Is mathematics a game?

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Abstract

We re-examine the old question to what extent mathematics may be compared with a game, realizing that a "motley of language games" is the more refined object of comparison. Our analysis owes at least as much to (late) Wittgenstein, updated by a Brandomian emphasis that the rules governing language games be *inferential*, as to Hilbert, supplemented with some empiricism taken from van Fraassen. Our "motley" provides a coat rack onto which various philosophies of mathematics may be attached and may even peacefully support each other. *Pure* mathematics is a language game incorporating aspects of formalism and structuralism, modified however by a different notion of truth. *Applied* mathematics corresponds to a different language game in which mathematical theories provide generalized yardsticks that "*measure*" (as opposed to: *represent*) natural phenomena. Thus our framework is non-referential for both pure and applied mathematics. The *certainty* of pure mathematics in principle resides in proofs, but in practice these must be "surveyable". Hilbert and Wittgenstein proposed almost oppositie criteria for surveyability; we try to overcome their difference.

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] (Our endnotes provide additional information–often bibliographical and/or historical–or English translations of quotes from the original German; they are not needed to follow the argument.) Whatever the answer, this question 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 nineteenh 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 of the analogy between mathematics and games like chess started with Frege's criticisms of Thomae (1898) and to a lesser extent Heine (1872) and Illigens (1893).[3] Frege wholeheartedly rejected the analogy; for he was primarily unable to comprehend how a mere game could describe any "thought".[4] Most 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* thoughts. But 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.
- On Frege's conceptualization, logical inference must take us from truth to truth. Therefore, without truth (which games allegedly lack) there is no deduction and hence no mathematics.
- The applicability of mathematics would be incomprehensible if it were merely a game, and also leads to irresolvable ambiguities between the formal and the applied sides.
- The analogy is inconsistent because even if mathematics initially were just a game, it also incorporates *the theory of the game* instead of only *being the game*. Similarly, there are theorems *about* chess.[5] This is something a game by itself could not accomplish.

All of this reflects Frege's particular branch of platonism. Things were brought to a head in the following exchange (which rapidly degenerated into an acrimonious polemic, omitted here):[6]

> Wer eine Arithmetik auf eine formale Zahlenlehre aufbauen will, eine Lehre, die nicht fragt, was sind und was sollen die Zahlen, sondern die fragt, was brauchen wir von den Zahlen in der Arithmetik, dem wird es erwünscht sein, auf ein anderes Beispiel einer rein formalen Schöpfung des menschlichen Verstandes hinweisen zu können. Ein solches Beispiel glaubte ich im Schachspiel gefunden zu haben. Die Schachfiguren sind Zeichen, denen im Spiel kein anderer Inhalt zukommt, als der ihnen durch die Spielregeln zuerteilt wird. Die Redeweise, die Zeichen sind leere, kann zu Mißverständnissen da führen, wo der gute Wille zum Verständnis fehlt. So glaubte ich auch die Zahlen in der Arithmetik, im Rechenspiel als Zeichen ansehen zu dürfen, denen eben im Spiel kein anderer Inhalt zukommt, als der ihnen durch die Rechnungs- oder Spielregeln zugeteilt wird. Das Zeichensystem des Rechenspiels wird aus den Zeichen 0 1 2 3 4 5 6 7 8 9 in bekannter Weise hergestellt.[7] (Thomae, 1906, pp. 434–435)

> Herr Thomae schreibt: "Das Zeichensystem des Rechenspiels wird aus den Zeichen
>
> 0 1 2 3 4 5 6 7 8 9
>
> in bekannter Weise hergestellt." Wenn er einfach sagte, das Rechenspiel habe als Spielgegenstände jene Ziffern, so wären wir zufrieden. Nun scheint er aber sagen zu wollen, daß die Spielgegenstände aus diesen Ziffern hergestellt werden, und zwar in bekannter Weise.

> Wie soll uns die Sache bekannt sein, da wir das Rechenspiel doch erst kennen lernen wollen? Herr Thomae macht hier den immer bei ihm wiederkehrenden Fehler, das als bekannt vorauszusetzen, wozu er erst den Grund legen will.[8] (Frege, 1908a, p. 52)

No one familiar with Frege will miss the analogy with his correspondence with Hilbert:[9]

> Sie sagen weiter: "Ganz anders sind wohl die Erklärungen in §1, wo die Bedeutungen Punkt, Gerade, . . . nicht angegeben, sondern 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.[10]
>
> (Hilbert to Frege, 29 December, 1899)

By way of introducing our third protagonist, Wittgenstein answered Frege's point as follows:[11]

> 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*. (Wittgenstein, *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 absence supposedly 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, then, 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.[12] In sum:

- For Frege, the use of symbols *follows from their meaning*, given by their external referents;
- For Wittgenstein, the use of symbols, as determined by certain rules, *is their meaning*.

As will be recalled in more detail below, late Wittgenstein lifted this principle to a more general theory of meaning, which applies not just to mathematical expressions but to more general linguistic expressions, whose meaning (allegedly) is not derived from the possible objects they represent (even if these exist), but from the rules that govern their use. Both the negative and the positive part of this position derive from long philosophical traditions: the former, going back at least to Kant, challenges what Sellars aptly called 'the Myth of the Given';[13] the latter seems more uniquely associated with Wittgenstein himself but is also related to forms of American Pragmatism.[14]

We already encountered our other protagonist, Hilbert: if (with the possible exception of Euclid) any mathematician in history contributed to the idea that mathematics is a rule-governed practice, it was him. Unfortunately, Hilbert's role in textbook philosophy of mathematics is merely that of a formalist, indeed as the pinnacle of formalism, culminating in what is called "Hilbert's program", i.e. his attempts to give a finitist proof of the consistency of classical mathematics.[15]

This "program" hardly plays a role for us, although it will be mentioned as an example of the idea that (inferential) rules could be different for "the game" itself and for Frege's "theory of the game" (which in this case would be Hilbert's metamathematics). From our point of view, it is Hilbert's much more general program (without scare quotes!) of *axiomatization* that provides various keys to understanding the precise way in which mathematics is a rule-governed activity:

1. Through his concept of an *implicit definition*, as exposed in the above fragment of his correspondence with Frege (as well as in Wittgenstein's comments on chess) Hilbert clarified the nature of (non-abbreviational) *definitions* as normative rules for the use of symbols.

2. Throughout his career (starting with Euclidean geometry and ending with quantum mechanics, covering a stupendous amount of mathematics and physics) Hilbert studied the *origins* of axioms in both physics and in sufficiently mature informal mathematical theories.

3. Hilbert's identification (following Frege and Russell, and followed by Gentzen's simplification through natural deduction) of first-order logic as the logic of deduction in (classical) mathematics was a decisive step in stating inferential rules for mathematical proof.[16]

See §3 for details and references. But even all of this is not enough to understand applied mathematics and mathematical physics within a view of mathematics as a rule-governed framework. The easy part is that, as explained by Hilbert, the underlying pure mathematics often originated in experience; this is even true if the application in question came only centuries after this origin and/or was unrelated to the original empirical source.[17] But the difficult part is to match a non-referential account of pure mathematics with its apparent representational role in describing the physical world.[18] This can be resolved by another Wittgensteinian idea, to the effect that mathematical theories do not represent the world, but merely provide *yardsticks* (or *objects of comparison*).[19] A yardstick does not *represent* reality, but merely *measures* it. Applied mathematics and mathematical physics therefore have a complicated relationship to reality that cannot be captured by some naive kind of referential theory, but requires a matching procedure in which the human agent is an intermediate between mathematics and the physical world: the latter is initially studied by extracting *data models* from physical phenomena as they appear to us, which the former tries to match with the predictions of *mathematical models*. Here we draw on ideas of van Fraassen (see §3). As recognized by Hilbert (from a different point of view), the power of applied mathematics and mathematical physics then comes from the fact that the process of proposing and using appropriate yardsticks is a progressive practice, in which the axiomatizations defining the yardsticks (which are mathematical theories) improve upon measuring "reality" with them.[20]

Using relevant parts of pure mathematics as yardsticks in applying mathematics to the physical world overcomes the problem (shared, *mutatis mutandis*, with platonism) that, once formalized as rule-governed language games, mathematics seems to be timeless, non-spatial, acausal, etc. (as the platonists have it), and yet can be applied to a causal world in space and time: the (dis)solution (once again due to Wittgenstein) is not to confuse properties of a yardstick (or other object of comparison) with properties of what it measures (or is compared with).

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*.[21] The motley we have in mind is a branched one, like a tree. At the multiple roots one finds foundational theories or overall formal and logical frameworks for mathematics like ZFC set theory.[22] Above these roots, interwoven individual areas of mathematics spurt out. Within most of these areas (or even within the foundational theories at the bottom, one has two absolutely crucial language games: one concerns "pure" mathematics with its formalized proofs, whereas the other governs "applied" mathematics (including mathematical physics).

Hence even the latter falls under the game analogy; but this is of course only possible because–despite the (intended) acoustic resemblance between *Sprachspiel* and *Schachspiel*–Wittgenstein's concept of a *language game* (see §2) vastly generalizes the concept of a game.

Our picture is dynamical. It 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 peacefully support each other:

- *Formalism* as the language game governing pure mathematics grounded in proofs.
- *Structuralism* entering both the "pure" language game via Hilbert's notion of implicit definitions and the "applied" one via van Fraassen's structural empiricism (see §3).
- *Philosophy of mathematical practice*, which resonates with our aim (which was also Wittgenstein's) to describe mathematics *as it is*, seeing it as a human practice grounded in history.[23]
- *Intuitionism* comes in twice: (i) as one of the possible language games giving a foundational framework; and (ii): even if the latter is classical, it may provide the logic of the metamathematics used to analyze the framework (and similarly for *finitism*, as Hilbert tried).

Rather than contradicting each other, such "philosophies" in fact complement and reinforce each other, as Hilbert himself clearly saw at least for the first and the last, whilst embodying the second. But note that despite our emphasis on rule-following we do not advocate some kind of deductivism.[24] Apart from the lack of embedding of this philosophy of mathematics into *both* pure and applied mathematics that is typical of its literature, even within the formalist language game for theorems and proofs, theorems (or what they say) are not "true" for us; as explained in §5 it would lead to considerable problems if they were. Theorems lack essential properties of things that have a truth value. What may be true is the claim *that some sentence is a theorem within a specific formal system*: it is not the *content of a theorem* that is true but the *proposition that it is a theorem*. This is how one finds truth through proof. Whilst admitting that this kind of truth is remote from the necessity that mathematics has in the eyes of a platonist, it shares the advantage that we should all agree about it: even Brouwer should admit that Hilbert's theorems are correct *by his own standards*, and *vice versa*. But . . . this is the case *provided we can indeed check that certain rules have been followed*. Like Hilbert and Wittgenstein, we therefore claim that the reliability or certainty of some proof (and for Wittgenstein even its existence) is determined by its surveyability. But this notion is problematic and in fact Hilbert and Wittgenstein held almost opposite views about it. Hence we reconsider surveyability using the more recent notion of proof verification.

In further support of our proposal, let us address the main questions in the philosophy of mathematics from our point of view, and contrast our answers with typical platonic ones.[25]

1. *Ontology:* What is mathematics *about*? We adopt the following definition of what it means for mathematics (or some language game) to be *about* certain things "external to it":

   > 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. (Mühlhölzer, 2012, p. 114)

   Refining the question: Do "abstract mathematical objects" exist? Here, abstraction may be taken to mean: not located in space and time and not participating in "causal relationships".

2. *Epistemology:* How, and what, can we know about mathematics? Is mathematics *a priori*, in that it does not rely on experience or experimentation? Do we *discover* or *invent* it?

3. *Truth:* What is the nature of mathematical truth? Is it ontological, or epistemic? In the first case, how can we know it? Either way, are the mathematical truths we (claim to) know "certain"? Are mathematical truths *necessary*, in that *they could not have been otherwise*?

4. *Applicability:* What makes applied mathematics and mathematical physics possible?

Platonists like Frege answer these question from the belief that mathematics is *referential* in a very specific way, namely that mathematical language refers to real and *abstract* mathematical objects that "exist" outside this language in the above sense.[26] Mathematicians refer to and describe these objects; they therefore *discover* (rather than *invent*) mathematical truths, which simply 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.[27]

This is not the place to criticize platonism in any detail; our purpose is to offer a viable alternative, which is especially intended to preserve a healthy balance between pure and applied mathematics (which has been a problem for platonism from the start), and explain the relationship between the two (and hence in particular account for the possibility of mathematical physics). Thus we emphasize the fourth question, but we hope to provide convincing answers to all questions. Let us announce and summarize these answers, to be fleshed out in the course of the paper.

First, we follow Wittgenstein in warning against the confusion that, despite their superficial structure, mathematical expressions are (primarily) referential. Numerous problems are created by assuming the ethereal "existence" of mathematical objects (or "thoughts") in the platonic sense. Moreover, we do not even believe that mathematical objects exist in the (natural) world, which has no mathematical–or for that matter linguistic–structure "in advance". The things mathematics *talks* about are similar to chess pieces. Their physical or spiritual embodiment is irrelevant. Yet mathematics *is* not about these pieces themselves in whatever incarnation. It is about the rules they are subject to; and the pure side of mathematics *is* these rules. Despite his completely different philosophical agenda, Husserl captured the nature of this kind of "existence", delicately balancing between the subjective and the objective as it does, quite well in terms of his notion of *identity*:[28]

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

In this view, we *know* about 'mathematical items' simply because we *invented* them! In other words, mathematics is seen as a human practice, so that the epistemological question hardly arises. Our answers to the third and fourth questions have already been summarized.

We elaborate our proposal in the remainder of this paper. We start with a summary of (late) Wittgenstein (§2), using both his philosophy of language and his philosophy of mathematics. We also review the Brandomian turn to *inferential* rule-following; without taking sides in the philosophy of language, this should be uncontroversial as an appropriate move in the philosophy of mathematics. This is followed (§3) first by a summary of Hilbert's views on the foundations of mathematics in so far as these are relevant to our program, second by the addition of some ideas of philosopher of science van Fraassen to these views, and third, by sketching our ensuing concept of applied mathematics. We then present our picture of mathematics as a "motley of language games" (§4). We continue with an analysis of truth (§5) and certainty (§6), to end with a Conclusion (§7).

2 Wittgenstein and Brandom: Rule-governed language games

For us, Wittgenstein's late philosophy is an important inspiration for answering the main (traditional) questions in philosophy of mathematics in a way diametrically opposed to Frege, who advocated a referential theory of meaning.[29] This is hardly surprising, since Wittgenstein's late philosophy partly originated in his middle-period reflections on the foundations of mathematics, which like his philosophy of language challenges referential ("Augustinian") theories.[30] In turn, Wittgenstein's late philosophy was preceded–and surely influenced–by his thinking about the abstract and axiomatized form of mathematics often associated with Hilbert.[31] In his middle period this initially led him to comparing mathematics favourably with chess, as our Introduction above recalls. But eventually such a view was replaced by attempts to ground mathematics in practice. The following passage provides an excellent summary of this crucial shift in Wittgenstein's philosophy of mathematics, which we need to understand in order to correctly incorporate his ideas:

> 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).

Apart from Plato, Frege, and Russell, Hardy was one of Wittgenstein's favorite targets,[32] and their exchange remains a good starting point for introducing some of Wittgenstein's philosophy:

> 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'. (Hardy, 1940, pp. 63)

An here is Wittgenstein's reply, from lectures on the foundations of mathematics in 1939:[33]

> Consider [Hardy (1929)] and his remark that "to mathematical propositions there corresponds—in some sense, however sophisticated–a reality". (...) 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. (Wittgenstein, LFM, pp. 239–240)

Wittgenstein claimed that philosophical problems originate in misunderstandings of language throughout his career (and hence disappear of these misunderstandings are clarified). In the *Philosophical Investigations* he specifically warns against mixing up different *language games*:[34]

> seltsam erscheint der Satz nur, wenn man sich zu ihm ein anderes Sprachspiel vorstellt als das, worin wir ihn tatsächlich verwenden. (Wittgenstein, PU, §195).[35]

As we shall see, the apparent tension between pure and applied mathematics has a similar root.

Although Wittgenstein himself refrains from giving a definition–and would surely regard any such definition as misguided, since games, languages, and language games are among his main examples of *family resemblances*, which somehow defy definition–we try nonetheless:[36]

1. A language game is a *practice* where certain words and symbols are *used*.[37]

2. The *meaning* of (most) words and symbols is given by their *use* within such a practice.

3. Sentences are the smallest units whose use counts as making a move in a language game.[38]

4. This *use* is determined by specific *rules* (forming the *grammar* of the language game).[39]

In mathematics, we add a further point, consistent with Wittgenstein but due to Brandom (2001):

5. These rules are *inferential*: that is, the meaning of sentences lies in their inferential role.

In fact, Brandom proposes this for language and social practices as a whole, but whatever the value of his proposal in such a general context,[40] its seems appropriate to mathematics. Indeed, one of the original sources of this idea was Gentzen's proof system of *Natural Deduction* in logic, where each logical symbol has an introduction rule and an elimination rule. These are rules of inference for its use, from which its 'usual' meaning follows.[41] For example, the introduction rule for the implication sign $\rightarrow$ states that $A \rightarrow B$ follows if from A *as a hypothesis* one can infer B, whereas its elimination rule is the *modus ponens* (i.e., A and $A \rightarrow B$ imply B). Since inferring B from A of course involves many other rules, this also shows that rules of inference form a network that can only be used as a whole. The shortest characterization of inferentialism is arguably that Brandom extends the attribution of meaning through use in making inferences from the logical to the non-logical vocabulary,[42] extending the idea of introduction and elimination rules to the effect that some sentence is either the premise or the conclusion of some inference. For example:

> The content of a concept such as temperature is, on this view, captured by the constellation of inferential commitments one undertakes in applying it: commitment, namely, to the propriety of all the inferences from any of its circumstances of appropriate application to any of its appropriate consequences of application. (Brandom, 2007, p. 653)

Once again, while in the philosophy of language this generality may be criticized, in mathematics this seems obviously correct (and yet Wittgenstein did not make this explicit, as far as we know).

In both cases, the key point found in both (late) Wittgenstein and Brandom is that language–or mathematics–is primarily a (rule-governed) *practice*; if one looks for a "foundation" of language–or of mathematics–then this practice (rather than some logical formalism) *is* its foundation.[43] Although in our view this is obvious for mathematics from its history, for platonists this is a scary thought. As a perhaps provocative illustration of what we sign up to, we quote from an article about free will that does not even refer to inferentialism, but nonetheless seems appropriate:

> Consider the category of things that are "fashionable." In any given culture, at any given period of time, there are a wide variety of things and/or doings (clothes, music, paintings, performances, manners, pastimes) that are fashionable. The fashionable is a real property-implicating category–that is to say, it picks out actual properties in the things and doings themselves. If we are appropriately in the know (i.e., understand the norms that govern what is fashionable), then we can certainly sort the various things we encounter into the fashionable and unfashionable, according to properties those things actually have. Happily, we can also change something that is unfashionable into something fashionable simply by altering its (objective) properties. Furthermore, in case this has gone unnoticed, the fashionable is clearly a socially significant category: It is important to us in a variety of ways, important enough that we have a number of emotional responses "appropriately" tuned into the relevant properties in our environment. (Not for nothing is the unfashionable regarded as "naff," "geekish," or "daggy", and our feelings towards those so designated, especially amongst certain constituencies, often a combination of pity and humorous contempt.) And yet despite all this, there are no objective features of things in the world that somehow shape or govern what norms to embrace regarding what is fashionable (deluded fashionistas may disagree with this point, but I trust the rest of us would not). The explanatory arrow goes the other way. It is the norms themselves–however these are determined–that determine or stipulate the features of things or doings that (here and now) make them fashionable. (McGeer, 2018, p. 303)

So far so good; mathematicians and philosophers frown on that kind of fashion. But continue:

> Consider the category of sentences that are "grammatical." In any given language, at any given period of time, certain sentences are grammatical; others are not. Again, this is a real category, picking out actual structural features of sentences in and of themselves. So if we understand the norms, or rules, that govern what is grammatical in a language, we can sort the sentences of that language accordingly; likewise, we can turn ungrammatical sentences into grammatical ones by adjusting the relevant (objective) properties. The grammatical is also a socially significant category: It is important to us in a variety of ways, important enough that we may even have emotional responses (whether pro or con) "appropriately" tuned into the relevant features of spoken and written language ("doesn't he sound like a toff, minding his p's and q's?!"). Yet here I trust it is even more obvious that there is nothing about the way words are put together in and of themselves that somehow shape or determine what norms (or rules) to embrace regarding what is grammatical in a given language. Again, the explanatory arrow goes the other way. It is the norms themselves–however these are determined–that determine or stipulate the features of sentences that (here and now) make them grammatical. (p. 303)

This sounds quite a bit more threatening, since the astute reader will have inferred at once that on our view this is going to apply also to mathematics! Mathematics is comparable to fashion, and its journals are similar to *Vogue*. In that light, comparisons with linguistic grammar may be taken as a compliment.[44] As already mentioned, having such an attitude towards mathematics seems to make it all the more difficult to explain how mathematics can be such a powerful tool for describing the physical world (whose existence we do not doubt). This is possible if we indeed regard mathematics as a tool, especially as a *yardstick*, as opposed to a *picture* or a *representation* of "reality". This idea may be found in Wittgenstein's late philosophy of both language and mathematics, and in our view is one if his most profound insights. Starting with the former:[45]

> §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.[46]
>
> §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.)[47] (Wittgenstein, PU)

Thus language games–or at least the 'clear and simple' ones–are tools of *examination*: they provide benchmarks or yardsticks with which some linguistic practice can be *compared*. Similarly, Wittgenstein considers mathematical theorems (or propositions) as yardsticks:[48]

> Ich will doch sagen: Die Mathematik ist als solche immer Maß und nicht Gemessenes.[49] (Wittgenstein, BGM §III.75h)
>
> 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.[50] (Wittgenstein, BGM, §VI.23c)
>
> 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.[51] (Ts-212,XV-114-5[1])

In other words, $25 \times 25 = 625$ just means that the computation was done according to the rules. This is very different from the platonic view that $25 \times 25 = 625$ refers to some external reality about ethereal numbers. Wittgenstein also notes that the formal result originates in experience, but has subsequently 'hardened into a rule'. Once it has become a rule, it has become a piece of mathematics that as such is no longer subject to checks by experience. Similarly, Gauss's measurement 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 showed that Euclidean geometry is an appropriate object of comparison for the local geography.[52]

The yardstick idea applies even to *counting*. Counting establishes a *comparison* between a finite "collection" A of say apples and some set $n = \{0, 1, \ldots, n-1\}$ defined in ZF set theory. Now unlike n, A is not a set in ZF, in which elements of a set are also sets. Hence n really *measures* A.

The result of such a comparison could be a perfect match, but usually it is at least as interesting to find out where the *differences* with the yardstick lie.[53] Wittgenstein illustrated this view only in elementary examples, but the yardstick view seems particularly appropriate in modern physics, at least if one does not believe in theories of everything (TOE) or other "final theories."[54]

For example, the *Standard Model of elementary particle physics* was developed in the 1960s and 1970s, and turned out to be astonishingly accurate, culminating in the discovery of the Higgs boson in 2012 at CERN. It was hailed as the truth. But today, physicists *hope* that experimental results from colliders or cosmic rays *violate* the predictions of the model! For that would be the only way forward: the model is a yardstick against which new empirical information is held.

This may be an extreme case; but upon reflection, *every* physical theory has this status. Even the emergence of non-Euclidean geometry in the 19th century may be (re)interpreted in this light: the original view of Euclidean geometry as a true description of the geometry of the world gave way to a number of alternative possibilities but remained viable as a yardstick.[55] Similarly for the exact solutions of Einstein's equations for general relativity:[56] Minkowski space-time is an accurate yardstick in most places in the universe; for the solar system we use the Schwarzschild solution; near a rotating black hole one uses the Kerr solution; the expanding universe is approximately matched by the Friedman (Lemaître–Robertson–Walker) solution, etc.

More generally, we would like to advocate the use of mathematical *theories* (as opposed to *theorems*) as yardsticks. Taking the last example, (the mathematical theory of) general relativity *as a whole* is held against the universe as an object of comparison, just like Newton's theory of gravity was in the past. The latter failed on initially small details, but also its conceptual framework eventually collapsed. Einstein's theory will surely have the same fate in due course.

Apart from this, as already alluded to in the Introduction, the above concept of applied mathematics and mathematical physics resolves the tension between mathematics as being timeless, non-spatial, acausal, etc., and yet applicable to our causal world in space and time. The Wittgensteinian solution is to point out that this apparent problem is due to a confusion between properties of some language game used as a tool of examination (i.e. comparison) with properties of the things that are examined.[57] Wittgenstein warns against this for example in §114 of the PI:

> Log. Phil. Abh. (4.5): "Die allgemeine Form des Satzes ist: Es verhält sich so und so." – Das ist ein Satz von jener Art, die man sich unzählige Male wiederholt. Man glaubt, wieder und wieder der Natur nachzufahren, und fährt nur der Form entlang, durch die wir sie betrachten.[58]

Finally, we recall that, for all his earlier sympathy for the analogy between mathematics and chess and his emphasis on rules, late Wittgenstein saw mathematics as applied *by definition*:[59]

> Ich will sagen: Es ist der Mathematik wesentlich, daß ihre Zeichen auch im *Zivil* gebraucht werden. Es ist der Gebrauch außerhalb der Mathematik, also die Bedeutung der Zeichen, was das Zeichenspiel zur Mathematik macht.[60] (Wittgenstein, BGM, §V.2)

Wittgenstein's turn from his middle period autonomous calculus view of mathematics to a necessarily applied activity was arguably related to his aim of grounding mathematics in practice, but this meant that he simply seems to have taken the possibility of applied mathematics for granted:

> Die Arithmetik aber kümmert sich (wie wir alle sehr wohl wissen) überhaut nicht um diese Anwendung. Ihre Anwendbarkeit sorgt für such selbst.[61] (Wittgenstein, PG §III.15)
>
> Die *Anwendung* der Rechnung muß für sich selber sorgen.[62] (Wittgenstein, BGM §III.4f)

In our view, this is not enough, especially in combination with Wittgenstein's foolhardy dislike of set theory and other signs that he was of touch with modern mathematics. Fortunately, the precise relationship between pure and applied mathematics, which is a central question for us, was also a major theme for Hilbert, to whom we now turn for further inspiration and guidance.

3 Hilbert and van Fraassen: Pure and applied mathematics

Hilbert was not a 'formalist' limited to pure mathematics. It was one of his 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, falls eine oder die andere von diesen Annahmen aufgehoben werden muß. Dies ist die *regressieve Aufgabe* der Mathematik.[63] (Hilbert, 1992, pp. 17–18).

By axiomatization,[64] 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 different 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 Axiome erfolgen müssen und die Schwierigkeit besteht darin, die Auswahl so zu treffen, daß alle beobachteten physikalischen Gesetze logische Folgen der ausgewällten Axiome sind.[65]
> (Hilbert, 1918, p. 411)

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. Indeed, as 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)

As Majer explains Hilbert, this is a consequence of an important difference between mathematics and physics in so far as axiomatization is concerned: the former usually considers single disciplines in what he calls 'maximal conceptual purity', whereas the latter often combines and intertwines a number of mathematical theories into a single highly complicated physical theory.

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.[66]

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 nineteenth 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).

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.[67] Thus it would reflect the spirit of Hilbert's 'zweifache Aufgabe' (two-fold task) of mathematics to only list two sources of rigour in mathematics and the mathematical sciences:

(i) Defining mathematical theories by *finding* appropriate axioms from heuristic considerations;

(ii) Proving theorems, *given* these axioms (including deduction rules, themselves axiomatized).

Combining (allegedly) "pure" and "applied" mathematics was natural for Hilbert, who emphasized the *unity* of mathematics and the mathematical sciences throughout his career:[68]

> 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.[69]
>
> (Hilbert, 1918, pp. 405–406).

Having quoted this with approval, one feels sorry to say that Hilbert did not have a satisfactory philosophy of applied mathematics. He relied on the vague notion of "*pre-established harmony*", a philosophical doctrine originally going back to the monadology of Leibniz.[70] This does not help:

> [although pre-established harmony was] one of the most basic concepts that underlay the whole scientific enterprise in Göttingen, 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". (Corry, 2004, pp. 393–394)

In fact, neither Hilbert nor Wittgenstein explained how natural phenomena acquire a mathematical structure, or what this structure (once in place) refers to. This requires more analysis, which we now attempt. As the most appropriate corresponding approach to mathematical physics 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 on this view the link between mathematical formalism and "reality" consists of three steps:[71]

1. A mathematical representation of natural phenomena by some ("surface") *data model*; think of a numerical table in which the position of a certain planet in the sky is recorded on a daily basis, or of the numerical outcome of a measurement of the angles in a triangle, or of the terabytes of data produced by a an experiment in the Large Hadron Collider (LHC) at CERN. In all cases, even in the most advanced sciences, the mathematical structure of data models–typically numerical tables or more complicated arrays of numbers–is elementary.

2. A mathematical *theory* of this data model, consisting of some *abstract structure*. For example, the mathematical theory in which the first data model of the previous point is embedded could be Kepler's laws, or Newton's theory of gravity, or Einstein's theory of general relativity. For the triangle we have Euclidean geometry. For the terabytes from the LHC we have the Standard Model. *Et cetera.* As van Fraassen states, data models do not stand on their own or come out of the blue; they are typically sought on the basis of such theories. Unlike the data models, these theories can be as mathematically advanced as one likes.

3. A "user" of the data model (which may be an entire team of scientists!) acting as a "middle man" between the mathematical theory and the (*a priori* non-mathematical) phenomena. The realization that some mathematical theory is related to the phenomena it tries to describe *via a user sitting on some intermediate data model* obviates the need for chimerical philosophical constructions like platonism or some other introduction of universals.

An astronomer compares planetary positions with Kepler's laws (etc.). Gauss compares the sum $\alpha+\beta+\gamma$ of the angles in a triangle with Euclidean geometry. A particle physicist compares the outcome of some collision in the LHC with the Standard Model. Van Fraassen (1992), pp. 3–4, summarizes all of this by asking *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. If it doesn't, it is rejected.[72]

Of course, the idea of regarding relevant parts of pure mathematics as yardsticks when applying mathematics to the physical world is closely related to the philosophy of models and idealizations;[73] indeed, in our approach every mathematical description of natural phenomena is both a model and an idealization! But it not an *approximation*, since there is nothing to approximate.

4 Mathematics as a motley of language games

Hilbert's view of axiom systems (especially for physical theories) as tentative and malleable, or even 'on the border of inconsistency', resonates well with Wittgenstein's idea of seeing mathematical theorems (or, as we prefer: theories) as yardsticks or objects of comparison used in 'measuring' the natural world (or at least empirical phenomena): *deviating* from the yardstick is often as interesting as *matching* it. Combining this with §§130–131 of the PI quoted in §2, in which language games are seen as yardsticks, too, it is a natural idea that mathematical theories are in fact special cases of language games. And this is indeed what we assume. Late Wittgenstein himself phrased the overall organization of mathematics in such terms a bit differently:

> Die Mathematik ist ein BUNTES *Gemisch* von Beweistechniken. – Und darauf beruht ihre mannigfache Anwendbarkeit und ihre Wichtigkeit.[74] (Wittgenstein, BGM, §III.46a)

This sounds bizarre, especially the combination of the two sentences–unless one understands what Wittgenstein means by 'Beweistechniken', which only becomes clear from the continuation:

> Und das kommt doch auf das Gleiche hinaus, wie zu sagen: Wer ein System, wie das R[ussell].sche, besäße und aus diesem durch entsprechende Definitionen Systeme, wie den Differentialkalkül, erzeugte, der erfände ein neues Stück Mathematik.
>
> Nun, man könnte doch einfach sagen: Wenn ein Mensch das Rechnen im Dezimalsystem erfunden hätte – der hätte doch eine mathematische Erfindung gemacht! – Auch wenn ihm Russell's Principia Mathematica bereits vorgelegen wären.[75] (Wittgenstein, BGM, §III.46bc)

In other words, by 'Beweistechniken' Wittgenstein means systems of definitions that, together with the logical deduction rules in *Principia Mathematica* form the basis of new proofs of new theorems. Equivalently, if he hadn't disliked set theory so much, the opening quote of this section could simply be that mathematics is a motley of its various branches, formalized within set theory (with its associated method of proof based on first-order logic). And this richness is indeed the key to its manifold applications and importance. For a modern mathematician, this is a platitude.

Nonetheless, this view of mathematics seems too limited. Mathematics 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),[76] 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 §3) 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".[77]

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

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

This remains well within the spirit of Wittgenstein.[78] In the spirit of §§130–131 of the PI just quoted, we propose this structure not as a *description* of mathematics, but as a yardstick against which mathematics can be held, accepting possible deviations as much as points of agreements.

In other words, the interpretation of language games themselves as objects of comparison with specific parts of some language–such as mathematics–is extended to motleys of language games set as an object of comparison against the entire language. In any case, the point, of course, is to flesh this out by explaining what actually makes mathematics a "motley of language games".

To start, let us take some piece of formalized mathematics,[79] such as ZFC set theory at the top level,[80] 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):[81]

- 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;[82]
- 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 explicitly mentioned by Weyl (1926), and should be attributed to Hilbert:[83]

> 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).[84]

Thus the point (which is essentially the same as in his earlier letter to Frege from 1899 quoted in §1) is that an 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).[85] As far as we know Hilbert rarely if ever mentioned chess, but Wittgenstein, who knew Hilbert's ideas in question, made practically the same point in explicit reference to chess:

> 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).[86]

Our key argument, grounded in our brief summaries in §2 and §3, is that in our view Hilbert (implicitly) plays (at least) what we interpret as three different language games on *the same* axioms or mathematical theories (which may be e.g. ZFC set theory as a whole, or some branch of mathematics like Peano Arithmetic that is either formally seen as a fragment of ZFC, or may be taken to be a stand-alone axiomatic theory perhaps based on second-order logic):

1. The cleanest language game is Hilbert's 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). The analogy between mathematics and chess applies here, 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 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.

2. Frege's "theory of the game", which became Hilbert's metamathematics, is one level above the previous one but it also squarely lies on the formal side: although here the object of investigation is a mathematical theory, its symbols remain uninterpreted. This language game is played on the same theories as the previous one (for example, arithmetic, as in "Hilbert's program"), but it need not follow the same logic as the original game. Here one need not think of the full scope of Hilbert's program; Gödel's completeness theorem is already metamathematical, as is its special case for propositional logic (Zach, 1999).

3. A different language game underlies applied mathematics and mathematical physics and hence is crucial in understanding the relationship between mathematics and the physical world. It is the one outlined in §2, where mathematical theories (or their theorems, or even their axioms) are seen as yardsticks or objects of comparison held against Nature.[87]

And of course there are numerous other language games that are especially relevant to mathematical practice; for example, in either trying to find proofs or in creating new mathematics even the most stubborn formalist will look for interpretations and perhaps visualizations in both applied and non-applied (or not-yet-applied) fields, for links with different fields of mathematics, etc. Similarly, learning mathematics is a language game by itself (much as learning a language is, as analyzed in the early parts of the *Philosophical Investigations*). And so on and so forth.

5 Truth

What does the "formalist" language game of §4 imply for the concept of *truth* in mathematics, and how would that resonate with the "applied" language game of §3? This is a difficult question, since we acknowledge both the existence of an external world and the fact that at the same time 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 were inspired by empirical phenomena. An approach like ours therefore faces the question where there could be any room for truth in mathematics, if not in the "real" world (be it natural, platonic, or otherwise)?

Let us return to chess for inspiration. It is meaningless to say that a position p in chess is "true", but it does make sense to claim that p is *legal*, in that it arose from a game played according to the rules R. This claim, call it $R \vdash p$, rather than p itself, could be said to be true or false, and this can be established by a proof in the form of an actual (legal) chess game leading to p, cf. §4. In normal games p even arises in this way; in so-called retrogade chess problems one has to reconstruct p.

Similarly, mathematical theorems cannot be true either, since in our non-referential ideology there is no objective state of affairs they could describe correctly.[88] Like in chess, truth in mathematics cannot lie in sentences φ (such as closed formulae in first-order logic), but only in claims $T \vdash \varphi$ stating that φ is a theorem within an ambient theory T, which is supposed to include rules of inference. And this is the case (by definition) iff there exists a proof of φ according to the rules of T. Thus the only thing we can say about mathematical truth in our framework is this:[89]

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 $T \vdash \varphi$. Our only compromise towards platonism is our belief that such truth (or falsehood) is a matter of *fact*, whether or not it is *known*.[90] But this is not the truth of platonism (or of naturalistic views of mathematics),[91] which concerns φ rather than $T \vdash \varphi$, backed by a correspondence theory of truth. Our claim should similarly be distinguished from the proposal that a sentence φ *itself* (as opposed to $T \vdash \varphi$) is true iff φ has a proof,[92] as in a *deductivist* philosophy of mathematics.[93] We find this dubious:

- 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 possible 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 virtues 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,[94] 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 $(\vdash \varphi)$ OR $\vdash (\neg\varphi)$.

  It may been argued against our proposal that one can "see" that the Gödel sentence G_T (which famously expresses its own unprovability in T) is "true"; and we do not account for this fact. This is, in fact, a classical mistake, going back at least to J.R. Lucas.[95] Even if we grant some version of platonism in which certain statements about natural numbers are "true", G_T is only true in this sense *provided the formal system T in question is consistent* (if not, anything can be proved, including G_T, which is thereby even *false*. And not even Hilbert, one of the greatest mathematicians of all times, could "see" that such a system was consistent. If C_T is a formal sentence expressing consistency of T, the actual implication is $C_T \to G_T$, which can be duly proved in T, so that we would say that $T \vdash (C_T \to G_T)$ is true.

Nonetheless, one needs to get used to the idea that say $7+5=12$ is not true (but neither is it false; it is just not the kind of mathematical statement that has a truth value), whereas

$$\text{PA} \vdash (7+5=12) \tag{5.1}$$

is true, i.e., the claim that $7+5=12$ is a theorem of Peano arithmetic.[96] Further to the arguments above, which were related to the "formal" language game, let us therefore explain why $7+5=12$ has no truth value from the point of view of our "applied" language game either, cf. §3.

What could $7+5=12$ mean? We reject the existence of platonic numbers 7 and 5 that can be platonically added to yield a platonic 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 the claim $\text{PA} \vdash (7+5=12)$ rather than $7+5=12$ itself (which we see as a sentence in PA).[97]

Let us try again: don't seven apples add up with five apples to yield twelve apples? They do; on the constructive empiricist account of §3 this is an example of a data model. But this model by itself expresses neither $7+5=12$ nor $\mathrm{PA} \vdash (7+5=12)$: the former, seen as a mathematical theory, is *held against it as a yardstick*, justified by the latter. Even though this yardstick yields perfect results (which is rare and may be restricted to counting and elementary arithmetic), all we have is a match between a data model and a mathematical theory. The latter *assumed* $7+5=12$ because $\mathrm{PA} \vdash (7+5=12)$ is true; the former is, in the Wittgensteinian parlance of §2, an 'empirical proposition hardened into a rule'. But this rule (i.e., $7+5=12$) cannot inherit any kind of truth from the empirical propositions that originally inspired it, since that would confuse the physical world with the role of mathematics as a yardstick invented by humans to understand it:

> 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, but they hold it because there seems to be no way out–I am trying to show in a very general 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.
>
> (Wittgenstein, 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 inaccessible cardinals on a par with $7+5=12$ as both being "true" sounds equally wrong. The only way to get around these problems seems to 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.

Here we took the strongest case that might potentially challenge our notion of truth, namely the simplest kind of theorem in arithmetic, and even one involving very small integers. Our case is even stronger if very (impossibly) large integers are concerned; yet stronger if instead of arithmetic we use Euclidean geometry; and still stronger again if we use the Standard Model of elementary particle physics (seen as a mathematical theory); especially for the latter no competent physicist would say that its theorems are true. But our argument is uniform for all these cases.

Finally, although Wittgenstein's (late) concept of truth was very different from ours (and essentially trivial),[98] the following words apply quite well also to our proposal:

> 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)

After the anti-platonism in the first sentence, the point of the second one is that declaring a mathematical proposition to be true is a move in a language game, hence an example of rule following. In line with his general concept of a rule (embedded in a "form of life"), what is crucial here is that 'they all agree in what they do'; what the agreement is about was spelled out around the same time, namely that even adherents of different systems agree that the others played the game correctly:

> Es bricht kein Streit darüber aus (etwa zwischen Mathematikern) ob der Regel gemäß vorgegangen wurde oder nicht. Es kommt darüber z.B. nicht zu Tätlichkeiten.[99] (PU §240)

In other words, despite their vitriolic disagreements, Brouwer should respect the fact that Hilbert's theorems are valid according to his own rules, and *vice versa*. *Short of* that kind of agreement, mathematics seems impossible. But *beyond* it no notion of truth in mathematics is possible either: for who are we to tell that either Brouwer's or Hilbert's principles, and hence the contents of their theorems (if they have any, beyond their role in a web of inferences) are correct? Or, for a different example, even within classical mathematics, who can tell that the continuum hypothesis is "true"? Perhaps Gödel, but then this truth is *relative* to his own universe of "definable" sets L. And this kind of relativity is exactly what is captured by our definition of truth.

This view also resonates with the Brandomian continuation of late Wittgenstein (cf. §2):

> Brandom's project is to make norms explicit; still, he rejects any Platonism about rules; we do not act on our conceptions of rules, or on explicitly formulated rules. Instead, it is the practice of the persons or the users of language that is the final court. The normative attitude that Brandom emphasizes includes the human activity of evaluating, hence, treating our own and others's utterances as correct or incorrect. (Haaparanta, 2019, p. 5)

A similar point is made in the continuation of our earlier quotation from McGeer (2020), cf. §2:

> (1) *Individual fallibility:* Individuals can have perfectly good knowledge about the objective properties of things and yet be wrong in their judgements as to whether something has the relevant response-dictated property for category membership. For instance, I may think that an ensemble composed of a tucked-in polyester shirt, bermuda shorts, white knee-socks, and tan hush-puppy shoes will cause my fashion-conscious neighbour to sigh with envy, but I am wrong! My understanding of the norms governing what is fashionable is just completely off-base.
>
> (2) *Collective infallibility:* It is not the case that everyone could be wrong in this way. So, for instance, if my fashion-conscious neighbour happens to be the editor of *Vogue* magazine, then she and her fashionista colleagues dictate what it is to be fashionable around here: they are the norm-setters. Consequently, if my ensemble meets her approval, I need do nothing more to enter the ranks of the fashionable that what I am already doing: lo and behold, white knee-socks and tan hush-puppy shoes do the trick! Of course, norms can be established in a variety of ways: top-down or bottom-up, through explicit instruction or informal undirected coordination, by reference to some specialized subset of a population or to the population as a whole, or some combination of these. But however this process goes, there is no sense to be made of collective fallibility about what (response-dictated) properties make for category membership, because the collective generates the norms that determine exactly what, at any given time, these properties actually are.

(3) *Norm-guided property recalibration:* What counts as the relevant response-dictated property for category membership will of course evolve or change as the constituting norms of the collective evolve or change. This will be obvious, I hope, from the examples given thus far. These examples should also make clear that norms for different response-dictated categories may evolve or change at different rates: The "fashionable" is considerably less stable in this regard than the "grammatical." Different factors will affect the stability of such norms, including whether stability itself is considered an asset or a liability to how the particular response-dictated category functions within a practice or form of life (e.g., designing/manufacturing/selling clothes vs. successful communication). But, again, no matter how sticky or stable the relevant norms may be, we should not let that blind us to the fact that category membership in a response-dictated category is just a function of what the norms themselves dictate. (McGeer, 2018, p. 303–304)

All of this can be seen, *mutatis mutandis*, throughout the history of mathematics. Mathematical books and articles are full of mistakes, even those by the greatest mathematicians of all times like Euclid and Hilbert. But is is exactly as McGeer (though writing about moral responsibility) has it: individual fallibility can be identified right because of collective infallibility, which on the other hand is a dynamical notion, being subject to recalibration. In our view Wittgenstein's famous analysis of rule following in the *Philosophical Investigations*, which of course forms much of the inspiration behind this paper, could hardly have been paraphrased more clearly.[100]

6 Certainty

The notion of *necessity* (or Wittgenstein's "logical must") adds little to our concept of mathematical truth, based on proof hence on correctly following rules. The *certainty* of mathematical truth, on the other hand, is far from trivial. For both Hilbert and Wittgenstein the certainty of mathematics (construed in their own very different ways) *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 completely 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*.[101] (Hilbert, 1922b, pp. 162–163).

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 bottomline is his notion of surveyability, which was also the basis of his eventual "finitism", cf. §3 and references therein.

For the present discussion it is a moot point if Wittgenstein was a finitist, too.[102] 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. [103]

(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.

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.[104]

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 also changed the debate.[105] 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 probably read only by the authors, like the classification of finite simple groups,[106] or the stability of space-time in general relativity.[107]

Without mentioning Hilbert, Wittgenstein, or the concept of surveyability, Avigad (2021) nonetheless captures 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.[108] How do we navigate between Scylla and Charybdis? Returning to Wittgenstein, the key property of a proof is, quite simply, the following:

"Der Beweis muß übersehbar sein" – heißt das nicht: daß es ein Beweis ist, muß zu sehen sein.[109] (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.[110] 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).[111] If it contains bugs, this "probably" would have been noted in verifying the dozens of theorems whose proofs have now been checked.

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).

7 Conclusion

> What is the conclusion to which we come? Modern mathematics and physics may seem to move in thin air. But they rest on a quite manifest and familiar foundation, namely the concrete existence of man in his world. (Weyl, 2009, p. 188)

Our aim was to re-examine the question to what extent mathematics may be compared with a game. Trying to answer this question led us essentially to Weyl's conclusion just quoted. To get there, we combined certain insights by Wittgenstein and Hilbert (as amplified by Brandom and van Fraassen, respectively, the former perhaps being unsurprising here). From Wittgenstein, we took the idea that mathematics is non-referential; from Hilbert, the idea that axiomatization (a strategy vastly underrated by Wittgenstein) enables both pure and applied mathematics (including mathematical physics): these correspond to different language games, of which the "applied" one also relies on Wittgenstein's remarkable idea of using mathematical theories as yardsticks (as opposed to representations). Mathematics also incorporates various other language games, together forming what in Wittgensteinian parlance is called a "motley".

We propose this motley of language games as a yardstick itself: we invite readers to compare the mathematics they have in mind with this picture, to see where it agrees and where it deviates. This essentially makes our overall proposal immune to objections, since any kind of critique contributes to its goal of just offering an object of comparison rather than a valid description! Moreover, as we argued in the Introduction, various mainstream philosophies of mathematics find a peaceful place within the motley; we take this aspect to be a major advantage of our proposal.

Having said this, platonists will like little of it, since they already reject the starting point of ultimately grounding mathematics in human practice. In this debate, short of settling probably irresolvable disagreements about the metaphysics we propose to look at our theory of truth in §5 as a battle ground, where we carefully argue our case. Formalists may have more sympathy for our view, since we claim to have moved the problem of understanding applied mathematics within a formalist framework a step forward; here, our case studies in §2 may be worth reviewing in support of the yardstick view we took from Wittgenstein (although also formalists may disagree with our concept of truth in mathematics). Logicists, formalists, and intuitionists may no longer exist in the original form, but what is left of these philosophies is also welcomed within the motley.

Finally, although our paper is by no means meant as an analysis of Wittgenstein's philosophy of mathematics (nor of Hilbert's), we hope to have implicitly answered certain objections to it by firstly doing some cherry-picking (i.e. tacitly removing some of his more extreme and outdated views) and secondly integrating these marbles with modern ideas appropriate to contemporary mathematics (especially ideas of Hilbert's, hopefully without falling into *his* traps either).

Acknowledgement: This work originated in a B.Sc. thesis of the second author (Singh, 2023), supervised by the first, which was greatly expanded afterwards. The authors are grateful to Jeremy Butterfield, Simon Friederich, Felix Mühlhölzer, Fred Muller, Tomasz Placek, Robert Thomas, and Freek Wiedijk for comments and help. They are indebted to ushar Menon for explaining some of the ideas of Robert Brandom, cf. Menon (2024), and finally Marc Slors for drawing their unexpected attention to McGeer (2020).

Notes

[1]The review by Epple (1994) provides an excellent historical and philosophical introduction to this question; see also Detlefsen (2005) and Weir (2022). We will not discuss the the actual mathematics of games in the usual sense of the word (Berlekamp, Conway, and Guy, 1982; Wells, 2012; du Sautoy, 2023), which may be seen either as a piece of metamathematics or as a branch of applied mathematics, like von Neumann's game theory. Either way, from our point of view it is a language game, too, cf. §4 and §3, respectively.

[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).

[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, and thus are possible truth carriers. Frege (1918) moves to an uncompromising platonism, e.g., 'Without wishing to give a definition, I call a thought something for which the question of truth arises. (...) A thought is something immaterial and everything material and perceptible is excluded from this sphere of that for which the question of truth arises.' (Frege, 1956, p. 292). He even uses the (no longer usable) term 'dritte Reich' (best translated as: third Realm) as the sphere beyond the material and perceptible.

[5]For example, a Bishop cannot move to a square of a different color: this is not a rule but a consequence of the rules (albeit a trivial one). A deeper example is the fact that chess games must end after finitely many moves.

[6]See Thomae (1906ab, 1908) and Frege (1906, 1908ab). Frege (1899) is also relevant (and very funny).

[7]'Anyone who wants to ground arithmetic in a formal theory of numbers, a theory that does not ask what numbers are and what they are supposed to do, but rather asks what we need from numbers in arithmetic, will want to look at another example of purely formal creation of the human mind. I thought I had found such an example in the game of chess. The chess pieces are symbols that have no other content in the game than what is assigned to them by the rules of the game. Saying that the signs are empty may lead to misunderstandings in the absence of any goodwill to understand. So I also believed that I could view the numbers in arithmetic, seen as a game of computation, as symbols that have no other content in the game than what is assigned to them by the rules of the game or the calculation. The system of symbols of the arithmetic game is made up of the symbols 0 1 2 3 4 5 6 7 8 9 in the usual known manner.'

[8]'Mr. Thomae writes: "The symbol system of the arithmetic game is made up of the characters 0 1 2 3 4 5 6 7 8 9 in a known manner." If he had simply said that the arithmetic game had those numbers as game objects, we would be satisfied. But now he seems to want to say that the game objects are made from these numbers, and that in a known way. How should we know the matter since we first want to get to know the arithmetic game? Here Mr. Thomae makes the recurring mistake of assuming that what he wants to lay the foundation for, is already known.'

[9] 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).

[10] '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.' See Gabriel et al., 1980, pp. 12, 13, and Hallett (2010), pp. 453–454.

[11]For the analogy with chess especially in Frege and Wittgenstein see also Kienzler (1997), Mühlhölzer (2008, 2010), Stenlund (2015, 2018), 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.

[12]As noted by Kienzler (1997), §4a, Wittgenstein himself 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 or refusal to see that the rules themselves comprise the meaning of chess (even though the pieces are meaningless); which was Wittgenstein's point.

[13]See e.g. O'Shea (2021) and Brandom (2002). The original source is Sellars (1963), Chapter 5, based on talks in London in March 1956, entitled: 'The Myth of the Given: Three Lectures on Empiricism and the Philosophy of Mind.'

[14]This, and also the previous aspect, is central to the philosophy of Brandom discusses below. See also Wischin (2019) for direct comparisons between Brandom and Wittgenstein.

[15]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. Detlefsen (2005) is a survey of formalism in a broad sense. 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. See footnote 64 for additional literature on Hilbert. Although his goal of proving the consistency of classical mathematics using finitist means was never achieved (or even well defined), it was very influential and formed the basis of proof theory, which remains an important discipline of logic.

[16]See Eklund (1996), Ferreirós (2001), Mancosu (2003), Mancosu, Zach, and Badesa (2009), and Ewald (2018).

[17]This phenomenon was famously emphasized by Wigner (1960). The three main examples are the use of the conical sections of the ancient Greeks by Kepler and Newton for describing planetary motion, Einstein's use of Riemannian geometry in his theory of general relativity, and von Neumann's use of Hilbert's spectral theory in quantum mechanics. See, respectively, Dijksterhuis (1961), Janssen and Renn (2022), and Landsman (2021).

[18]There is a fine distinction between reference and representation, which is well explained by Menon (2024), p. 3: 'Roughly speaking, representationalism is the (family of) view(s) according to which the meaning of a linguistic expression is grounded in the relation of representation that obtains between an expression and the proper part of the world that that expression picks out. "Referentialism" is the special case of representationalism where the relation is reference, and the relata are, respectively, terms and parts of the world. It is the default semantic position for scientific realism.' Much of what we say about (or rather: against) reference (which we use to be on the safe side) also applies to representation, but the difference is mostly insignificant for our arguments.

[19]Replacing 'mathematical' by 'logical', this may even be claimed to be one of the key differences between the *Tractatus* and the *Philosophical Investigations* (Kuusela, 2019a, 2022).

[20]Continuing footnote 17: this insight challenges Wigner's claim that the effectiveness of mathematics in physics is 'unreasonable' and that 'the miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift we neither understand nor deserve'. For yardsticks are not within nature, like mathematical structures on naive views, but are tools to analyze it. The more abstract the mathematics, the more universal the tool. And indeed, the two deepest applications of mathematics in physics so far, viz. the last two examples in footnote 17, followed the "modernist transformation of mathematics" in the nineteenth century alluded to at the beginning of our Introduction, after which the pinnacle of abstraction–and hence, applicability!–had been reached.

[21]Here 'motley' is Anscombe's translation of Wittgenstein's 'buntes Gemisch'. See Mühlhölzer (2005), p. 66, footnote 15, for a critique of this translation. We cite from an email from Felix Mühlhölzer to one of the authors: 'Peter Hacker wrote this to me: "Anscombe's translation of 'ein buntes Gemisch' as 'a motley' is perfectly accurate. The word 'motley' has two distinct meanings. Here it means 'a great diversity of elements' or 'a heterogeneous assemblage." But all other people I consulted only saw the other meaning: referring to the traditional costume of the court jester or fool, as you say.' We find both meanings appropriate and hence use Anscombe's translation. On a different note, Gerrard (1996), p. 174, writes that 'In the middle 1930s Wittgenstein's views changed: he began to look at mathematical language as a motley of language-games.' See also Severn (1990). But as we shall see, Wittgenstein never explicitly proposed this, and although clearly in his spirit, our proposal very considerably differs from what he (or the authors just cited) did propose. See the remainder of this Introduction and §2 for further details.

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

[23]For introductions to the philosophy of mathematical practice we refer to e.g. Mancosu (2008), Hamami & Morris (2020). Pérez-Escobar (2022) argues that this philosophy resonates well with the later Wittgenstein.

[24]ISee Paseau & Fregel (2023).

[25]For a somewhat different list see Linnebo (2017), §1.1. See also Tait (2001).

[26]See e.g. Balaguer (1998), Panza & Sereni (2013), Landry (2023), and Linnebø (2023) for studies of platonism.

[27]One of the clearest statements of this belief may be found in Hardy (1940), pp. 63–64.

[28]The best (and also last) primary source is Husserl (1954). From the vast secondary literature we just refer to Hartimo (2021) for Husserl's general philosophy of mathematics, and to Hacking (2009) for the work just cited. After 40 years of study, Rota, a mathematician, began studying Husserl in 1957 and wrote the article from which we quote shortly before his death in 1999. Among top mathematicians, Weyl and Gödel (a platonist!) were also spellbound by Husserl, cf. Da Silva (2017) and Tieszen (2011), respectively. Husserl (1954) himself explained an interesting aspect of this permanence that also applies to our scenario. Historically, mathematics originated in experience and applications (and perhaps also in some kinds of mysticism, as argued by Meschkowski (1979)), which subsequently underwent a process of 'idealization', in that mathematicians encounter different drawings of a circle but identical circles, so to speak. After this first step of 'idealization' (which is relevant in the specific context of Euclidean geometry that he discusses but can be replaced by formalizing any piece of mathematics), 'objectification', or rather 'permanence' (*Immerfort-Sein*) takes place by taking into account humanity seen as an 'emphatic and linguistic community' (*Einfühlungs- und Sprachgemeinschaft*) or 'communication community' (*Mitteilungsgemeinschaft*). Permanence happens via written communication, followed by 'reactivation' (*Reaktivierung*). Everyone who has both done (research)

mathematics and played (tournament) chess will recognize this description (and may agree this is the best one can say).

[29]Scheppers (2023) argues that it is already a mistake to read Wittgenstein's philosophy of mathematics as an attempt to answer these questions, since Wittgenstein had his own questions and program. But his program and goals are not ours; we merely use certain insights from his philosophy as a source of inspiration.

[30] Weiberg & Majetschak (2022) define Wittgenstein's middle period as 1929–1936, so that *Wittgenstein und der Wiener Kreis: Gespräche* (Wittgenstein, 1984b), which records conversations between 1929–1931 and contains most of his comments on chess, fall into it, as does the *Philosophische Grammatik, Teil III: Grundlagen der Mathematik*, compiled from 1930–1933 (Wittgenstein1984c). His late philosophy is then located within 1936–1946 (or even 1951), during which he compiled the main sources of his (unfinished) philosophy of mathematics, namely *Bemerkungen über die Grundlagen der Mathematik* (BGM), 1937–1944 (Wittgenstein, 1984d) and *Lectures on the Foundations of Mathematics, Cambridge 1939* (LFM; Diamond, 1975). The *Philosophische Untersuchungen* (PU) were written between 1936–1946 (Wittgenstein, 1984a). Part I of BGM was originally intended to follow §§1–188 of the PU and hence form the second part of those, but Wittgenstein dropped this plan. 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), Schroeder (2021), and Scheppers (2023), with useful summaries by Gerrard (1996), Potter (2011), Rodych (2018), Bangu (undated), and the Introduction of Floyd & Mühlhölzer (2020).

[31]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.' ('Even in BGM III, a certain lack of relevant mathematical knowledge becomes clear from time to time, and W.'s extensive focus on the writings of Frege and Russell sometimes make his considerations a little outdated.') 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).

[32]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* 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.

[33]Following common practice, we refer to Wittgenstein's works by standard abbreviations: in the case at hand, LFM is *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, which in our bibliography is Diamond (1975). Similarly, BGM is *Bemerkungen über die Grundlagen der Mathematik* (Wittgenstein, 1984d), and PI or PU refers to the *Philosophische Untersuchungen* (Wittgenstein, 1984a). Cited only once, PG is the *Philosophische Grammatik* (Wittgenstein, 1984c). See footnote 30 for more information.

[34] See also e.g. Baker & Hacker (2009a), especially §I.4, and Mühlhölzer, 2010, especially §I.4. Wittgenstein introduced language games as a central tool of his analysis of language in his middle period (notably in the *Blue and Brown Books* from 1933–1935), generalizing this concept in the PI.

[35] '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.' (Wittgenstein, PI, §195)

[36]The closest Wittgenstein himself comes to at least a characterization of language games is his list of examples in §23 of the PI: 'Giving orders, and acting on them; Describing an object by its appearance, or by its measurements; Constructing an object from a description (a drawing); Reporting an event; Speculating about the event; Forming and testing a hypothesis; Presenting the results of an experiment in tables and diagrams; Making up a story; and reading one; Acting in a play; Singing rounds; Guessing riddles; Cracking a joke; telling one; Solving a problem in applied arithmetic; Translating from one language into another; Requesting, thanking, cursing, greeting, praying.'

[37]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.'

[38]Quoted *verbatim* from Brandom (2007), p. 652.

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

[40]This is the subject of an immense literature. Brandom (2007), Peregrin (2014) and Beran, Kolman, and Koreň (2018) are good points to start.

[41]von Plato (2013) is a nice introduction.

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

[43]For Wittgenstein's philosophy of mathematics this view is developed in detail in Mühlhölzer (2010).

[44] Brandom also stresses the *normative* character of grounding meaning in the use of inferential rules, talking about undertaking and attributing 'commitments', etc. We discuss the relevance of normativity for mathematica later in this section, when we return to Wittgenstein, and once again in our disucssion of truth at the end of §5.

[45]See Kuusela (2019a), Chapter 5, for a detailed exegesis of these paragraphs, as well as Kuusela (2022), Chapter 3.

[46]'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.'

[47]'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.)'

[48]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.

[49]'What I want to say is: mathematics as such is always measure, not thing measured.'

[50] '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.'

[51]'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`.

[52]See Bühler (1981) for Gauss. 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.

[53]See Kuusela (2019a). What Hempel wrote about Carnap's notion of *explication* also applies to such comparisons: '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)

[54]The *locus classicus* is Weinberg (1994).

[55]See for example Gray (2007) and Toretti (2019).

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

[57]See Kuusela, 2019a, §4.6, for the case of language games.

[58]'Tractatus Logico-Philosophicus (4.5): "The general form of propositions is: This is how things are." – That is the kind of proposition one repeats to oneself countless times. One thinks that one is tracing nature over and over again, and one is merely tracing round the frame through which we look at it.'

[59]On this topic see also Gerrard (1991), Rodych (1997), Mühlhölzer (2010), and Dawson (2014).

[60]'I want to say: it is essential to mathematics that its signs are also employed in *mufti*. It is the use outside mathematics, and so the meaning of the signs, that makes the sign-game into mathematics.

[61] 'But (as we all know well) arithmetic isn't at all concerned about this application. Its applicability takes care of itself.'

[62] 'The application of the calculation must take care of itself.'

[63] '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.'

[64] 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); see endnote 15 for references and the end of §3 for a brief discussion.

[65] '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.'

[66] Even in mathematics itself Hilbert acknowledged the appearance of contradictions as a historical phenomenon. But unlike physics, he apparently found contradictions unacceptable in mathematics, give his obsession of proving the consistency of classical mathematics. This marks a major difference with Wittgenstein, whose cheerful acceptance of inconsistent theories, repeated comments on the indeterminateness of decimal expansions (e.g. of π), relaxed attitude towards the possibility of rejecting a correct proof, and whose insisting that proofs change or even define the nature of what was proved, sound out of touch with modern mathematics and hence are inappropriate for our program.

[67] 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).

[68] 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. For another example see Hilbert (1900), pp. 296–297.

[69] '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.'

[70] See e.g. Pyenson, (1982), Kragh (2015), and Corry (2004).

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

[72] The division between "elementary" and "advanced" mathematics inherent in van Fraassen's program–i.e., the former being used for data models and the latter for the mathematical theories supposed to explain such models–seems to mirror Hilbert's finitist mathematics held against the ideal mathematics that is used in practice (see references in footnote 15). Either way, the intuition is that there must be some starting point for doing mathematics whose origin by definition lies outside mathematics. This starting point typically consists of numbers (Lakoff & Núnez (2000) provide an interesting though controversial perspective on this from cognitive science).

[73] See e.g. Norton (2012), de Regt (2017), Bueno and French (2018), Frigg (2022), and Schech (2023).

[74] 'Mathematics is a MOTLEY of techniques of proof. – And upon this is based its manifold applicability and its importance.'

[75] 'But that comes to the same thing as saying: if you had a system like that of Russell and produced systems like the differential calculus out of it by means of suitable definitions, you would be producing a new bit of mathematics. Now surely one could simply say: if a man had invented calculating in the decimal system–that would have been a mathematical invention!–Even if he had already got Russell's Principia Mathematica.'

[76] 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'.

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

[78]Although Wittgenstein himself is sometimes taken to have proposed that a mathematics is a "motley of language games" (cf. endnote 21), such a claim is difficult to find in his published works or in his *Nachlass*. See also Baker & Hacker (2009c), Essay I: Two fruits upon one tree, and Mühlhölzer (2010), §§I.6, II.8.

[79]Wittgenstein would already regard this starting point as misguided! Cf. Mühlhölzer (2010) and Scheppers (2023).

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

[81]What is admittedly missing in the analogy between mathematics and chess 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", after repeated failure to prove some theorem). 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.

[82] 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).

[83]This is Hilbert's famous concept of *implicit definition*. Giovannini & Schiemer (2021) call such definitions *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. The idea is also very clearly stated by von Neumann (1925), p. 220: '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.' ('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).

[84] '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.'

[85]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.' 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.See Giovannini & Schiemer (2021), §4.2, and references therein, as well as von Plato (2017).

[86]'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.'

[87]Note that 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 (Friederich proposed this on the formal side, but it equally well applies to the applies side). 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 (in classical physics one may think of (partial) differential equations like those of Newton, Maxwell, or Einstein). As noted by Friederich, this resonates particularly well with Hilbert's implicit definitions (see §3).

[88]Tarski-style truth in the sense of model theory is irrelevant here; it lies within mathematics. See footnote 67.

[89] What we propose is an example of a *disquotational definition of truth* (Künne, 2003; David, 2022).

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

[91]See Maddy (2014), whose attempt to list Wittgenstein on the naturalistic ticket is criticized by Gustafsson (2015).

[92]See e.g. Dieudonné (1971), Tait (1986), and Weir (2010). The so-called BHK (Brouwer–Heyting–Kolmogorov) interpretation (or semantics) of intuitionistic logic is also often taken to mean that a sentence is true iff it has a proof (Artemov & Fitting, 2021; Iemhoff, 2022). We reject this, too, but even so one may still support the more modest BHK interpretation of the logical connectives in terms of proofs (Aloni, 2023; van Atten, 2023).

[93]See e.g. Paseau & Pregel, (2023). Deductivism is a branch of formalism.

[94]See Paseau & Pregel (2023), §9, and references therein.

[95]See Franzén (2005), §2.9 and references therein; this book discusses many misunderstandings of Gödel's theorems.

[96]In some crazy theory T where $T \vdash (7+5=10)$, this would still be true on our criterion (as long as the proof in T is correct!). This theory might be an interesting game but it would be a useless yardstick in applied mathematics.

[97]This is the well-known "Truth/Proof problem", see e.g. Tait (1986).

[98]§136 of the PI is an important source for Wittgenstein's late concept of truth. As in the quotation from §140 in the main text, declarations of truth are themselves examples of a rule, which we endorse, but unlike our proposal Wittgenstein makes no difference between a proposition p and the statement 'p' is true, and hence Wittgenstein would call mathematical theorems themselves true. Similarly in §6 of Anhang II of BGM. As explained by Mühlhölzer (2012), at least middle Wittgenstein did not like metamathematics and hence would be reluctant to ascribe any value (let alone a truth value) to statements like $T \vdash \varphi$, which, after all, are statements in metamathematics.

[99]'Disputes do not break out (among mathematicians, say) over the question whether a rule has been obeyed or not. People don't come to blows over it, for example.'

[100]See especially §§185–242 of the PI. Baker and Hacker (2009c) remains the authoritative exegesis. For a briefer discussion we also recommend Mühlhölzer, (2010), §I.5. McGeer's 'norm-guided property recalibration' resonates with Wittgenstein's last work, *Über Gewißheit* (*On Certainty*), notably §97: 'Die Mythologie kann wieder in Fluß geraten, das Flußbett der Gedanken sich verschieben. Aber ich unterscheide zwischen der Bewegung des Wassers im Flußbett und der Verschiebung dieses; obwohl es eine scharfe Trennung der beiden nicht gibt' ('The mythology may change back into a state of flux, the river-bed of thoughts may shift. But I distinguish between the movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other.') Ironically, Wittgenstein's *On Certainty* is much less relevant to our next section on ... Certainty!

[101] '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 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*.'

[102]This question is discussed for example by Rodych (1997) and Marion, (1998),

[103] '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.'

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

[105]See Appel & Haken (1977), Robertson *et al.* (1997), Haken (2006), and Gonthier (2008) for the four-colour theorem, and Tymoczko (1979) and Shanker (1987), pp. 143–160, for the debate.

[106]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`.

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

[108] 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. Dutilh Novaes (2011) identifies eight different ways in which formality and rules can be interpreted.

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

[110] 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.

[111] 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.

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