiring into the nature of $X$ (Hood, 2010).

[27] See e.g. Falcon (2023) for an discussion of Aristotle's four causes, which may be also seen as explanations.

> of scientific knowledge. And the originative source of science grasps the original basic premiss, while science as a whole is similarly related as originative source to the whole body of fact. (*Posterior Analytics* II.19, translated by G.R.G. Mure)

Thus indemonstrable definitions are supposed to be grasped by intuition, claimed to be even more true than scientific knowledge; and what is grasped is 'known by induction' (which Aristotle elsewhere describes as 'argument from the particular to the universal'). But this seems at odds with the idea that premisses are supposed to be certain, a property hardly associated with either intuition or inductively gained knowledge. Already Euclid struggled with this tension, as is obvious from his attempts, widely regarded as unsatisfactory already in antiquity, to define what in his approach logically speaking should have been indefinables,[28] such as points, lines, and surfaces.

Eventually, Aristotle's scheme gave way to our current hypothetico-deductive view of scientific procedure,[29] in which no premiss of some scientific demonstration is regarded as strictly true, self-evident, or necessary, and the flow is both in a positive sense from some empirically welcome conclusion to an increased belief (for realists) or acceptance (for empiricists), and in a negative sense from the possible falsification of some conclusion by experiment to the falsehood of one or more of the premisses. But this does not make Aristotle's analysis of definition irrelevant; as we shall see, in mathematical physics the search for essence remains an important goal of definition.

What has changed since Aristotle's work on definition, then, is especially that:

1. definitions should no longer be regarded as true, necessary, or self-evident;
2. the four Aristotelian causes supposed to ground every definition in its goal of providing essence have been replaced by other ingredients (such as mathematics and logic);
3. indemonstrable definitions, or "indefinables", are dealt with via implicit definition.

## 4 Definition: Frege to Hilbert

We now skip 2200 years, since despite the importance of intermediate events, Frege and Hilbert provide a valid point of entry into the modern era.[30] In Frege's *Begriffsschrift* from 1879, a definition was an abbreviation, i.e., an arbitrary stipulation by which a new sign is introduced to take the place of a complex expression whose meaning we already know. But *Grundlagen der Arithmetik* from 1884 is a sus-

[28] See Heath (1908), pp. 155–194. This was fully clarified only by Hilbert (1899), see also below.

[29] See Pulte (2001, 2005) and more briefly Paseau and Wrigley (2024), summarized in Landsman (2025).

[30] For a glimpse of what happened during these 2200 years one may consult Anstey and Bronstein (2025). In our brief account cannot possibly do justice to Frege either; apart from the original sources (Frege 1879, 1884, 1903) see for example Dummett (1996), chapter 2, Horty (2007), Shieh (2008), and Hallett (2021).

tained quest for the definition of number, and more generally for the "definition of a definition". In the best Platonic tradition the former is not found, but about the latter:

> The basic picture of definition in *Grundlagen* is this. We start with an expression that has been in use for some time, and in which we have not discerned any logical structure — call such expressions 'simple'. A definition of this expression rests on an analysis of the concept that it expresses as logically structured from simpler concepts; the *definiens* reflects this logical structure by including occurrences of truth-functional connectives and quantifiers — call such expressions 'complex'. Using such a definition, one can *import additional* logical structure into those propositions which have been in use prior to the analysis and in which the *definiendum* occurs. In virtue of the presence of the additional logical structure, we can discern deductive relationships to and from these propositions that we could not discern prior to the analysis-based definition. In this sense, the definition enables us to prove things we could not prove without it. (Shieh, 2008, p. 997)

This points to a much loftier goal of defining than mere abbreviating, namely *concept formation*. This might also be called *explication* (a term more often used in connection with Carnap), viz.

> a reconstruction of the meaning of some expression already in use in terms of others that are better understood, or in some other ways less problematic. (Horty, 2007, p. 35)

Here one may think of 19th century definitions of continuity, or of real numbers, etc.: the availability of an advanced logical language is crucial for the clarity that this kind of definition brings. The more interesting ones among Frege's definitions therefore have dual nature: formally, they are abbreviations or notational conventions, but if the *definiendum* was already in use, typically somewhat informally and hence without having been defined before (even in mathematics!), then the *definiens* not only clarifies it but is even meaning-constitutive. This duality is echoed by a famous passage from *Principa Mathematica* (whose debt to Frege is generally enormous):

> Theoretically, it is unnecessary ever to give a definition: we might always use the definiens instead, and thus wholly dispense with the definiendum. Thus although we employ definitions and do not define "definition," yet "definition" does not appear among our primitive ideas, because the definitions are no part of our subject, but are, strictly speaking, mere typographical conveniences. Practically, of course, if we introduced no definitions, our formulae would very soon become so lengthy as to be unmanageable; but theoretically, all definitions are superfluous. In spite of the fact that definitions are theoretically superfluous, it is nevertheless true that they often convey more important information than is contained in the propositions in which they are used. This arises from two causes. First, a definition usually implies that the definiens is worthy of careful consideration. Hence the collection of definitions embodies our choice of subjects and our judgment as to what is most important. Secondly, when what is defined is (as often occurs) something already familiar, such as cardinal or ordinal numbers, the definition contains an analysis of a common idea, and may therefore express a notable advance. Cantor's definition of the continuum illustrates this: his definition amounts to the statement that what he is defining is the object which has the properties commonly associated with the word "continuum," though what precisely constitutes these properties had not before been known. In such cases, a definition is a "making definite": it gives definiteness to an idea which had previously been more or less vague. For these reasons, it will be found, in what follows, that the definitions are what is most important, and what most deserves the reader's prolonged attention. (Russell and Whitehead, 1910, pp. 11–12).

Dummett (1991), following G.E. Moore, refers to this duality or ambiguity in definition as the 'paradox of analysis': if the *definiendum* is not obviously "the same" as the *definiens*, how can we maintain we have given a correct definition? For example, while there was never any doubt about the meaning of a prime number or a finite group, finding the right definition of the continuum was definitely a struggle that divided the greatest mathematicians like Hilbert, Brouwer, and Weyl.

How does Frege deal with indemonstrable definitions? To start, in his view 'we must never present as a definition something that is in need of proof or of some other confirmation of its truth',[31] so in that sense all his definitions are indemonstrable. Nonetheless, he does face a problem similar to Aristotle, in that at the end of the day some terms in some basic definitions will not themselves be defined. Frege's solution is not to appeal to 'intuition' but to 'elucidation':[32]

> One can also recognize a third proposition, elucidatory propositions, but I would not want to count them as part of mathematics itself but refer them to the antechamber, the propaedeutics. They are similar to the definitions, in that they too are concerned with fixing the meaning of a sign (or word). But in addition, they contain elements whose meaning cannot be assumed as known completely and beyond question, perhaps because they are used variously or ambiguously in the language of everyday life. In the cases where a meaning is to be given to a sign which is logically simple, then one cannot give a definition proper, but one must content oneself with fending off the unwanted meanings which crop up in the use of language, indicating the one intended. In doing this, certainly one must always count on a cooperative understanding trying to hit upon the meaning. Such statements of elucidation can- not be used in the same way that the definitions can, because they lack the necessary precision. For this reason, as I said, I confine them to the antechamber. (Frege to Hilbert, 27 December 1899)

As noted by Hallett (2021), since Frege insisted throughout his career that at least in mathematics words without fixed meaning have no meaning at all, relying on something as vague as 'elucidation' is disastrous for Frege's entire logicist program, indeed at least as damaging as Russell's paradox (which is usually seen as the death blow to this program).[33] Frege even follows Aristotle in calling 'axioms propositions that are true but are not proved because our knowledge of them flows from a source very different from the logical source, a source which may be called spatial intuition.'[34]

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[31] Frege to Hilbert, 27 December 1899; Gabriel *et al.* (1980), p. 36. The Frege–Hilbert correspondence, to which we will return, lasted from 1895 to 1903, centered around Hilbert's *Grundlagen der Geometry* (*Foundations of Geometry*) from 1899, in which he developed and analyzed new axioms for Euclidean geometry in a style of mathematics which became dominant in the twentieth century. The correspondence is reprinted with commentary in Gabriel *et al.* (1980). See also Hallett (2010, 2021), Blanchette (2018), and Rohr (2023) for the Frege–Hilbert debate as a whole.

[32] See Hallett (2021), pp. 171–173 for what follows.

[33] Speaking of Russell: his concept of 'knowledge by acquaintance' (i.e., direct awareness without the intermediary of inference or any knowledge of truths, cf. Hasan and Fumerton, 2024) addressing what he calls ' indefinables' is hardly more convincing than Frege's 'elucidation' or Aristotle's 'intuition', especially in connection with modern mathematical physics. This poverty is all the more remarkable since Russell considered the discussion of indefinables the chief part of philosophical logic; see Russell (1903), Preface, quoted in this context by Hallett (2021), p. 177.

[34] Frege to Hilbert, 27 December 1899; Gabriel *et al.* (1980), p. 37.'

Neither elucidation nor spatial intuition was necessary for Hilbert, who circumvented both:[35]

> You [Frege] say further: 'The explanations in sect. 1 are apparently of a very different kind, for here the meanings of the words "point", "line", . . . are not given, but are assumed to be known in advance.' This is apparently where the cardinal point of the misunderstanding lies. I do not want to assume anything as known in advance; I regard my explanation in sect. 1 as the definition of the concepts point, line, plane – if one adds again all the axioms of groups I to V as characteristic marks. If one is looking for other definitions of a "point", e.g., through paraphrase in terms of extensionless, etc., then I must indeed oppose such attempts in the most decisive way; one is looking for something one can never find because there is nothing there; and everything gets lost and becomes vague and tangled and degenerates into a game of hide-and-seek. (Hilbert to Frege, 29 December, 1899; Gabriel *et al.* (1980), p. 39)

> In my opinion, a concept can be fixed logically only by its relations to other concepts. These relations, formulated in certain statements, I call axioms, thus arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the definitions of the concepts. I did not think of this view because I had nothing better to do, but I found myself forced into it by the requirements of strictness in logical inference and in the logical construction of a theory. I have become convinced that the more subtle parts of mathematics and the natural sciences can be treated with certainty only in this way; otherwise one is only going around in a circle. (Hilbert to Frege, 22 September 1900; Gabriel *et al.* (1980), p. 51)

It is hard to find a better expression of the opposition between a referential semantics of mathematics, where the basic objects of mathematics are supposed to be 'given in advance' (notably by definitions) prior to axiomatization,[36] and an inferential semantics where these objects—the alleged "indefinables"— are introduced and defined *by* the system of axioms in which they appear. Unnecessarily conservatively, Hilbert initially interpreted the logical connectives in the traditional way; but as suggested by his student Gentzen, even these are implicitly defined by the rules for logical inference.[37] A similar attitude towards Frege was displayed by "middle" Wittgenstein,[38] whose comments on the analogy between mathematics and chess provided strong (but probably unintended) philosophical support for Hilbert's position, for example:

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

[35] Hilbert's reference to 'sect. 1' etc. in the first excerpt below is to his *Grundlagen*.

[36] It is worth specifying what exactly one means by objects 'given in advance'. A useful criterion was proposed by Mühlhölzer (2012), p. 114: 'An object is *given in advance* iff the criteria of identity for the object which the language game is about are *not* completely stated or presented by the language game itself; and it is *not given in advance* iff the criteria of identity for the object are completely stated or presented in the language game – [so] that this identity is given by the language game alone and by nothing else.'

[37] See Giovannini & Schiemer (2021), §4.2, and references therein, as well as von Plato (2013).

[38] Weiberg & Majetschak (2022) define Wittgenstein's middle period as 1929–1936. Kienzler (1997), Mühlhölzer (2008, 2010), Stenlund (2015), and Max (2020) discuss some of Wittgenstein's comments on chess. As reviewed by Epple (1994), serious discussion (and criticism) of the analogy between mathematics and chess started with Frege. In the context of implicit definitions connections between Hilbert and Wittgenstein have been discussed before by Muller (2004), Mühlhölzer (2008, 2010), and Friederich (2011). See also Landsman and Singh (2025).

> just as in language the rules of syntax define the logical element of a word. (Wittgenstein, 1967, p. 134)

This idea of *implicit definition* has a tangled history. A French line starting with Gergonne (1818) ended in Poincaré, whose debate with Russell mirrored the one between Hilbert and Frege,[39] e.g.,

> For Russell, shape is just the sort of "indefinable" basic term of which we have an unanalyzable, immediate grasp, and asking for a definition of it is like asking for "the spelling of the letter A". For Poincaré, that allegedly immediate grasp may be relevant to the psychology of the individual. But it cannot help us to understand how a concept functions within a coherent system of principles, so that two persons who may not associate the same immediate intuition with a concept can nonetheless reason with it in accord with one another. That requires that both apply the same systematic criteria that constitutes its implicit definition. This has two notable consequences: that the vague intuitive notion can be made precise by articulation the principle that is implicitly assumed in our use of it; and that such a principle, once isolated, allows us to treat the subject as a formal one that is independent of intuition altogether. (. . . )
>
> So [Kant's] transcendental principles, conditions under which intuitions can provide objective knowledge of relations in space, cannot transcend the intuitions whose order they constitute. It was Poincaré who understood that if there is such an order, or "form of intuition," it must be implicit in the concepts that we impose upon intuition. Those concepts are implicit in the rules that guide our intuitive practice. (DiSalle, 2006, pp. 85–86)

Hilbert, partly following Dedekind and Pasch,[40] was the high point of a German line, and there was also an independent Italian route involving among Burali-Forti and Peano at an earlier stage, which culminated in the work of Enriques (1913).[41] Here is a summary of the idea by Enriques:

> A geometrical theory may be regarded as a system of logical relations, holding for certain concepts designated by the words "point," "line," etc. We may attribute to these words an abstract and indeterminate meaning, thus regarding them as the symbols of unknown concepts, but such as formally satisfy the fundamental propositions which express geometrical relations. It is then allowable to decide at will, by some convention, the meaning of our symbols, provided this be done in a way that will fulfil the formal conditions already stated. In this way we obtain an infinite number of possible concrete interpretations of abstract geometrical theories. (Enriques 1906, p.109)

The Frege–Hilbert debate did clarify some respects in which implicit definition is ambiguous:

1. One may or may not desire a unique reference for the terms that are implicitly defined by the axioms (up to an appropriate kind of isomorphism). One may want this in some (non-Wittgensteinian) philosophies of language, and perhaps

[39] See e.g. Coffa (1991), Friedman (1999), Ben-Menahem, (2006), Stump (2015), Warren (2020), etc.

[40] See Pollard (2010), chapter 4. Giovannini & Schiemer (2021) call implicit definitions *structural*, since the words 'implicit' and 'explicit' are adjectives for certain technical definitions in logic (which Beth's definability theorem actually identifies). Although Hilbert himself did not use this term, we will follow Enriques (1913) and subsequently the logical positivists (see below) in using 'implicit definitions'. See also Torretti (2012) and Schlimm (2013).

[41] See Mancosu (2016), chapter 2, and Biagioli (2024) for more information.

sometimes also in mathematics,[42] but in mathematics and mathematical physics this is usually as impossible as it is undesirable (and boring if it happens to be the case). Here is the most famous passage from Hilbert:

> You [Frege] say that my concepts, e.g. 'point', 'between', are not unequivocally fixed; e.g. 'between' is understood differently on p.20, and a point is there a pair of numbers. But it is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. If in speaking of my points I think of some system of things, e.g. the system: love, law, chimney sweep . . . and then assume all my axioms as relations between these things, then my propositions, e.g. Pythagoras' theorem, are also valid for these things. In other words: any theory can always be applied to infinitely many systems of basic elements. [This] can never be a defect in a theory, and it is in any case unavoidable. (Hilbert to Frege, 29 December, 1899; Gabriel *et al.* (1980), p. 40)

2. At the semantic level, one may conceive implicit definitions as either:[43]

   - Determining the *meaning* of the undefinables or primitives in the axioms, for example by simply be declaring that this has been done by postulating their mutual logical relations (which was the opinion of e.g. Peano and Enriques), or by fixing their possible interpretations (as would be made more precise about three decades later in model theory),[44] which was arguably the view of Hilbert himself, as well as of Poincaré:[45]

     > [His] analysis reveals the foundation of geometry in a "disguised" or implicit definition. But having this foundation does not make geometry an uninterpreted structure. For the definition itself constitutes an interpretation of a specific type of phenomenon as instantiating a specific mathematical structure. (DiSalle, 2002, p. 181)

     This highlights an interpretation of geometry (or even mathematics in general) that also Wittgenstein adopted in his later work, namely that it is not *descriptive* of some external reality but *normative* for possible descriptions thereof through convention.[46]

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[42] Indeed, Hilbert (1900) himself famously defined the real numbers as a particular ordered field in what is now called a categorical way, i.e., with a unique model (up to isomorphism), but this was done in second-order logic. Even the Dedekind–Peano axioms do not characterize the natural numbers, but even if they did, both examples would be misleading in so far as mathematical physics as well as much of mathematics is concerned.

[43] We follow Giovannini and Schiemer (2021), which contains details and references. See also Sereni (2024).

[44] See Eder and Schiemer (1918).

[45] Poincaré's views on what he called "disguised" definition was developed in a large number of his writings (of which the most famous one is his popular book *Science and Hypothesis*), which can be traced from Gray (2013).

[46] Here is an example from Wittgenstein: 'What about the sentence "The sum of the angles in the triangle is 180°'? If it does not turn out to be 180°' in a measurement, I will assume a measurement error. The sentence is therefore a postulate about the manner of description.' (Ts-212,XV-114-5[1], where all Typescripts (Ts-) can be found online via `http://www.wittgensteinsource.org`). Baker & Hacker (2009), §VII.4) write that mathematical theorems are '*norms of representation*', in the sense that theorems are not (primarily) *descriptive*, as in a platonic view; instead they are *normative for possible descriptions*. Similarly, Mühlhölzer (2012), p. 109, says that mathematical

   - Defining a *structure* (which was Frege's view, who called it a 'second-level concept'), cashed out, in later parlance, by its models. In each of these the undefinables are actually defined in a traditional sense, namely by their interpretation in the given model.

3. It is common in the philosophical literature to interpret implicit definitions as 'truths [that] are merely reflections of what we mean — or which concepts we employ' (Warren, 2022, p. 28), so that the axioms, or, in ordinary language, sentences containing the *definienda*, determine the meaning of the latter by the 'free stipulation of the truth' of these sentences.[47] We do not attribute any kind of truth to definitions, including implicit ones.[48] Instead, we follow at least this aspect of Poincaré's conventionalism as paraphrased by Ben-Menahem:

   > 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. (. . . ) . The axioms are considered to bestow meaning on the implicitly defined terms in the sense that they fix a range of possible interpretations. On this construal, there is no postulation of truth by convention. (Ben-Menahem, 2006, p. 140)

To avoid confusion between the various kinds of conventionalism on the market (and especially their different concepts of truth), on the above view we therefore propose to write 'hypothetical' instead of 'conventional'; this terminology especially respects the decisive historical transition from the 'Euclidean' (= Aristotelian) model to the 'hypothetico-deductive' model, cf. §1.

As usual, Wittgenstein's take on these matters is interesting and helpful. As we saw, his comments on chess are in the spirit of implicit definition, as is the entire concept of a language game in the *Philosophical Investigations*. In both cases certain rules define the use of the indefinables, which in turns provide their meaning. The very nature of this procedure makes further justification impossible:

> Once I have exhausted the justifications, I have reached bedrock, and my spade is turned. Then I am inclined to say: "This is simply what I do." (Wittgenstein, 2009, §217)

Wittgenstein returned to this issue in his last work, *On Certainty*. Here he distinguishes between what we are certain of and what is true. In many cases (among which we may include theories of mathematical physics as discussed in this essay) our certainties correspond to the rules that constitute a 'picture of the world'. Consistent with our interpretation of conventionalism, such rules are neither true nor false themselves, but if they are accepted they enable true and false statements:

> But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false. (Wittgenstein, 1975, §94)

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propositions are *preparatory* for descriptions of empirical states of affair, instead of being *about* such states. . See also Bangu (2025).

[47] This kind of truth attribution is also shared by the neo-logicists, see e.g. Hale and Wright (2000)

[48] As explained in Landsman (2025), this is part of a more general discussion about the possible truth of the conventions that form the *a priori*.

## 5 Definition: Mathematical physics

As we just saw, implicit (or disguised) definitions first arose in the context of spatial (i.e., Euclidean and non-Euclidean) geometry through the work of Hilbert, Poincaré, Enriques, and others, providing not only a solution to the problem of indefinables but *en passant* turning the traditional referential interpretation of mathematics in which primary objects are supposed to be known before mathematics starts describing them, into an inferential one is which the meaning of symbols is given by their use according to the (inferential) rules following from their (implicit) definitions.

Unlike Hilbert's views at least initially, but like Kant's and Helmholtz's in earlier phases of the discussion, Poincaré's views on geometry were closely related to physics, with explicit discussions of Newtonian mechanics and later relativity in the light of his conventionalism (which is inseparable from his use of implicit or "disguised" definitions).[49] Although Enriques, who like Poincaré's was a mathematician, a philosopher and a popularizer of science, partly opposed the latter's views, also he saw the possibility of a vast expansion of the idea of implicit definition:

> In Enriques's usage, implicit definitions of the basic concepts of geometry indicate that the meaning of the abstract terms designating them in an axiomatic system is determined relative to the formal implications of the theory, along with its possible instantiations in concrete scientific domains. Enriques also gave a fully general interpretation of the notion of implicit definition by identifying the basic concepts of all disciplines as abstract concepts, whose meaning is determined in part by a formal theory and in part by empirical circumstances. (Biagioli, 2024, p. 19)

Inspired rather by by Hilbert and (early) Wittgenstein, Schlick (1918/2009), §I.11, also suggested such generalizations. Providing more detail than our previous authors, his idea was to relate ostensive definitions of objects in reality, which are connected through a 'network of empirical judgements', with implicit definitions of objects in some mathematical theory, of which however the only "example" Schlick gives is that of 'the basic equations of physics'. These two networks of definitions 'completely agree' in that they are uniquely connected by a coordination procedure. Although Schlick's book followed his earlier one on relativity within a year, the above program wasn't even carried out for special or general relativity, let alone for the rest of (modern) physics.[50]

Nonetheless, despite this false start the idea to extend implicit definition from pure mathematics to physics seems sound: it means that theories of mathematical physics (such as Newtonian mechanics or general relativity) implicitly define the physical concepts appearing therein (such as space, time, and motion), assuming the mathematical concepts, such as a Euclidean or more general manifold and a specific fixed or variable metric, are understood via their own axioms and definitions. Returning to the Frege–Hilbert opposition, it is as if Frege would insist that concepts

[49] It is rather within mathematics that Poincaré restrained himself in this respect, arguing (similar to Brouwer) that arithmetic had a completely different origin from geometry, namely in the human experience of time, cf. Gray (2013).

[50] See Coffa (1991) and DiSalle (2006) for especially critical reviews; for history also see Wagner (2022).

like space and time are understood *prior to* the establishment of the theory in which they (subsequently) appear, whereas Hilbert (whom we obviously side with) would define these concepts *by* such a theory (and hence is subject to change if the underlying theory changes). In particular, implicit definitions are taken to be meaning-constitutive for the indefinables, in the Wittgensteinian sense that meaning is defined by use according to the rules set by the theory in question.[51] One might even argue that this pragmatic "meaning = use" point of view goes back to Newton:

> Relative quantities, therefore, are not the actual quantities whose names they bear but are those sensible measures of them (whether true or erroneous) that are commonly used instead of the quantities being measured. But if the meanings of words are to be determined by usage, then it is these sensible measures which should properly be understood by the terms "time," space,", "place," and motion, and the manner of expression will be out of the ordinary (. . . ) (*Principia*, Scholium in Book 1; Cohen and Whitman, 1999, pp. 413–414)

Indeed, we follow DiSalle (2006) in his claim that Newton, in *Principia*, realized that the key conceptual ingredients of his mechanics such as space, time, mass, and force should be embedded in his theory as *definitions* that ground his edifice. One might say that some of Newton's attempts to define things explicitly tend to be as unsuccessful (in being vague and/or circular) as Euclid's; what does work is taking his definitions and axioms/laws as a whole in mutual dependence.[52]

Furthermore, the (mistaken) idea of the logical positivists that (implicit) definitions are *arbitrary* conventions has already been answered by DiSalle particularly in connection with Newton:

> That his arguments concern definitions, rather than making metaphysical claims, might seem incompatible with what seems to be an incontrovertible fact, namely, that Newton's views of space and time are essential to the ontological basis for his theory — part of his understanding of how the world really is. This incompatibility is only apparent. It comes from the assumption that such definitions are arbitrary, as the positivists suggested, and adopted because of the simplicity and general usefulness of the conceptual frame work in which they occur; it overlooks the fact that Newton's definitions emerge from a conceptual analysis. Newton undoubtedly believed in a world of real things, including God as well as material objects, things whose real causal interactions are governed by the laws of nature, whatever those might be. He therefore attempted to define space, time, and motion in such a way that this picture of the world might make sense — that is, not to stipulate the character of space and time, but to discover, by analysis of what we do know about the world, how they *must* be defined in order to make sense of such a world. (DiSalle, 2006, p. 40)

Also from later authors (within our canon) the picture arises that far from being arbitrary conventions, implicit definitions in mathematical physics are the result of applying conceptual analysis to empirical phenomena. In particular, Wittgenstein

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[51] Following Brandom we take these rules to be inferential, cf. Landsman and Singh (2025) and Landsman (2025).

[52] See also Cohen and Whitman (1999). DiSalle (2006), chapter 2, partly following Stein (1967), explains how Newton's definitions of absolute time and space in the Scholium following Definitions 1–8 in Book 1 make sense (only) in connection with his laws of motion. Similarly, his explicit definitions of mass and force only make good sense in combination with—are implicitly defined by— his laws of motion, which in turn rely on mass and force. This was also Poincaré's view (DiSalle, 2002; Gray, 2013, pp. 370–371).

argued that ‘empirical propositions are hardened into rules’ (cf. §2), and according to Poincaré, ‘empirical generalization are exalted to become a postulate’.[53] Hilbert, in explaining his program of axiomatization, made a similar point:

> it is important to give the theories formed on the basis of experience a firmer structure and a basis that is as simple as possible. For this it is necessary to clearly work out the prerequisites and to differentiate exactly what is an assumption and what is a logical conclusion. In this way, one gains clarity about all unconsciously made assumptions, and one recognizes the significance of the various assumptions, so that one can oversee what modifications will arise if one or the other of these assumptions has to be eliminated. (Hilbert, 1920/1992, pp. 17–18)

Having crossed the line from the *a posteriori* to the *a priori*, such definitions then become normative or meaning-constitutive for the correct use of the concepts thus defined. As argued in in Landsman (2025), the basic physical concepts then appear with the meaning a specific theory has already endowed them with in the models of the underlying theories.[54]

## 6 Synthesis and examples

In his thesis on definitions in mathematical practice (which covers the modern era), Coumans (2023), p. 212, gives the following ‘partial taxonomy of definitions based on their roles’:[55]

1. *Abbreviational definitions* help to efficiently write down a mathematical text.
2. *Narrative definitions* structure the narrative of a mathematical text.
3. *Functional definitions* are needed to complete various steps in a proof.
4. *Essential definitions* capture the essence of a possibly preformal concept.
5. *Explanatory definitions* provide a fruitful perspective on a particular concept.

These categories overlap, making it natural to see “definition” as a family resemblance:[56]

> I can think of no better expression to characterize these similarities than “family resemblances”; for the various resemblances between members of a family: build, features, colour of eyes, gait, temperament, etc. etc. overlap and criss-cross in the same way. (Wittgenstein, *Philosophical Investigations*, §67)

As we have seen, the first point in the list is the one initially declared by Frege as well as by Russell and Whitehead to be the only “official” possibility; the second is

[53] See DiSalle (2006), p. 84, as well as DiSalle (2002), *passim*.

[54] Here we take a relaxed view of what a model of a theory of mathematical physics amounts to, cf. Landsman (2025), but even on a strict view (i.e. as an exact solution to the exact equations of the theory a case like general relativity shows the richness of the class of models (in that case seen as solutions to the Einstein equations): even in vacuum they range from flat spacetime to rotating black holes. How boring would it be to have just one model!

[55] We changed the order and added a numbering.

[56] See also Slug (2006) and Baker and Hacker (2009a), Chapter XI.

a more relaxed version thereof. The third was highlighted by Lakatos (1963/2015). The fourth was taken to be the higher goal of definition not just by Aristotle but also by Frege, Russell, and Whitehead. As Coumans explains, essential definitions should align with previous intuition about the *definiendum*, whereas explanatory definitions need not do so; but in practice they are hard to distinguish. We may add:

> A more promising notion of definition by conceptual analysis is based not on the question, "what do we typically mean by *X*?", but rather on the question, "what conception of X is implicit in our established empirical judgments and practices?" (DiSalle, 2002, p. 179)

This gives a Kantian twist to Aristotle's essential definitions, unique to mathematical physics:

6. *Transcendental definitions* clarify what is implicit in our established empirical judgments and practices.

To this list we also add a further distinction between explicit and implicit definition, as we see it:

- *explicit* definitions are always abbreviational and may also belong to any other category.
- *implicit* definitions paradoxically "define undefinables" via axiom systems in which they occur; as such, recent as they may be, they belong to the Aristotelian categories 4, 5, and 6.

In pure mathematics, set theory arguably provides the clearest examples of implicit definition:

> 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. (von Neumann, 1925/1967, p. 395).

At one stroke, this replaced half a century of failed attempts by Dedekind, Cantor, Frege, and others to define sets explicitly or intuitively; von Neumann effectively declares the concept of a set to be indefinable in the traditional sense, but definable via implicit definition,[57] falling into the Aristotelian category 4 above. Note that the membership relation $\in$ is part of this implicit definition (much as the equality sign $=$ used in set theory is defined implicitly by the axioms of first-order logic), upon which all of the usual set-theoretic symbols like $\subset$ and $\cap$ etc. are defined explicitly. Also more generally, explicit definitions ultimately rely on implicit ones.

In mathematical physics 'axioms systems' are better seen as mathematically rigorous theories of physics such as (precise) formulations of classical mechanics, general relativity, quantum (field) theory, and statistical mechanics. Moreover, implicit definition should be construed in a more relaxed way than in pure mathematics. In the latter, symbols like $\in$ and $=$ and words like 'set' are implicitly defined as indicated by von Neumann; in the original example of Hilbert (1899) it is the words 'point',

[57] See Ferreirós (2008) for history and Muller (2004) specifically on von Neumann (1925/1967).

'line', and 'plane' that are thus defined. In mathematical physics, on the other hand, the typical situation is like the one in general relativity: space-time is first defined *explicitly* within mathematics (i.e., as a connected $4d$ Lorentzian manifold, possibly time oriented).[58] However, *that* the mathematical objects thus defined play the role of physical space-time, including the physical meaning thereof, is *implicitly* defined by the theory as a whole. Similarly: in *Principia* Newton gave explicit definitions of time, space, and force (in words); but *that* these explicit definitions indeed define time, space, and force is only clear from his theory as a whole, including his laws of motion (see footnote 52). To take a simpler example involving just the meaning of a constant: in special relativity one may explicitly define "the speed of light" as a positive constant $c$ entering the metric $ds^2$ as well as Maxwell's equations; but *that* this constant defines the physical speed of light is only clear from special relativity and electrodynamics as a whole. The point, as in all such cases, is that there isn't a well-defined concept like "the speed of light" *prior* to its appearance in some theory, which subsequently is described by some theory (such as special relativistic electrodynamics), although there may be a preformal idea about it; instead, the speed of light only becomes rigorously defined *by* a suitable theory. Or, taking an example from Knox and Wallace (2023): viscosity enters the Navier–Stokes equations as a real constant $\eta$ in a specific place; but *that* this constant captures the essence of the preformal concept of viscosity is implicit in these equations and the theory of their solutions (that is, in hydrodynamics) as a whole.[59] But this applies not just to constants but even to e.g. space and time. As such, the discussion is similar to the Frege–Hilbert and Russell–Poincaré debates about spatial geometry mentioned earlier.

Once the basic "category 6" concepts have thus been defined explicitly by some mathematical stipulation but implicitly by the entire physical theory (or some relevant fragment thereof), one may try, on that basis, to define both concepts *within* the theory, such as black holes, or concepts *about* the theory, such as determinism. Both of these fall into the Aristotelian categories 4 and/or 5 in the above list. The same point seems to apply here: though defined explicitly in a mathematical sense, the physical meaning of these concepts is defined by the theory as a whole.

For example, the definition of a black hole in general relativity rests on two independent ideas:[60] Penrose's definition of an (absolute) event horizon as, roughly speaking the boundary of the region in spacetime where light-rays can escape to (null) infinity (which is what makes a black hole "black"); and a slow and painful process, starting with Einstein himself, led to the identification of a spacetime singularity with an incomplete and inextendible causal geodesic (which is the "hole"). As proposed more generally in Landsman (2025), concepts like this are defined in an ambient *theory* (such as in this case general relativity), upon which specific *models* of this

[58] See e.g. Hawking and Ellis (1973), §3.1, or Landsman (2022), Definition 5.3.

[59] More generally, the 'constitutive spacetime functionalism' proposed by Knox and Wallace (2023) seems, apart from their realist agenda, close to the use of definitions we propose here. See also Butterfield and Gomes (2023).

[60] See Landsman (2022ab) for details and references. The technical details are by themselves a major achievment.

theory (such as: spacetimes satisfying the vacuum Einstein equations) may or may not have the property thus defined (like the Schwarzschild or Kerr spacetimes having a black hole and Minkowski spacetime not having one). Likewise, a property like determinism should be *defined* at the level of general relativity, allowing of course the notion of a model in describing what the said property *means*, upon which specific models may or may not in fact *be* deterministic, according to the given definition. Although the jury is still out at the time of writing,[61] the procedure itself is rigorous.

[61] See Manchak *et al.* (2025) and references therein to earlier work on determinism in general relativity.

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