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Was Wittgenstein a radical conventionalist?

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> Turing doesn't object to anything I say. He agrees with every word. He objects to the idea he thinks underlies it. He thinks we're undermining mathematics, introducing Bolshevism into mathematics. But not at all.
>
> —L. Wittgenstein,
> to his students in
> Cambridge, 1939

According to Michael Dummett's infamous reading of Wittgenstein's philosophy of mathematics, Wittgenstein was a *radical conventionalist* who held that "the logical necessity of any statement is a direct expression of a linguistic convention" (Dummett 1959, p. 329) and not just "consequences of conventions, but individually conventional" (Putnam 1979, p. 424, as cited by Dummett 1993). On this view, our mathematical practices determine directly, however that is specified, for each mathematical proposition individually, that it is true or false.

Dummett's reading of Wittgenstein's philosophy of mathematics is a profound one, but despite being well-grounded in the text and expounded by a philosopher with deep knowledge of Wittgenstein's philosophy, it has been almost universally rejected, mostly for philosophical reasons, however, rather than exegetical ones, and it often feels as if the reasons for rejecting it amount to nothing more than an *argumentum ad lapidem*: radical conventionalism is plainly false, and therefore it simply cannot be that a great philosopher like Wittgenstein ever held it.

In this paper, I defend the reading of Wittgenstein as a radical conventionalist, even though I deviate from Dummett in important ways. I will focus mostly on the *Lectures on the Foundations of Mathematics*,[1] and argue that there, Wittgenstein's view can best be described as one where *our agreement about the particular case* is constitutive of our mathematical concepts, and hence that *each* true statement of mathematics is directly conventional in Dummett's sense, and not a consequence of a prior adoption of a convention or rules.

After laying out my reading of Wittgenstein, I will argue that his view is actually able to withstand some of the most difficult objections that have been brought forward against the view, including those of Dummett himself. The view can therefore be made more philosophically palatable than hitherto has been supposed possible. My goal is therefore not merely exegetical, but to argue that radical conventionalism has been prematurely excluded from consideration by philosophers of mathematics and should be reconsidered.

I Defining radical conventionalism

Despite the well-known quotations I gave in the introduction, where Dummett emphasises the individual conventionality of each mathematical truth, those familiar with his reading of Wittgenstein might raise an immediate objection: While it is true that Dummett does say these things, he also emphasises that Wittgenstein thought that the necessity of a given statement is always due to our having "expressly decided to treat that very statement as unassailable" (Dummett p. 329) and that the truth of each mathematical truth is for Wittgenstein therefore an explicit *decision* on our part. Indeed, it seems likely that a major reason for the widespread rejection of Dummett's reading is the be-

1. Henceforth LFM. I will refer to the *Remarks on the Foundations of Mathematics* as RFM and the *Philosophical Investigations* as PI.

lief that this notion of 'decision' is an intrinsic part of Dummett's conception of what radical conventionalism is.

In this section, I will argue that on the contrary, the view is best understood by contrasting it with more moderate forms of conventionalism whereby each truth is indeed a *consequence* of a given convention and not individually conventional. We should accept Dummett's first two definitions which define radical conventionalism as the view that each mathematical truth is *individually* conventional and a *direct* expression of a linguistic convention. The defining aspect of radical conventionalism is thus that there is no criterion of correctness or truth for our mathematical statements outside of our mathematical practices, even when application is taken into account, and that each truth is directly determined by those practices. I will further argue that the emphasis on decision is neither here nor there, *even by Dummett's own lights*, and hence that Dummett's emphasis on choice doesn't express what is really essential about his reading.

Now, conventionalism about a particular domain is the view that the propositions of that domain owe their truth-value, in some sense or another, to linguistic conventions (see e.g. Quine 1966; Glock 2008; Warren 2015; Topey 2019), or as it is often put, that they are true in virtue of meaning (Glock 2003; Warren 2016). Dummett's own definition of radical conventionalism is motivated by what he sees as the failure of more moderate forms of the view, and radical conventionalism can therefore best be understood through the contrast with these moderate, more orthodox forms of conventionalism.

The main argument Dummett has in mind against orthodox conventionalism is what has been called *Quine's regress problem* (Quine 1966).[2] Here is how Dummett poses the problem:

> It appears that if we adopt the conventions registered by the axioms, together with those registered by the principles of inference, then we must adhere to the way of talking embodied in the theorem; and this necessity

2. It might be noted here that Putnam (1979, p. 424) dubs this the 'Quine-Wittgenstein objection' to orthodox conventionalism, as he sees Wittgenstein's considerations of rule-following as structurally similar.

> must be one imposed upon us, one that we meet with. It cannot itself express the adoption of a convention; the account leaves no room for any further such convention. (Dummett 1959, p. 329)

The orthodox conventionalist, Dummett thinks, relies on the notion of logical consequence to explain how we move from the axioms to the theorems, but logical consequence is what (among other things) the theory itself is purporting to explain and is on this view something that is imposed upon us from without, independent of our actual practice, and not a mere convention.

The dilemma then is, that we either land in a regress, explaining those background notions as being explicitly stipulated rules, leading to the same problem again, or we take these background notions for granted, at the cost of having thereby thrown away the conventionalism—as well as the very motivation for the view. We'd have, as Putnam puts it, reduced the conventionalist claim that logic is true by convention to the less exciting claim that "logic is true by conventions *plus logic*" (Putnam 1979, p. 424). According to Dummett, Wittgenstein avoids this unfortunate consequence by going in for radical conventionalism (although Dummett calls it "full-bloodied conventionalism" in his earlier paper). Dummett's first pass at explaining this view is as the view that all necessary statements are so because they have been adopted directly as conventions. For Wittgenstein, Dummett writes,

> the logical necessity of any statement is always the direct expression of a linguistic convention. (Dummett 1959, p. 329)

This definition is the one I will be adopting for the rest of the paper, and indicates, one might say, that what mostly separates the orthodox conventionalist from the radical is that for the latter, the source of the necessity of every mathematical statement is the same: there are no privileged statements from which further truths derive their necessity, everything is on one level, and each truth is determined *directly* by the convention, but is not a *consequence* of that convention.

Another way of putting the same point, is that if conventionalism relies in some sense on agreement (however that is specified) being the source or ground of truth, then the radical conventionalist position is that agreement *about the particular case*—not general rules or stipulations—is that ground. This contrasts with the moderate position, where the axioms are adopted by convention and the rest follows.

Should my reading of Wittgenstein as a radical conventionalist then not be rejected outright, given that I do not think that 'decision' was an important part of Wittgenstein's view? That would be too rash, I believe, as Dummett's notion of 'decision' is not what's *essential* about his reading of Wittgenstein, namely the contrast between orthodox conventionalism and its radical counterpart.

We can see this when we look at what Dummett thinks about conventionalism more generally. His first, and most general, definition of conventionalism does not mention stipulations, but characterises the view as one where truths (of a certain domain) are not due to an external reality, but language. But when he describes the view further, he seems to have something like explicit stipulation on Quine's model in mind and thinks of the view as one where truths are a result of having chosen certain statements (or their consequences, in the case of moderate forms) as true and exempt from counter-examples. Dummett therefore thinks that it is characteristic of conventionalism *in general* that there is an element of choice involved, since for him, the axioms are stipulated. It is therefore only natural for Dummett to speak in the same way about radical conventionalism, if he thinks that choice and stipulations play an essential role for the moderate conventionalist.

However, there is no reason for us to follow Dummett in this, as it is not necessary to think that conventionalism relies on explicit stipulations in the first place. For example, we might be conventionalists about the norms of etiquette and adopt something like Lewis's theory of convention to explain how such conventions arose and continue to exist (see Lewis 1969). There is therefore room in logical space for conventionalism without stipulation, and were we to take Dummett to seriously, we'd have not way to express this possibility.

There is a further reason for keeping the terminology. For Dummett, our normal intuitions tell us, which he himself shares, that as soon as we have laid down our rules of inference or in this case arithmetical rules, we do not have any "further active part to play" in what follows, but as Wittgenstein's considerations about rules make clear (on Dummett's interpretation) we are free to accept or reject a proof at any step, as there is

> nothing in our formulation of the axioms and of the rules of inference, and nothing in our minds when we accepted these before the proof was given, which of itself shows whether we shall accept the proof or not; and hence there is nothing that *forces* us to accept the proof. (Dummett 1959, p. 330)

If, Dummett concludes, we accept the proof, "we confer necessity on the theorem proved; we 'put it in the archives' and will count nothing as telling against it" (p. 330).

In Dummett's later paper, he does not take himself to deviate from his previous interpretation, but considers an objection to the view presented here, in order to clarify it. The objection goes as follows. Suppose, for instance, that we have accepted a proof that a cylinder intersects a plane in an ellipse. According to radical conventionalism, we have therefore acquired a new criterion for applying the term "ellipse" which we might appeal to in certain cases to say that some figure, while not looking like an ellipse, must nonetheless be one. It could then be objected that there could therefore never arise a circumstance in which a counter-example might lead us to doubt our theorem, and subsequently discover a mistake in the proof. This is ruled out, because the correctness of the proof is simply taken to be our acceptance of it as a proof, and since that very acceptance is supposed to make us rule out any counter-example *a priori*, we could never doubt our own proof. But, the objection goes, this has in fact happened many times in the history of mathematics, and so Wittgenstein is merely confusing necessity with certainty (Dummett 1993, p. 447).

But, the later Dummett argues, this is not what we should take the view to entail. We should *modify* our understanding of the view so that what we have already proved should only be taken to be *provisionally* compelling, admitting perhaps of a counter-proof or an empirical counter-example. What is important, Dummett says, is that we do not introduce the *ideal* into the account—saying that what is necessary is what the ideally competent mathematician would call necessary, for example, as that would entail that there are external standards of judgement that exist independently of us and our own mathematical practice, and *that*, Dummett thinks, is what the radical conventionalist denies.

It should be emphasised here how much Dummett's own reply to this objection jettisons the emphasis on decision as integral to radical conventionalism. If "decision at each step" really were the defining element of the view, and not the individual conventionality of each truth, as well as the rejection of external standards or the ideal, then Dummett's reply to the objection simply would not work, as the correct step is, on that reading, defined as what we have decided, and hence we would have *no* criteria at all, not even internal to the practice, to later doubt our choice, not even an empirical counter-example. Dummett himself has therefore implicitly rejected "decision at each step" as being a defining aspect of radical conventionalism (or perhaps even explicitly, as he seems to be aware of this and calls for modifying the definition of the view).

What is important, even for Dummett, is that "the logical necessity of any statement is a direct expression of a linguistic convention" (Dummett 1959, p. 329) and *not* that we have expressively decided that a given truth is one. The latter emphasis is simply ruled out by Dummett's own arguments in his later paper.

And since Dummett's own general definition of conventionalism about some domain does not depend on the notion of choice, but rather on the idea that there is nothing external to our language that determines the truth of the propositions of that domain ("all necessity is imposed by us not on reality, but upon our language") and that his argument in the latter paper against the objection that we could never reform our practice according to radical conventionalism, the idea that conventionalism in general requires deliberate choice is neither here nor there, even for Dummett himself. We should therefore not be too worried about Dummett's talk of decisions when we define radical conventionalism.[3]

For the rest of the paper, I will argue that Wittgenstein held a version of this view in the *Lectures* and I will refer to it as *radical conventionalism.* The core idea that Wittgenstein seems to defend there is that it is our agreement about the *particular case* which is constitutive of the concepts we use, and hence their correctness. This agreement is somehow rooted in our practice—what we actually do, or would find natural to do (as Wittgenstein puts it), and is meant to be understood as agreement 'in action', rather than the agreement of opinions.

II The rule-following paradox and Wittgenstein's notion of 'naturalness' in the *Lectures*

In this section, I will discuss Wittgenstein's remarks on rule-following in the *Lectures* and argue that Wittgenstein's position is that human agreement *about the particular case* plays a *constitutive* role in the determination of meaning. This idea of meaning is then carried over into the philosophy of mathematics by seeing mathematical truths as conceptual truths that only depend on the meaning of the propositions, resulting in radical conventionalism about mathematics.

The arguments and examples that make up the rule-following considerations of §§185–242 of the *Philosophical Investigations* are quite prominent in the *Lectures* and Wittgenstein returns to them time and time again. The first use Wittgenstein makes of these kinds of arguments is to demonstrate that understanding is not an occurrent mental

3. Yemima Ben-Menahem makes a similar point in her book on conventionalism (see Ben-Menahem 2006, p. 258f).

state (the subject of very much of Lecture I). He points out that if he were to teach a student how to square numbers, the criterion for the student having correctly learnt the technique cannot be that he (i.e. Wittgenstein) had thought of every step before and that the students actions match this prior anticipation—and further that he does not know anything fundamentally different about himself and his ability to follow the rule than he does about the student. We are, Wittgenstein says, inclined to think of "*meaning* as a kind of queer mental act which anticipates all futures steps before we take them" (LFM II, p. 28). This, Wittgenstein denies.

Nevertheless, Wittgenstein does not seem to want to say that every step is therefore *not* determined. He asks:

> Should one then say that if I write $y = x^2$, where *x* is to take all the intergers, that it is not determined what is to happen at any particular point? (*LFM* II, p. 28)

The ensuing discussion parallels the discussion of §189 and later in the *Philosophical Investigations* in many ways. There, as well as here, Wittgenstein seems to only allow two senses of the word "determine", namely that it may mean (a) that "people trained in a certain way generally go on writing down a certain series" and that "they all act in the same way when confronted with this formula and asked to write down its series" (*LFM* II, p. 28) or (b) as a statement about the mathematical form of the formula, i.e. to contrast formulas like $y = x^2$ and $y = x^z$—where the former does determine a unique series, while the latter determines infinitely many series, each depending on the value of z.

One might think that Wittgenstein is changing the subject matter here: we do not want know about how people *learn* mathematics nor about the mathematical question of whether a formula determines a series, but about how it is determined for every value of *x* what the combination of symbols '$y = x^2$' refers to? However, after being pressured on the point by one of his students, he says:

> "Does the formula '$y = x^2$' determine what is to happen at the 100th step?"
>
> This may mean "Is there any rule about it?"—Suppose I gave you the training below 100. Do I mind what you do at 100? Perhaps not. We might say, "Below 100, you must do so-and-so. But from 100 on, you can do anything." This would be a different mathematics.
>
> If it means, "Do most people after being taught to square numbers up to 100, do so-and-so when they get to 100?", it is a completely different question. The former is about the operations of mathematics but the latter is about people's behaviour. (*LFM* II, p. 29)

This reply is not much clearer, unfortunately, and there seems to be a tension in what Wittgenstein says here. At first, he seems to be allowing that there is a third way to understand the question, showing that he's not simply forgetting this possibility, but then again only seems to offer these two alternatives: either the question is about people's behaviour (that there is a rule about it seems to be reducible to people's training) or it is a question internal to mathematics. After a brief interlude about the role of intuition in rule-following, he says:

> But a man is only said to know by intuition that $25 \times 25 = 625$ if 625 is in fact the result which we all get by calculation. But a man is said to know $1 + 1 = 2$ not because two is in fact the result which we can reach by calculation—for what sort of calculation should we use?—but because he says with the rest of us that $1 + 1 = 2$.
>
> The real point is that whether he knows it or not is simply a question of whether he does it as we taught him; it is not a question of intuition at all. (*LFM* II, p. 30)

He then goes on to say that following '|, ||' by '|||' or going from '1 to 2 to 3, etc.' is "more like an act of decision than of intuition". That does certainly seem like evidence in favour of Dummett's reading of Wittgenstein's radical conventionalism, rather than mine, namely that there is an act of decision involved, but Wittgenstein then immediately clarifies:

> But to say "It's a decision" won't help [so much] as: "We all do it the same way". (*LFM* II, p. 31)[4]

Again, the idea that 'we all do it the same way' seems to be how Wittgenstein wants to solve the problem of rule-following—that somehow the very fact that we all do it in the same way is constitutive of the correctness conditions of the rule, and at the start of the next lecture, Wittgenstein summarises his discussion of the matter in a way that makes his emphasis on training come out quite clearly:

> We saw that the word "determine" can be used in two different ways. One can ask "Does my pointing determine him to go in a certain direction" and mean by that question either "Will he (or most people) go in a certain direction when I point?" or "Is one trained in such a way that, when I point, it is correct to go in a certain direction and incorrect to go in other directions?" (*LFM* III, p. 32)

Wittgenstein returns to the question of rule-following again in Lecture VI in the context of a discussion about the notions 'same', 'analogous' and 'similar'. Here, Wittgenstein gives perhaps the most explicit formulation of the view I'm attributing to him in the *Lectures*, namely that training and human agreement about a particular case is constitutive of correctness in the use of symbols. After describing a case where one shows a partner how to do certain movements and then asking them to do the same thing, and explaining how they might then misunderstand any such instruction, but typically don't,

4. It should be noted that there is some doubt that this section was reconstructed correctly from the notes of the students. I'm relying on the *content* having been reported correctly, which seems plausible given that Wittgenstein says similar things often (see editor's footnote on p. 31 of LFM).

Wittgenstein says, and it is worth quoting at some length:

> Similarly one can show a child how to multiply 24 by 37, and 52 by 96, and then say to it, "Now multiply 113 by 44 analogously." The child may then do one of many things. If he can't justify his action, we should go through it again and again, until we converted him to doing the same as us. *The only criterion for his multiplying 113 by 44 in a way analogous to the examples is his doing it in the way in which all of us, who have been trained to do it the same as us, would do it.* If we find that he cannot be trained to do it the same as us, then we give him up as hopeless and say he is a lunatic. (*LFM* VI, p. 58. Emphasis mine.)

Here, Wittgenstein clearly and explicitly says that the only criterion for whether or not a rule has been followed (i.e. continued in the analogous way relative to the set of examples shown) in the *particular case of multiplying 113 by 44* is whether or not the rule-follower in question does it in the same way as *all of us* who have received the same training would do it. Wittgenstein's use of the counterfactual is also significant, since he doesn't seem to be saying that this agreement needs to be manifested in every case to do the work it is meant to do.

Later in the same lecture, Wittgenstein discusses this picture in relation to mathematical proofs and explicitly uses the word "convention" to describe his view:

> Mathematical conviction might be put in the form, 'I recognize this as analogous to that'. But here "recognize" is used not as in "I recognize him as Lewy" but as in "I recognize him as superior to myself". He indicates his acceptance of a convention. (*LFM* VI, p. 63)

And given what Wittgenstein had just said, that the only criterion for doing the same thing in a particular case is to do what others do, it seems reasonable to suppose that view he is advancing is indeed what I described as the one of the natural components of the radical conventionalist position—that without agreement about a particular case, there is no right or wrong, correct or incorrect.

In Lecture X, Wittgenstein essentially makes this point in a discussion on the difference between experiment and calculation. He has made the point that in calculations there is such a thing as right and wrong, while in experiments there is not, and then considers a case where multiplication is being invented and that so far only numbers below 100 have been multiplied together. He then considers a particular case, namely 123×489, and suggests that we might ask someone to do the same thing for these two numbers as we did for the numbers below 100. This, Wittgenstein says, would be an experiment, but one whose result we might adopt as a calculation. He explains:

> What does that mean? Well, suppose 90 per cent do it all one way. I say, "This is now going to be the right result." The experiment was to show what the most natural way is—which way most of them go. Now everybody is taught to do it—and *now* there is a right and wrong. Before there was not. (*LFM* X, 94)

Here, Wittgenstein is again explicitly considering a particular case and says that *if* everyone is taught to do it that way, then that is the correct way—and further that the correctness is constituted by that agreement ("before there was not").

The alternative reading, that Wittgenstein is in fact not considering agreement about a particular case as constitutive in these examples, but the general way how subjects of the experiment handle numbers greater than 100, is not plausible, for two reasons. The first is that Wittgenstein even more explicitly considers another particular case a little later in his discussion, namely the case of '$12 \times 12 = 144$':[5]

5. Severin Schroeder, for example, reads Wittgenstein in this way and criticises the alternative reading as incoherent. He writes:

> Empirically speaking, there is no social agreement on this particular sum, there is social agreement only on the general principles of multiplication (Schroeder 2017, p. 95)

True enough, but what are we agreeing about when we agree that the general principles of multiplication go *like this*? Surely it means, in some sense, that we agree about individual cases, for suppose we do not

> Russell said, "It is possible that we have always made a mistake in saying $12 \times 12 = 144$." But what would it be like to make a mistake? Would we not say, "This is what we do when we perform the process which we call 'multiplication'. 144 is what we call 'the right result' "?
>
> Russell goes on to say, "So it is only probable that $12 \times 12 = 144$." But this means nothing. If we had all of us always calculated $12 \times 12 = 143$, then that would be correct—that would be the technique. (*LFM* X, p. 97)

Here, Wittgenstein is clear that *if* our practice were such that we would all say that '$12 \times 12 = 143$', a particular case different from what we actually do, then that would be correct. The point is quite obscure, I believe, because it is not expressed with much care for the distinction between use and mention. The claim is not that we decide what the outcome is, but rather that our agreement about the particular case determines to *which* concept the symbol '$\times$' refers to in the first place—if we would all say that '$12 \times 12 = 143$', then '$\times$' would refer to the concept which gives that as the correct answer, and if we all say '$12 \times 12 = 144$', then that concept is *multiplication*. We are not simply agreeing *that* $12 \times 12 = 144$, but rather it is our agreement about this particular case that constitutes the fact that our practice is multiplication, and not a deviant one.[6]

This part of the lectures is pivotal for Wittgenstein's argument, I believe, so it is worth going over the point once more. We can helpfully imagine that words referring to concepts behave like rigid designators in that they pick out the same concept in every possible world, given their meaning in the actual world. In the actual world, the symbol '$\times$' and the term *multiplication* refer to a function where '$12 \times 12 = 144$'—that is what multiplication is. Wittgenstein's claim, as I read him, is that if our practice was such that we always calculated '$12 \times 12 = 143$', then the symbol '$\times$' (and our word

agree—then there must be some *particular* case that we do not agree on, otherwise we would agree on every case. This shows that agreement about how a rule goes *in general* depends on agreement in particular cases.

6. Wittgenstein is clear that if a single individual in the community would say that $12 \times 12 = 143$, contravening this consensus, then what they did should count as a mistake. But he does not say what the consensus consists in—is it a simple majority? Everyone except one?

'multiplication') would refer to that different function. That function is not *multiplication* (since that refers to a different function in the actual world, namely one where '$12 \times 12 = 144$') but *if* our practice was such, then the word 'multiplication' would have referred to that function—and hence had a different meaning in the actual world.

Our agreement about the particular case, Wittgenstein seems to be saying, is constitutive of what our practice is, that if we would all do it one way, then *that* would be our practice, rather than a mistake in another practice—our actual practice where $12 \times 12 = 144$. That is to say, if we let $\times_{144}$ be the multiplication function and $\times_{143}$ be a function that agrees with the multiplication function in every place, except let $12 \times_{143} 12 = 143$, then the claim is that our agreement determines which of these we are in fact referring to when we use the symbol '$\times$', and so if we would agree to the proposition '$12 \times 12 = 143$', then that would thereby show that the function we refer to by '$\times$' is not $\times_{144}$, but $\times_{143}$—our agreement fixes the reference of the symbol '$\times$', not *that* $12 \times 12 = 143$. If we did so agree, then that would be our practice, and hence correct.

This is tantamount to claiming that nothing outside our mathematical practice, not even prior committments, can serve as a criterion of correctness in individual cases, since otherwise it *would* be conceivable that we could make such a mistake as Russell suggests—and that is of course just an expression of radical conventionalism. The point is worth emphasising: For Wittgenstein, it is inconceivable that we would all make a mistake in a relatively basic case, since if we would all judge that $12 \times 12 = 143$, for example, that would therefore be our practice in use of that symbol, and hence correct. The practice is here the only criterion of correctness, as per the definition of radical conventionalism. It does *not* follow, however, as per Dummett's discussion of the case where we might change our mind about a previously accepted proof, that we cannot make a mistake in *every* case.[7]

7. Although, it is quite unclear *how* Wittgenstein thinks that we can make mistakes in mathematics, both as individuals or as a community. I say more about this below, but I believe that his notion of 'naturalness'

The other reason for rejecting the reading that Wittgenstein means that agreement about the general way we proceed is constitutive of concepts, and not our agreement about the particular case, is that he discusses this explicitly and argues against it, via a regress argument. Turing had pointed out that we could never stipulate that every single multiplication is true ("deposit them all in the archives" to use the terminology of the *Lectures*), prompting Wittgenstein to reply:

> Well, what then?—This is like counting to a number which has not been counted to. Now what is it that we are going to deposit in our archives? We might say, "We are not going to deposit single multiplications, but only general rules." (LFM XI, p. 105)

He then says:

> But let us go into this question. We have the metre rod in the archives. Do we also have an account of how the metre rod is to be compared with other rods? There might be a point sometimes in putting an account—say, a picture—of the way in which we compare them; or instruments used for this purpose. Couldn't there be in the archives rules for using these rules one used? Couldn't this go on forever? (LFM XI, p. 105)

Because of this regress, Wittgenstein seems to suggest, general rules can therefore not be what lies at the bottom of our rule-following practices, even if it might sometimes be useful to make use of such things. Instead, Wittgenstein says, that as a matter of fact, we are all inclined to continue the finite examples we have seen in the same way.

But how do we move from facts about all of us agreeing to a certain result—itself an empirical fact, not necessarily manifested in our behaviour—to the corresponding mathematical proposition according to Wittgenstein? This is important, because mathematical propositions aren't *about* our agreement that is meant to ground them. On this

can be made out to be thick enough to allow for the possibility of mistakes.

matter, he says, and it is again instructive to quote at some length:

> It has been said: "It's a question of general consensus." There is something true in this. Only—what is it that we agree to? Do we agree to the mathematical proposition, or do we agree in *getting* this result? These are entirely different. [...] Mathematical truth isn't established by their all agreeing that it's true—as if they were witnesses of it. Because they all agree in what they do, we lay it down as a rule, and put it in the archives. Not until we do that have we got to mathematics. One of the main reasons for adopting this as a standard, is that it's the natural way to do it, the natural way to go—for all these people. (*LFM* XI, 107.)

This difference in kinds of consensus, that something is a matter of opinion or "agreement in witnessing" on the one hand, and agreement in what we do on the other, is one that Wittgenstein often emphasises (see below and e.g. *PI* §241). It is not clear, however, what this difference is, and Wittgenstein often struggles to articulate it. From the context, however, it is clear that he thinks that it can solve the problem that Turing had brought up and he again emphasises 'naturalness' in proceeding as a constitutive factor in concept determination.[8]

In a later lecture, Wittgenstein made a similar point, emphasising the different kinds of agreement. He first points out that people tend to react in the same way after having gone through the same training:

> If you have learned a technique of language, and I point to this coat and say to you, "The tailors now call this colour 'Boo' ", then you will buy me a coat of this colour, fetch one, etc. The point is that one only has to point to something and say: "This is so-and-so", and everyone who has

8. It does not follow from this characterisation that the view is not a species of conventionalism, since we know, again from the example of Lewis, that a convention doesn't have to be seen as an agreement of opinions (on Lewis's account, a strategy selection is not being of the *opinion* that it is best).

> been through a certain preliminary training will react in the same way. If I just say "This is called 'Boo' " you might not know what I mean; but in fact you would all of you automatically follow certain rules.

He then asks:

> Ought we to say that you would follow the *right* rules?—that you would know the meaning of "boo"? No, clearly not. For which meaning? Are there not 10,000 meanings which "boo" might now have?—It sounds as if your learning how to use it were different from knowing its meaning. *But the point is that we all make the SAME use of it*. To know its meaning is to use it in the same way as other people do. "In the right way" means nothing. (*LFM* XIX, p. 183)

Here it might seem that Wittgenstein is expressing meaning scepticism, the view that there is no such thing as correctness in our linguistic practice, since he says there is no such thing as "the right way"—but that reading would be too quick, since immediately after having made this remark, he goes on to say that this is the same for continuing the series of cardinal numbers (and presumably any other series) and here the criterion of correctness *is* doing it in the same way as everyone else:

> Is there a criterion for the continuation—for a right and a wrong way—except that we do in fact continue them in that way, apart from a few cranks who can be neglected? (*LFM* XIX, p. 183)

His previous denial of there being a correct way should therefore be understood as the radical conventionalist claim that there is no such criterion outside of our practice. That reading is supported by his conclusion:

> This has often been said before. And it has often been put in the form of an assertion that the truths of logic are determined by the consensus of

> opinions. Is this what I'm saying? No. There is no *opinion* at all; it is not a question of opinion. They are determined by a consensus of action: a consensus of doing the same thing, reacting in the same way. There is a consensus, but it is not a consensus of opinion. We all act the same way, walk the same way, count the same way. (LFM XIX, p. 183–184)

This last remark might at first glance be read as a rejection of conventionalism, however, rather than an affirmation—since Wittgenstein says that the truths of logic are *not* determined by a consensus of opinions. However, mathematical statements are true, we are told, because of the other kind of consensus, the consensus of *action* (the same distinction Wittgenstein had been trying to make before). (And we can point to at least one account of convention, namely that of Lewis, that does not equate the selection of a convention with its participants being of the *opinion* that it is the best.)

Wittgenstein's answer in the *Lectures* seems to be that a "consensus in action" (LFM XIX, p. 184), "the natural way to do it" for the people engaged in the practice (e.g. LFM XI, p. 107) or "doing it in the way in which all of us [...] would do it" (LFM VI, p. 58) provides the answer—i.e. constitutes the correctness of one way of continuing. Wittgenstein's emphasis here is always on the particular case and this agreement, however it is in the end spelled out, is rooted in our common nature and training.[9]

I do not want to understate the difficulties providing a fully satisfactory account of mathematical practice that is able to meet this definition: How can our practice, a finite thing, possibly settle infinitely many cases on a case-by-case basis? How does our agreement about the specific case manifest itself, if not on an analogy with a referendum or

9. I should mention that the famous §201 of the *Philosophical Investigations* supports this reading, even though it has not been traditionally read that way. Anscombe's original translation reads:

> What this shews is that there is a way of grasping a rule which is *not* an *interpretation*, but which is exhibited in what we call "obeying the rule" and "going against it" in actual cases.

while the most recent translation has, instead of "actual cases", the more confusing and less readable " from case to case of application".

The original, "von Fall zu Fall", might be more idiomatically rendered as "on a case-by-case basis".

a vote, as prominent commentators have asked (Schroeder 2017; Gerrard 2018)?[10] What does the distinction between agreement in action and agreement in opinions amount to? I do not have any answers to these questions here, but if my overall thesis is correct, that this is Wittgenstein's considered position, I would claim that this a problem to be solved by Wittgenstein exegetes and interested philosophers, rather than a reason to dismiss it as a reading of Wittgenstein.[11]

III Wittgenstein's radical conventionalism in the *Lectures*

Wittgenstein's rejection of mathematical platonism (i.e. the view that mathematical propositions are descriptions of an external mathematical reality) is well known. Despite this rejection, however, Wittgenstein does not deny that mathematical statements are objective nor even claim that locutions such as "a reality corresponds to our mathematical propositions" are necessarily false or meaningless.

It is rather, Wittgenstein thinks, that a "wrong picture goes with them" (LFM XIV, 141) and for Wittgenstein, we can give such ways of speaking meaning if we provide an explanation of this correspondence. Otherwise, we will have simply said something meaningless or empty. In Lecture XXV, Wittgenstein gives what is perhaps the clearest expression of this aspects of his philosophy of mathematics:

> Suppose we said first, "mathematical propositions can be true or false". The only clear thing about this would be that we affirm some mathematical propositions and deny others. If we then translate the words "It is true..."

10. Schroeder even claims, with some good reason, that the very idea of radical conventionalism is nonsense—that conventionalism requires a kind of generality that the radical variety denies. I hope that the overall thesis of this paper shows that this is not the case.

11. Here, I agree with Ian Hacking, who pointed out that pieces of Wittgensteinian jargon, such as *practice*, *agreement* and *custom*, are "often cited as if they were at the end, not in the middle, of a series of thoughts" (Hacking 2014, p. 2).

For an attempt at answering these questions, see Berg 2022. The account of rule-following given there purports to do both, preserve objectivity and allow for the possibility of mistakes and avoids the problem of 'voting' by providing a game-theoretic account whereby agreement means that dispositions to judgement are in a coordinated equilibrium.

> by "A reality corresponds to..." – then to say that a reality corresponds to them would say only that we affirm some mathematical propositions and deny others. We also affirm and deny propositions about physical objects. [...] If that is all that is meant by saying that a reality corresponds to a mathematical propositions, it would come to saying nothing at all, a mere truism: if we leave out the question of how it corresponds, or in what sense it corresponds. (LFM, XXV, 239)

A little later, Wittgenstein emphasises that mathematical statements are objective and claims that we are tempted to say such things as "a reality corresponds to..." in relation to mathematics because the form of mathematical statements suggests a comparison with empirical statements:

> Or to say this [that mathematical statements correspond to a reality] may mean: these propositions are responsible to a reality. That is, you cannot just say anything in mathematics, because there is the reality. This comes from saying that propositions of physics are responsible to that apparatus – you can't say any damned thing. It is almost like saying, "Mathematical propositions don't correspond to moods; you can't say one thing now and one thing then". Or again: "Please don't think of mathematics as something vague that goes on in the mind". [...] And if you oppose this you are inclined to say "a reality corresponds". (LFM XXV, 240)

Wittgenstein then distinguishes between two different senses we could attach to the phrase "a reality corresponds..." (LFM XXV, 241). The first is what we've just seen, that some mathematical propositions can be the result of certain calculations, and not others, or that some propositions can be derived from our axioms via inference rules, etc. The second way is that of how the whole of mathematics, not just individual statements, can be said to correspond or be responsible to something.

Here, Wittgenstein introduces two constraints, one of a psychological nature and the other practical, having to do with the usefulness of our mathematical theories. The first constraint is that if we use a word in a particular way, we are inclined to use it in certain ways in future cases, where some ways to proceed are 'unnatural'. Here, Wittgenstein is surprisingly explicit:

> Suppose I said, "If you give different logical laws, you are giving the words the wrong meaning". This sounds absurd. What is the wrong meaning? Can a meaning be wrong? There's only one thing that can be wrong with the meaning of a word and that is that it is unnatural. (LFM XXV, 243)

What does Wittgenstein mean by 'unnatural' here? We've seen a few examples so far, but let's look at a few more.

In Lecture XXI, Wittgenstein discusses how we come to accept logical principles, for example, the law of non-contradiction. He first says:

> You might ask: What are we convinced of when we are convinced of the truth of a logical proposition? How do we become convinced of, say, the law of contradiction?[12]
>
> We first learn a certain technique of using words. Then the most natural continuation for us is to eliminate certain sentences which we don't use—like contradictions. This hangs together with certain other techniques. (LFM XXI, 201.)

Later in the lecture, Wittgenstein makes the same point again:

> How do we get convinced of the law of contradiction?—In this way: We learn a certain practice, a technique of using language; and then we are all inclined to do away with this form—on which we do not act naturally in

12. See Berg 2021 for a more detailed discussion of how this relates to contradictions. I will mostly avoid the subject here, since it would take us too far afield.

> any way, unless this particular form is explained afresh to us. (LFM XXI, 207.)

The point is, I believe, that when we learn to use language, we learn to use the words 'and' and 'not' in certain ways when describing things, i.e. when we assert empirical propositions. We come to understand sentences of the form '$\neg p$', '$p \wedge \neg q$', etc. as describing certain situations and the most natural analogy of extending the use of these symbols into new cases we have not seen before makes us exclude the combination where a proposition and its negation are asserted: there is simply nothing we find it natural to describe as being of that form.

We could, however, if certain facts were different, adopt a practice of using certain sentences of the form '$p \wedge \neg p$' to describe certain situations. For example, we could use the sentence 'it is raining and it is not raining' to describe the situation where it is drizzling. The point here is not that sentences of the form '$p \wedge \neg p$' are not necessarily false, but that they are depends on our actual practice of using such sentences—and that is one where certain analogies guide us into new and new cases and where we find it natural to exclude sentences of this form. We could have a different practice where analogies lead us down a different path.

In a different lecture, Wittgenstein had discussed a similar example and suggested that when we invent such cases, it gives the impression that we are being cheated:

> Suppose that we give the rule that "Do so-and-so and don't do it" always means "Do it". The negation doesn't add anything. So if I say "Sit down and don't sit down", he is to sit down. If I then say, "Here you are, the contradiction has a good sense", you are inclined to think I am cheating you. This is an immensely important point. Am I cheating you? Why does it seem so? (LFM XIX, 185)

Alan Turing and Norman Malcolm, who attended the lectures, subsequently suggest that the feeling of being cheated comes from the fact that we wanted to discuss the law of non-contradiction as used in ordinary language, "not in connexion with language modified in some arbitrary way", as Turing puts it (LFM XIX, 185).

Wittgenstein agrees, but suggests that the explanation of this feeling of being cheated is because "I have made a wrong continuation". Turing then offers the following analogy:

> [Turing:] Could one take as an analogy a person having blocks of wood having two squares on them, like dominoes. If I say to you "White-green", you then have to paint one of the squares on the domino which I give you white and the other green. [...] – Your suggestion comes to saying that when I say "White-white" you are to paint one of the squares white and the other grey.

Wittgenstein accepts this analogy but claims that there is nothing internal to this practice that makes it *wrong*:

> Yes, exactly. And where does the cheating come in? What is wrong with the continuation I have suggested? Why is this continuation in your analogy a wrong continuation? Might it not be the ordinary jargon among painters? The point is: Is it or is it not a case of one continuation being natural for us?

Wittgenstein then goes on to argue that the reason Turing finds this objectionable is that he sees rules as operating independently of our practice. For Wittgenstein, on the other hand, it is not logically impossible to give meaning to statements of the form '$p \wedge \neg p$', but rather a different logic would result, if we did do so.

This example is a particularly clear one of how Wittgenstein thinks of the constitution of concepts, even if it came from Turing, and thus quite important for our purposes. The idea is, I believe, this: the painters are taught to paint blocks of woods with two squares by instruction and seeing examples. We could then imagine that a painter gets a command to paint a block where the two colours are completely new or in a new combination. According to Wittgenstein, what determines the correct action in such a case is how the painters find it natural to continue, even if they have never seen such a case before. The painter's training thus determines what is correct in unseen cases, without pointing outside of the practice itself. Given the painter's practice and their form of life, they would find it natural to paint both squares white when given the command "white-white", but if they have been specifically taught to paint one square white and another gray when they hear this command, they might find that quite natural. There is nothing that shows that this is *incorrect*, even if that is not what they would have done had this case not been given special treatment ("explained afresh").

With this in mind, we can give a similar explanation for simple arithmetical concepts. We are taught how to add, for example, by learning simple algorithms and seeing examples. At each step in carrying out such an algorithm, the correct step is determined by what we would find natural to do. If the algorithm is simple enough, it is then not particularly mysterious that we would find *something* natural at each step, even if the steps are potentially infinite. Our arithmetical training can therefore determine the correctness of infinitely many answers, even if there is nothing outside our practice that constrains us. This avoids Quine's regress that so plagued more moderate forms of conventionalism.

It is worth pausing to see why. As we saw at the beginning of the paper, Quine's argument is in effect that if we say that the correctness of a given theorem is explained by the explicitly stipulated axioms, we still have to explain how we move from the axioms to the theorems—explaining what follows from what. We can then either stipulate further rules, leading to a regress, or throw out the conventionalism. On this view, however, the correctness of every truth is determined directly by how we find it natural to project what we have done so far into novel cases. There is no regress of rules, because the *content* of the rule is determined by what we find natural to do in particular cases.

The last example I'd like to consider is by far the most important, however, and one where Wittgenstein frankly discusses an explicit case of mathematical truth on this picture, that of Goldbach's conjecture—the unproven conjecture that every even integer greater than 2 can be expressed as the sum of two primes. There, he claims that to believe that Goldbach's conjecture is true without having a proof of it is to believe that we will find it most natural to extend our mathematics in that way:[13]

> Suppose someone had a hunch that "every even number greater than 6 is the sum of two primes". If you have a hunch it will come out right, you have a hunch that the mathematical system will be extended this way—that is, that it will be best or most natural to extend the system in such a way that *this* will be said to be right. (*LFM* XIV, p. 137)

If we agree that our agreement about a particular case is constitutive of our use of our symbols and that there is nothing to the truth of a mathematical statement other than our language and mathematical practices, it then follows that our agreement about a particular case like Goldbach's conjecture is constitutive of the truth of that statement in the sense that our concepts would be fixed that way: our practice *could* be extended so that the conjecture is true and it *could* be extended so that it is false. And that is precisely what Wittgenstein says next:

> Suppose someone said: "What you, Wittgenstein, say comes to saying we could *also* extend arithmetic in such a way as to prove this is not so, or to make it a primitive proposition." I'd say: certainly. (*LFM* XIV, p. 137)

He continues:

> Because of course you haven't made this extension. The road is not yet actually built. You could if you wished assume it isn't so. You would get into an awful mess. (*LFM* XIV, p. 137)

13. See also Lecture XI, p. 104:"[In mathematics, there] is nothing there for a higher intelligence to know—except what future generations will do. We know as much as God does in mathematics". Here again, Wittgenstein seems to insist that to conjecture that a proposition is true is to make a conjecture of how people would find it most natural to proceed. Cf. also PI §352.

In my view, there could hardly be a clearer statement of radical conventionalism: there is nothing in our prior practice, Wittgenstein seems to be saying, that makes it so that only one way of extending arithmetic could possibly be correct and we could go either way at any step—albeit with the caveat that we'd get "into an awful mess" if we go one way, rather than another. (More on that below.)

A little later, Wittgenstein states that having a hunch that the conjecture is true is "a hunch that people will find it the *only* way of proceeding" (*LFM* XIV, p. 138), that is to say, that predicting that a theorem is true is really a prediction about how people would in fact proceed (and not, say about mathematical facts independent of our practice.)

It would not be an understatement to say that this view is extremely counter-intuitive and does not fit well with what we think we are doing when we are doing mathematics—the phenomenology of proof. Furthermore, we tend to think that if something is a mathematical fact, it is because it could not have been otherwise—if Goldbach's conjecture is true, it is because that is how the structure of the natural numbers *really* is, independently of us.

Wittgenstein is of course aware of this:

> You might say, "Wittgenstein, this is bosh. For if the system will be extended in such a way, it must be *capable* of being extended in such a way." If this is so, then the person who has a hunch that Goldbach's theorem is correct has a hunch about the possibilites of extension of the present system—that is, he believes something about the essence, the nature, of the system, something mathematical about it. (*LFM* XIV, p. 137)

Wittgenstein attributes this view to Turing, and says to him:

> If you say, "The mere fact that a proof could be found is a fact about the mathematical world", you're comparing the mathematician to a man who has found out something about a realm of entities, the physics of mathe-

> matical entities. If you say, "You can this way or that way", you say there is no physics about mathematics. (*LFM* XIV, p. 138)

Wittgenstein is here contrasting his view with the Platonist view that mathematical statements are descriptions of an external reality, a view that he had previously associated with his Cambridge colleague, G. H. Hardy. He goes on:

> Professor Hardy says, "Goldbach's theorem is either true or false."—We simply say the road hasn't been built yet. At present you have the right to say either; you have a right to postulate that it's true or that it's false.— If you look at it this way, the whole idea of mathematics as the physics of the mathematical entities breaks down. For which road you build is not determined by the physics of mathematical entities but by totally different considerations. (*LFM* XIV, p. 138–9)

But even if Wittgenstein does think that our mathematics can be extended either way, in some sense at least, he does not think that we are completely unconstrained in what we do, for these other considerations, even if they do not logically determine what we find natural, they do constrain our mathematical practice:

> The mathematical proposition says: The road goes here. Why we should build a certain road isn't because mathematics says that the road goes there—because the road isn't built until mathematics says it goes there. What determines it is *partly practical considerations and partly analogies in the present system of mathematics*.
>
> But the fact that a proof of the theorem is *possible* may seem to be a mathematical fact—not a fact of convenience etc. (*LFM* XIV, p. 139. Emphasis mine.)

It seems that the view Wittgenstein is advancing is that even though our practice *could* be extended such that the conjecture is true and extended in such a way that is false, we will in practice only find one way of extending it natural or practical (but see the last section for complications). There are no *external constraints* in how we might extend it, but facts about ourselves and our practice make it so that it can as a practical matter only be extended in one way—at least when things go well.

These are of course startling claims—and on the face of it, ludicrous. How could this position possibly be reconciled with the objectivity of mathematics? Either Goldbach's conjecture is true or not, we want to say with Hardy, and that does not depend merely on what we find natural to say, but the mathematical facts themselves. Furthermore, only one way is consistent, since if there were counterexamples to Goldbach's theorem, it's negation would be true.

Despite the philosophical difficulties, however, the example just given is the clearest example of Wittgenstein professing his radical conventionalism about mathematical truth, and seems to me to be quite definitive as an exegetical matter: Our agreement about the particular case is what explains the truth of mathematical theorems, even in substantial cases like Goldbach's conjecture, and this agreement is generated by how we find it natural to proceed, which in turn is determined by various different things, most prominently our biological nature (the brute fact that we respond in certain ways to stimuli) and the linguistic training we receive.

IV Objections to radical conventionalism about mathematical truth

In this section, I will examine a number of strong objections to the view just outlined and argue that they can be at least partially answered by examining the way our mathematical practice actually proceeds and how we are trained to do mathematics. This is of course fitting, since Wittgenstein constantly emphasises these aspects of mathematics to foundational to their truth. My goal here is not, however, to defend radical conventionalism as the definitive view in the philosophy of mathematics, but rather to show that it should perhaps not be so quickly dismissed as philosophers working in the philosophy of mathematics have tended to do.

The picture of mathematical truth Wittgenstein develops in the *Lectures* can roughly be described as follows: Our agreement in action—how we do in fact calculate, infer, count, etc.—is constitutive of the respective mathematical concepts. That is to say, if we all calculate in such a way that $12 \times 12 = 144$, then that is constitutive of the fact that our concept of multiplication is one where that is correct—or perhaps rather, that the symbol '$\times$' refers to the multiplication function, rather than another function. Similar considerations apply to our inference rules, etc.[14]

We extend these concepts into new and unforeseen cases by doing what we all find natural. What we find natural in each case is in turn determined by various contingent and empirical facts, e.g. about our biology and psychology, facts about our practice, and most importantly, how we are taught the relevant concepts. To say that a certain theorem is true, or a given calculation has such and such an answer, is therefore to believe that if we were to extend our practice so that these new cases are covered, that would be how people would naturally extend them. To take another example, we find it natural to say, after having gone through the steps of an algorithm, that "$57 + 68 = 125$" and unnatural to say that "$58 + 68 = 5$", and so, '+' refers to the addition function and not Kripke's quus function.

And since nothing outside of our mathematical practices and languages grounds the truth of mathematical statements on Wittgenstein's view, mathematical truths are therefore conceptual truths where our agreement about the particular case is constitutive of those very concepts. The facts which determine this agreement are not fixed once and for all, but are contingent and depend on reasons and causes that may vary immensely, there

14. To be absolutely strict with regards to the distinction between use and mention, we would perhaps rather have to say that if our practice is such that after some process, we utter the statement "$12 \times 12 = 143$", then the symbol '$\times$' as used in that statement refers not to the multiplication function, but rather a similar function where the output is 143 with the two inputs 12 and 12.

is therefore no external constraint on how these concepts may be constituted. Necessity, for Wittgenstein, is grounded in and dependent on contingent facts.

The account just outlined has two main elements: The first (1) is a theory of how concepts are constituted. Here, Wittgenstein's view is that it is our agreement about the particular case that determines which concepts we are in fact using. The second (2) is the claim that mathematical statements are not descriptions of facts independent of our practice, and which our concepts have to fit. They combine into the claim that our agreement about the particular case is constitutive of our mathematical concepts and therefore that each truth is individually conventional.

The first part of this account (1) is Wittgenstein's response to his own rule-following paradox (as his discussion in the *Lectures* makes clear) and can be described as a community account of rule-following. As such, it is vulnerable to objections such accounts have faced in the literature. The first objection of this sort I want to consider is the objection that community solutions cannot do for the community itself what they do for the individual agents, namely, provide a standard of correctness. The following is a fairly standard version of this objection:

> Any given individual's use of an expression is correct only if it is acceptable to the rest of the community. If the individual's use is unacceptable to the rest of the community, that use is incorrect. But the dispositions of the community taken together do not track an investigation-independent property either. Therefore, there is no possibility of mistake for the community as a whole. We may all be disposed to call some non-square things 'square'. (Hattiangadi 2007, p. 93)

There are at least two readings of this objection that are worth highlighting. The first is that we might simple get it wrong—that it could be that given the community's previous commitments, it should determine, for example, that $12 \times 12 = 144$, but for some reason or another, it settles on the wrong answer that $12 \times 12 = 143$.

However, given Wittgenstein's discussion of this example, as well as his discussion of Goldbach's conjecture, the objection thus put would simply be a confusion of use and mention, since in the latter case, the symbol '$\times$' would refer, not to the multiplication function, but a different function where that would be the right answer. For Wittgenstein, there are no right or wrong mathematical concepts ("there's only one thing wrong with the meaning of a word and that's that it is unnatural") and thus for him, the objection is simply misguided.

There is a different sense in which the community should be able to make a mistake, however. On this second reading of the objection, the possibility of mistake should be relative to the concept the community it takes itself to be employing—if we intend to multiply (i.e. really be using the function $\times$ and not $\times_{143}$), we should also be able to fail to multiply—and not just give the right answer, relative to a different concept. The answer to the previous objection seems to rule that possibility out. Another way of putting this objection would be as follows: any individual agent can miscalculate and give the wrong answer to a mathematical problem and if so, it should also be logically possible that *every* agent makes the same mistake. But then radical conventionalism would seem to predict that the mistaken answer was the correct answer, ruling out this possibility.

I think, however, that Wittgenstein's notion of naturalness need not be so thin as that. We could imagine that an agent finds a particular answer natural, but nevertheless fails to give that answer. If so, it should also be possible that *every* agent finds a particular answer natural, but fails to give it, in which case, the actual answer given and the answer that the community finds natural might come apart, making room for mistakes.

This brings us to the second part (2) of Wittgenstein's account, that mathematical truths are conceptual truths that are not constrained by reality. One might say, in the light of previous answers to objections, that it simply cannot be that for Wittgenstein, mathematical truths are not constrained by reality, since empirical counterexamples can always show that we were wrong. For example, if we would line up 12 rows of 12 toy soldiers, we would presumably find that there are 144 toy soldiers by counting them. This would show that $12 \times 12 = 144$—and what we find natural in calculating a particular sum would not matter, we would simply be wrong.[15]

I don't think this follows. If we make a distinction between the *arithmetical* statement that $12 \times_{143} 12 = 143$ and the empirical statement that *these rows of twelve toy soldiers are 144 toy soldiers*, there is no direct contradiction and the latter does not directly falsify the latter. This is not as strange as might seem at first glance. For example, physicists use vector arithmetic to calculate how forces combine and subtract. It is not, however, *a priori* that forces can be aptly described by the means of vector arithmetic, despite the apparent success of such models, and if it were discovered that such was not the case, no proposition of vector arithmetic would thereby be falsified.[16] It would most likely force us to abandon the practice of *using* vector arithmetic to describe forces, but that does not amount to the same thing. Similarly, the discovery that the local geometry of space is not Euclidian would not thereby falsify Euclidian geometry or any of its propositions.[17]

On this view, then, apparent empirical counterexamples could only suggest a change in our practice, whereby mathematical concepts would be deemed unfit for empirical use, and not such that particular mathematical statements could be falsified (after all, if the symbol '$\times$' refers to a function such that $12 \times 12 = 143$, then the sentence '$12 \times 12 = 143$' is trivially true). One could nevertheless ask if this complete separa-

15. This is of course related to Wittgenstein's 'inference-ticket' view, according to which, mathematical statements are 'rules of description' that allow us to make inferences from one empirical statement to another, for example, that if I know I have 12 rows of 12 toy soldiers, I can infer that I have 144 toy soldiers, without counting them all up.

16. See for example Lecture XII, where Wittgenstein's student had given him a case where a mistake in logarithmic tables leads to a bridge falling down:

> The point is that these tables do not by themselves determine that one builds the bridge in this way; only the tables together with a certain scientific theory determine that. (LMF XII, p. 110)

Presumably it would be a part of this theory that logarithmic tables are useful for this purpose. He then adds that this would be similar to the case of $12 \times 12 = 143$.

17. See Pérez-Escobar 2023 for a discussion of how such arithmetical statements resist falsification in light of empirical evidence.

tion between mathematical statements (e.g. "$2 + 2 = 4$") on the one hand, and the corresponding empirical claims (e.g. "two apples and two pears are four pieces of fruit") on the other doesn't make a complete mystery out of the fact that we use mathematical statements in all sorts of non-mathematical practices? I believe Wittgenstein's discussion in the *Lectures* contains the seed of an answer to this question.

Throughout the *Lectures*, Wittgenstein claims that our mathematical calculi have their origins in experience, but have since been made independent of them (see e.g. LFM IV, pp. 43–44 and LFM V, p. 55). This is not only textual evidence for the view just described, where our mathematical calculi are indeed independent of our experience, but could also offer a way out of the present problem. We adopt certain techniques and rules, e.g. a way of counting and measuring, for example, for practical ends and these are chosen so that they give the right result in easy and known cases, but in more complicated ones, the outcomes are used to judge our experience (which is a point Wittgenstein makes often as well). But since the way we project these rules and techniques is based on our agreement about the particular case, there is no guarantee that the rules we have come up with will fit our experience in all future cases. However, since these mathematical techniques and rules do have their origin in our experience of the world, it is not particularly *surprising* that they work well in practice—that is what they are designed for.[18]

It could still be objected that this is not enough. After all, our basic arithmetical discourse has an intended model: the real world. It's not the case, we might say, that we can so cleanly separate the claim that e.g. 7 + 5 = 12 from the claim that seven apples and five oranges are twelve pieces of fruit, because when we utter propositions like the former in natural language, we mean something like 'seven objects added to five objects are twelve objects'.[19]

18. This view is similar to the view found in Jenkins 2008. For Jenkins, arithmetical truths are conceptual truths, but we acquire our arithmetical concepts through experience and interacting with the world which in turn has an arithmetical structure.

19. There are accounts in the literature that hold that arithmetical sentences in natural language of the

This may very well be true, but only seems to be particularly relevant for sentences containing small enough numbers. Firstly, there are more arithmetical truths than there are possible combinations of physical objects, namely infinitely many. It can therefore not be that the real world is the intended model for *all* arithmetical truths, unless one wants to be a finitist about arithmetic. Second, we do not have direct acquaintance with or direct grasp of the truth of arithmetical propositions (or even their content!) where the numbers involved are large enough. For example, if I claim that 135 664 objects added to 37 863 objects are 173 527 objects, my justification for this claim is based on a given calculation (in this case, a simple algorithm) and not any direct experience with objects. Such statements must, in some way or another, be mediated through arithmetical concepts and techniques, even if the intended interpretation of such utterances is that they are about the actual world.

Such considerations are of course one reason why philosophers have found platonism compelling—if the objects are not in this world, then maybe in the next. The point is, however, that we must find some other way to ground arithmetical truths, both ontologically and epistemologically, than to simply point to objects in the world. Here, Wittgenstein's view, as I have described it, at least has an advantage by locating the genesis of our mathematical concepts in experience.

The next objection to radical conventionalism I want to consider is due to Dummett and is one of the last arguments he presents against radical conventionalism, but nevertheless one of the most forceful. It goes as follows: If we think that nothing external to our practice nor what we've done in the past determines what we should do in future cases and, it seems that we are compelled to accept a very counter-intuitive picture of what a proof is. Namely, if we assume that a proof (or a calculation) always composed of tiny little steps from the premises to the conclusion (and similarly for a calculation) and each of these steps is a direct expression of a convention, it seems that we have to

form 'two and two is four' should be interpreted along adjectival lines, i.e. as something like 'two objects and two other objects are in total four objects'. See Roberts and Shapiro 2017 for an overview of the issues.

accept either that (i) it is not determinate what counts as right when we take each of the little steps or (2) that the combination of all the little steps is not transitive and that we could therefore accept all the little steps and reject the whole proof. Here's Dummett's version of the objection:

> Suppose the calculation in question is an ordinary addition. One of the rules that make up the computation procedure is that, if one of the two final digits is 7 and the other 8, you write 5 in the digits column of the sum and carry 1 to the tens column. To maintain that there is no determinately correct result of the calculation, you must say one of two incredible things. Either you must say that, until someone has done it, it is not determinate what would count as writing down 5 and carrying 1; or you must say that, although it is determinate what the outcome of each application of one of the constituent rules would be, it is not determinate what would be the outcome of a large but finite number of such applications. I do not know how many of the followers of Wittgenstein really believe either of these things; for myself, I cannot, and conclude that the celebrated 'rule-following considerations' embody a huge mistake. (Dummett 1993, p. 460)

This point is well-taken. I believe, however, that we can take the sting out of this objection by looking at how Wittgenstein conceives of our mathematical practice and how we acquire concepts in his view. The core idea is that correctness in a mathematical practice is determined by our agreement in the particular case and this agreement is of what we find natural to say in each case. At first glance, it might then seem like a real possibility that we would all find it natural to say that, for example, $17 + 18 = 5$, and not 35, despite having performed a calculation that ends in the way Dummett describes it.

However, a key part in concept acquisition on this picture is how we learn the relevant concepts and what we would find natural to say. A crucial component of this process is that mathematical practice, both calculation and proof, proceeds in small steps and not in big jumps. It is therefore not the case, given how our practice actually is, that what we would find it natural to say that $17 + 18 = 5$ and in general our agreement about the particular case would not be *sui generis* in this sense—it would always have to be accompanied by a calculation or a proof, and given how we learn these, we wouldn't find it natural to give any other answer than 35 after such a process. *In this sense, it is determined in advance that the outcome of the calculation is 35*, because we will only find one way of proceeding to be natural, given the our training and the simplicity of the algorithm. Our agreement determines that the symbol '+' refers to addition and this agreement is fixed, even before it is manifested in practice.[20]

Of course, this is not a *logical* impossibility: it is not contradictory to suppose that we would form such a *sui generis* judgement about this case and that the process of actually calculating it would not override it. In that sense, Dummett is correct. However, Wittgenstein's account predicts that we would then simply be using *other* concepts than we actually do, and in that sense, the outcome would be correct, relative to *those* concepts. But given that we would have to imagine changes to our whole mathematical practice and form of life, describing our practice to be one that employs different concepts wouldn't seem to be that far off the mark in any case: if we have to imagine our whole mathematical practice to be different, why not say that our concepts are different too?

There are, therefore, two different ways to understand Dummett's use of 'determine' when he says that we'd have to accept that it is not determined what we say after having performed a calculation like he describes. There is the strong sense, according to which there is some kind of rule, definition or meaning which settles *a priori* and independently of our practices and language what the answer should be, then Wittgen-

20. This also throws light on some very puzzling passages in *RFM* concerning the expansion of real numbers (e.g. RFM V, §9).

Wittgenstein seems to be saying that it is not determined in advance whether or not a certain sequence occurs in the expansion of a given number or not. On this reading, that is inaccurate: For Wittgenstein, the expansion of real number is associated with a technique or rule of expanding it, and our agreement in the particular case also determines reference here.

stein would not deny that Dummett is right; for Wittgenstein, after all, the practice itself is prior to these things. But if we understand it in a weaker sense, as referring to the way that the practice, understood as a complex, structured interplay between different agents, can provide a determinately correct answer in each case, Wittgenstein can claim that it does—and since there is a fact of the matter what we *would* find natural to say in a given case, even if we have never been faced with it before, it is even determined in advance what our agreement about that particular case would be, an hence what the correct outcome of the calculation is.

The example of Goldbach's conjecture is similar. We would not find it natural to say that Goldbach's conjecture is true, unless we had a proof, and like a calculation, such a proof would be extended in small steps, not big leaps.[21] Now, the theorem says that every even natural number greater than 2 is the sum of two prime numbers. For the theorem to be true, there need therefore to be infinitely many calculations that do not provide a counter-example to the theorem. It could then be objected that since the criterion of correctness for each of these calculations is independent from the criterion of correctness of the theorem, it could be that we would find it natural to say that there exists a number greater than 2 such that it is not a sum of two prime numbers *and* that we would find it natural to say that every even natural number greater than 2 is the sum of two prime numbers. We would, as Wittgenstein said, get into an awful mess.

There is indeed nothing in the account that could rule out such a case, but if it were to occur, that would simply mean that our mathematics would be inconsistent. This possibility, however, cannot be counted as an objection against radical conventionalism, since *no* theory of mathematical truth can guarantee that mathematics is consistent either. This does, however, raise an important question about the role of the law of non-contradiction on Wittgenstein's account. Wittgenstein's philosophy of mathemat-

21. Finding the conjecture very plausible and being inclined to accept it is not 'to find it natural to say' in the relevant sense. For Wittgenstein, 'natural' seems to be a technical term, taking all of our practice into account. And it is simply not the practice of mathematicians to accept theorems without proof.

ics is notorious for what has been taken as a flippant attitude towards contradictions in mathematics and logic. There is not space here to cover this topic in any depth, but I think it is clear that Wittgenstein was not as cavalier towards contradictions as many have supposed—or even as his remarks seem to suggest.[22]

One point of contention is that for Wittgenstein, a practice can be inconsistent, without it thereby being useless or needing revision. This is not as controversial at it would first seem. For example, we can imagine a game whose rules are inconsistent, but that the inconsistency could only be made manifest under very specific circumstances, requiring super-human ability from the players. It would seem perverse to therefore declare the game useless for that reason, since we could still play it perfectly well. This is comparable to the situation in chess, which is known to be solvable (i.e. there exists a strategy such that White or Black always wins, or always draw). At first glance, it might seem that chess would become an unplayable game, if we knew what the optimal strategy was. Presumably, however, no human player would be able to execute it, and so the difference in practice is negligible.[23] Consistency is no different—it would still be possible to play such an inconsistent game.

This possibility does not mean that inconsistency is not serious business according to Wittgenstein. He's clear that in the general case, an inconsistent practice is likely to fall into confusion or could lead to catastrophe.[24] But since his account of mathematical truth is such that mathematical truths to not correspond to anything external to our practice, and that an inconsistency is not necessarily fatal to a practice, there is room on his account for inconsistent mathematics—with the caveat that such mathematical

22. See Berg 2021 for a detailed discussion.

23. Wittgenstein's own example is the statute of country that say that on feast days the vice-president is supposed to sit next to the president *and* that they're supposed to sit between two women. Wittgenstein seems to assume that the president is not a woman.

24. However, Wittgenstein does not think that if our *mathematical* practices are inconsistent, then the *contradiction* will cause mayhem. The reason is, which he gives in his debate about the matter with Turing, is that if we use an inconsistent mathematical calculus to model a physical phenomenon, then the model will simply not be accurate: if p is the case, then the problem with deriving $p \wedge \neg p$ is not that it is a contradiction, but that it depends on us deriving $\neg p$—the contradiction is always parasitic on a falsehood.

theories are not trivial.[25]

And indeed, the existence of inconsistent mathematics shows that this is in fact possible. In the realm of pure mathematics, there is, for example, work on inconsistent models of arithmetic (Priest 1997, Priest 2000), set theory (Weber 2012), analysis (McKubre-Jordens and Weber 2012) and even geometry (Mortensen 2010).[26] But perhaps even more crucially, the phenomenon extends into applied mathematical theories as well. Esther Ramharter points out in a similar context, for example, that the use of the Dirac δ-function in physics produces contradictions that do not interfere with the practice of doing physics (Ramharter 2010) and Johannes Lenhard has analysed a number of examples from the history of engineering that show the use of inconsistent mathematical theories with great success.[27]

Why do mathematicians then avoid inconsistency, on Wittgenstein's view? Unfortunately, Wittgenstein does not say much about this, except merely claiming that it is one of the rules of our mathematical practice (LFM XXI, 208). Given the picture of mathematics Wittgenstein develops in the *Lectures*, I believe we can give an answer: Mathematical truths are conceptual truths, but our mathematical concepts have their genesis in experience and are subsequently made independent of it. We use these concepts to judge our experience. There is an in-built connection to reality via the process of concept-acquisition, which takes place through learning and participation in mathematical practices (e.g. counting, adding, weighing, etc.) and because of that, we have no use for contradictions in how we use these concepts to describe reality—we do not find it natural to use statements of the form '$p \land \neg p$' as descriptions and thus do not extend such practices in a way that includes such forms.

25. In Lecture XXIII, Wittgenstein does advocate the abandonment of the ex falso-rule. Turing objected that there are indirect derivations in a given proof system that can derive an arbitrary statement without using such a rule, but the existence of well-behaved paraconsistent logics gives Wittgenstein a decisive reply to Turing.

26. See also Weber's recent book, *Paradoxes and Inconsistent Mathematics* (Weber 2021).

27. Among these are Heaviside's "operational calculus", used successfully in electrical engineering, despite being inconsistent, and a more recent example from engineering thermodynamics (see Lenhard, In preparation.) See also Bueno 2005 for discussion of the Dirac δ-function.

The insistence of mathematicians to avoid contradictions is then, for Wittgenstein, (i) a practical requirement for those parts of mathematical practice that lie very close to experience, e.g. those that are closely connected with counting, adding, weighing and other practical activities; (ii) an outgrowth of those parts of mathematics described in (i) and where the avoidance of contradiction acquires an aesthetic interest (cf. RFM I, §167), and finally (iii) a sociological norm that becomes entrenched because of (i) and (ii), leading to what Wittgenstein called mathematicians' "superstitious dread" of contradictions (RFM I, app. III, §17).

The law of non-contradiction is thus a *requirement* we impose on our mathematical practice, but not a *description* of external mathematical facts. It is therefore not *necessary* that we could not have mathematical concepts such that we accept Goldbach's conjecture while accepting counterexamples, but it is rather a part of our practice as it is actually done that we avoid such outcomes: our practice is such that a mathematical conjecture is true if its negation is inconsistent with other statements we have accepted.

This is related to another serious objection to radical conventionalism due to Putnam (Putnam 1979, p. 427). Putnam points out that if mathematical truth is due to our agreement in the particular case, then it is a further fact whether or not our agreement results in a contradiction or not, and hence there is at least one mathematical fact which is not due to our agreement.

Putnam's objection is ingenious, but I do not think we need to worry about it: if, as a result of our agreement, we could derive a sentence of the form p and a sentence of the form not-p, then Putnam is of course right that it is *objectively* true that our practice would be inconsistent. However, that it would be is just what it *means* for our practice to inconsistent. And if we assume that the reference of the word 'inconsistent' is also determined by our agreement in the particular case, the radical conventionalist theory of meaning can easily accommodate this further fact.[28] It was never a part of the view that

28. There is not space to discuss this further here, but I believe this is Wittgenstein's point with his otherwise obscure discussion of mathematical facts about chess (see Lecture XV).

mathematical truths aren't objective anyway, and thus put, Putnam's objection is rather a demonstration of how the account can accommodate objectivity.

The last objection I want to consider for now is the objection that radical conventionalism, as I've described it, is not a kind of conventionalism at all. This is suggested by Barry Stroud's reaction to Dummett's reading of Wittgenstein, which is, I believe, quite typical among commentators. Here's Putnam's paraphrase of Stroud's objection (see Stroud 1965):

> ...the position Dummett calls, "radical conventionalism" cannot possibly be Wittgenstein's. A convention, in the literal sense, is something we can legislate either way. Wittgenstein does not anywhere say or suggest that the mathematician proving a theorem is legislating that it shall be a theorem (and the mathematician would get into a lot of trouble, to put it mildly, if he tried to "legislate" it the opposite way) (Putnam 1979, p. 424–425).

The first aspect of this objection can be easily dismissed, since as we just saw in the last section, Wittgenstein is, on the contrary, quite explicit that mathematicians could, in theory, go either way, even if he does not describe it in terms of legislation—and this he's not compelled to do, if the view is to count as a kind of conventionalism, as is quite possible to imagine that mathematical truths are conventional, without thereby imagining a process of legislation or stipulation, to return to a point made before in the present essay.

This brings us to what I belive is the real bite of Stroud's objection: For something to be a convention, there has to be an alternative that could have been adopted instead of the one that was *actually* adopted and given that the meaning of mathematical expressions is meant to be grounded in our nature and form of life, there seems to be only one alternative on offer. Hence, the view cannot count as a species of conventionalism, radical or otherwise.

Fortunately, I do not think Stroud's objection hits the mark. For Wittgenstein, it is an empirical fact that we do indeed always proceed in one way when we do mathematics, but that is contingent, not logically necessary.[29] We could have, in fact, gone another way, if certain contingent facts would have been otherwise. Mathematical truths are necessary, given our mathematical practice, but it is contingent that we have the practices we do have, even if we did not make any choices that we ourselves were conscious of (see Burge 1975 for a similar point).

This leads to a different worry, however. Since our agreement about the particular case is what determines the reference of our mathematical symbols, the meaning of our symbols would change if our practice regarding that symbol changed. If, for example, we'd all agree to the *sentence* "$12 \times 12 = 143$", then our practice would not be that of multiplication. There is in this sense no alternative way of multiplying, because multiplication is the very practice that it is. We could have had different *multiplication-like* practices, but they would not be multiplication. I do not think we should be overly worried about this. After all, it is commonplace to define conventionalism as the view that mathematical statements are true in virtue of meaning, or depended on us and how we speak and not determined by external reality. This is the definition I've been using here—and the view that Dummett ascribed to Wittgenstein in his original article.

There are more objections to consider, and what I have said is far from the last word on any of them that have been discussed here, but what I hope to have shown is that radical conventionalism should not be rejected out of hand by theorists in the philosophy of mathematics; that there is more to be said about radical conventionalism, and it therefore deserves more attention than it hitherto has received.

29. Or rather, most of the time. As I've described mathematical practice on Wittgenstein's view, if we would end up in a contradiction, mathematicians would go back and tinker with one of the concepts to resolve the tension.

V Final remarks

In this paper, I've defended the view that Wittgenstein was a radical conventionalist and argued that the view has been prematurely rejected by philosophers of mathematics. For the radical conventionalist, mathematics is essentially a human social practice. We engage in mathematical practices and those very practices are what shape and constitute our mathematical concepts, themselves the ground of mathematical truth. Mathematical practices, however, do not exist in a vacuum. The most basic of them (e.g. counting, measuring length, weighing, inferring, and so on) are interwoven with numerous other practices, both quotidian and scientific, and thus have one foot—so to speak—in the physical world. Thus, while mathematical truth is not grounded in the physical world directly, it influences and shapes our mathematical concepts.[30] This way of looking at Wittgenstein's philosophy of mathematics can also serve to explain a number of puzzling passages in the *Remarks* and through them connect his work to more recent work in the philosophy of mathematical practice. There is not space here to discuss all of them, but I'd like to mention one theme that has perplexed commentators, namely those passages that discuss mathematics as concept-formation.[31]

On the radical conventionalist view, mathematical concepts are determined by our agreement in the particular case. We extend these concepts into new cases by doing what we find natural, given our training and form of life. So far, we've mostly focused on simple examples, e.g. calculations and simple inferences. In such cases, we would only find one way of proceeding natural, and hence we can say that in a way, it is already determined what the outcome is—what we would all find natural to say after we've performed the calculation or drawn the inference. The contours of the relevant concepts are already drawn, because it is fixed what we would do.

In general, however, Wittgenstein speaks as if the conceptual boundaries are not

30. Here is a point of connection with the work of Kitcher (1984) and Ferreirós (2016). For them, elementary mathematics arise from interactions with the physical world. See also Jenkins 2008.

31. For a contemporary view in this direction, see Tanswell 2018.

drawn *until* we get there (cf. for example RFM III, §31 and RFM V, §45). This has suggested that perhaps Wittgenstein does not think that mathematics is objective or that he is a finitist.

If we take inspiration from Lakatos' work on mathematical practice, however, and regard mathematics as a process of 'improvement of guesses by speculation and criticism' and not one by which mathematical truth grows "through a monotonous increase of the number of indubitably established theorems" (Lakatos, 2015 [1976], p. 5), we can still think that our agreement in the particular case is indeed what determines the boundaries of our mathematical concepts, without throwing the baby out with the bathwater. It's just that the process which establishes this agreement is far more dynamic than what Lakatos calls the 'deductivist model' of mathematical practice would suggest.[32] This allows us to make a distinction between simple calculations, where it is already fixed in advance what is correct, and what Fogelin called "the constructive frontiers of mathematics where genuine disputes can arise" (Fogelin 1987, p. 161).

But the connection goes the other way, too. Philosophers of mathematical practice have until now shown little interest in foundational matters in the philosophy of mathematics, e.g. accounts of what mathematical truth is, preferring to focus on other philosophical questions raised by actual mathematical practice. The foregoing suggests, however, that radical conventionalism may be a natural foundational theory for such work. The philosophy of mathematical practice may, after all, have important metaphysical consequences.[33]

32. For a recent paper on how Wittgenstein's later work on the philosophy of mathematics can shed light on Lakatos's work an vice versa, see Pérez-Escobar 2022.

Especially interesting is Pérez-Escobar's discussion of how Wittgenstein's view that mathematics creates the form of our description can, through feedback loops between mathematical and non-mathematical components of scientific practices, provide the kind of counterexamples in mathematics that Lakatos discusses. In conjunction with Wittgenstein's contention that our mathematical calculi have their origins in experience, this may enable us to make progress in explaining "the unreasonable effectiveness of mathematics" (Wigner 1960. See Grattan-Guinness 2008 and Ferreirós 2016, p. 43 for a responses to Wigner's problem that also emphasise a worldly connection), as well as a further discussion in Pérez-Escobar 2023.

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