Is mathematics a game?

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Abstract

We re-examine the old question to what extent mathematics may be compared to a game. Under the spell of Wittgenstein, we propose that the more refined object of comparison is a "motley of language games", the nature of which was (implicitly) clarified by Hilbert: via different language games, axiomatization lies at the basis of both the rigour and the applicability of mathematics. In the "formalist" game, mathematics resembles chess via a clear conceptual dictionary. Accepting this resemblance: like positions in chess, mathematical sentences cannot be true or false; true statements in mathematics are *about* sentences, namely *that they are theorems* (if they are). In principle, the *certainty* of mathematics resides in proofs, but to this end, in practice these must be "surveyable". Hilbert and Wittgenstein proposed almost oppositie criteria for surveyability; we try to overcome their difference by invoking computer-verified proofs. The "applied" language game is based on Hilbert's axiomatization program for physics (and other scientific disciplines), refined by Wittgenstein's idea that theorems are yardsticks to which empirical phenomena may be compared, and further improved by invoking elements of van Fraassen's constructive empiricism. From this perspective, in an appendix we also briefly review the varying roles and structures of axioms, definitions, and proofs in mathematics. Our view is not meant as a philosophy of mathematics by itself, but as a coat rack analogous to category theory, onto which various (traditional and new) philosophies of mathematics (such as formalism, intuitionism, structuralism, deductivism, and the philosophy of mathematical practice) may be attached and may even peacefully support each other.

Contents

1 Introduction

The aim of this paper is to re-examine the old question to what extent mathematics may be compared with a game (like chess).[1] Whatever the answer, it is a good point of entry into the philosophy of mathematics, and even into its history; for example, it is no accident that (barring some "playful" earlier allusions) the idea of such a comparison originated in the late 19th century, since that was the time of the "modernist transformation" in which mathematics gradually lost its connection with physical reality, intuition, and visualizability; these were replaced by abstraction and rigorous proof.[2] Serious analysis started with Frege's criticisms of Thomae (1898), Heine (1872), and Illigens (1893).[3] Frege fiercely rejected the analogy between mathematics and chess; for he was primarily unable to comprehend how a mere game could describe any "thought".[4] All of his more specific reasons for disapproving of the analogy essentially reduce to this inability, such as:

- Mathematics is *meaningful*, since it refers to real objects. Games are meaningless.
- Thus the rules of mathematics originate in reality, whereas for games they are arbitrary.
- Grounded in reality, there is *truth* in mathematical theorems, which games lack.
- The applicability of mathematics would be incomprehensible if it were merely a game.
- Mathematics also incorporates *the theory of the game* instead of only *being the game*.[5]

Such arguments go to the heart of the philosophy of mathematics. Seeing mathematics as a game provides a yardstick for answering the main (traditional) questions in this area, such as:[6]

1. What is mathematics *about*?[7] Do "mathematical objects" exist? If so, are they "*abstract*"?[8]
2. Is mathematics *a priori*, in the sense that it does not rely on experience or experimentation?
3. What is the nature of mathematical truth? How can we know it? Is what we know certain?
4. Are mathematical truths *necessary*, in the sense that *they could not have been otherwise*?
5. How is applied mathematics/mathematical physics possible (and why is it so powerful)?

[1] The review by Epple (1994) provides a nice historical and philosophical introduction to this question; see also Detlefsen (2005). For the analogy with chess especially in Frege and Wittgenstein see also Kienzler (1997), Mühlhölzer (2008, 2010), Stenlund (2015), Max (2020), and Lawrence (2023). Wittgenstein's reflections on Frege in general (which fed much of his thought throughout his career) are reviewed by many authors. Apart form the above literature, see e.g. Reck (2002), Baker & Hacker (2009ac), Kienzler (2012), Dehnel (2020), Potter (2020), Schroeder (2021), etc.

[2] See, for example, Mehrtens (1990), Heintz (2000), Gray (2008), and Maddy (2008).

[3] See Frege (1903), §§86–137, translated in Geach and Black (1960). This degenerated into a hilarious public polemic between Thomae (1906ab, 1908) and Frege (1906, 1908ab). Frege (1899) is also relevant (and very funny).

[4] This was the central ingredient of his philosophy of both mathematics and language. The word 'thought' (*Gedanke*) is ambiguous in both English and German. Frege (1892) clarifies that for him, thoughts do not refer to the subjective act of thinking, but to the objective content thereof, which can be shared by many people.

[5] The argument seems to be that this is something a game could not accomplish, whence the analogy breaks down.

[6] This list is mostly paraphrased from Linnebo (2017), §1.1, except for the last question. See also Tait (2001).

[7] The question what it means that mathematics (or rather some language game) is *about* certain objects external to it is a difficult one. A useful answer has been given 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.'

[8] Abstraction may be taken to mean: not located in space and time and not participating in causal relationships.

Platonists like Frege answer these question from the belief that mathematics is *referential* in a very specific way, namely that mathematical language refers to *abstract* mathematical objects that "exist" outside this language and independently of those using it. In particular: *there are real mathematical objects, which mathematicians refer to and describe.* Thus mathematicians *discover* (rather than *invent*) mathematical truths, which consist of correct descriptions of these mathematical objects and their properties. Being discoverers, mathematicians are supposed to be similar to physicists who unearth properties of nature, with the flattering difference that what mathematicians discover is both *a priori* and necessary. As Hardy put it:[9]

> I have often used the adjective 'real', and as we use it commonly in conversation. I have spoken of 'real mathematics' and 'real mathematicians', as I might have spoken of 'real poetry' or 'real poets', and I shall continue to do so. But I shall also use the word 'reality', and with two different connotations.
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> In the first place, I shall speak of 'physical reality', and here again I shall be using the word in the ordinary sense. By physical reality I mean the material world, the world of day and night, earthquakes and eclipses, the world which physical science tries to describe.
>
> I hardly suppose that, up to this point, any reader is likely to find trouble with my language, but now I am near to more difficult ground. For me, and I suppose for most mathematicians, there is another reality, which I will call 'mathematical reality'; and there is no sort of agreement about the nature of mathematical reality among either mathematicians or philosophers. Some hold that it is 'mental' and that in some sense we construct it, others that it is outside and independent of us. A man who could give a convincing account of mathematical reality would have solved very many of the most difficult problems of metaphysics. If he could include physical reality in his account, he would have solved them all.
>
> I should not wish to argue any of these questions here even if I were competent to do so, but I will state my own position dogmatically in order to avoid minor misapprehensions. 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. This view has been held, in one form or another, by many philosophers of high reputation from Plato onwards, and I shall use the language which is natural to a man who holds it. A reader who does not like the philosophy can alter the language: it will make very little difference to my conclusions. (Hardy, 1940, pp. 63–64)

See Balaguer (1998), Panza & Sereni (2013), Landry (2023), and Linnebø (2023) for studies of platonism. Our goal is not to dispute this ideology in any detail, if only because the above summary is very superficial. But here is the main point of criticism: its ontology is obscure, and because of that it is also difficult to explain how we can know anything about it (as Aristotle already demurred against Plato).[10] Its explanation of the applicability of mathematics is also at best shadowy.

[9]And similarly: 'It seems to me that no philosophy can possibly be sympathetic to a mathematician which does not admit, in one manner or another, the immutable and unconditional validity of mathematical truth. Mathematical theorems are true or false; their truth or falsity is absolute and independent of our knowledge of them. In some sense, mathematical truth is part of objective reality. "Any number is the sum of 4 squares"; "any number is the sum of 3 squares"; "any even number is the sum of 2 primes". These are not convenient working hypotheses, or half-truths about the Absolute, or collections of marks on paper, or classes of noises summarising reactions of laryngeal glands. They are, in one sense or another, however elusive and sophisticated that sense may be, theorems concerning reality, of which the first is true, the second is false, and the third is either true or false, though which we do not know. They are not creations of our minds; Lagrange discovered the first in 1774; when he discovered it he discovered some- thing; and to that something Lagrange, and the year 1774, are equally indifferent.' (Hardy, 1929, p. 4). Yet Hardy (1940) takes the analogy with chess quite seriously, but rejects it: not on grounds of ontology or epistemology, but *triviality*: 'A chess problem is genuine mathematics, but it is in some way "trivial" mathematics. However ingenious and intricate, however original and surprising the moves, there is something essential lacking. Chess problems are *unimportant*.' (p. 88)

[10]See especially his *Metaphysics*, books M and N (μ and ν), available in many editions and translations.

More recently, Benacerraf (1973) argued that one cannot get both the ontology and the epistemology of mathematics right,[11] so that some philosophies of mathematics bend towards a sound ontology at the expense of the epistemology, whilst others do the opposite. Platonism seems to be the unique philosophy of mathematics that manages to get neither of the two right. Hence despite its apparent popularity, even among top mathematicians of the 20th century like Gödel (Kennedy, 2020), Connes (2001), and Penrose (2004), platonism is, in our view, best seen as a foil (like solipsism), to which a philosophy of mathematics based on games provides a useful antidote.[12]

In analyzing this cure for platonism, Wittgenstein's late philosophy is an important source, especially since it partly originated in his thoughts and reflections on the foundations of mathematics, whilst his philosophy of language constantly challenged referential ("Augsutinian") theories, see below. Our other protagonist is Hilbert, whose formalism—broadly taken and *correctly interpreted*—provides the key to modern mathematics and thence to our analysis.[13] Adding a crucial ingredient of the constructive empiricism of van Fraassen, we arrive at a synthesis to the effect that, in Wittgensteinian parlance, our answer to the question in the title is that mathematics may be seen as —or at least may be favourably compared with—a so-called *motley of language games*.

The motley we have in mind is like a branched tree. At the multiple roots (some would say: at the top) one finds foundational theories or overall frameworks like ZFC set theory.[14] Above (or below) these roots, interwoven individual areas of mathematics spurt out. Within most of these areas (or even within the foundational theories at the top), one has two absolutely crucial language games: the first concerns formalized proofs, whereas the second governs applications.[15]

This picture (or tool of comparison) is not meant as a philosophy of mathematics by itself, but rather as a coat rack onto which various (traditional and new) philosophies of mathematics may be attached and may even support each other. Among many, the basic ones we have in mind are:

- *Formalism* as the starting point of any game-theoretic picture of mathematics, cf. §A.1.
- *Intuitionism* as one of the possible language games giving a foundational framework. Even if the latter is classical (as in ZFC), intuitionism may provide the logic of the metamathematics used to analyze the framework (and similarly for *finitism*, as Hilbert tried).
- *Structuralism* entering pervasively in the proposed picture, starting with Hilbert's notion of implicit definitions, and moving to the structural empiricism as just explained.
- *Deductivism* governing our concept of mathematical truth.
- *Philosophy of mathematical practice* (Mancosu, 2008; Hamami & Morris, 2020), which is also favourably disposed towards the later Wittgenstein (Pérez-Escobar, 2022).

[11]See also Nutting (undated) and references therein for an excellent review of "Benacerraf's problem". His argument is based on the same assumption as Frege's, namely that mathematical reference is similar to ordinary linguistic reference, and hence is vulnerable to a late Wittgensteinian critique (Barco, 2018). We only use it rhetorically.

[12]The broader notions would be "constructivist" or "anti-realist" philosophies of mathematics. In the more general setting of realist versus non-realist philosophies of mathematics, Shapiro (1997) and Rush (2022) argue that this contrast is less than it seems, once these positions have been made sufficiently precise.

[13]Please note that this paper is *not* supposed to be a study of their respective philosophies of mathematics as such.

[14]Their axioms are what Feferman (1999) calls *foundational*. The others are *structural*. See also Schlimm (2013).

[15]From a Kantian point of view, we might say that the *a priori* side of mathematics is one language game, and the *a posteriori* side is another. These may also alternate, as in the case where some area of mathematics originates in experience (as all the traditional areas do), then becomes "purified" via formalization, and subsequently may be applied again, perhaps in a different context. Kant's analytic/synthetic distinction is less relevant for modern mathematics, except perhaps in propositional logic, where the tautologies could be called "analytic", and, throughout mathematics, in the context of *explicit* definitions, see §A.3. But invoking the latter distinction would add little to our discussion.

Rather than contradicting each other, such philosophies in fact complement and reinforce each other, as Hilbert himself clearly saw (at least for the first two, whilst embodying the third).[16] This unifying role of the motley of language games may be comparable to the fruitful way category theory (or more generally topos theory) is a coat rack for numerous mathematics disciplines; cf. Krömer (2007) and Marquis (2009) for philosophically informed histories of this area.

To summarize this picture, let us answer the five traditional questions in the philosophy of mathematics listed in the beginning of this Introduction, from the point of view developed here:

1. There are no mathematical objects in the platonic sense. Whatever its origins, once mathematics has become mathematics, it is a dynamical collection of families of rules. In Hilbert-style proof theory certain families of rules become the object of study, once again via specific (new) rules; in that case, the rules themselves become the object of (meta)mathematics.

2. Historically, all of mathematics originated in experience. Some areas subsequently took a life of their own, like a game, but even these remain a *human practice*. Others areas always remained close to experience, and many alternate between "pure" and "applied" (as a consequence of which this distinction seems to make little sense). In applied mathematics and mathematical physics, it is not so much the *mathematics itself* that relies on experimentation but the *match* between the rules inherent in it and the intended application.

3. Theorem as such are not "true". What may be true is the claim that some sentence is a theorem. This kind of truth is intersubjective: we should all agree about it. By definition, we can find truth through proof (which in turn is an example of strict rule following). Like Hilbert and Wittgenstein, we claim that the certainty of some proof is determined by its surveyability, but we redefine this using the more recent notion of proof verification.

4. Such truths are necessary, given the set of rules in which they originate. But this necessity is admittedly remote from the necessity that mathematics has in the eyes of a platonist.

5. Applied mathematics and mathematical physics—indeed all of mathematics—originate in attempts to axiomatize sufficiently mature theories of physics, space, and quantity, etc. Their power comes from the fact that this is a progressive practice, in which the axiomatizations improve upon checks against "reality", represented by data models which in turn improve.

The plan of the remainder of this paper is as follows. We start with a summary of what we learnt from Wittgenstein (§2), and then walk straight into our "motley" (§3). This is backed up by a study of what we take to be Hilbert's main views on the foundations of mathematics in so far as these are relevant to our program (§4).[17] Combining Hilbert, Wittgenstein, and van Fraassen (as we see them), we move on to the associated concepts of applied mathematics (§5), and finish with an analysis of truth (§6) and certainty (§7) in this light. These are the main sections of the paper.

The appendix of the paper, which may be skipped on a first reading, is a crash course in the philosophy of axioms (§A.2), definitions (§A.3), and proofs (§A.4), in so far as this is relevant to our approach. These topics are preceded by a short survey of formalism (§A.1), since this concept is often misunderstood (especially by philosophers and notably in connection with Hilbert).

Throughout this paper, readers who are cognizant of the (philosophical) literature on Hilbert and Wittgenstein may feel that they are on familiar territory, but even those readers might find some new analysis or context. Serious examples from physics must be left to a successor paper.

[16] Even platonism is not excluded by our proposal, although it would be unnatural, given that it was a foil.

[17] What is called "Hilbert's program", i.e. his attempts to give a finitist proof of the consistency of classical mathematics, plays no role here; its goal was never achieved or perhaps even well defined. See footnote 50 for references.

2 Wittgenstein

The main sources of Wittgenstein's philosophy of mathematics are *Wittgenstein und der Wiener Kreis: Gespräche* (Wittgenstein, 1984b), which records conversations between 1929–1931; *Philosophische Grammatik, Teil III: Grundlagen der Mathematik*, compiled from 1930–1933 (Wittgenstein1984c); *Bemerkungen über die Grundlagen der Mathematik* (BGM) from 1937–1944 (Wittgenstein, 1984d); and *Lectures on the Foundations of Mathematics, Cambridge 1939* (LFM; Diamond, 1975).[18] For our purposes, Wittgenstein's reply to Hardy (above) seems a good way to start:[19]

> Consider [Hardy (1929)] and his remark that "to mathematical propositions there corresponds—in some sense, however sophisticated–a reality". (The fact that he said it does not matter; what is important is that it is a thing which lots of people would like to say). Taken literally, this seems to mean nothing at all—*what* reality? I don't know what this means.—
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> But it is obvious what Hardy compares mathematical propositions with: namely physics. Suppose we said first, "Mathematical propositions can be true or false." The only clear thing about this would be that we affirm some mathematical propositions and deny others. If we then translate the words "It is true ..." by "A reality corresponds to ..."—then to say a reality corresponds to them would say only that we affirm some mathematical propositions and deny others. We also affirm and deny propositions about physical objects.—But this is plainly not Hardy's point. If this is all that is meant by saying that a reality corresponds to mathematical propositions, it would come to saying nothing at all, a mere truism: if we leave out the question of *how* it corresponds, or in what sense it corresponds.

[18]Weiberg & Majetschak (2022) define Wittgenstein's middle period as 1929–1936, so that BGM, LFM, and the *Philosophische Untersuchungen* (PU), written between 1936–1946 (Wittgenstein, 1984a), fall in his late period. Except for the *Lectures* and the *Wiener Kreis* volume, the original notebooks and typescripts from which these published works were assembled may be found in the *Wittgenstein's Nachlass: The Bergen Electronic Edition*, available online in Open Access at `http://www.wittgensteinsource.org/` or `https://wab.uib.no/transform/wab.php?modus=opsjoner`. English translations of the published works just mentioned (with the same exceptions) may be found online (via paid library subscriptions) in the *The Collected Works of Ludwig Wittgenstein. Electronic Edition*, at `https://www.nlx.com/collections/121`. Pichler et al. (2011) is a very useful survey of all major Wittgenstein editions until 2011. Wittgenstein did not prepare BGM for publication himself; the kind of selection and polishing process that led to the PU therefore did not take place, which makes the quality uneven (Mühlhölzer, 2010; Hawkins & Potter, 2022). Recent secondary literature includes e.g. Mühlhölzer (2006, 2008, 2010, 2012), Floyd (2021), and Schroeder (2021), with useful summaries by Gerrard (1996), Potter (2011), Rodych (2018), and Bangu (undated).

[19]We side with Gerrard (1991) that a good way to understand at least the start of Wittgenstein's philosophy of mathematics is to put his critique of the Augustinian conception of language at the beginning of the *Philosophical Investigation* (cf. Baker & Hacker, 2009a, Essay I) next to his critique of what Gerrard calls the Hardyian Picture of mathematics: 'Wittgenstein, in fact, had two chief post-Tractatus accounts of mathematics. I have labelled these the calculus conception and the language- game conception. The calculus conception dominated Wittgenstein's thought from 1929 through the early 1930s, although in some areas (such as contradiction) its influence lasted longer. In the middle 1930s, his views began to change to the language-game conception, and by the early 1940s, the view of mathematical language as a nexus of language-games had completely overturned the calculus view. In the transitional (calculus) period Wittgenstein saw mathematics as a closed, self-contained system. The rules (construed extremely narrowly) alone determine meaning, and thus become the final and only court of appeal. In the more mature (language-game) period, meaning and truth can be accounted for only in the context of a practice, and mathematics is examined by seeing what special role it plays in our lives and its special relationship to other language-games. But not everything changed; throughout all the stages of Wittgenstein's work on the philosophy of mathematics, he remained opposed to and tried to undermine what he considered to be a misleading picture of the nature of mathematics. According to this opposing picture, mathematics is somehow transcendental: a mathematical proposition has truth and meaning regardless of human rules or use. According to this picture there is an underlying mathematical reality which is independent of our mathematical practice and language and which adjudicates the correctness of that practice and language. This plays a similar (negative) role for Wittgenstein's philosophy of mathematics as does the Augustinian Picture for his later philosophy of language. For reasons given in the next section, I call this the "Hardyian Picture" after the mathematician G. H. Hardy. The Hardyian Picture helps to give a structure to Wittgenstein's work on the philosophy of mathematics and to unite seemingly disparate discussions. Regardless of what else he was doing, Wittgenstein always kept this picture in mind and tried to distance himself from its temptations and confusions.' (Gerrard, 1991, p. 127).

> We have here a thing which constantly happens. The words in our language have all sorts of uses; some very ordinary uses which come into one's mind immediately, and then again they have uses that are more and more remote. For instance, if I say the word 'picture', you would think first and foremost of something drawn and painted and, say, hung up on the wall. You would not think of Mercator's projection of the globe; still less of the sense in which a man's handwriting is a picture of his character. A word has one or more nuclei of uses which come into every body's mind first; so that if one says so-and-so is also a picture—a map or *Darstellung* in mathematics—in this lies a comparison, as it were, "Look at this as a continuation of that." So if you forget where the expression "a reality corresponds to" is really at home— What is "reality"? We think of "reality" of something we can *point* to. It is *this, that*. Professor Hardy is comparing mathematical propositions to propositions of physics. This comparison is extremely misleading. (LFM = Diamond, 1975, pp. 239–240)

Indeed, apart from Plato, Frege, and Russell, Hardy was one of Wittgenstein's favorite targets.[20]

On the positive side, in his middle period (notably in the *Blue and Brown Books* from 1933–1935) Wittgenstein introduced *language games* as a central tool of his analysis of language, which he generalized later on in the *Philosophical Investigations*. Although he refrains from giving a definition—and would regard any such definition as misguided, since games, languages, and language games are among his main examples of a *family resemblance*, cf. §A.3 below—we try:

1. A language game is a *practice* where certain words and symbols are *used* (operationally);[21]

2. This *use* is determined by specific *rules* (forming the *grammar* of the language game);[22]

3. The *meaning* of (most) words and symbols is in turn determined by their *use*.

This inverts the traditional ('Augustinian') view in which the meaning of words is determined by the external objects that they are supposed to refer to (PI, §1). The example in §2 of the PI then shows that such a reference, even if it exists, is insufficient to determine the use of words.[23]

[20] Wittgenstein's opposition to Frege and Hardy aligns with his objections to Plato (Kienzler, 1997, 2013). He commented on the dialogues *Sophist, Theaetetus, Charmides, Philebus*, and *Cratylus* (and undoubtedly read others). The first four are, roughly speaking, searches for the definition of sophistry, knowledge, temperance, and pleasure, whilst the last is a quest for the nature of names and signs, asking questions about conventions and reference very similar to those analyzed about 2300 years later by Frege, Russell, and Wittgenstein. In Plato (as well as Aristotle), defining is seen as a search for essence, although ironically, almost all such attempts fail. Wittgenstein's concept of a *family resemblance* (see also §A.3) may even be seen as his answer to the Socratic quest for definitions through essence. Wittgenstein is often negative about Plato ('Wenn man die Sokratischen Dialoge liest, so hat man das Gefühl: welche fürchterliche Zeitvergeudung! Wozu diese Argumente die nichts beweisen & nichts klären.' Ms-120,25r[2]), particularly about his efforts to *transcend* language; what Plato sought in the lofty realm of forms, Wittgenstein simply found on earth in the use of language, obviating the need for Plato's ethereal reifications (Kuusela, 2019b). Nonetheless, it seems that Wittgenstein never *denied* the existence of some external reality, even a mathematical one (Gerrard, 1991; Dawson, 2014). His point was that the correctness of a mathematical proposition (or some other linguistic utterance) is not established by comparing it with such a reality (if it exists), but with some linguistic practice (such as a proof). But in the direction of philosophical therapy he could have argued that a lack of real mathematical objects also relieves us of the obligation to explain what these exactly would be and how we could possibly access them.

[21] Floyd (2015) makes the following point: 'In English we lack the German word "*Praxis*" (...). "Practice" is a poor substitute, because it carries a connotation of something contingent and coordinative, a convention, or a mere matter of "what we choose to do"—which is of course vague until spelled out, and impossible to describe without theorizing in some way or other, for every description explains to some extent, and every explanation describes to some extent. (...) But in German *Praxis* forms part of what any theory must itself manage to theorize and incorporate, critically.'

[22] Kuusela (2019a), Chapter 6, holds that language games *need* not be based on rules; but those in mathematics are.

[23] In §2 of the *Philosophical Investigations*, an assistant *A* has to pass building-stones to a builder *B* on a construction site. For this purpose they use a language consisting of the four words 'block', 'pillar', 'slab', and 'beam. After *B* calls them out, *A* brings the stones, 'as he has learnt'. Here the use lies in the *action* involved.

Wittgenstein's examples also suggest that there are natural as well as invented language games, and that language games may be open-ended or "alive": e.g., new words, symbols, as well as rules may be added, and old ones changed. Furthermore, the *existence* of a rule does not automatically *impose* it: both the rules and the habit of following them form part of the language game in question and hence are matters of (in our case: mathematical) "practice". This may require strict adherence to the rule, or some leeway, see §7 for the case of proofs as an example of rule following.[24]

We will use language games in mathematics as indicated by Wittgenstein for languages:[25]

> §130. Unsere klaren und einfachen Sprachspiele sind nicht Vorstudien zu einer künftigen Reglementierung der Sprache, gleichsam erste Annäherungen, ohne Berücksichtigung der Reibung und des Luftwiderstands. Vielmehr stehen die Sprachspiele da als *Vergleichsobjekte*, die durch Ähnlichkeit und Unähnlichkeit ein Licht in die Verhältnisse unsrer Sprache werfen sollen.[26] (*Philosophische Untersuchungen*)
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> §131. Nur so nämlich können wir der Ungerechtigkeit, oder Leere unserer Behauptungen entgehen, indem wir das Vorbild als das, was es ist, als Vergleichsobjekt sozusagen als Maßstab hinstellen; und nicht als Vorurteil, dem die Wirklichkeit entsprechen *müsse*. (Der Dogmatismus, in den wir beim Philosophieren so leicht verfallen.)[27] (*idem*)

Thus language games—or at least the 'clear and simple' ones—are tools of *examination* rather than tools of *description* (or even of *representation*): they provide benchmarks or yardsticks with which some linguistic practice can be *compared*. The result of such a comparison could be a perfect match, but it is equally interesting to find out where the differences lie (Kuusela, 2019a).[28]

Throughout his career, Wittgenstein claimed that philosophical problems originate in misunderstandings of the logic of language. In the *Philosophical Investigations*, such misunderstandings are traced to the confusion that arises if different language games are mixed up, in the sense that rules determining the use (and hence the meaning) of some linguistic structure (like a word, or sentence, or grammatical form) in one language game are mistakenly used in a different one:

> seltsam erscheint der Satz nur, wenn man sich zu ihm ein anderes Sprachspiel vorstellt als das, worin wir ihn tatsächlich verwenden. (*Philosophische Untersuchungen*, §195).[29]

Phrased during his work on the *Philosophical Investigations*, Wittgenstein's comments on Hardy (quoted above) follow this pattern. More briefly, the following pair of sentences misled e.g. Frege:

"The Earth is very old" "17 is a prime number".

These have the same structure, which suggests that the singular terms "Earth" and "17" both refer to objects that really exist. But this is based on a confusion between different language games.[30]

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[24] In §185 of the PI the interlocutor asks a pupil to continue a series $2, 4, 6, 8, \ldots$. Surprisingly, as soon as the pupil has reached 1000, (s)he continues with $1000, 1004, 1008, 1012$, claiming this is correct. The literature on this is enormous: see e.g. Baker & Hacker (2009c), pp. 116–134 in general, and Mühlhölzer (2010), §§I.5, I.6, for mathematics.

[25] See Kuusela (2019a), Chapter 5, for a detailed exegesis of these paragraphs, which we endorse.

[26] 'Our clear and simple language-games are not preliminary studies for a future regimentation of language a 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.'

[27] 'For we can avoid unfairness or vacuity in our assertions only by presenting the model as what it is, as an object of comparison a as a sort of yardstick; not as a preconception to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.)'

[28] What Hempel wrote about Carnap's notion of *explication* also applies to such comparisons, namely that the situation is not black and white: 'Thus understood, an explication cannot be qualified simply as true or false; but it may be adjudged more or less adequate according to the extent to which it attains its objectives.' (Hempel 1952, p. 12)

[29] 'the sentence seems odd only only when one imagines it to belong to a different language-game from the one in which we actually use it.'

[30] See e.g. Baker & Hacker (2009a), especially §I.4, and Mühlhölzer, 2010, especially §I.4.

Another mistake is to confuse properties of some language game used as a tool of examination (i.e. comparison) with properties of the things that are examined (Kuusela, 2019a, §4.6). This is a key mistake of the platonists: the language of mathematics (or, for Wittgenstein, of logic) is *abstract*, in the sense of acausal, aspatial, and atemproal. The things for which mathematics is a tool of examination, such as (actual) circles or triangles or numbers, are not: any of those is present somewhere in space and time, and may be causally effective. And yet we can apply mathematics to them: in §5 we describe in detail how this is done without postulating a platonic realm.

3 Mathematics as a motley of language games

Although Wittgenstein is sometimes taken to have proposed that a (natural) language is a "motley of language games", such a claim is difficult to find in his published works or in his *Nachlass*, and indeed would be at odds with §§130–131 of the PU just quoted. What he did say was:

> Die Mathematik ist ein BUNTES *Gemisch* von Beweistechniken. — Und darauf beruht ihre mannigfache Anwendbarkeit und ihre Wichtigkeit.[31] (BGM, §III.46)

But this limited view of mathematics seems insufficient: apart from proofs, it involves a lot more:

1. *A long history:* from numerical tables in Mesopotamia almost 4000 years ago to the rigorous concept of a function in the 19th and 20th centuries; from quantitative methods of surveying to Riemannian geometry; from counting to class field theory, *et cetera*. It has thereby led to:

2. A number of different *formal foundations of mathematics*, like ZF or ZFC or BNG set theory, intuitionistic set theory, λ-calculus, topos theory, homotopy type theory, *et cetera*.

3. Associated *notions of proof* ranging from the informal reasoning of ancient Babylonian and Chinese mathematicians to the pseudo-axiomatic setting of Euclid (which lacked explicit rules of deduction) to the advanced logical apparatus of Frege, Russell, Hilbert, and Gödel. But even the logic differs not only between the formal foundational systems just mentioned (and others), but also includes considerable diversity in what is being tolerated within each of them, from informal rigour to bending the rules. See also Wittgenstein's quote above.

4. Within each of these foundational systems: a wide collection of mathematical theories (also called areas, branches, disciplines, or fields),[32] each with its own (sub) community, goals, and standards of proof. These areas typically also overlap (e.g. Lie groups combine group theory and differential geometry; functional analysis combines linear algebra and topology, etc.). Following Hilbert (see below) we find it hard to maintain the traditional distinction between "pure" and "applied" mathematics (although many mathematics departments do).

5. The meta-theory of the axiomatized theories (i.e. Frege's "theory of the game"), including *both* formal aspects like proof theory *and* informal aspects like "the strategy of the game".[33]

It would therefore be better to answer our title question '*Is mathematics a game?*' by:

[31] 'Mathematics is a MOTLEY of techniques of proof. — And upon this is based its manifold applicability and its importance.' A motley is the traditional costume of the court jester or fool, which seems well in the spirit of Wittgenstein! See, however, Mühlhölzer (2005), p. 66, footnote 15, for a critique of this translation.

[32] See the *Mathematics Subject Classification* (MSC) at `https://mathscinet.ams.org/mathscinet/msc/msc2020.html`, or the 'Branches of Mathematics' listed in Gowers (2008). To get an idea, under the letter *a* the latter sums up: 'algebraic numbers; analytic number theory; algebraic geometry; arithmetic geometry; algebraic topology'.

[33] Developing the formals aspect of thist, i.e., metamathematics, was of course Hilbert's achievement.

Mathematics is a motley of language games of a very specific (formal) kind.

This remains well within the spirit of Wittgenstein,[34] especially if, encouraged by §§130–131 of the PU just quoted, it is taken as a proposal to merely *compare* mathematics with a motley of language games, seen as a yardstick which in part describes mathematics well, and in part may not (although the latter is hard to find). But the point, of course, is to flesh this out by explaining what is meant by "mathematics" and showing what actually makes it a "motley of language games".

To start, let us take some piece of formalized mathematics, such as ZFC set theory at the top level,[35] or some formalized theory within it (or taken separately), such as Peano arithmetic or (Hilbert-style) Euclidean geometry, including its (logical) rules of inference (e.g. first-order logic). We then favour the specific analogy between chess and mathematics proposed by Weyl (1926):[36]

- The *axioms* of some theory are analogous to the starting position of a game of chess;
- The *deduction rules* (à la Natural Deduction) are analogous to the possible moves;[37]
- 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;
- A *proof* is like a game leading to that position, played according to the rules;
- A *definition* resembles the idea that chess pieces are defined by the rules of chess.

The last point was not mentioned by Weyl (1926) and should be attributed to Wittgenstein:

> Es ist übrigens sehr wichtig, daß ich den Holzklötzchen auch nicht ansehen kann, ob sie Bauer, Läufer, Turm, etc. sind. Ich kann nicht sagen: Das ist ein Bauer und für diese Figur gelten die und die Spielregeln. Sondern die Spielregeln bestimmen erst diese Figur: Der Bauer ist die Summe der Regeln, nach welchen er bewegt wird (auch das Feld ist eine Figur), so wie in der Sprache die Regeln der Syntax das Logische im Wort bestimmen.
> (Wittgenstein, 1984b, p. 134).[38]

This was part of Wittgenstein's general criticism of Frege's arguments against the chess analogy:

> *Frege* hat sich mit Recht gegen die Auffassung gewendet, daß die Zahlen der Arithmetik die Zeichen sind. Das Zeichen "0" hat doch nicht die Eiegnschaft, zu dem Zeichen "1" das Zeichen "1" zu ergeben. In dieser Kritik hatte Frege recht. Nur hat er nich das andere gesehen, was am Formalismus berechtigt ist, daß die Symbole der Mathematik nicht die Zeichen sind,

[34] See e.g. Baker & Hacker (2009c), Essay I: Two fruits upon one tree, and Mühlhölzer (2010), §§I.6, II.8.

[35] Omitting the axiom of choice would be like omitting e.g. castling from the rules of chess.

[36] This works both at a purely logical level and in interpretations of syntactic theories in set theory (cf. footnote 56). What is admittedly missing here is a translation of the goal of *winning* in chess: there seems to be no analogue of checkmate in mathematics (although there is an emotional analogue of resigning, i.e., "giving up".). Indeed, in the latter the goal is to establish the counterpart not of a winning position but of an arbitrary legal position (i.e. a theorem). Perhaps the shared aspect of beauty in both games of chess and proofs somewhat compensates for this discrepancy.

[37] In a Hilbert-style calculus (Hilbert & Ackermann, 1928), most deduction rules are seen as axioms, *modus ponens* being the only deduction rule. What we here have in mind is the opposite: the axioms are supposed to describe some specific mathematical theory (such as set theory, or arithmetic, or Euclidean geometry) whereas all deduction rules are logical in character and, with a few exceptions, are universal for all fields of mathematics. See e.g. von Plato (2017).

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

> aber doch keine Bedeutung haben. Für Frege stand die Alternative so: Entweder wir haben es mit den Tintenstrichen auf dem Papier zu tun, oder diese Tintenstriche sind Zeichen *von etwas*, und das, was sie vertreten, ist ihre Bedeutung. Daß diese Alternative nicht richtig ist, zeigt gerade das Schachspiel: Hier haben wir es nicht mit den Holzfiguren zu tun, und dennoch vertreten die Figuren nichts, sie haben in Freges Sinn keine Bedeutung. Es gibt eben noch etwas drittes: die Zeichen können verwendet werden wie im Spiel. Wenn man hier (beim Schachspiel) von 'Bedeutung' reden wollte, so wäre es am natürlichsten zu sagen: Die Bedeutung des Schachspiels ist das, was alle Schachspiele gemeinsam haben.[39]
>
> (Wittgenstein, 1984b, p. 105)

> Frege ridiculed the formalist conception of mathematics by saying that the formalists confused the unimportant thing, the sign, with the important, the meaning. Surely, one wishes to say, mathematics does not treat of dashes on a bit of paper. Frege's idea could be expressed thus: the propositions of mathematics, if they were just complexes of dashes, would be dead and utterly uninteresting, whereas they obviously have a kind of life. And the same, of course, could be said of any proposition: Without a sense, or without the thought, a proposition would be an utterly dead and trivial thing. And further it seems clear that no adding of inorganic signs can make the proposition live. And the conclusion which one draws from this is that what must be added to the dead signs in order to make a live proposition is something immaterial, with properties different from all mere signs.
>
> But if we had to name anything which is the life of the sign, we should have to say that it was its *use*. (*Blue Book*, p. 4).

In other words, Frege (allegedly) just saw two possibilities: either symbols refer to something in reality, in which case the game is meaningful (which, in his view, doesn't apply to chess since it lacks "thoughts", whose alleged absence blocks any analogy with mathematics), or they don't (which for Frege applies to chess but not to mathematics), in which case the game is meaningless. Wittgenstein's point is that Frege overlooked the possibility that even *a priori* meaningless symbols might "come alive" by their use, as governed by the rules they are subject to:[40]

- For Frege, the use of symbols, i.e., the rules they are subject to, follows from their meaning. This seems almost reactionary, given the "post-modernist revolution" in mathematics.
- For Wittgenstein, on the other hand, the rules are primary and whatever meaning the symbols have follows from these rules. This resonates well with Hilbert, to whom we now turn.

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[39] '*Frege* was right in objecting to the conception that the numbers of arithmetic are signs. The sign "0" , after all, does not have the property of yielding the sign "l" when it is added to the sign "I" . Frege was right in this criticism. Only he did not see the other, justified side of formalism, that the symbols of mathematics, although they are not signs, lack a meaning. For Frege the alternative was this: either we deal with strokes of ink on paper or these strokes of ink are signs of something and their meaning is what they go proxy for. The game of chess itself shows that these alternatives are wrongly conceived—although it is not the wooden chessmen we are dealing with, these figures do not go proxy for anything, they have no meaning in Frege's sense. There is still a third possibility: the signs can be used the way they are in the game. If here (in chess) you wanted to talk of "meaning" , the most natural thing to say would be that the meaning of chess is what all games of chess have in common.'

[40] As noted by Kienzler (1997), §4a, Wittgenstein overlooks or ignores the fact that Frege (1903) does note this 'other possibility', for in footnote 1 on page 83 (which is part of §71) he says: 'Es besteht freilich auch eine Meinung, nach der die Zahlen weder Zeichen sind, die etwas bedeuten, noch auch unsinnliche Bedeutungen solcher Zeichen, sondern Figuren, die nach gewissen Regeln gehandhabt werden, etwa wie Schachfiguren. Danach sind die Zahlen weder Hülfsmittel der Forschung noch Gegenstände der Betrachtung, sondern Gegenstände der Handhabung. Das werd später zu prüfen sein.' ('Of course, there is also an opinion according to which numbers are neither symbols that mean something nor nonsensical meanings of such symbols, but rather figures that are handled according to certain rules, for example like chess pieces. According to this, the numbers are neither aids for research nor objects of observation, but rather objects of handling. This will have to be checked later.') Indeed, in §95 Frege (1903) complains that the rules of chess do not endow the chess pieces with any *content* that would be the consequence of these rules, 'like the name "Sirius" designates a certain fixed star.' This suggests a stubborn inability to see that the rules themselves comprise the meaning of chess (even though the pieces are meaningless); which of course was Wittgenstein's view.

4 Hilbert

Essentially the same idea lies behind Hilbert's concept of an *implicit definition*,[41] which comes from Hilbert (1899) but is most clearly explained in his fascinating correspondence with Frege:[42]

> Meine Meinung ist eben die, dass ein Begriff nur durch seine Beziehungen zu anderen Begriffen logisch festgelegt werden kann. Diese Beziehungen, in bestimmten Aussagen formulirt, nenne ich Axiome und komme so dazu, dass die Axiome (ev[tl]. mit Hinzunahme der Namengebungen für die Begriffe) die Definitionen der Begriffe sind. Diese Auffassung habe ich mir nicht etwa zur Kurzweil ausgedacht, sondern ich sah mich zu derselben gedrängt durch die Forderung der Strenge beim logischen Schliessen und beim logischen Aufbau einer Theorie. Ich bin zu der Überzeugung gekommen, dass man in der Mathematik und den Naturwissenschaften subtilere Dinge nur so mit Sicherheit behandeln kann, anderenfalls sich bloss im Kreise dreht. (Hilbert to Frege, 22 September 1900; Gabriel *et al,*, 1980, p. 23).[43]

Thus Hilbert's idea is that some axiom system—like the one given by Hilbert (1899) himself for Euclidean geometry—*defines* all non-logical things that occur in this system (via certain arbitrary symbols).[44] As illustrated by a famous earlier excerpt from the Frege–Hilbert correspondence, this view was diametrically opposite to Frege's, who maintained that the things in axioms should *already* be defined in advance, so that the (compelling) axioms simply record their properties.

> Sie sagen weiter: "Ganz anders sind wohl die Erklärungen in §1, wo die Bedeutungen Punkt, Gerade, ... nich angegeben, sonders als bekannt vorausgesetzt werden." Hier liegt wohl der Cardinalpunkt des Missverständnisses. Ich will nichts als bekannt voraussetzen; ich sehe in meiner Erklärung in §I die Definition der Begriffe Punkte, Gerade, Ebenen, wenn man wieder die sämtlichen Axiome der Axiomgruppen I–V als die Merkmale hinzunimmt. Wenn man nach andern Definitionen für "Punkt", etwa durch Umschreibungen wie ausdehnungslos etc. sucht, so muss ich solchem Beginnen allerdings aufs entschiedenste widersprechen; man sucht da etwas, was man nie finden kann, weil nichts da ist, und alles verliert sich und wird wirr und vage und artet in Versteckspiel aus. (...) Sie sagen meine Begriffe z.B. "Punkt", "zwischen" seien nicht eindeutig festgelegt; z.B. [auf] S. 20 sei "zwischen" anders gefasst und dort sei der Punkt ein Zahlenpaar. Ja, es ist doch selbstverständlich eine jede Theorie nur ein Fachwerk oder Schema von Begriffen nebst ihren nothwendigen Beziehungen zu einander, und die

[41] Giovannini & Schiemer (2021) call these *structural*, since the words 'implicit' and 'explicit' are adjectives for certain technical definitions in logic (which Beth's definability theorem actually identifies). See Hodges (1993), §6.6.

[42] The Frege–Hilbert correspondence lasted from 1895 to 1903, centered around Hilbert (1899). See for example Gabriel *et al.* (1980), Shapiro (2005), Hallett (2010), Burke (2015), Blanchette (2018), and Rohr (2023).

[43] '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.'

[44] Hilbert held this view at least since 1891, see Blumenthal (1970), pp. 402–403: 'In einem Berliner Wartesaal [in 1891] diskutierte er mit zwei Geometrn (wenn ich nicht irre, A. Schoenflies und E. Kötter) über die Axiomatik der Geometrie und gab seiner Auffassung das ihm eigentümlich scharfe Gepräge durch den Ausspruch: 'Man muß jederzeit an Stelle von "Punkte, Geraden, Ebenen" "Tische, Stühle, Bierseidel" sagen können.' Seine Einstellung, daß das anschauliche Substrat der geometrische Begriffe mathematisch belanglos sei und nur ihre Verknüpfung durch die Axiome in Betracht komme, war also damals bereits fertig.' 'In a Berlin waiting room [in 1891] he discussed the axioms of geometry with two geometers (if I am not mistaken, A. Schoenflies and E. Kötter) and gave his opinion a sharp thrust typical for him through the statement: 'One must be able to say "tables, chairs, beer mugs" instead of "points, lines, planes" at any time.' His attitude that the intuitive substrate of the geometric concepts was mathematically irrelevant and that only their connection through the axioms should be taken into account was already present at that time.'

> Grundelemente können in beliebiger Weise gedacht werden. Wenn ich unter meinen Punkten irgendwelche Systeme von Dingen, z.B. das System: Liebe, Gesetz, Schornsteinfeger ..., denke und dann nur meine sämmtlichen Axiome als Beziehungen zwischen diesen Dingen annehme, so gelten meine Sätze, z.B. der Pythagoras auch von diesen Dingen.[45]
>
> (Hilbert to Frege, 29 December, 1899)

The idea is also clearly stated by von Neumann (1925), p. 220:[46]

> Man wendet, um diesen [naiven] Begriff [der Menge] zu ersetzen, die axiomatische Methode an; d. h. man konstruiert eine Reihe von Postulaten, in denen das Wort "Menge" zwar vorkommt, aber ohne jede Bedeutung. Unter "Menge" wird hier (im Sinne der axiomatischen Methode) nur ein Ding verstanden, von dem man nicht mehr weiß und nicht mehr wissen will, als aus den Postulaten über es folgt. Die Postulate sind so zu formulieren, daß aus ihnen alle erwünschten Sätze der Cantorschen Mengenlehre folgen, die Antinomien aber nicht.[47]

Unnecessarily conservatively, Hilbert did initially interpret 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.[48] In compensation, it was one of Hilbert's deepest conceptual insights that:

Both the rigour and the applicability of mathematics originate in axiomatization.

> Der Mathematik kommt hierbei eine zweifache Aufgabe zu: Einerseits gilt es, die Systeme von Relationen zu entwickeln und auf ihre logischen Konsequenzen zu untersuchen, wie dies ja in den rein mathematischen Disziplinen geschieht. Dies ist die *progressive Aufgabe* der Mathematik. Andererseits kommt es darauf an, den an Hand der Erfahrung gebildeten Theorien ein festeres Gefüge und eine möglichst einfache Grundlage zu geben. Hierzu ist es nötig, die Voraussetzungen deutlich herauszuarbeiten, und überall genau zu unterscheiden, was Annahme und was logische Folgerung ist. Dadurch gewinnt man insbesondere auch Klarheit über alle unbewußt gemachten Voraussetzungen, und man erkennt die Tragweite der verschiedenen Annahmen, so daß man übersehen kann, was für Modifikationen sich ergeben,

[45] 'You 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. (...) You say my concepts, e.g., "point", "between", are not unambiguously determined; e.g., on p. 20, "between" is taken differently and there a point is a number-pair. – Yes, it is obvious that any theory is actually only a framework or a schema of concepts, together with the necessary relations of these concepts to each other, and the base elements can be thought of in an arbitrary way. If, as my points, I think of some system of things, e.g., the system: love, law, chimney sweep ... and then assume my axioms as relations between these things, then my theorems, too, hold of these things, e.g., the Pythagorean Theorem.' See Gabriel et al., 1980, pp. 12, 13, and Hallett (2010), pp. 453–454.

[46] It is no accident that von Neumann went to Göttingen to work with Hilbert in 1926, soon after this paper.

[47] '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.' See also Muller (2004) and Schlimm (2013).

[48] See Giovannini & Schiemer (2021), §4.2, and references therein, as well as von Plato (2017). This idea is especially clear in Gentzen's own proof system called *Natural Deduction*, where each logical symbol has an introduction rule and an elimination rule. For example, the elimination rule for the implication sign $\rightarrow$ is *modus ponens* (i.e., A and $A \rightarrow B$ imply B), whilst its introduction rule states that $A \rightarrow B$ follows if from A *as a hypothesis* one can deduce B.

falls eine oder die andere von diesen Annahmen aufgehoben werden muß. Dies ist die *regressieve Aufgabe* der Mathematik.[49] (Hilbert, 1992, pp. 17–18).

By axiomatization,[50] Hilbert meant the identification of certain sentences (becoming axioms) that form the foundation of a specific field in the sense that its theoretical structure (*Fachwerk*) can be (re)constructed from the axioms via logical principles. The epistemological status of the axioms differs between fields. For example, Hilbert considered geometry initially a natural science that emerged from the observation of nature (i.e. experience), which then turned into a mathematical science through axiomatization (Corry, 2004, p. 90). This does not mean that he treated the axioms of geometry as "true" (as Euclid had done): Hilbert often stressed the tentative and malleable nature of axiom systems—just look at the seven editions of *Grundlagen der Geometrie*!

> Wie man aus dem bisher Gesagten ersieht, wird in den physikalischen Theorien die Beseitigung sich einstellender Widersprüehe stets durch veränderte Wahl der Aziome erfolgen müssen und die Schwierigkeit besteht darin, die Auswahl so zu treffen, daß alle beobachteten physikalischen Gesetze logische Folgen der ausgewällten Axiome sind.[51]
> (Hilbert, 1918, p. 411)

Or, as historian Majer powerfully summarized Hilbert's view on the axiomatization of physics:

> physical theories live, as it were, on the border of inconsistency (Majer, 2014, p. 72)

For Hilbert, the axiomatization of physical theories is therefore never a static process: it moves on as physics itself moves on (Corry, 2004; Majer, 2014). Axiomatization may lead to the exposure of contradictions via a purely logical analysis, whose removal is then an important step forward.

Axiomatization, then, contributes in two very different ways to the *rigour* of mathematics:

1. via syntactic proofs from the axioms (whose symbols remains uninterpreted);
2. via the axiomatization of sufficiently mature informal theories of mathematics.[52]

[49] 'Mathematics has a two-fold task here: On the one hand, it is necessary to develop the systems of relations and examine their logical consequences, as happens in purely mathematical disciplines. This is the *progressive task* of mathematics. On the other hand, it is important to give the theories formed on the basis of experience a firmer structure and a basis that is as simple as possible. For this it is necessary to clearly work out the prerequisites and to differentiate exactly what is an assumption and what is a logical conclusion. In this way, one gains clarity about all unconsciously made assumptions, and one recognizes the significance of the various assumptions, so that one can overlook what modifications will arise if one or the other of these assumptions has to be eliminated. This is the *regressive task* of mathematics.'

[50] Primary sources for Hilbert's program of axiomatization include Sauer & Majer (2009, 2024), and Hilbert (1900, 1918). For secondary literature see especially Corry (2004, 2018), Majer (2001, 2006, 2014) and Majer & Sauer (2014). Unfortunately, the power of this program for the philosophy of mathematics got somewhat lost because of what is generally perceived to be the failure of "Hilbert's program" (i.e. of proving the consistency of classical mathematics using finitistic methods). Here, important primary sources are Ewald & Sieg (2013), Hilbert (1926), and Hilbert & Bernays (1934, 1939). See also Volkert (2015), Hallett & Majer (2004), and Ewald et al. (2012) for Hilbert's early formalism, which predated his "program" by about two decades. The large secondary literature on "Hilbert's program" includes Detlefsen (1986), Franks (2009), Sieg (2013), Tapp (2013), and Zach (2023), and references therein. Proof theory (von Plato, 2018; Rathjen & Sieg, 2022) is still seen as a positive outcome of this program, but its interest seems limited to logic. We emphasize the relevance of Hilbert's program (without scare quotes!) of axiomatization for *applied mathematics*, which—following Hilbert—we take to include the "quantitative" and "spatial" sciences altogether.

[51] 'As can be seen from what has been said so far, in physical theories the elimination of contradictions that arise will always have to be done by changing the choice of axioms and the difficulty lies in making the selection in such a way that all observed physical laws are logical consequences of the selected axioms.'

[52] Even in mathematics itself Hilbert grudgingly had to acknowledge the appearance of contradictions as a historical phenomenon. But unlike physics, he apparently found contradictions unacceptable in mathematics, give his obsession of

Similarly, axiomatization is also the key to the *applicability* of mathematics, namely:

3. via the axiomatization of sufficiently mature theories of physics, space, quantity, etc.

In fact, it seems neither possible nor necessary to sharply distinguish between the second and third activities: for example, are Euclid's axioms (more precisely: his so-called postulates and common notions–whatever their clarity and worth from a modern point of view) attempts to axiomatize earlier informal geometry, or some physical theory of space? Or, for a more recent example, did the axiomatization of number theory by Dedekind, Peano, and others in the late 19th century serve to make earlier informal mathematics rigorous (at least by the standards of the time) or did it formalize non-mathematical theories of quantity? Even the axiomatization of set theory in the early twentieth century tried to bring rigour into both the informal set theories of Riemann, Dedekind, and Cantor, and the genuine efforts by Frege, Russell, and others to understand sets as ingredients of the physical universe or at least the human mind (Ferreirós, 2008); see also §A.3.

The following excellent paragraphs by Wittgenstein,[53] originally meant to counter one of Frege's objections to the analogy between mathematics and chess, as well as a similar point raised by Hardy (who as we saw in part endorsed the said analogy) confirm the artificiality of any sharp distinction between nos. 2 and 3, and hence between pure and applied mathematics:

> Wenn es Menschen auf dem Mars gäbe und sie so Krieg miteinander führten wie die Figuren auf dem Schachfeld, dann würde der Generalstab die Regeln des Schachspiels zum Prophezeien benutzen. Es wäre dann eine wissenschaftliche Frage, ob sich der König bei einer bestimmten Spielkonstellation matt setzen läßt, ob er sich in drie Zügen matt setzen läßt und so weiter.[54] (Wittgenstein, 1984b, p. 104)

> Wenn die menschliche Kriegsführung dem Schachspiel ähnlicher wäre als sie tatsächlich ist so könnte man versuchen eine Schlacht auf dem Schachbrett darzustellen und mathematische Probleme die die Möglichkeiten der Schlacht betreffen auf dem Schachbrett zu lösen. Freilich nur mathematische Probleme, denn Experimente über den Vorgang der Schlacht könnte man mit den Schachfiguren nicht vornehmen da sie sich anders verhalten als die Menschen. Wenn also das Problem gelöst würde etwa von einer bestimmten Position ausgehend den Anderen in N Zügen matt zu setzen, so wäre das die Lösung eines mathematischen Problems des Krieges.[55] (Wittgenstein, MS 108: 162 f.)

proving the consistency of classical mathematics. This marks, incidentally, a major difference with Wittgenstein, whose cheerful acceptance of inconsistent theories, his repeated comments on the indeterminateness of decimal expansions (e.g. of π), his relaxed attitude towards the possibility of rejecting a correct proof, and his insisting that proofs change the nature of what was proved, sound out of touch with modern mathematics and hence inappropriate for our program.

[53] For comparisons of Hilbert and Wittgenstein see Muller (2004), Mühlhölzer (2006, 2008, 2010, 2012) and Friederich (2011, 2014). There is no evidence that Hilbert read the *Tractatus*. Wittgenstein's middle and late work came too late for Hilbert, but it is unlikely that he would have been moved by it. Conversely, even in the 1930s Wittgenstein's mathematical education and reflections still relied on Frege and Russell. Mühlhölzer (2010), p. 10 writes that 'Auch in BGM III wird hin und wieder ein gewisser Mangel an relevantem mathematischen Wissen deutlich, und W.s weitgehende Konzentration auf die Schriften Freges und Russells verleiht seinen Überlegungen manchmal etwas Antiquirtes.' Nonetheless, Wittgenstein was well aware of Hilbert and his program (Mühlhölzer, 2006, 2008, 2010; Floyd, 2023). For example, BGM III, §81 reflects on Hilbert (1922a) (without citation, as always). But Wittgenstein regarded Hilbert's aim of proving the consistency of classical mathematics as 'completely vapid' (Shanker, 1987, Chapter 6).

[54] Translation: If there were people on Mars and they waged war with each other like the pieces on a chess board, then the General Staff would use the rules of chess to prophesy. It would then be a scientific question whether the king can be checkmated in a certain game constellation, whether he can be checkmated in three moves, and so on.

[55] See `https://wab.uib.no/transform/wab.php?modus=opsjoner`.). Quoted by Max (2020), p. 198. Translation: If human warfare were more similar to chess than it actually is, one could try to represent a battle on the chessboard and solve mathematical problems relating to the possibilities of the battle on the chessboard. Of course, only mathematical problems, because you couldn't carry out experiments on the battle process with the chess pieces because they behave differently from people. So if the problem were solved, for example starting from a certain position, to checkmate the other person in N moves, then that would be the solution to a mathematical problem of war.

Thus the key difference is between numbers 1 on the one hand and 2–3 on the other: the first is formal and focuses on proofs, whereas numbers 2 and 3 both take us outside (formal) mathematics.[56] Combining (allegedly) "pure" and "applied" mathematics was natural for Hilbert, who emphasized the *unity* of mathematics and the mathematical sciences throughout his career:[57]

> Die genannten Probleme sind nur Proben von Problemen; sie genügen jedoch, um uns vor Augen zu führen, wie reich, wie mannigfach und wie ausgedehnt die mathematische Wissenschaft schon heute ist und es drängt sich uns die Frage auf, ob der Mathematik einst bevorsteht, was anderen Wissenschaften längst widerfahren ist, nämlich daß sie in einzelne Teilwissenschaften zerfällt, deren Vertreter kaum noch einander verstehen und deren Zusammenhang daher immer loser wird. Ich glaube und wünsche dies nicht; die mathematische Wissenschaft ist meiner Ansicht nach ein unteilbares Ganze, ein Organismus, dessen Lebensfähigkeit durch den Zusammenhang seiner Teile bedingt wird. Denn bei aller Verschiedenheit des mathematischen Wissenstoffes im Einzelnen, gewahren wir doch sehr deutlich die Gleichheit der logischen Hülfsmittel, die Verwandtschaft der Ideenbildungen in der ganzen Mathematik und die zahlreichen Analogieen in ihren verschiedenen Wissensgebieten. Auch bemerken wir: je weiter eine mathematische Theorie ausgebildet wird, desto harmonischer und einheitlicher gestaltet sich ihr Aufbau und ungeahnte Beziehungen zwischen bisher getrennten Wissenszweigen, werden entdeckt. So kommt es, daß mit der Ausdehnung der Mathematik ihr einheitlicher Charakter nicht verloren geht, sondern desto deutlicher offenbar wird.[58] (Hilbert, 1900, pp. 296–297)

> So ordnen sich die geometrische Tatsachen zu einer Geometrie, die arithmetischen Tatsachen zu einer Zahlentheorie, die statischen, mechanischen, elektrodynamischen Tatsachen zu einer Theorie der Statik, Mechanik, Elektrodynamik oder die Tatsachen aus der Physik der Gase zu einer Gastheorie. Ebenso ist es mit den Wissensgebieten der Thermodynamik, der geometrischen Optik, der elementaren Strahlungstheorie, der Wärmeleitung oder auch mit der Wahrscheinlichkeitsrechnung und der Mengenlehre. Ja es gilt von speziellen rein mathematischen Wissensgebieten wie Fläichentheorie, Galoisscher Gleichungstheorie, Theorie der Primzahlen nicht weniger als für manche der Mathematik fern liegende Wissensgebiete wie gewisse Abschnitte der Psychophysik oder die Theorie des Geldes.[59]
> (Hilbert, 1918, pp. 405–406.

[56] We follow Tait (1986) in seeing models in set theory (or even topos theory) as internal to mathematics, and hence the distinction between syntax (as e.g. in formulating mathematical sentences in first-order logic) and their interpretation in set theory is irrelevant for our theme. See also Baldwin (2018) and Button & Walsh (2018).

[57] He did so against many others, including Frege, who claimed well into the 19th century that e.g. arithmetic and geometry had a fundamentally different epistemological status.

[58] 'The problems mentioned are merely samples of problems, yet they will suffice to show how rich, how manifold and how extensive the mathematical science of today is, and the question is urged upon us whether mathematics is doomed to the fate of those other sciences that have split up into separate branches, whose representatives scarcely understand one another and whose connection becomes ever more loose. I do not believe this nor wish it. Mathematical science is in my opinion an indivisible whole, an organism whose vitality is conditioned upon the connection of its parts. For with all the variety of mathematical knowledge, we are still clearly conscious of the similarity of the logical devices, the relationship of the ideas in mathematics as a whole and the numerous analogies in its different departments. We also notice that, the farther a mathematical theory is developed, the more harmoniously and uniformly does its construction proceed, and unsuspected relations are disclosed between hitherto separate branches of the science. So it happens that, with the extension of mathematics, its organic character is not lost but only manifests itself the more clearly.'

[59] 'Thus the geometric facts are arranged into some geometry, the arithmetic facts into some number theory, the static, mechanical, electrodynamic facts into some theory of statics, mechanics, electrodynamics or the facts from the physics of gases into some gas theory. And similarly with the disciplines of thermodynamics, geometric optics, elementary radiation theory, heat conduction, or even probability calculation and set theory. Yes, it applies as much to special purely mathematical areas of knowledge such as the theory of surfaces, Galois' theory of equations, and the theory of prime numbers, as to some areas of knowledge that are remote from mathematics, such as certain sections of psychophysics or the theory of money.'

Hilbert (implicitly) plays two different language games with *the same* axioms. The cleanest one is the first in the list 1, 2, 3 above: this is his famous emphasis on the meaninglessness of mathematical symbols *in so far as proofs and other formal aspects of axiom systems are concerned* (such as consistency and completeness), cf. §A.2. The analogy between mathematics and chess is particularly clear in this language game, given the formal, deductive notion of proof shared by Frege, Russell, and Hilbert, in which (unlike in Euclid—let alone 17th and 18th century mathematics) not only the axioms but also the rules of deduction are formalized and clearly stated. This is, of course, a pristine example of rule following. Thus formalizing proofs is one of the language games played in mathematics—in which one ignores whatever meaning the symbols may have.

5 Applied mathematics

A very different language game underlies nos. 2–3 in §4. This one is sometimes played in mathematics itself (no. 2), but it is especially important in applied mathematics and mathematical physics (no. 3), or, in other words, in understanding the relationship between axiom systems and the physical world. Conceptually, this game is much more complicated than the previous one (technically, it is the other way round!). Indeed, it is not at all clear what it means to extract some mathematical structure from natural phenomena through axiomatization. According to (late) Wittgenstein,[60]

> Die Rechtfertigung des Satzes $25 \times 25 = 625$ ist natürlich daß, wer so und so abgerichtet wurde, unter normalen Umständen bei der Multiplikation $25 \times 25 = 625$ erhält. Der arithmetische Satz abes sagt nicht *dies* aus. Er ist so zu sagen ein zur Regel verhärteter Erfahrungssatz. Er bestimmt, daß der Regel nur dann gefolgt wurde, wenn dies das Resultat des Mulitplizierens ist. Er ist also der Kontrolle durch die Erfahrung entzogen, dient aber nur dazu, die Erfahrung zu beurteilen.[61] (BGM, §VI.23c, p. 325)

> Wie ist es mit dem Satz "die Winkelsumme im Dreieck ist 180°"? (...) ich werde wenn sie sich bei einer Messung nicht als 180° erweist einen Messungsfehler annehmen. Der Satz ist also ein Postulat über die Art und Weise der Beschreibung.[62] (Ts-212,XV-114-5[1])

> Mathematical truth isn't established by their all agreeing that it's true—as if they were witnesses to it. *Because* they all agree in what they do, we lay it down as a rule, and put it in the archives. (LFM, Lecture IX, p. 107)

Thus the claim that *if we give 25 apples to 25 people, then in total they hold 625 apples,* just means that the operation we carried out was really multiplication and that we didn't make a mistake. This is very different from the platonic view that $25 \times 25 = 625$ is a description of some external reality about ethereal numbers. Similarly, the theorem from Euclidean geometry stating that the sum $\alpha + \beta + \gamma$ of three angles in a triangle is 180° means the following for an actually drawn triangle: if the sum of its angles equals 180° (presumably within some error bound determined by the accuracy of the drawing and our ability to measure angles), then Euclidean geometry and in particular its concept of an angle has been used correctly to represent the situation. Thus Gauss's measurement

[60] See also Steiner (2009). Although Wittgenstein said this about mathematics in general, we only use this idea for applied mathematics and mathematical physics, where we include elementary arithmetic and geometry in the former.

[61] 'The justification of the proposition $25 \times 25 = 625$ is, naturally, that if anyone has been trained in such-and-such a way, then under normal circumstances he gets 625 as the result of multiplying 25 by 25. But the arithmetical proposition does not assert that. It is so to speak an empirical proposition hardened into a rule. It stipulates that the rule has been followed only when that is the result of the multiplication. It is thus withdrawn from being checked by experience, but now serves as a paradigm for judging experience.'

[62] '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.' Typescripts Ts- (or manuscripts Ms-) can be found online via `http://www.wittgensteinsource.org`.

of the sum of the angles in the triangle with vertices Brocken, Inselberg, and Hohenhagen (near Göttingen) between 1821–1825, resulting in 180° within his measurement accuracy (Bühler, 1981) showed that Euclidean geometry could be used to represent the local geography as appropriate.

This is in the spirit of §§130–131 of the *Philosophical Investigations* quoted earlier, with the difference that those paragraphs described the role of language games as objects of comparison for some natural language, whereas here we are talking about (say) the applied side of Euclidean geometry as a mathematical language game being applied to the physical world as a yardstick.[63]

However, Hilbert talked about axioms, whereas Wittgenstein talks about theorems (or 'propositions'). We follow Friederich (2011, 2014) in transferring Wittgenstein's interpretation of *theorems* to *axioms*, and hence in using axioms as "irreducible" yardsticks. After all, axioms are special instances of theorems, and conversely, they imply the theorems. It should therefore be enough to 'harden' certain *key* empirical regularities into the *axioms* of a physical theory.[64] As noted by Friederich, this resonates particularly well with Hilbert's implicit definitions (see §4).

Even so, neither Hilbert nor Wittgenstein explained how empirical phenomena acquire a mathematical structure, or what this structure (once in place) refers to.[65] This requires more analysis. As the most appropriate corresponding approach to the philosophy of science we suggest *constructive empiricism* (van Fraassen, 1980, 1992, 2008; Morton & Mohler, 2021). In particular:

> 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. But those theoretical models are abstract structures, even in the case of the practical sciences such as materials science, geology, and biology—let alone in the advanced forms of physics. All abstract structures are mathematical structures, in the contemporary sense of "mathematical", which is not restricted to the traditional number-oriented forms. And mathematical structures, as Weyl so emphatically pointed out, are not distinguished beyond isomorphism—to know the structure of a mathematical object is to know all there is to know. (...) Essential to an empiricist structuralism is the following core construal of the slogan that all we know is structure:
>
> 1. Science represents the empirical phenomena as embeddable in certain *abstract structures* (theoretical models).
> 2. Those abstract structures are describable only up to structural isomorphism.
>
> (...) 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 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)

Thus in this view the link between mathematical formalism and "reality" consists of three steps:

[63] Baker & Hacker (1989a), §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 propositions are *preparatory* for descriptions of empirical states of affair, instead of being *about* such states.

[64] In classical physics one may think of (partial) differential equations like those of Newton, Maxwell, or Einstein.

[65] Hilbert never got beyond the vague notion of "pre-established harmony", a philosophical concept originally going back to Leibniz. See e.g. Pyenson, (1982), Kragh (2015), and Corry (2004), pp. 393–394, who even claims that it was 'one of the most basic concepts that underlay the whole scientific enterprise in Göttingen', adding that 'Hilbert, like all his colleagues in Göttingen, was never really able to explain, in coherent philosophical terms, its meaning and the possible basis of its putative pervasiveness, except by alluding to "a miracle".'

1. A mathematical representation of natural phenomena by some ("surface") *data model*;[66]
2. A mathematical *theory* of this data model, consisting of some *abstract structure*.[67]
3. A "user" (which may be an entire team of scientists!) acting like a "middle man" between the mathematical theory and the (*a priori* non-mathematical) natural phenomena.

The realization that some mathematical theory is related to the phenomena it tries to describe via an intermediate data model constructed by some user obviates the need for chimerical philosophical constructions like platonism or some other introduction of universals. Like Newton's absolute space and time, some platonic realm housing numbers and perfect circles may be postulated, but it is unnecessary and leads to insurmountable ontological and epistemological problems (cf. §1).

In sum: Wittgenstein asks about the outcome of the multiplication 25×25 or the addition $\alpha + \beta + \gamma$ *according to the standard rules of arithmetic or Euclidean geometry, respectively*. Van Fraassen (1992), pp. 3–4, asks *what the world would be like according to some (mathematical) theory*. If it meets the "empirical regularities" the theory is accepted—which word is literally used by van Fraassen, as opposed to "believed", which a realist would use—as a valid representation of the phenomenon in question (i.e., the performance of someone doing the multiplication or the outcome of appropriate measurements on a particular landscape). If it doesn't, it is rejected.[68]

6 Truth

What does the "formalist" language game of §3 imply for the concept of *truth* in mathematics, and how would that resonate with the "applied" language game of §5? This is a difficult question, since although we acknowledge both the existence of an external world and the fact that mathematics has endless representational capacities with regard to it, we have just seen that such representations are much more subtle than naive realism would suggest. As noted before, Wittgenstein clearly realized the tension between the abstract nature of mathematics (and many other language games) and the worldly nature of the world (or of some natural language) described by it (Kuusela, 2019a). At the same time, like chess, mathematics has historically always been based on certain *man-made* rules, for which there are even many different possibilities; and this is the case whether or not these rules are inspired by empirical phenomena. So where could there be any room for truth in mathematics, if not in the real world (which is *a priori* unmathematical, let alone platonic) or in man?

Her is our view. Much as it is meaningless to say that a position in chess is "true", it makes no sense to say that mathematical theorems are true either, since there is no objective state of affairs they could describe correctly.[69] Truth in chess can only reside in the claim that some position is *legal*, in that it arose from a game played according to the rules. Similarly, truth in mathematics can only reside in the claim " $T \vdash \varphi$ " that some sentence φ is a theorem within the theory T in which φ is expressed as a well-formed formula.[70] And this is the case (by definition) iff there exists a proof of φ according to the rules of T (which is supposed to include rules of inference).

[66] Think of a numerical table in which the position of a certain planet in the sky is recorded on a daily basis. One may equally well think of the result of some calculation of 25×25 or a measurement of $\alpha + \beta + \gamma$ as data models.

[67] The mathematical theory in which the data model of the previous footnote is embedded could be Kepler's laws, or Newton's. As van Fraassen states, data models do not stand on their own but are typically informed by such theories.

[68] Something similar happens in the brain according to what is called *predictive processing theory* (also called *predictive coding*). This theory replaces traditional views (according to which the brain just *records* sense data) by the idea that all the time the brain actively produces *predictions* about the world, which are compared with incoming sense data, and then updated if necessary. What one consciously experiences then results from a combination of both the "outgoing" predictions and the "incoming" sense data. See e.g. Hohwy (2013) and Millidge, Seth, & Buckley (2022).

[69] Tarski-style truth in the sense of model theory is irrelevant here; it lies within mathematics. See footnote 56.

[70] This might be varied by defining $T \vdash \varphi$ to be true if a proof of φ *is known*, as in intuitionistic mathematics.

Thus the only thing we can, in our opinion, say about mathematical truth is this:[71]

Mathematical truth resides not in theorems *but in claims* that some sentence is a theorem.

This makes a proof of φ in T the truth-maker of the truth-bearer $T \vdash \varphi$. This idea should be distinguished from what e.g. Dieudonné (1971), Tait (1986), Weir (2010), and others propose, namely that a sentence φ *itself* (as opposed to $T \vdash \varphi$) is true iff φ has a proof. Against this:

- Unless one believes (with e.g. Gödel) that there is a single "true" foundational system for all of mathematics (such as ZF set theory with additional axioms) such proposals endorse a *coherence theory of truth* (Young, 2018), in which each such system would come with its own set of truths. We leave this to politicians. On our proposal, although people may differ about the worths of different foundational systems, given an unambiguous concept of proof they cannot reasonably differ about the theorems in each of these systems.

- One encounters difficulties with (ironically) Gödel's first incompleteness theorem,[72] according to which (under the usual assumptions) there are sentences φ such that neither φ nor $\neg\varphi$ is provable. Yet at least in classical logic $\varphi \vee \neg\varphi$ is provable for every formula φ. If this is taken to mean that $\varphi \vee \neg\varphi$ is true, then this can be the case (namely for undecidable φ) without either φ or $\neg\varphi$ being true. But there is no problem if $\vdash (\varphi \vee \neg\varphi)$ is true without either $\vdash \varphi$ or $\vdash \neg\varphi$ being true, since $\vdash (\varphi \vee \neg\varphi)$ is quite different from $(\vdash \varphi) \vee \vdash (\neg\varphi)$.

Nonetheless, one needs to get used to the idea that say Kant's famous example $7+5=12$ is not true (but neither is it false; it is just not the kind of mathematical statement that has a truth value), whereas $\mathrm{PA} \vdash (7+5=12)$ *is* true, i.e., the claim that $7+5=12$ is a theorem of Peano arithmetic.

Adding to the arguments above, which were related to the "formal" language game, let us therefore try to explain why $7+5=12$ has no truth value from the point of view of our "applied" language game either, cf. §5. Truth should come from the alleged fact that $7+5=12$, but what could that mean? We reject the existence of platonic numbers 7 and 5 that can be platonically added to yield a similarly ethereal number 12. But even if this were to make any sense, proof would be the only access to the alleged truth of $7+5=12$; yet proof by construction establishes $\mathrm{PA} \vdash (7+5=12)$ rather than $7+5=12$ itself.[73]

Let us try again: don't seven apples add up with five apples to yield twelve apples? They do; take this as our data model. But this by itself expresses neither $7+5=12$ nor $\mathrm{PA} \vdash (7+5=12)$: it says that seven *apples* add up with five *apples* to yield twelve *apples*. On the constructive empiricist account above, the user of the mathematical theory PA compares the latter theorem with the stated data model for counting and adding apples, and finds a match. This match is between a data model and a formal theory; again, there is no need for abstract/platonic numbers.

As far as we can judge, this analysis seems reinforced by what Wittgenstein taught in 1939:

> one asks such a thing as what mathematics is about—and someone replies that it is about numbers. Then someone comes along and says that it is not about numbers but about numerals; for numbers seem very mysterious things. And then it seems the mathematical propositions are about scratches on the black board. That must seem ridiculous even to those who hold it, bu they hold it because there seems to be no way out—I am trying to show in a very general

[71] This is idea is part of what is called *deductivism* in the philosophy of mathematics (Paseau & Pregel, 2023). In general philosophy (Künne, 2003; David, 2022), it is an example of a *disquotational definition of truth*.

[72] This theorem is also used *against* deductivism, cf. Paseau & Pregel (2023), §9, the idea being that the Gödel sentence in some formal system is "seen" to be true although it cannot be proved in the given system. But it can duly be proved in the standard interpretation of this system in the natural numbers Franzén (2005).

[73] This is the "Truth/Proof problem", which forms another challenge to platonism (Tait, 1986).

way how the misunderstanding of supposing a mathematical proposition to be like an experiential proposition leads to the misunderstanding of supposing that a mathematical proposition is about scratches on the blackboard.

> Take "$20 + 15 = 35$". We say this is about numbers. Now is it about the symbols, the scratches? That is absurd. It couldn't be called a statement or proposition about them; if we have to say that it is a so-and-so about them, we could say that it is a rule or convention about them.—One might say, "Could it not be a statement about how people use symbols?" I should reply that that is not in fact how it is used—any more than as a declaration of love.
>
> One might say that it is a statement about numbers. Is it wrong to say that? Not at all; that is what we call a statement about numbers. But this gives the impression that it's not about some coarse thing like scratches, but about something very thin and gaseous.—Well, what is a number, then? I can show you what a numeral is. But when I say it is a statement about numbers it seems as though we were introducing some new entity somewhere.
>
> (LFM, Lecture XII, p. 112)

The unity of mathematics emphasized by Hilbert provides an additional argument for the lack of truth of $7+5=12$. If it were true, then, being the content of a theorem, every theorem in mathematics should be true. This leads to a problem discussed earlier: since different foundational systems may yield contradictory results, just one of these systems could be "true". The history of mathematics suggests this is dubious. Even if only one of them ultimately comes out be correct (e.g. since the others unexpectedly are inconsistent), putting esoteric result in ZFC set theory about crazy cardinals on a par with $7+5=12$ as both being "true" sounds equally wrong. The only way to get around these problems is to—indeed—treat all theorems from all foundational systems on a par; but instead of declaring them all true, the ensuing notion of truth is expressed much better by saying that the claim $T \vdash \varphi$ that φ can be deduced from T is true, rather than φ itself.

7 Certainty

The notion of *necessity* (or Wittgenstein's "logical must") adds nothing to our deductivist concept of mathematical truth. The *certainty* of mathematical truth, on the other hand, is far from trivial.

For both Hilbert and Wittgenstein the certainty of mathematics originates in proofs, but proof is not enough: both add the requirement that these proofs be *surveyable* (Shanker, 1987, Mühlhölzer, 2006; Floyd, 2023). But they mean very different things by this. Starting with Hilbert:

> Wie wir sahen, hat sich das abstrakte Operieren mit allgemeinen Begriffsumfängen und Inhalten als unzulänglich und unsicher herausgestellt. Als Vorbedingung für die Anwendung logischer Schlüsse und die Betätigung logischer Operationen muß vielmehr schon etwas in der Vorstellung gegeben sein: gewisse außerlogische diskrete Objekte, die anschaulich als unmittelbares Erlebnis vor allem Denken da sind. Soll das logische Schließen sicher sein, so müssen sich diese Objekte vollkommen in allen Teilen überblieken lassen und ihre Aufweisung, ihre Unterscheidung, ihr Aufeinanderfolgen ist mit den Objekten zugleich unmittelbar anschaulich für uns da als etwas, das sich nicht noch auf etwas anderes reduzieren läßt. (...) Hierin liegt die feste philosophische Einstellung, die ich zur Begründung der reinen Mathematik — wie überhaupt zu allem wissenschaftlichen Denken, Verstehen und Mitteilen — für erforderlich halte: *am Anfang — so heißt es hier — ist das Zeichen*.[74] (Hilbert, 1922b, pp. 162–163).

[74] 'As we have seen, abstract operation with general concept-scopes and contents has proved to be inadequate and uncertain. Instead, as a precondition for the application of logical inferences and for the activation of logical operations, something must already be given in representation: certain extra-logical discrete objects, which exist intuitively as immediate experience before all thought. If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation, their diff erence, their succession (like the objects

Vielmehr ist als Vorbedingung für die Anwendung logischer Schlüsse und für die Betätigung logischer Operationen schon etwas in der Vorstellung gegeben: gewisse, außer-logische Konkrete Objekte, die anschaulich als unmittelbares Erlebnis vor allem Denken da sind. Soll das logische Schließen sicher sein, so müssen sich diese Objekte vollkommen in allen Teilen überblicken lassen und ihre Aufweisung, ihre Unterscheidung, ihr Aufeinanderfolgen oder Nebeneinandergereihtsein ist mit den Objekten zugleich unmittelbar anschaulich gegeben als etwas, das sich noch auf etwas anderes reduzieren läßoder einer Reduktion bedarf.Dies ist die philosophische Grundeinstellung, die ich für die Mathematik wie überhaupt zu allem wisseschaftlichen Denken, Verstehen und Mitteilen für erforderlich halte. Und insbesondere in der Mathematik sind Gegenstand unserer Einstellung zufolge unmittelbar deutlich und wiedererkennbar ist.[75] (Hilbert, 1926, p. 171)

Thus Hilbert's understanding of the certainty of mathematics relies on the certainty of logical inference, which in turn relies on the use of very simple signs. This was also the basis of his eventual "finitism", which called for a final mark of certainty in the form of a finitist proof of the consistency of classical mathematics (which goal was never achieved or even well defined).[76]

For the present discussion it is a moot point if Wittgenstein was a finitist, too (cf. Marion, 1998). What matters is that his notion of 'surveyability' of a proof was almost the opposite of Hilbert's. He explains this notion in §III.1 of his *Remarks on the Foundations of Mathematics*:

'Ein Mathematischer Beweis muß übersichtlich sein.' "Beweis" nennen wir nur eine Struktur, deren Reproduktion eine leicht lösbare Aufgabe ist. Es muß sich mit Sicherheit entscheiden lassen, ob wir hier wirklich zweimal den gleichen Beweis vor uns haben, oder nicht. Der Beweis muß ein Bild sein, welches sich mit Sicherheit genau reproduzieren läßt. Oder auch: was dem Beweise wesentlich ist muß sich mit Sicherheit genau reproduzieren lassen. Er kann z.B. in zwei verschiedenen Handschriften oder Farben niedergeschrieben sein. Zur Reproduktion eines Beweises soll nichts gehören, was von der Art einer genauen Reproduktion eines Farbtones oder einer Handschrift ist.

Es muß leicht sein, *genau* diesen Beweis wieder anfzuschreiben. [77]

(Wittgenstein, 1984d, p. 143)

These conditions are studied in detail by Mühlhölzer (2006), who summarizes them as follows:

S1 The surveyability of a proof consists in its possibility of reproduction.

S2 This reproduction must be an easy task.

themselves) must exist for us immediately, intuitively, as something that cannot be reduced to something else. The solid philosophical attitude that I think is required for the grounding of pure mathematics — as well as for all scientific thought, understanding, and communicatio — is this: *In the beginning was the sign*.'

[75] 'As a further precondition for using logical deduction and carrying out logical operations, something must be given in conception, viz., certain extra-logical concrete objects which are intuited as directly experienced prior to all thinking. For logical deduction to be certain, we must be able to see every aspect of these objects, and their properties, differences, sequences, and contiguities must be given, together with the objects themselves, as something which cannot be reduced to something else and which requires no reduction. This is the basic philosophy which I find necessary, not just for mathematics, but for all scientific thinking, understanding, and communicating. The subject matter of mathematics is, in accordance with this theory, the concrete symbols themselves whose structure is immediately clear and recognizable.'

[76] See the references in footnote 50 on Hilbert's program.

[77] 'A mathematical proof must be perspicuous.' Only a structure whose reproduction is an easy task is called a "proof". It must be possible to decide with certainty whether we really have the same proof twice over, or not. The proof must be a configuration whose exact reproduction can be certain. Or again: we must be sure we can exactly reproduce what is essential to the proof. It may for example be written down in two different handwritings or colours. What goes to make the reproduction of a proof is not anything like an exact reproduction of a shade of colour or a hand-writing. It must be easy to write down exactly this proof again.'

S3 We must be able to decide with certainty whether the reproduction produces the same proof.

S4 The reproduction of a proof is of the sort of a reproduction of a picture. (p.59)

The feature that for Hilbert ensured surveyability of a proof, namely the consistent use of elementary signs or strokes, was precisely the reason why Wittgenstein considered such proofs *non*-surveyable and hence not even proofs. And likewise for the infamous proof of $1 + 1 = 2$ given by Russel and Whitehead (1910), *54.43, which including all preparation takes hundreds of pages.[78]

This theme influenced and subsequently became strongly influenced by Turing's concept of computability (Floyd, 2023). Four decades onwards, computer-assisted proofs, like the famous one of the four-colour theorem (Appel & Haken, 1977; Robertson *et al.*, 1997; Haken, 2006; Gonthier, 2008) also changed the debate (Tymoczko, 1979; Shanker, 1987, pp. 143–160). Indeed, such proofs can hardly be called surveyable in either Hilbert's or Wittgenstein's sense; and this is also the case for proofs entirely done by hand but involving large teams of mathematicians publishing their work in dozens of papers whose length adds up to thousands of pages, like the classification of finite simple groups,[79] or the stability of space-time in general relativity.[80]

Without mentioning Hilbert, Wittgenstein, or the concept of surveyability, Avigad (2021) nonetheless describes the essence of their opposition. An informal proof is more likely to be understandable (and would arguably be surveyable in the sense of Wittgenstein), but alas, it is not rigorous. A formal Hilbert-style proof, on the other hand, will hardly be understandable if only because of its length, which also enormously increases the probability of error. Thus the universal habit among "working mathematicians" of not writing out proofs according to the rules of logic is essential to their readability (and often even reliability), but this habit obviously sacrifices rigour. Conversely, formal rigour sacrifices readability and in the worst case introduces the errors Hilbert so desperately hoped to avoid. See also Thurston (1994), Weir (2014), Hamami (2018), Ording (2019), Burgess & De Toffoli (2022), Stillwell (2022), and Hamami & Morris (2023) for perspectives on the wide variety of styles of proof in mathematics. Even within the formal setting, Dutilh Novaes (2011) identifies eight different ways in which formality and rules can be interpreted.

How do we navigate between Scylla and Charybdis? Returning to Wittgenstein, the key property of a proof that is relevant for the certainty of mathematics is, quite simply, the following:

> "Der Beweis muß übersehbar sein" — heißt das nicht: daß es ein Beweis ist, muß zu sehen sein.[81] (Wittgenstein, MS 122: p. 105; Mühlhölzer, 2010, p. 574)

On a relaxed interpretation of 'sehen' (seeing), Wittgenstein and Hilbert may both get their way if computer-verified proofs (which they did not live to see) are invoked.[82] See e.g. Geuvers (2009). These greatly enhance the certainty of mathematics—perhaps without driving it up to full certainty. The underlying proof assistants rely on a so-called logical kernel, which ultimately has to be trusted (verifying it would lead to infinite regress).[83] If it contains bugs, this "probably" would have been noted in verifying the dozens of theorems whose proofs have now been checked.

[78] The blog by Dominus (2006) gives a very nice discussion of this proof.

[79] Wikipedia gives an excellent summary of this classification. The special issue on formal proof of the *Notices of the AMS*, December 2008, available at `https://www.ams.org/notices/200811/200811FullIssue.pdf`, covers both computer-verified and computer-assisted proofs. See also `https://www.cs.ru.nl/~freek/100/index.html`.

[80] See Dafermos *et al.* (2019), and Dafermos *et al.* (2021), and references therein.

[81] '"The proof must be surveyable" — doesn't this mean: it should be visible that a proof is a proof.'

[82] To avoid confusion, we note that computer-*verified* proofs are produced by so-called proof *assistants*, which have nothing to do with computer-*assisted* proofs of the kind mentioned above.

[83] See `https://en.wikipedia.org/wiki/Proof_assistant`. Their list of proof assistants also tells us if the kernel is 'small': the smaller, the better. The authors are grateful to Freek Wiedijk for information about this topic.

Of course, this argument is as weak as the analogous argument for the consistency of set theory (namely that *so far* it has not produced a contradiction). Bugs in the compiler for the programming language in which the proof assistant is written are also possible, but the likelihood of such errors can be reduced by using a number of different compilers, as has indeed been done to good effect.

In sum: short of absolute guarantees, the jury is still out on the certainty of mathematics. But if it applies, it must come from proof, as both Hilbert and Wittgenstein maintained. Interestingly, the historical circle closes at this point, since proof assistants (obviously) rely on a complete formalization of mathematics as envisaged by Hilbert (Nederpelt & Geuvers, 2014).

All of this confirms the language-game view of mathematics; like a game it is based on rules, but one has to take the *actual practice of rule following* into account to get a complete picture (whereas in a game like chess, deviations from the rules would end the game at once):

> Warum nenne ich die Regeln des Kochens nicht willkürlich; und warum bin ich versucht, die Regeln der Grammatik willkürlich zu nennen? Weil 'Kochen' durch seinen Zweck definiert ist, dagegen 'Sprechen' nicht. Darum ist der Gebrauch der Sprache in einem gewissen Sinne autonom, in dem das Kochen und Waschen es nicht ist. Wer sich beim Kochen nach andern als den richtigen Regeln richtet, kocht schlecht; aber wer sich nach andern Regeln als denen des Schach richtet, spielt *ein anderes Spiel*; und wer sich nach andern grammatischen Regeln richtet, als den unsern, spricht darum nichts Falsches, sondern *von etwas Anderm*.[84] (Wittgenstein, Ts-228,108[3]et109[1])

A Formalism, axioms, definitions, and proofs

A.1 Formalism

Though often associated with Hilbert,[85] there isn't a canonical notion of "formalism" in the philosophy of mathematics. Here is the rather sterile version defined e.g. by Linnebø (2017):

> *Formalism* is the view that mathematics has no need for semantic notions, or at least none that cannot be reduced to syntactic ones. (Linnebø, 2017, p. 39)

This is the version attacked by Frege (cf. the extracts from his correspondence with Hilbert above). But it is a straw man that certainly shouldn't be associated with Hilbert, for whom it is only *in the context of proofs and in the analysis of axiom systems regarding consistency etc.* (and hence, in metamathematics) that mathematics is seen as a deductive enterprise in which symbols have no meaning (outside the rules they are subject to). A much broader view of formalism is promoted by Detlefsen (2005), who characterizes it by five key components (some of historical interest only):[86]

1. Formalism rejected a representational role for mathematical language, making mathematical reasoning independent of the actual content of the words and symbols in this language:

   > The pivotal commitment of formalism (. . .) is a view concerning the nature of language—namely, that it can serve as a guide to thought even when it does not function descriptively. (Detlefsen, 2005, p. 251)

[84] 'Why don't I call the rules of cooking arbitrary; and why am I tempted to call the rules of grammar arbitrary? Because 'cooking' is defined by its purpose, whereas 'speaking' is not. That is why the use of language is autonomous in a sense in which cooking and washing are not. Anyone who follows rules other than the correct ones when cooking cooks poorly; but anyone who follows rules other than those of chess is playing a different game; and anyone who follows grammatical rules other than ours is not speaking falsely, but rather about something different.'

[85] The classical exposition of formalism by von Neumann (1931) is entirely devoted to Hilbert's program.

[86] We put Detlefsen's fourth criterion (which he traces this back to Berkeley) first, since it is the key point.

2. Formalism rejected the traditional (Aristotelian) *division* of the mathematical sciences into arithmetic (as the science of multitude), geometry (as the science of magnitude), and the mixed sciences (notably optics, harmonics, mechanics, and astronomy).[87]

3. It also rejected the traditional (Euclidean) concept of *proof* (which is sometimes called *genetic*), which was based on the *construction* of some object claimed to "exist", followed by an explicit verification of the properties claimed by the theorem to be proved.

4. As a corollary of the previous point, against the previous standards of proof going back to Euclid (residing in visualization and intuition, based on the actual meaning of the symbolic situation), formalism advocated rigour through abstraction and algebraic deduction.

5. Finally, formalists emphasized the almost unlimited freedom and creative force of mathematicians to propose (consistent) mathematical theories.

None of these ideas truly originated with Hilbert,[88] but he was certainly the most important mathematician who broadly championed formalism (in the sense described by Detlefsen above), and thus took mathematics from the 19th into the 20th century. We already discussed the first and second points. At least since his instantly famous proofs of the Basis theorem (1890) and the *Nullstellensatz* (1893), Hilbert was a non-constructivist.[89] Hilbert (1899) made the third and fourth ingredients widely known and acceptable, and the fifth was stressed by him throughout his career.

Wittgenstein's take on formalism has been analyzed in detail by Mühlhölzer (2008, 2010). It should be obvious that Wittgenstein supported ingredient 1 of formalism (in the above list from 1 to 5), i.e. the representational role of mathematical language (as opposed to its *constitutive* role); this may even be said to be the whole point of his late philosophy. He did not (as far as we know) comment on the historically oriented second ingredient on the division of mathematics. He might be ambiguous on the nature of proof as meant in the third point, since on the one hand he is often seen as a constructivist, whereas on the other his outspoken aim was to leave mathematical *practice* as it is (and just provide therapy for those who are confused about its *foundations*).[90] Writings from his middle phase, where his calculus-conception of mathematics reigned (Gerrard, 1991), cf. §2, seem to support the fourth goal of increasing the rigour of proofs.[91] Finally, the unlimited freedom and creative force of mathematics also seems compatible with his views. It therefore seems fair to say that although Wittgenstein opposed all "isms", such as logicism, platonism, intuitionism, and formalism,[92] the latter was nonetheless closest to his heart, also because of his sympathy for the closely related analogies with chess (Max, 2020a; Mühlhölzer, 2010, §I.7). See also §§2–4.

[87] See also Katz (2018) and Mendell (2019).

[88] For example, a telling quotation is: 'They who are acquainted with the present state of the theory of Symbolic Algebra, are aware of the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination.' (Boole, 1847, Preface). Indeed, the full title of the book from which this is taken is *The Mathematical Analysis of Logic: Being an Essay Towards a Calculus of Deductive Reasoning*. See also Mehrtens (1990), Heintz (2000), Gray (2008), and Maddy (2008).

[89] His later adoption of intuitionistic logic and even finitism was restricted to *metamathematics* and was intended to justify non-constructive proofs in classical mathematics.

[90] Here is an example of the first point: 'Ich glaube, die Mathematik wird, wenn die Grundlagenstreit beendent sein wird, das Gesicht annehmen, das sie auf der Volksschule hat (...) Er braucht in keiner Weise verbesster zu werden' (Wittgenstein, 1984b, pp. 105–106). 'I believe that when the foundational dispute is over, mathematics will take on the appearance that it has in elementary school (...) It does not need to be improved in any way.'

[91] All this is blurred by various ideas Wittgenstein forwarded on mathematical proof that are out of touch with modern mathematics, like the idea that the meaning of a theorem lies in its proof, or that a proof changes the meaning of a theorem. As noted before Wittgenstein did not connect to Hilbert-style proofs and proof theory. According to Mühlhölzer (2005), p. 128, Wittgenstein was not so much interested in formal proofs but in proofs that are carried out in (non-logical) mathematical practice.

[92] For example: 'In dem Kampf zwischen dem 'Formalismus' & der 'inhaltlichen Mathematik', was behauptet

A.2 Axioms

Hilbert's main tool, which Detlefsen (2005) might (and perhaps should) have included in the above list, was *axiomatization* (Corry, 2004; Majer, 2001, 2006, 2014; Schlimm, 2013; Majer & Sauer, 2014; Steingart, 2023). As already mentioned (and illustrated by quotations), Hilbert's program of axiomatization was meant to secure both the *rigor* and the *applicability* of mathematics:

1. the former partly by formalizing the notion of a mathematical proof (which led to his glib identification by a number of philosophers as a sterile formalist), and partly by the axiomatization of what might be called immature mathematical theories;

2. the latter by the axiomatization of physics.

All of this was in place around 1900 (and even earlier in Hilbert's lectures, cf. footnote 50). Both aspects of the first point were manifest in Hilbert (1899), as well as in his correspondence with Frege from 1899 and 1900. The second came out into the public especially through his *sixth problem* (from the famous list of 23 problems in 1900):[93]

> Mathematische Behandlung der Axiome der Physik.
>
> *Durch die Untersuchungen über die Grundlagen der Geometrie wird uns die Aufgabe nahe gelegt, nach diesem Vorbilde diejenigen physikalischen Disciplinen axiomatisch zu behandeln, in denen schon heute die Mathematik eine hervorragende Rolle spielt; dies sind in erster Linie die Wahrscheinlichkeitsrechnung und die Mechanik.*[94] (Hilbert, 1900, p. 272)

In the next two decades Hilbert extended this ambition quite significantly, e.g.,

> Ich glaube: Alles, was Gegenstand des wissenschaftlichen Denkens überhaupt sein kann, verfällt, sobald es zur Bildung einer Theorie reif ist, der axiomatischen Methode und damit mittelbar der Mathematik. Durch Vordringen zu immer tieferliegender Schichten von Axiomen im vorhin dargelegten Sinne gewinnen wir auch in das Wesen des wissenschaftlichen Denkens selbst immer tiefere Einblicke und werden uns der Einheit unseres Wissens immer mehr bewußt. In dem Zeichen der axiomatischen Methode erscheint die Mathematik berufen zu einer führenden Rolle in der Wissenschaft überhaupt.[95] (Hilbert, 1918, p. 414).

Hilbert (1918) *begins* his essay on axiomatic thought (of which the above words are the *end*) by stressing the importance of the connection between mathematics and neigbouring fields like physics and epistemology, introducing the axiomatic method as the key to this connection:

denn jeder Teil? Dieser Streit ist so ähnlich dem zwischen Realismus & Idealismus! Darin z.B., daß er sehr bald obsolet geworden sein wird & daß beide Parteien entgegen ihrer täglichen Praxis Ungerechtigkeiten behaupten.' (Ms-112,15v[2], quoted in Mühlhölzer (2010), p. 72, from BT, p. 535). 'In the battle between 'formalism' & 'contentful mathematics', what does each side claim? This dispute is similar to that between realism and idealism! For example, in that it will very soon become obsolete and that both parties, contrary to their daily practice, claim injustice.'

[93] See e.g. Corry (2004, 2018) and references therein. It is puzzling that Hilbert did not mention Newton's *Principia* in this light, which was surely the first explicit and successful axiomatization of physics.

[94] '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 probability theory and mechanics.*'

[95] 'I believe this: as soon as it is ripe for theory building, anything that can be the subject of scientific thought at all falls under the scope of the axiomatic method and hence indirectly of mathematics. By penetrating into ever deeper layers of axioms in the sense outlined earlier we also gain insight into the nature of scientific thought by itself and become steadily more aware of the unity of our knowledge. Under the header of the axiomatic method mathematics appears to be called into a leading role in science in general.'

> Wie im Leben der Völker das einzelne Volk nur dann gedeihen kann wenn es such allen Nachbarvökern gut geht, und wie das Interesse der Staaten es erheischt, daß nicht nur innerhalb jedes einzelnen Staates Ordnung herrsche, sondern auch die Beziehungen der Staaten unter sich gut geordnet worden müssen, so ist es auch im Leben der Wissenschaften. In richtiger Erkenntnis dessen haben die bedeutendsten Träger der mathematischen Gedankens stets groß es Interesse an den Gesetzen und der Ordnung in den Nachbarwissenschaften bewiesen und vor allem zu Gunsten der Mathematik selbst von jeher die Beziehungen zu den Nachbarwissenschaften, insbesondere zu den großen Reichen der Physik und der Erkenntnistheorie, gepflegt. Das Wesen dieser Beziehungen und der Grund ihrer Fruchtbarkeit glaube ich, wird am besten deutlich, wenn ich lhnen diejenige allgemeine Forschungsmethode schildere, die in der neueren Mathematik mehr und mehr zur Geltung zu kommen scheint: ich meine die *axiomatische Methode*.[96] (Hilbert, 1918, p. 405).

A.3 Definitions

The concept of a definition goes back to Socrates, Plato, and Aristotle,[97] who tried to define things (words or concepts) by finding their *essence*.[98] Turning to our three protagonists, Frege promoted various concepts of definition. In his *Begrifsschrift* from 1879, a definition was an abbreviation:[99]

> Wäre nun (69) ein synthetisches Urteil, so wären es auch die daraus abgeleiteten Sätzen. Man kann aber die durch diesen Satz eingeführten Bezeichnungen und daher ihn selbst als ihre Erklärung entbehren: nichts folgt aus ihm, was nicht auch ohne ihn erschlossen werden könnte. Solche Erklärungen haben nur den Zweck, durch Festsetzung einer Abkürzung eine äusserliche Erleichterung herbeizuführen. Ausserdem dienen sie dazu eine besondere Verbindung von Zeichen aus der Fülle der möglichen hervorzuheben, um daran einen festern Anhalt für die Vorstellung zu gewinnen.[100] (Frege, 1879, p. 56).

In other words: A definition is an arbitrary stipulation by which a new sign is introduced to take the place of a complex expression whose meaning we already know. But also note the last sentence! All of this is echoed in *Principa Mathematica* (whose debt to Frege is generally enormous):

[96] 'Just as in the life of nations the individual nation can only thrive when all neighbouring nations are in good health; and just as the interest of states demands, not only that order prevail within every individual state, but also that the relationships of the states among themselves be in good order; so it is in the life of the sciences. In due recognition of this fact the most important bearers of mathematical thought have always evinced great interest in the laws and the structure of the neighbouring sciences; above all for the benefit of mathematics itself they have always cultivated the relations to the neighbouring sciences, especially to the great empires of physics and epistemology. I believe that the essence of these relations, and the reason for their fruitfulness, will appear most clearly if I describe for you the general method of research which seems to be coming more and more into its own in modern mathematics: I mean the *axiomatic method*.'

[97] Further to the recent *Stanford Encyclopedia of Philosophy* article by Gupta & Mackereth (2023), the *Encyclopedia of Philosophy* entry by Abelson (1967) also remains worth reading, as is the even older monograph by Robinson (1954).

[98] Cellucci (2018) regards 'Galileo's decision to abandon Aristotle's aim to penetrate the essence of natural substances' and replace this aim by stipulative definitions via mathematics as the beginning of modern science.

[99] In this quote, '(69)' refers to a certain symbolic definition: if some object δ has some property F and for some function f the object $f(\delta)$ also has the property F (in case this is defined), then we say that the 'f-sequence' $(\delta, f(\delta), \ldots)$ inherits F. The details are irrelevant for what follows, including the Kantian terminology. All that matters is that the notion of 'inheritage' (expressed symbolically) is defined in terms of known things (expressed symbolically).

[100] 'If (69) had been a synthetic judgement, then so would have been the theorems derived from it. But we can do without the notation introduced by this sentence, and hence without the sentence itself as its definition; nothing follows from the sentence that could not also be inferred without it. Our sole purpose in introducing such definitions is to bring about an extrinsic simplification by stipulating an abbreviation. Apart from this, such definitions serve the purpose of highlighting special combinations of symbols from the wealth of all possibilities, so as to anchor them in our imagination.'

> 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 & Whitehead, 1910, pp. 11–12).

Returning to Frege, despite the quotation preceding the one just given, his *Grundlagen der Arithmetik* from 1884 is a sustained quest for the essence of number, although the author explicitly declares himself to be a logicist rather than an essentialist. Starting with the very first sentence (which states that 'the number one is a thing' is unsatisfactory because the first article is definite whereas the second is not),[101] the *Grundlagen* contains a large number of examples of what definitions should *not* be. The only positive characterization is that a 'a definition must be logical' (p. IX). And finally, in a more general context Frege defined (*sic*) definitions by their extension:

> Eine Definition eines Begriffes (möglichen Prädikates) muss vollständig sein, sie muss für jeden Gegenstand unzweideutig bestimmen, ob er unter den Begriff falle (ob das Prädikat mit Wahrheit von ihm ausgesagt werden könne) oder nicht. Es darf also keinen Gegenstand geben, für den es nach der Definition zweifelhaft bliebe, ob er unter den Begriff fiele, wenn es auch für uns Menschen bei unsern mangelhaften Wissen nicht immer möglich sein mag, die Frage zu entscheiden. Man kann dies Bildlich so ausdrücken: der Begriff muss scharf begrenzt sein.[102] (Frege, 1903, §56)

This is vintage Frege (and surely also early Wittgenstein): definitions should be unambiguous and the world is a place in which every *Gegenstand* (= object) is sharply defined by its properties.[103]

[101] 'Auf die Frage, was die Zahl Eins sei, oder was das Zeichen 1 bedeute, wird man meistens die Antwort erhalten: nun, ein Ding. Und wenn man dann darauf aufmerksam macht, dass der Satz "die Zahl Eins ist ein Ding" keine Definition ist, weil auf der einen Seite der bestimmte Artikel, auf der anderen der unbestimmte steht, dass er nur besagt, die Zahl Eins gehöre zu den Dingen, aber nicht, welches Ding sie sei, so wird man vielleicht aufgefordert, sich irgendein Ding zu wählen, das man Eins nennen wolle.' (Frege, 1884, p. I). Ironically, even Frege's definition of the number one, which eventually relied on his entire subsequent program, turned out to be wrong in view of Russell's paradox.

[102] 'A definition of a concept (of a possible predicate) must be complete; it must unambiguously determine, as regards any object, whether or not it falls under the concept (whether or not the predicate is truly assertible of it). Thus there must not be any object as regards which the definition leaves in doubt whether it falls under the concept; though for us men, with our defective knowledge, the question may not always be decidable. We may express this metaphorically as follows: the concept must have a sharp boundary.'

[103] Continuing footnote 101: Used in set theory, unlimited definition by extension eventually led to Frege's downfall, since his comprehension principle leads to Russell's paradox.

This is also inherent in the logical atomism of Russell (and the *Tractatus*, whose world is modeled on propositional logic and its binary truth tables). Against this (and the entire preceding tradition in philosophy), the later Wittgenstein forwarded his famous concept of a *family resemblance*:[104]

> Hier stoßen wir auf die große Frage, die hinter allen diesen Betrachtungen steht. – Denn man könnte mir nun einwenden: "Du machst dir's leicht! Du redest von allen möglichen Sprachspielen, hast aber nirgends gesagt, was denn das Wesentliche des Sprachspiels, und d.h. der Sprache, ist. Was allen diesen Vorgängen gemeinsam ist und sie zur Sprache, oder zu Teilen der Sprache macht. Du schenkst dir also gerade den Teil der Untersuchung, der dir selbst seinerzeit das meiste Kopfzerbrechen gemacht hat, nämlich den, die allgemeine Form des Satzes und der Sprache betreffend." Und das ist wahr. – Statt etwas anzugeben, was allem, was wir Sprache nennen, gemeinsam ist, sage ich, es ist diesen Erscheinungen gar nicht Eines gemeinsam, weswegen wir für alle das gleiche Wort verwenden, – sondern sie sind miteinander in vielen verschiedenen Weisen verwandt. Und dieser Verwandtschaft, oder diesen dieser Verwandtschaften wegen nennen wir sie alle "Sprachen". Ich will versuchen, dies zu erklären. (...)
>
> Wir sehen ein kompliziertes Netz von Ähnlichkeiten, die einander übergreifen und kreuzen. Ähnlichkeiten im Großen und Kleinen. (...) Ich kann diese Ähnlichkeiten nicht besser charakterisieren, als durch das Wort "Familienähnlichkeiten"; denn so übergreifen und kreuzen sich die verschiedenen Ähnlichkeiten, die zwischen den Gliedern einer Familie bestehen: Wuchs, Gesichtszüge, Augenfarbe, Gang, Temperament, etc. etc. – Und ich werde sagen: die 'Spiele' bilden eine Familie.[105] (Wittgenstein, 1984a, §§65–67).

Except for some rather vague allusions involving numbers and propositions (Baker & Hacker, 2009a, §XI.7) Wittgenstein did not apply this idea to mathematics. There are two relevant layers:

1. Like a (language) game, mathematics *as a whole* could be seen as a family resemblance.
2. Concepts *within* mathematics, notably definitions and proofs, might be family resemblances.

[104] As noted for example by Sluga (2006), to understand Wittgenstein's motivation for the idea of a family resemblance it is very useful to go back to an earlier passage from the *Blue Book* (pp. 19–20): 'The idea that in order to get clear about the meaning of a general term one had to find the common element in all its applications has shackled philosophical investigation; for it has not only led to no result, but also made the philosopher dismiss as irrelevant the concrete cases, which alone could have helped him to understand the usage of the general term. When Socrates asks the question, "what is knowledge?" he does not even regard it as a preliminary answer to enumerate cases of knowledge.' Essay XI in Baker & Hacker (2009a) provides a historical perspective on family resemblances. Nietzsche already described a 'family resemblance' between specific groups of languages and even ways of philosophizing in these languages. But: 'It is one thing to family resemblances between different languages and to group the various languages into families according to their genesis. It is quite a different thing, however, to extend the notion of a family resemblance to *concepts* (including the concept of language), i.e. to argue that the extension of a concept may be united not by common characteristics but by overlapping similarities between the members.' (Baker & Hacker, 2009a, p. 210).

[105] 'Here we come up against the great question that lies behind all these considerations.– For someone might object against me: "You take the easy way out! You talk about all sorts of language-games, but have nowhere said what the essence of a language-game, and hence of language, is: what is common to all these activities, and what makes them into language or parts of language. So you let yourself off the very part of the investigation that once gave you yourself most headache, the part about the general form of propositions and of language." And this is true.–Instead of producing something common to all that we call language, I am saying that these phenomena have no one thing in common which makes us use the same word for all, – but that they are related to one another in many different ways. And it is because of this relationship, or these relationships, that we call them all "language". I will try to explain this. (§65) we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail. (§66) 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.– And I shall say: 'games' form a family.' (§67)

The first would mean that mathematics is a concept with various different instantiations, such as Babylonian mathematics, Chinese mathematics, Greek mathematics, 17th century European mathematics, 18th (...), 19th (...), and 20th century mathematics (finally a homogeneous worldwide practice) are seen as different members of the mathematical family. This is a possible view, but in the context of our question to what extent mathematics may be seen as a game (which is the driving force of this article), we prefer to see mathematics as a single notion, which we interpet as a “motley of language games”. *That* is a family resemblance, which, switching to the second point, in case of mathematics for example involves axioms (§A.2), definitions, and proofs (§A.4).

All of these, but especially the second, are family resemblances. Indeed, one finds all kinds of definitions in mathematics. See Coumans (2023), p. 212, for a classification of definitions.

A.4 Proofs

In the formal(ist) language game played by Hilbert, proofs are examples or rule following. But where do the rules (of deduction) come from? Hilbert answers this in a famous passage:[106]

> Das Formelspiel, über das BROUWER so wegwerfend urteilt, hat außer dem mathematischen Wert noch eine wichtige allgemeine philosophische Bedeutung. Dieses Formelspiel vollzieht sich nämlich nach gewissen bestimmten Regeln, in denen die *Technik unseres Denkens* zum Ausdruck kommt. Diese Regeln bilden ein abgeschlossenes System, das sich auffinden und endgültig angeben läßlt. Die Grundidee meiner Beweistheorie ist nichts anderes, als die Tätigkeit unseres Verstandes zu beschreiben, ein Protokoll über die Regeln aufzunehmen, nach denen unser Denken tatsächlich verfährt. Das Denken geschieht eben parallel dem Sprechen und Schreiben, durch Bildung und Aneinanderreihung von Sätzen. Wenn irgendwo eine Gesamtheit von Beobachtungen und Erscheinungen verdient, zum Gegenstand einer ernsten und gründlichen Forschung gemacht zu werden, so ist es diese hier — liegt es doch in der Aufgabe der Wissenschaft, uns von Willkür, Gefühl und Gewöhnung freizumachen und vor dem Subjektivismus zu bewahren, der sich schon in den Anschauungen KRONECKERS bemerkbar gemacht hat und der, wie mir scheint, in dem Intuitionismus seinen Gipfelpunkt erreicht.[107] (Hilbert, 1928, pp. 79–80)

Hilbert’s view on the origin of logic (which is his and Frege’s tool for deductive proofs in mathematics), then, is that its rules express the *technique of our thinking*. This view keeps Hilbert at some distance from both Frege and Wittgenstein. Frege emphatically insisted that the laws of logic are *not* the psychological laws by which we think and reason, but the laws by which reasoning is justified (Soames, 2014, p. 30), or even the most general laws of truth, which as such are *normative* for correct thinking (rather than *descriptive* of it), for which ordinary language is too imprecise (Kuusela, 2019a, §I.1). For Wittgenstein, on the other hand, the ‘technique of thinking’ is grounded in practice and it is this practice that leads to the laws of logic (Mühlhölzer, 2010, p. 75). Claiming that practice is ruled by our way of thinking, Hilbert seems to put Wittgenstein’s view on its head, as the latter maintained that our way of thinking is ruled by some practice.

[106] Many other authors have used this, or parts of it; e.g. Detlefsen (1986), p. x; Mühlhölzer, 2010, p. 74.

[107] ‘The formula game that Brouwer so deprecates has, besides its mathematical value, an important general philosophical significance. For this formula game is carried out according to certain definite rules, in which the technique of our thinking is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds. Thinking, it so happens, parallels speaking and writing: we form statements and place them one behind another. If any totality of observations and phenomena deserves to be made the object of a serious and thorough investigation, it is this one-since, after all, it is part of the task of science to liberate us from arbitrariness, sentiment, and habit and to protect us from the subjectivism that already made itself felt in Kronecker’s views and, it seems to me, finds its culmination in intuitionism.’

This seems a long way from Frege's lofty faith in the supreme status of logic, which in turn encouraged Russell (who followed Frege in many ways, but left him at the bitter end) to write:

> The old logic put thought in fetters, while the new logic gives it wings. It has, in my opinion, introduced the same kind of advance into philosophy as Galileo introduced into physics, making it possible at last to see what kinds of problems may be capable of solution, and what kinds must be abandoned as beyond human powers. (...) It is in this way that the study of logic becomes the central study in philosophy: it gives the method of research in philosophy, just as mathematics gives the method in physics. And as physics, which, from Plato to the Renaissance, was as unprogressive, dim, and superstitious as philosophy, became a science through Galileo's fresh observation of facts and subsequent mathematical manipulation, so philosophy, in our own day, is becoming scientific through the simultaneous acquisition of new facts and logical methods. (Russell, 1914, pp. 48, 194)

See also Kuusela (2019a), p. 14. But if, following PI §§130–131, we regard language games as logical tools of examination or comparison in the study of language (and hence in philosophy); and, as advocated in this paper, regard mathematics as consisting of specific language games that can be used in the study of physics in a very similar way, namely as *yardsticks* rather than *descriptions*, then this late Wittgensteinian perspective seems to confirm Russell's analogy between the use of logic as *the* method in philosophy and the use of mathematics as *the* method in physics.

Acknowledgement: This work originated in a B.Sc. thesis of the second author (Singh, 2013), supervised by the first, which was greatly expanded afterwards. The authors are grateful to Jeremy Butterfield, Thomas Friederich, Fred Muller, Tomasz Placek, and Freek Wiedijk for comments and further help with this paper.

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