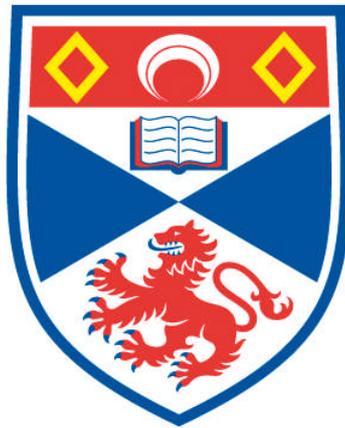


ON WITTGENSTEIN'S NOTION OF THE OBJECTIVITY OF MATHEMATICAL PROOFS

Martha Alicia Trevino-Tarango

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On Wittgenstein's notion of the objectivity of mathematical proofs

Martha Alicia Trevino-Tarango

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Abstract

This work analyses and defends Wittgenstein's definition of mathematical objectivity, looking particularly at his account of mathematical proofs, of what makes them normative, and what role mathematical and linguistic practices play in their establishment. It aims to provide a clearer view of Wittgenstein's idea that the objectivity of proofs is inextricably rooted in empirical regularities, detected within what he calls our 'form of life', and which are in turn constituted by both regularities in nature and regularities in human practices. To accomplish this, the thesis makes an exhaustive analysis of Wittgenstein's distinction between proofs and experiments. Drawing from Wittgenstein's philosophy of language, this paper addresses important criticisms that have hindered a serious study of his remarks on mathematics, and vindicates his arguments as cogent, solid accounts of mathematical necessity and practice. This exploration will show that mathematical proofs do not have to be regarded as tools for the discovery of mathematical truths, of any sort, that depict mathematical facts, but can be correctly characterised as norms that produce new understanding, forming new concepts to deal with reality, and which ultimately affect the very limits of intelligibility, of what we can think, express and do. Thus, a philosophically relevant link between mathematical proofs and possibilities of action is set.

On Wittgenstein's notion of the objectivity of mathematical proofs

Introduction

This paper analyses the account of mathematical proofs that Wittgenstein develops in later works like the *Remarks on the Foundations of Mathematics* (1937-1944, henceforth RFM) and the *Lectures on the Foundations of Mathematics* (1939, henceforth LFM). Wittgenstein's remarks on mathematical proofs have been widely discussed and included in many debates, but I believe some of the most valuable insights to be gathered from them are either still written in fragmentary form or get somewhat lost in broader arguments, like in Wittgenstein's criticism against the philosophical search for foundations for mathematics. I argue here that we can gain important philosophical results through a study that centres on his analysis of proofs and mathematical practice, one that carefully examines Wittgenstein's claim that proofs produce conceptual changes in our understanding, a claim which in turn criticises the more standard definitions of mathematical objectivity and argues it is ultimately anchored in practice and mathematical abilities. By highlighting the relevance of the role mathematical practice plays in the establishment of proofs, the thesis also makes important links between philosophy of language and philosophy of mathematics.

The first chapter introduces general mathematical concepts, proofs and theorems with which we will work throughout the paper. Section 1.2 and its subsections present the reader with Wittgenstein's account of language and mathematics in order to gain a better grasp on his remarks on the normativity of proofs and the relation between the latter and actual mathematical practice. Section 1.3 ends the chapter on a critical note on Wittgenstein's views, mainly highlighting remarks on their alleged conventionalism and lack of correction criteria, on the one hand, and on the other also noting the apparent incoherence of considering proofs

conceptual changes, that is, how this contradicts the intuitive notion of mathematical discovery.

The second chapter addresses such criticisms and dispels misinterpretations of Wittgenstein's remarks on proofs, drawing from relevant connexions between his philosophy of language and philosophy of mathematics. It digs deeper into the meaning of notions that are usually left undefined or neglected as unimportant, even mistaken, in his philosophy, such as 'form of life' and the 'hardening' of empirical regularities. This section also develops a positive account of Wittgenstein's philosophy of proofs by clearing up its solid definition of mathematical objectivity, drawing a clear distinction between the latter and the objectivity of empirical sciences. Section 2.1 contains the replies to the criticisms of the first chapter and a defence of Wittgenstein's account. Section 2.2 elaborates on Wittgenstein's distinction between proofs and experiments using the concepts of 'form of life' and 'conceptual change' examined in the first chapter. Yet it also traces important links between empirical and mathematical statements by explaining how mathematical rules are shaped by empirical regularities and, in turn, how mathematical order gives shape to every intelligible expression, including of course any statement that describes the empirical world.

The final chapter summarises the achievements and shortcomings of Wittgenstein's account of mathematical proofs. It pays special attention to the visual metaphors that his account draws, like the idea that a proof does not reveal new truths but rather makes us see new physiognomies, and explains how such observations relate methodologically to the view that philosophical progress consists in providing a clear view over the entanglements of language, a synoptic vision that clarifies the correct use of expressions. This also motivates a reflection on Wittgenstein's arguments' heavy reliance on geometrical metaphors, and suggests his account must cover more ground and extend to arithmetic and other areas of

mathematics if it is to be convincing. Finally, some concluding remarks and questions are added regarding the need to clarify further what kind of modification could a proof produce in mathematics, and if such is indeed a conceptual change.

I will use the standard abbreviations of Wittgenstein's works:

TLP – *Tractatus Logico-Philosophicus*

PI – *Philosophical Investigations*

RFM – *Remarks on the Foundations of Mathematics*

PG – *Philosophical Grammar*

LFM – *Lectures on the Foundations of Mathematics* (Cora Diamond, ed.)

OC – *On Certainty*

Chapter I

Understanding mathematical proofs

There is an open philosophical discussion questioning if and how mathematical proofs establish truths about mathematics. At first glance, questioning if proofs establish truths at all may seem odd, for we certainly have solid, growing, mathematical knowledge, whose tenets we regard as necessary, irrefutable; and this is due, presumably, to their unquestionable truth. Mathematicians, engineers and every person acquainted with basic arithmetic use mathematical operations with utmost certainty. After some education, we understand that the results we obtain in our basic, day to day calculations or geometrical constructions can be rigorously proved if needed, for there is a set of axioms and theorems that firmly supports them and justifies our use of them. But, regardless of this solid trust in mathematical knowledge, we are less prepared to explain what would seem like very basic features of it: what are mathematical statements about? What do mathematical laws rule over? How, if at all, can they be distinguished in detail, for instance, from laws of nature or laws of logic? It soon becomes difficult even to clarify what a mathematical proof says in a non-redundant way, that is, without appealing to the principle that is established by it or to the inference rules that were used to derive it. This would just be restating the proof, which may turn out to be the only answer we can provide in the end, but this dissertation joins the efforts that question if it is possible to know more about what a proof establishes, aiming to understand what it means within mathematics and as a new piece of knowledge.

This chapter introduces a general, standard notion of mathematical proofs and presents some examples to get a better idea of what proofs look like, how their steps are derived, and the philosophical questions they raise regarding what they are about, what they demonstrate and also why they are endowed with logical necessity. I will then introduce the

way Wittgenstein approaches such questions. To be able to understand and criticise his analysis, two crucial clarifications are in order: first, what does Wittgenstein contend by defining mathematics first and foremost as an *activity* which is embedded within a particular 'form of life'? Is a 'form of life', whatever it may mean, a relevant concept to understand mathematics? And secondly, how, if at all, could we make sense of mathematical proofs being 'conceptual changes'? How could a proof change mathematics?

Having obtained a better grasp of Wittgenstein's research questions, his philosophical approach and the arguments he develops to explain the norm-like nature of mathematical proofs, the chapter addresses some of the most relevant criticisms of Wittgenstein's views. I will point out their importance as much as their strengths and flaws. The point of this extensive, albeit necessary, preliminary groundwork is to eliminate misunderstandings many commentators have raised and that have become long-standing exegetical obstacles that have hindered a serious study of Wittgenstein's philosophy of mathematical proofs. It is also useful to keep these criticisms properly spelled out and close at hand, since they must be dealt with to locate Wittgenstein's position in the philosophy of mathematics' landscape, which is on its own a rather complex task. But more importantly, understanding these criticisms will provide guidance to see what Wittgenstein's remarks are really up to, where they fall short of sound explanations and also where they strike a particular vein of promissory, fruitful philosophical research. It will become evident that the crux of this debate between Wittgenstein and his critics is the concept of mathematical objectivity. Very simply put, Wittgenstein's critics firmly deny that a cogent account of mathematical proofs, with a clear definition of the nature of mathematical objectivity, can be accomplished within his unorthodox outlook. The last part of the chapter brings together and structures the criticisms that have been advanced, and introduces the next chapter proposing a way of reading Wittgenstein's

views on proofs that does preserve a solid account of mathematical objectivity.

1.1 Some preliminaries

It is commonly accepted that mathematics is, to put it in neutral terms, a sector of research in the quest for truth (cf. Dummett, in Jacquette, 2002; and Brown, 1999), in a similar way as empirical sciences are also involved in such quest. But we need a more detailed explanation of how mathematics specifically contributes to it. A common standpoint is to say that mathematical statements assert something about numbers, geometrical figures, angles, spaces and the like, in an analogous way as propositions of the empirical sciences assert truths about the empirical world, energy, subatomic particles, tectonic plates and so on. Both mathematical and empirical statements seem to assert that something obtains. The former state that, say, an equivalence, a numerical expansion, a pattern or order, is the case. Yet mathematical statements are also different from empirical propositions because whatever is equalled, expanded or ordered is nothing that we observe in space and time, nothing affected by causality, nothing contingent. Moreover, mathematical statements can be logically proved, beyond doubt, as correct.

Philosophy has tried to make sense of this elusive, non-empirical content of mathematical proofs. Depending on the philosophical stance adopted, one might understand that mathematical proofs lead to discoveries of abstract objects, their properties and relations; or perhaps to new mathematical structures or more basic and effective rules to formalise mathematical knowledge. One might consider that mathematics describes an abstract realm which is different from and independent of our empirical reality, and equally independent of our thoughts. This last idea is the fundamental tenet of mathematical platonism, a philosophical and mathematical view in which 'mathematical objects are there and stand in

certain relations to one another, independently of us, and what we do is to discover these objects and their relations to one another. [...] For the platonist, the meaning of a mathematical statement is to be explained in terms of its truth-conditions; for each statement, there is something in mathematical reality in virtue of which it is either true or false.' (Dummett, 1959, p. 325) To cite an important alternative to platonism, the formalist account of mathematics, on the other hand, defends the existence of mathematical objects, but considers these are the very mathematical formulae and symbolism we use to express truths in mathematical language, so there is no need to talk about objects like perfect circles or the set of real numbers. Proofs, on the other hand, are legitimate objects.

In any case, we can see that images of a 'mathematical domain' where mathematical facts obtain are not hard to conjure up. To illustrate this more convincingly, let us consider a particular example, the proof that $\sqrt{2}$ is irrational. In the proof, we make the hypothesis that the opposite holds, i.e. we entertain the possibility that $\sqrt{2}$ is rational, and then demonstrate that this would be logically contradictory, forcing us to reject the hypothesis and accept that $\sqrt{2}$ is indeed irrational. The reasoning we will use to derive the proof is rigorous and based on clearly defined concepts.

We can start by defining the kinds of number in question:

A *rational number* can be expressed as the ratio of two integers, p and q , or in other words the fraction p/q , where p and q are integers and $q \neq 0$. p and q have no common divisors.

Since it is possible that $q = 1$, all integers are rationals. (That is, $1 = 1/1$, $2 = 2/1$, $3 = 3/1$...)

Rational numbers can also be expressed as decimals of finite length or decimals that show a repeating pattern.

Examples of rational numbers: 8, $3/5$, 0.3125, 0.2954545454..., -27.

Irrational numbers cannot be expressed as fractions, nor as finite-length decimals nor as repeating-pattern decimals. Examples: π , 2.645751311..., 1.4142135623...

Rational and irrational numbers together make up the set of *real numbers*.

A square root of p is a number q such that $q^2 = p$. Any positive integer has 2 square roots, one positive, one negative. Example: $\sqrt{16} = 4$ and -4 , since $(4)^2$ and $(-4)^2 = 16$.

We also must note that the square of an even number is even, that is,

For an integer n , the square of the number $2n$ is $4n^2$, which is evidently even

And the square of an odd number is odd,

For an integer n , the square root of $2n + 1 = 4n^2 + 4n + 1$, which is odd

Now, to the proof. We will use these definitions to prove that the hypothesis that $\sqrt{2}$ is rational contradicts itself. This move is a *reductio ad absurdum*:

Suppose we can express $\sqrt{2}$ as the ratio of two numbers, p and q , which, as explained above, have no common divisors. We would have

$$(1) \sqrt{2} = p/q$$

therefore,

$$(2) 2 = (p/q)^2$$

$$(3) 2q^2 = p^2$$

Now, we are dealing here with squares of even numbers, since we are multiplying one side of the equation by 2. Since the square of even numbers is even, we would have

$$(4) p = 2r$$

r is another integer. Now we substitute this in the equations above. We have

$$(5) 2q^2 = 4r^2, \text{ therefore}$$

$$(6) q^2 = 2r^2$$

Now, q is also even, so that

$$(7) q = 2s$$

and s is another integer.

p and q , as we can see in lines (4) and (7), do have the common factor 2, which contradicts our hypothesis that $\sqrt{2}$ can be expressed as the ratio of two numbers which have no common divisors. Therefore, to avoid this contradiction, we must accept that $\sqrt{2}$ is irrational.

The *reductio*, as we can see, crafts a very compelling argument, established with full certainty, for we cannot turn our back on what is logically correct and fall into contradiction, invalidating all the reasoning we've built. The effect of the *reductio* is something like this: it is as if we traced a path of sound reasoning and arrived at a crossroad where, if we affirm the hypothesis that we've been entertaining, we would end up losing our own ground, our own traced path, so we have no option but to go the other way and deny the hypothesis in order to stay on track, so to speak. That is, we rather retrace our steps than lose our very possibility of moving. But what, then, constitutes these grounds we cannot afford to lose?

A platonist account would take the proof at face value and consider it an ontological statement that reports that there cannot exist a rational number r such that $r = \sqrt{2}$, or that $\sqrt{2}$ has the property of being irrational. These statements become a description of how the number-theoretical landscape is constituted, what can, as it were, fit within it and what cannot.

Many mathematicians have adopted such stance, affirming that there exists such a landscape and that '[m]athematical theorems are true or false; their truth or falsity is absolute and independent of our knowledge of them' and that '[i]n *some* sense, mathematical truth is part of objective reality.' (Hardy, quoted in Brown, 1999, p. 10)

But Benacerraf's (1973) old dilemma reminds us that sticking to the platonist picture in mathematics has its costs. The platonist must explain what mathematical truths are about and how we can apprehend them, i.e. how we have mathematical knowledge. They must also clarify if we are to have one uniform notion of truth, one that is equally applicable to any meaningful sentence in language, so that we can say, for instance, that it is true that $2+2=4$, and it is also true that sodium and chloride make salt. In other words, we must be able to explain if the truth-conditions of empirical and mathematical judgements have the relevant parallels (Benacerraf, 1973, p. 663), even if they are not identical. One way to give these explanations may begin by arguing that mathematics simply deals with more fundamental aspects of reality. For example, while chemistry studies interactions between particles, mathematics reveals the very geometric nature of their spins and molecular arrangements. That would explain mathematical statements' higher degree of certainty: however molecules may arrange, they will arrange in an identifiable geometrical shape. On the other hand, the platonist can also object against Benacerraf's constraints on mathematical truth. After all, a demand for an epistemology of mathematics that is continuous with that of the physical sciences ignores significant differences between them.

Furthermore, it is possible to reject platonism and endorse another philosophical and mathematical account that still cares to preserve a notion of mathematical truth. As we briefly introduced above, formalism understands mathematical knowledge as knowledge of the rules to manipulate logical symbols and formulae, to master logical syntax in proof-derivations. In

Hilbert's formalism, for instance, statements about infinity, which platonism counts as legitimate, are seen as problematic and ultimately are rejected. For infinity is nowhere to be found in reality, not in nature, nor in rational thought (Hilbert, in Benacerraf and Putnam, 1998, p. 201). The mathematical truths that are upheld in the formalist view do not affirm that mathematical facts obtain in the mathematical landscape, but talk about the consistency of mathematical structures. A proof, rather than a number or a triangle, is an example of a concrete and identifiable mathematical object, which can be described and which has properties (like that it does not lead to contradiction) (Hilbert, in Benacerraf and Putnam, 1998, p. 199).

In the following pages, I will argue that it is both possible and fruitful to analyse the epistemological contributions of mathematical proofs without notions of truths about abstract or formal objects, but rather, following Wittgenstein's approach, as conceptual developments that transform the possibilities of thought and action, so that mathematics must be understood as a relevant constituent of the way we think, develop understanding, form concepts, and act. This approach offers the possibility of making a sharper distinction between truth-apt and non-truth-apt propositions, of highlighting the unique nature of mathematical necessity, as well as taking a closer look at the relation between a proof and mathematical practice proper.

1.2 Wittgenstein's approach: conceiving mathematics as a rule-governed activity

Wittgenstein strongly disagrees with the philosophical analyses briefly described above. He does not merely reject platonism and formalism to provide a better account of what mathematical proofs talk about, but rather considers that the very research questions of the mentioned programmes are wrongly put. That is, he questions if it is at all cogent to say that proof-derivations assert some kind of content (i.e. that they say something about abstract or

formal objects), or in other words to say that proven statements are truth-apt (that, for instance, the proof of the irrationality of $\sqrt{2}$ expresses a truth about the properties and constitution of irrationals), and that it is philosophy's task to explain what these contents are. His criticism of the content of mathematical statements can be traced back to writings as early as the *Tractatus Logico-Philosophicus* (published in 1922, henceforth TLP). In 6.21 he argues that truth and falsity can only be attributed to two kinds of propositions: logical propositions, which are tautologous or contradictory, i.e. always true or always false; and factual propositions, which can be true or false depending on what they depict, that is, if they do or do not picture a fact that effectively obtains in the physical world¹. Notoriously, he does not classify mathematical propositions (and explicitly calls them pseudo-propositions, TLP 6.2) into either of these categories. He considers them neither logical nor factual, though does not fully explain why. Interestingly, yet too briefly, he mentions that philosophy must pay special attention to the use of expressions in mathematics, that asking ourselves what an expression *does* will lead to productive results (TLP 6.211), perhaps hinting at future developments in his writings concerning the links between the uses of an expression and the determination of its meaning. In any case, at this point, mathematical pseudo-propositions are regarded not as propositions which have a content, which are 'about something', but as vehicles which guide us from contentful expressions to the derivation of more contentful expressions, preserving their sense. That is, much like logical inferences, mathematical operations would preserve the truth-aptness of factual propositions. To put a very simple example, I can report that, say, there are 3 apples in my basket. Then if someone else tells me they added 2 more apples to my basket I will rightly make the judgement that there are now 5 apples in my basket, and this

¹Of course, strictly speaking, he also discusses 'elementary propositions' and gives a complex explanation of what makes them true, but I believe this conception is endemic to the *Tractatus* and will not affect the current investigation on proofs, so we need not stop to give a full account of them.

would be a true judgement. Of course, this is because I follow the arithmetical operation $3 + 2 = 5$. If I look inside my basket and see there are not 5 apples there, but only 4, I will judge, again truly, that the person who reported they had added 2 more was lying or that one apple was removed before I checked, etc. The operation I will not put in doubt, the operation that will be a guideline in my judgements, will remain $3 + 2 = 5$.

In the *Philosophical Investigations* (published in 1953, henceforth PI) Wittgenstein analyses language as a practice. He investigates the possibilities of expressing meaningful statements in common scenarios of linguistic exchanges, interested in the way linguistic rules are actually followed, and on determining what is the actual source of the logical compulsion that, say, prevents the simultaneous use of mutually contradictory judgements. Importantly, Wittgenstein continually draws strong links between language and mathematics, and notices the wider, similar philosophical questions we can ask about the meaning of linguistic and mathematical statements. Take, for instance, the fact that senseless combinations of words, like 'Colourless green ideas sleep furiously' can easily be formulated in language and, furthermore, are structurally correct. This makes it difficult to rigorously determine the meanings of expressions (i.e. if and how the meanings of constituent parts add up to give meaning to the sentence) and the correct use of language, and has led philosophers and logicians to construct formal languages in which there is lesser risk of ambiguities. But then, Wittgenstein asks, can we also say that we can *think* senseless things? (PI §513) How would we do that? In mathematics, he goes on, we find that some sentences do not immediately strike us as senseless, though after thorough investigation it turns out they are, in a way. This happens with mathematical conjectures, i.e. unproved mathematical statements which look like sentences we can understand, yet, Wittgenstein remarks, whose *sense* is to be determined. This way of understanding conjectures, of course, stands opposed to the

platonist understanding, in which the mathematical conjecture will be very much like a scientific hypothesis: a truth-apt statement whose *truth* is to be determined, its sense is already clear. Here's an example of what Wittgenstein wants to highlight, he asks

'Does the sequence 7777 occur in the development of π ?' (PI §516)

The question perhaps invites to picture the sequence '7777' inserted in a row of random numbers at some point in the expansion of π , so we could say it is a perfectly intelligible idea. Yet it takes more than imagining '7777' just as easily as we can imagine '4159' occurring in π . As it turns out, it is more than highly likely that such sequence occurs in π , but there is no available proof that it does. Therefore, if there were a proof that '7777' cannot occur in π , then we would have to conclude that some mathematical proofs may lead us to conclude we cannot imagine something we thought imaginable (PI §517). To put a more tangible example, suppose we learn how to construct a pentagon with ruler and compass, we master how to do the measurements, the lines, the circles, and once we have done it a couple of times we are quite entitled to imagine the process is fairly similar if we want to construct other polygons. We think, for instance, that we can construct a heptagon and even 'set our minds to it', that is, imagine where some lines and vertices would go, the circles we would trace, etc. Yet, for all that, it is impossible to construct a heptagon with ruler and compass.

The relevant questions here are a) if it is always clear what a mathematical statement is about, and b) what would the consequences be if it isn't. Is '7777' already there in π , just 'too far away' for us to see it? Could Fermat really have known what his famous conjecture was about? Is the latter question another way to ask 'did he actually have a proof?'. Questions concerning the 'aboutness' of mathematics are difficult to deal with when it comes

to open mathematical statements like conjectures. This, Wittgenstein would want to say, is a shortcoming of the philosophical analyses that read mathematical statements as if they expressed truths. Wittgenstein suggests an alternative approach: to analyse mathematics as a practice, and mathematical expressions as rules that govern such practice. This has proven to be a double-edged initiative. On the one hand, analysing the practice of mathematics is an important philosophical task, and one must be prepared to account for the undoubtedly effective use of mathematical calculations and models to investigate the universe. Yet, a seemingly undivided focus on the practical aspects of mathematics makes many doubt the seriousness of Wittgenstein's intentions. In other words, philosophers doubt if Wittgenstein's approach neglects primordial, more abstract areas of mathematics, higher mathematics and pure mathematics. But before passing judgement, let us look at what he really is up to.

Wittgenstein wants to analyse mathematics in a similar way as he analyses language, looking into 'rule-following' and communal, public linguistic use. He observes that mathematical rules share many features with linguistic ones: they can be followed in a precise enough way, precise enough for specific purposes, and this is in great part due to perceptions of sameness and regularity shared by communities, features of language we often dismiss as trivial. Wittgenstein, however, puts them at the very centre of philosophical debates about the meaning of expressions when he affirms that '[t]o understand a sentence means to understand a language. To understand a language means to be a master of a technique.' (PI §199) In other words, to understand what a sentence means I must have a grasp of the linguistic rules that govern it. Grasping these rules requires that I know my way around the language where the sentence belongs. And finally knowing my way around it means I know how to use the language effectively. Wittgenstein takes the above notion of technique to mean something similar in mathematics, that is, mastering a mathematical technique will be

considered an ability to 'move around' mathematics, to understand and manipulate meaningful expressions within it.

Late in the RFM, Wittgenstein reminds us that '[o]nly in the practice of a language can a word have a meaning' (VI §41). He writes this in the context of an exposition that stresses that we must understand and analyse mathematics as essentially a rule-governed human activity, intertwined with language, and part of other activities and institutions that may not appear salient to us, but that are fundamental for any mathematical reasoning to take place. For example, he asks '[c]ould there be arithmetic without *agreement* on the part of calculators? Could there be only one human being that calculated? Could there be only one that followed a rule?' (RFM VI §45, emphasis mine) 'Language', for Wittgenstein, 'relates to a way of living. In order to describe the phenomenon of language, one must describe a practice, not something that happens once' (RFM VI §34). Philosophy's task is to make such descriptions of the practices that make up language and mathematics. To do so, Wittgenstein claims, one must understand that mathematics is set within a form of life.

1.2.1 Form of life

To imagine a language, Wittgenstein argues, we must imagine the form of life where it has a use (PI §19). The point of this remark is to draw our attention to the fact that language is an activity that does not occur in a vacuum, that there are physical and cultural contingencies, as well as practical purposes, that have necessarily shaped what language can and cannot express, and to highlight that this is not a trivial matter. The language we use becomes something like a home we inhabit and a platform from which we articulate our judgements. We distinguish if someone is using a language by comparing their use of expressions to the way we communicate. 'The common behaviour of mankind is the system of reference by

means of which we interpret an unknown language.' (PI §206) This means there has to be enough regularity and order in a language for us to recognise it as such.

We must also understand that there are multiple uses for different expressions in different linguistic contexts, many languages, as it were, within language. Wittgenstein explains that this variety produces different, dynamic *language-games*. The purpose of the analogy with games is to give an idea of the variety of uses of words and phrases, of the malleability of language. Just as activities as varied as chess, rugby and 'ring a ring o' roses' can be considered games we play, so do activities like giving and obeying orders, describing and measuring objects, constructing objects from a description, reporting and speculating about events, doing scientific investigations, joking, translating, etc. (PI §23). Terms like 'world' or 'reality', for example, play different roles when we predict facts and when we make them up. Wittgenstein's purpose is to contrast this account of language against philosophical aims to unveil the one underlying logical structure of language, as if language functioned for homogeneous purposes.

Wittgenstein argues that, according to our necessities, new types of languages, or language-games, come into existence, and others become obsolete and get forgotten (*ibid.*). Think of how the scientific discourse on astronomy, for instance, has changed through history. Galaxies and elliptic orbits were unheard of, and now they are a common concept even for the non-experts. They are now necessary to describe the universe, whereas concepts that presupposed the Earth was the centre of the universe have been neglected. Interestingly enough, in this same section, where he underlines the varieties of language-games, Wittgenstein compares these linguistic dynamics to mathematical changes, though, again, he does not elaborate on the comparison. I will now elaborate on the relation between language-games, mathematics and mathematical changes, and the concept of 'form of life'.

One major problem is that the account of 'form of life' is as ambiguous as it is ambitious. It is supposed to do nothing less than supply the standards of mathematical objectivity, and explain how mathematics is a rule-governed activity, yet more than a run-of-the-mill kind of game, indeed an activity on which intelligibility hinges. But at the same time it is difficult to tell if a form of life is more than just a sociological account of human development or if it is a legitimate philosophical concept, if it plays a relevant role in the constitution of mathematics or if actually mathematics gives it its form. In short, it is hard to tell if mathematics is how it is due to our form of life or if it is the other way around.

Now, Wittgenstein understands that our form of life is not just a minimal condition, prerequisite or setting needed for mathematics to occur. The notion of form of life simply takes into account that language and mathematics form and develop the way they do due to specific physical and human settings into which linguistic and mathematical agents are born and where they develop uses, customs, expectations, fears, certainties. The purpose is to highlight that mathematics is an activity which arises in our form of life and therefore interacts with it, shaping the possibilities of action, imagination, thought. Avigad (2008) points out that there is something of a reciprocal relationship between mathematics and understanding, that just as we speak of understanding theorems, proofs, solutions, definitions, concepts, and methods, 'at the same time, we take all these things to contribute to our understanding.' (pp. 320-321) So, going back to the question above, Wittgenstein seems to argue that our form of life is constituted at the same fundamental level as our mathematics, that they have to be understood in unison, instead of one depending on the other.

It is not Wittgenstein's interest to do a historical or anthropological study of what were the particular events that constituted forms of life in which language arose and how they specifically came to be, but simply to point out that such forms of life are given and we cannot

ignore them (PI II xi, p. 226), but rather must actively include them in our philosophical investigations. We simply have to work with them. To accept them, however, does not, and in fact cannot, amount to passively resign ourselves to our lot and take this acceptance as an enlightening explanation of why the world is how it is, why we think how we do and why mathematics happens to be the way it is. There remains to explain what role mathematics plays in our lives. We have a special conception of mathematics, 'an ideal of its position and function' (RFM VII §19), and this needs clarification.

Wittgenstein asks

'What is the criterion for the way [a] formula is meant? Presumably the way we always use it, the way we were taught to use it. We say, for instance, to someone who uses a sign unknown to us: "If by «x!2» you mean x^2 , then you get this value for y , if you mean \sqrt{x} , that one". –Now ask yourself: how does one *mean* the one thing or the other by «x!2»? *That* will be how meaning it can determine the steps in advance.' (RFM I §2, and a very similar version in PI §190)

'And how does *the way* we mean it come out? Doesn't it come out in the constant practice of its use?' (RFM I §10)

Just what we take as our 'constant practice' or constant use of expressions needs to be more clearly defined. There is a risk of being too ambiguous and leave room for considering that mathematical expressions assert truths, that there is a notion of mathematical content behind our practices, something which Wittgenstein definitely wants to avoid. Such stance involves thinking that we follow rules and formulae because they correspond to some (empirical or abstract) reality. Wittgenstein warns that in our daily use of language we talk as if some propositions already follow from others even if we haven't framed them explicitly in syllogistic form; or we describe things as if we believed, say, that a straight line already connects two points; or assume that the transitions that correspond to the continuation of a series, like the

sequence of natural numbers, are already done, and that we are just, in a sense, merely 'tracing' all of these relations (RFM I §21). We think that they depict facts that obtain. Just think of simple, apparently harmless statements like 'the shortest distance between two points *is* a straight line', '5 *is* the successor of 4'. They seem perfectly true and uncontroversial. What is not so simple is the whole picture of mathematics they seem to carry with them. From the way these statements are framed it is easy to assume that, and certainly very easy to talk about, the *fact* that one number is *really* the successor of another, that is, that they share a relation of proximity perhaps analogous to the relation between two substances that are always found together in nature, or mixed in a definite proportion or ratio that is always the same, like in ionic compounds. We can understand it as the fact that '5 always follows 4 in the natural number series', as if the 'act of following' occurred in some sense, and that the relation '*b* follows *a*' indeed happens due to some fact or property that makes one readily and uniquely available to place after the other. As in the proofs explained above, we conjure up an image of mathematical objects, properties and relations obtaining.

Instead, Wittgenstein argues that when we perform a mathematical operation, like when we work out a calculation, but also when we derive a proof, we should not consider that the reason we derive unique, necessary results is because the rules of inference *correspond* to some sort of reality. 'Here what is before our minds in a vague way is that this reality is something very abstract, very general, and very rigid. Logic is a kind of ultra-physics, the description of the "logical structure" of the world' (RFM I §8). We invoke superlative philosophical facts, like that all the movements a machine could possibly do are already present in the machine's diagram, comparing its future movements to objects that are lying in a drawer and we take out (PI §193). Against these deceiving pictures, Wittgenstein affirms that philosophical explanations of the way we signify in language and calculate in

mathematics, or reasons why we follow rules as we do, will at some point give out and not because we have reached the most logically simple components or foundations of language or mathematics, but rather because all that sustains language and mathematics is our agreement in action, which is how it is thanks to our shared form of life (PI §211, §217).

Wittgenstein elaborates on this in RFM III §58: to say that it is a fact that, say, 129 is divisible by 3 or that it is true that it is divisible by 3 is not problematic so long as we recognise that we are not asserting a fact dependent on mathematical reality and independent of our mathematical techniques. He suggests we try conceiving it more like a fact of the calculating technique, reminding us that properties of numbers do not exist outside the calculating. It seems natural to think we discover that 129 is divisible by 3 some time in our mathematical training. But on a second look, we have to admit that if we followed the techniques of multiplication and division correctly, then it is no surprise we got such a result. '129 is divisible by 3' is then more accurately described as a correct application of the operation of division, not a true statement that refers to a discovery about numbers and their properties. It certainly looks a lot like a discovery, for instance if we are looking for a way to shelve 129 magazines in 3 cabinets of the same size. We make the calculation and we are entitled to say we 'found' a way to sort them. While it is also true that we do not foresee or already apprehend the results of relatively long mathematical operations, it is slightly misleading to call them discoveries as if we stumbled upon them unexpectedly. Even if we indeed ignore how much is, say, 762 times 2521, we cannot say its result surprises us, for we figure out the result employing a well-known technique we employ daily, not by noticing some sort of fact. Mathematical statements, Wittgenstein wants to say, are not truths, not even overdetermined truths, but rather rules. 'This must be the result' means there is no open question left about the calculation. There is only one possible output given the rules of transformation in the

technique. Because of this normative character, every correctly performed operation is itself a rule, in a sense. We will expand on this below, on the next chapter, on the distinction between proofs and experiments.

It remains, no doubt, an empirical fact that we calculate in certain ways, that is, we can describe how we physically, day by day, calculate, prove and measure on blackboards, paper and computer programmes. Yet, Wittgenstein writes, that does not make the propositions used in calculating into empirical propositions (RFM VII §18). Again, the empirical ways we happen to write down numbers, carry sums, draw symbols, are the way they are for some historical reasons, but that is not the philosophical answer we are looking for when we enquire about the nature of mathematics.

To explain the role played by our form of life in mathematics Wittgenstein highlights how heavily the meaning of our expressions depends on the use we give them. This is also why he has been read as a conventionalist about mathematics and why it is difficult to make sense of what, if anything, he defines as mathematical objectivity. One way to breach a trail is by being clear on how Wittgenstein characterises linguistic usage. In the following I will attempt to trace this in the briefest way possible.

First, Wittgenstein explains language use, then how such use is embedded in our form of life, and finally how our form of life sustains mathematical objectivity, that is, that the objectivity of proofs is ultimately answerable to regulated actions. Wittgenstein remarks that as children we begin to acquire language through very simple practices, at times very much like two builders who only use a couple of words to designate construction materials. One yells 'brick' and the other brings a brick to him. So we learn that some words designate certain objects, and we begin to correlate them, identify their distinctive features, learn their relevant synonyms, and so develop new possibilities of expressing statements. Now, this describes a

part of language very well but, Wittgenstein goes on to say, there is a great variety of language-games within language, some more simple, some more complex, that we can construct for different purposes. It is through engaging in these games that we learn to master a language. Bringing up the image of a game shows an intuitively accurate process of the interactions needed in learning to use language. We learn language in a similar way we learn to play a game. We start off without knowing how to play, we see others do it. As in a game, people's linguistic interactions are sanctioned by communal accords, say like when we agree that we are playing one game and not another, when we agree that we have started and when we finish, when someone wins and when someone makes an invalid move. These practices make us understand that there is a right and a wrong way to follow instructions. What confers meaning to expressions and what ultimately makes the multiple language-games work and interconnect is the form of life where they are embedded and used. The meanings of 'agreement', 'rule' and 'same' are intertwined (PI §§ 224-225), and agreement and rule-following can only take place within a community where the use of expressions is publicly prescribed and sanctioned. 'One does not learn to obey a rule by first learning the use of the word "agreement". Rather, one learns the meaning of "agreement" by learning to follow a rule [i.e. the rules that govern the use of the term 'agreement']'. If you want to understand what it means to "follow a rule", you have already to be able to follow a rule' (RFM VII §39), since to be able to achieve any understanding at all, we must be able to follow explanations, discern their scope, we must be trained in reading and learning patterns and in discerning correct ways of performing these from incorrect ones. In this sense, proceeding in accordance to a rule is founded on, or presupposes, agreement (RFM VII §26). Now, there is obviously a problem with explaining what it is to follow a rule by saying that one *already* knows how to follow a rule. We may say that to be able to follow a rule is perhaps a prerequisite for

knowledge, but Wittgenstein's particular observations still need to be spelled out more clearly.

Wittgenstein admits it is difficult to avoid redundancies when defining rule-following, but also remarks that we have to accept that our language works within a framework of agreements, and it is hard to account for them without already mentioning them: this is the extent to which language depends on agreements and regularities of the use of expressions. 'The application of the concept "following a rule" presupposes a custom. Hence it would be nonsense to say: just once in the history of the world someone followed a rule (or a signpost; played a game, uttered a sentence, or understood one; and so on).' (RFM VI §21)

Now, communal agreement does not determine the facts that constitute the world, but it is crucial that we agree on what we call 'fact' and 'true' to learn what does. It is not a trivial demand to ask that we must have a common criterion to find out if some sentence is true or false. We know that, usually, most of our sentences must refer to some fact and that we need to be able to check, in some way, if such fact obtains, make sure we are not being deceived, etc. In other words, we have learned to use the concept 'truth', to call some things possibly true and understand that others cannot be true. This agreement happens within the language we use, it is brought about by the very use of language: it is not an arbitrary agreement of opinions, but an agreement in form of life (PI §241). This point needs to be discussed because such agreement in form of life will provide the objective criteria for the correctness of mathematical results. The reasoning goes something like this: it is quite independent from us that, say, $2 + 2 = 4$ (PI II xi, p. 226), such operation is actually an objective feature of our calculus. Yet this is a calculus which is made possible because of standards of correctness and certainty that stem from our form of life. Being in common accord is necessary to follow derivations properly. The objection Wittgenstein wants to block is the question 'if everyone believed that $2 + 2 = 5$, would $2 + 2 = 4$ remain the correct operation?' Wittgenstein

challenges our imagination: it is not at all trivial that we can imagine the conditional, that we can make sense of everyone believing that $2 + 2 = 5$, for this is not just a belief, it touches upon practice. An addition is not a fact of experience because, by the rules that govern it, it can't be otherwise. That is what a rule is, an unquestionable procedure by which we build all judging (RFM VI §28), all questioning and inferring hinges on it. People believed for a long time that the Earth was the centre of our solar system, then they were proved wrong. Yet, there was nothing unimaginable about it, nothing that would lead to logical contradiction. It was simply an imaginable, possible fact that did not obtain. We cannot judge the 'possibility' that ' $2 + 2 = 5$ ' in a similar fashion, for the statement runs against the rules of logic and mathematics. The implications of such hypothetical widespread belief turn on themselves, to add $2 + 2 = 5$ would entail an unsustainable, inoperable mathematics. It is not the case that we would have a mathematics just as ours, except for that 'minor' change, there would be no mathematics at all.

A good analogy to better understand Wittgenstein's concepts of rules and rule-following considers comparing the model of a machine, which describes how the machine works and moves, with an actual machine built under such model's guidelines. Trained as an engineer, Wittgenstein is more than aware that a machine's parts become worn out, some more quickly than others, that the machine is susceptible to temperature changes, friction, etc. (RFM I §122) Just as we need to see the machine operating to fully understand what it does and how it does it, we need to analyse language in practice, as an activity, surrounded by empirical contingencies, yet logically, internally, determined to do certain operations. To work out how a mechanism will operate is similar to working out a multiplication rule: the outcome is normatively determined, but is not, as it were, already present. Saying that the machine's action seems to be in it from the start is like saying that the future movements of a machine

are like objects which are already lying in a drawer and which we then take out (*ibid.*). A similar claim is made about mathematical formulae, like the rule for the expansion of π for instance, as if the numbers in the expansion existed and, say, we could check if the pattern '7777' is part of those numbers or not.

The philosophical point of this analogy with a machine is that mathematical laws determine possibilities for action just as a diagram or description of how a mechanism functions determine the actual workings of a machine. There are empirical constraints, no doubt, on the construction of the actual machine as well as on the development of the mathematical techniques we have, and though we cannot foresee how the machine will actually, in detail, work, we can perfectly well picture the movement the machine is supposed to have. The actual movement will undoubtedly be very similar. Such movement is determined, in a sense, we see it in the dotted lines and arrows, we can understand how it will proceed. We understand that a machine is, for instance, making a rotating movement even if the actual rotations vary because, say, some piece spins around more loosely or tighter than it seemed in the diagram, etc. But even if different people model the machine and build it with different materials, the outcome remains basically the same (PI §194). We can understand it so well we can figure out unforeseen possible applications for it before building it, or ways in which it could improve. The possibility of a machine's movement thus stands in a unique relation to the movement itself, closer than that of a picture with its subject. We can say that the empirical world, experience, will determine whether some drawing of Cologne's Cathedral really pictures the actual cathedral. Yet it is not an empirical fact that a machine has precisely a certain possibility of movement (*ibid.*). The possibility Wittgenstein is talking about here is different from the empirical conditions of the movement. This suggests that he refers to a necessity beyond the empirical constraints of the machine, something closer to the order or

logic that governs the movement, beyond laws of friction or gravity. This is the idea behind arguing that mathematical statements are not quite accurately described as contentful or truth-apt, for, similarly, we can argue that there is no fact or set of facts that could picture what mathematical statements establish, that is, nothing that could depict the character which makes them *norms* that dictate possibilities of understanding, rather than ultra-general, abstract *truths* which talk about abstract objects and entities of the sort.

When Wittgenstein argues that the correctness of mathematical operations depends on our constant, regular practice, he does not say that mathematics depends on whatever we do that reflects our mood or intentions at the time, but rather that we participate in a practice that has set specific rules for formulas, standards for their correct use, sanctions to diverging results, etc. In RFM I §34 he explains that the final argument against someone who refuses to follow a proof that has been derived correctly or to accept a correct result is just to remark 'This is how it goes, can't you see? Look!' and, rightly acknowledged, he adds that that is not an argument. This first seems unhelpful. It seems to point out that we must follow the instruction fully and blindly *because* that is the way we do it, thus begging the question. But on a closer reading we see there is supporting evidence for Wittgenstein's case. It is true enough that mathematical techniques have proved to pay, that they are effective tools, yet this is not what justifies them. Like a game, for which there may be a cause of its being played, mathematics may be used because it has powerful, effective tools that pay off. This may be, causally, why we keep employing mathematical techniques, but this does not have to be its ground (OC §474), its justification or the reason why it works, what makes mathematics normative, in a word. Wittgenstein makes the following distinction: on the one hand we have empirical facts that indeed play a causal part in constituting our language, determining how words and discourses take shape, as well as why language looks, sounds and works the way

it does. Empirical characteristics of language, for instance the phonetics and syntax of actual languages in the XXI century, are not what grounds linguistic necessity, but they are needed for regularities to develop within certain limits, to become salient to us, for us to become accustomed to them and make them standards of measuring, counting, grouping, etc. On the other hand we have the normative dimension of these characteristics, the fact that they constrain the way we think and act, like a machine's constitution. Though parts can be added or removed, there is a movement limitation established by the way the machine is built.

Practising mathematicians like Philip Davis and Reuben Hersh acknowledge that mathematicians have only one way to prove the meaning of theorems to sceptical outsiders: by introducing the latter to their way of thinking. If a sceptical student manages to absorb their way of thinking then they are no longer critical outsiders. If they don't understand it, then they get 'flunked out'. If they get it and still think that the mathematical arguments are wrong, then they are simply dismissed as misfits (*The Mathematical Experience*, 1981, p. 44; quoted in Tymoczko, 1984, p. 463). Similarly, we can only understand and inquire into the meaning of mathematical proofs assuming there is a background community of users of proof-expressions. A community provides institutions, structures and training that sanction our mathematical operations. Now, we may be taken aback at a first glance with this sort of explanation. As Tymoczko points out, it is easy to think that communities of mathematicians come second to well-established mathematical knowledge, which an individual may obtain in isolation. It makes sense to think that the obtaining of mathematical knowledge is prior to its distribution or communication via communities (1984, p. 449). Being set in a community does not seem essential to mathematics, or it may be a trivial requisite: we can end up saying that, for that matter, we need a community for research in every area of knowledge, so no real explanatory work is done here. Even if it is necessary to have a community to have

mathematics, it seems not to be a sufficient condition for mathematical objectivity. Communities may, after all, misconceive mathematical truths. But Wittgenstein reminds us that calculations are ratified by an agreement of ratifications (RFM VII §9). In practice, due to the complexity of the subject, mathematicians need to collaborate, compare results, even work together in large numbers to tackle a single problem. The role mathematics plays as a human activity is stressed once again by noting that there has to be understanding among the calculators to determine right and wrong results.

Usually, when we start philosophically analysing how mathematics works, we will be referred back to, and need to scrutinise, more and more basic, elementary, previously established logical principles on which our every-day sums and measurements, as well as higher mathematics, depend. But Wittgenstein points out that beyond these 'first principles' we need a starting point of agreement that transcends mathematical formalisation, or that does not appeal to logic as an underlying foundation. He argues this because it seems that to provide an answer about mathematics that calls upon formalisation is begging the question: one would already have to have bought a stance on how mathematics works to ask, with the vocabulary, concepts and tools of that stance, how mathematics works. The 'form of life' account that Wittgenstein develops aims to answer these lingering questions by analysing mathematical practice, how we learn and teach the concept of mathematical necessity, and inquiring into its origin in practice itself.

As Wittgenstein puts it, we cannot ignore that our form of life has made mathematical agreement possible in the first place, and note that such agreement has been carried further by the proofs (RFM IV §30). That is, proofs reinforce mathematical order, they derive new concepts that are rooted in the most basic agreements we share regarding space, time, proportion, equality, sameness, rhythm. For instance, in the above proof of the irrationality of

$\sqrt{2}$, we learn rules to operate in arithmetic, relations of proportion between even and odd numbers and their squares, as well as rules that govern the expansion of irrationals, which in turn form a part of the rules that structure the reals. We learn the certainty of this proof so that if we should find ourselves in the middle of deriving another apparently correct proof in number theory that, among other things, assumed that $\sqrt{2}$ or the square root of another prime was rational, and this was a pivotal assumption for the rest of the alleged proof to work, then we would rightly dismiss such chain of derivations.

Wittgenstein explains that a proof *channels* our experience (RFM IV §34), it directs our actions and thoughts. In this sense, he actually sides with Frege in arguing that mathematical laws apply not only to facts, but to anything we can intelligibly frame (Dummett, in Jacquette, 2002, p. 20). Wittgenstein's position, thus, emphasises both the undeniable relation between mathematics and empirical reality, in that the former frames the latter, as well as the absolute necessity of mathematics. The insight we gain into a proof has repercussions in human action (RFM IV §32), this is why it affects our form of life. The concept that is formed guides our experience into particular channels (RFM IV §33), and so makes us see necessary connexions, between shapes and numbers, for example, or between different equations, which will together encompass our view of necessary mathematical relations that impinge upon the ways we conceive the world. In following a rule, continuing a mathematical series for instance, the transformation we produce is a kind of establishment of an identity (RFM VI §29). For example, by learning how to operate with irrational numbers, we understand their meaning, their place, their different character in regards to the natural numbers, the different kind of proportion or quantity they compass in comparison to the integers, etc.

Mathematics defines the possibilities of thought, the modes of description of actual and possible phenomena, as well as the methods of discovery of truths, indeed everything we can

think and do. Mathematics teaches 'not just the answer to a question, but a whole language-game with questions and answers.' (RFM VII §18) Instead of learning new facts with mathematics, we learn to identify, give identity to, forms of facts. We define a play in billiards with trigonometry, the growth of a plant with integral calculus, and so on.

Relative to mathematics, physical theories are more open to revision. If we discover, say, an elementary particle that does not behave as it should, then we do not just ignore it exists and let it accommodate on its own, we have to account for it. Perhaps it will turn out that it is a particle we already knew, we just misplaced some data and thought it was a different one. But if our theory cannot explain it, then we will have to change it to accommodate the finding somehow. Granted, a similar thing can occur in mathematics. Say, we derive an odd consequence in a proof, an affirmation that does not fit with the rest of the statements or which would imply some breach of a law of inference. The relevant difference is that in mathematics we are not just talking about an anomaly, but rather something that indeed challenges our mathematical conceptions, our very way of thinking. An unsuspected finding in a theory in physics could, most of the time, still be explained with the existing scientific tools. Of course, scientific revolutions do occur, but they are called that way precisely because they become a watershed in the way science is done, and they presuppose that there are long periods of stable science practice. A contradicting statement in mathematics, however, directly challenges the very tools we are using. That is, if in a logical derivation we derive both p and $\neg p$ then, distractions and typos excluded, there *must* be a mistake in it, and we refuse to think along its guidelines, or to be more precise, we find out we simply cannot think that way. A mathematical proposition, like a proven mathematical statement, is not just an object of knowledge, but something that 'shapes our behaviour' (Avigad, 2008, p. 329), that changes what we think conceivable, and even what we can do. In

practice this means we abandon techniques that lead us to contradiction, dismiss them as useless, whereas physical theories with conflicting results deserve our attention until one of them is falsified.

In a public form of life, different mathematical proof-techniques take their shape, they extend, combine and transform. We get used to the uniformity of their results. These active practices, our ways of engaging with the world, change with a mathematical change. A proof does precisely this, it makes us see mathematics in a new light. It does not discover a mathematical entity or depict a matter of fact, it creates concepts to regulate our forms of expression. A proof can, for example, connect techniques, like those in algebra with those in geometry to define a two-dimensional sphere in a three-dimensional Euclidean space; or it can introduce the concept of elliptic curves to provide a fundamental aid in the proof of Fermat's Last Theorem, which is itself a problem in number theory. These connexions, which are not accumulations of truths but rather conceptual adjustments, constitute mathematical progress.

Going back to the concept of form of life, we can sum up that Wittgenstein uses it to explain, in a very simple and uncontentious way of putting it, that if there were not complete agreement among calculating agents, mathematicians, mathematics teachers, we would not learn any mathematical technique. Indeed, there would be no mathematics at all, for mathematics originates and develops as a practice. This does not mean, however, that global agreement is sufficient to do correct mathematics, it only points out that agreement is needed, and that such requisite should not be overlooked.

1.2.2 Conceptual changes

As mathematical techniques develop, they in turn change the way we deal with the world,

they enrich our understanding, we find more and more applications for them. This kind of progress, Wittgenstein argues, must be understood as a conceptual change. Let us look at an example, say, the law of associativity. When we learn it, we slightly change our concept of addition and multiplication, namely because they now have to adjust them to the law. Now we have to incorporate what the law dictates to all sums and multiplications. Now we can read long strings of numbers as we group them out associatively in our sums and multiplications. In a way, the road we can travel with addition and multiplication has been expanded into new directions, and even some shortcuts have been set, because we can make these combinations. Mathematics forms concepts that help us comprehend things, to deal with situations (RFM VII §67), it gives us geometrical explanations to understand our location, to build structures; it gives us a sense of infinity, of duality, of negation, which we use daily.

The formation of a concept, let us recall, guides our experience into defined, particular channels, to see experiences in a new way. Just like 'an optical instrument makes light come from various sources in a particular way to form a pattern.' (RFM IV §33) The instrument here is the proof, the pattern of light is the proved proposition. Some mathematical proofs, as explained above, lead us to say that actually we cannot imagine something we thought we had clearly imagined, which leads us to 'revise what counts as the domain of the imaginable' (PI §517). For example we learn that, unlike in Euclidean geometry, in hyperbolic geometry the parallel postulate² does not hold; or, as another example, we can prove the impossibility of constructing a heptagon with ruler and compass. But in non-Euclidean geometries we do not discover new spaces, it is more accurate to say we regard space in a new way. When I figure out what a proof is saying, and I figure out that this must be so, I am making something

² The parallel postulate establishes that for any straight line and a point not on it, there exists only one straight line which passes through that point and never intersects the first line. In other words, the two lines described can be extended to any length and would not intersect.

into the criterion of identity (RFM IV §29). This is why we decree, for example, 'we will now operate with hyperbolic geometry', we will take into account certain curves and other features we did not cover when we worked with Euclidean space. We see our mathematical tools differently thanks to the new geometrical concepts.

To sum up, Wittgenstein defines mathematical development or progress as a conceptual change based on two reasons. First, as we have seen in the definition of form of life, mathematics is an activity. Our form of life and the mathematics that develops with it are not fixed, but dynamic; likewise, one is not the basis for the other, they are rather interconnected, arise together. A proof transforms the activities we can perform, so we may learn to do more things with curves, plot different surfaces, and this does not mean that those curves and surfaces are newly discovered features of mathematical geography, but that we now know more about curves and space, and we can picture and use them in a wider range of situations. The key idea is that it is in the course of deriving a mathematical proof, i.e. in the process of drawing inferences and establishing connexions, understanding more fully what the research is about, as well as its consequences and potential for even more future research, that we learn to see mathematics in new ways. Why cannot we just say we discover more mathematics? Presumably, Wittgenstein would argue, because a mathematical change does not, strictly speaking, add knowledge. It is rather a command or instruction that directs our perception, instead of being guided by it. A new mathematical concept introduces a new paradigm among the paradigms of the language, 'the proof changes the grammar of our language, changes our concepts. It makes new connexions, and it creates the concept of these connexions. (It does not establish that they are there; they do not exist until it makes them.)' (RFM III §31)

Secondly, the reason why it is accurate to define mathematical proofs as conceptual

changes and, at the same time, the reason to define a proven statement as necessary, is neatly expressed in the following clarification: Wittgenstein notes that when someone says 'If you follow the rule you must get this result', the person that gives the instruction has not any clear concept of what the experience would correspond to the opposite (RFM IV § 29). Here he highlights his long-held view that genuine (factual) propositions and the truth-falsity dichotomy belong together. A proposition, he seems to conclude, is truth-apt iff it depicts a possible state of affairs. But truth-false bivalence does not apply in mathematics. We need to regard mathematical statements as concepts because they dictate how we are to operate, how to calculate, and it is unintelligible that we should calculate any other way. A wrong addition is not a false addition, it is just not addition at all, it is not mathematics at all. Mathematical knowledge differs from scientific knowledge in that respect. If we understand the necessary character of mathematical proofs in their full rigour then, in the case of a conjecture or an open mathematical question, we really cannot say that their proofs are out there, regardless of our arriving at them. 'It must be like this, does not mean: it will be like this. On the contrary: "it will be like this" chooses between one possibility and another. "It must be like his" sees only one possibility.' (RFM IV §31) It is only when we understand this one and only possibility, when we spell it out and realise what are its full implications, i.e. when we construct its proof or disproof, that we have a proper piece of mathematical knowledge.

1.3 Wittgenstein's critics: on the objectivity of proofs

By now it should be fairly obvious that Wittgenstein's stance, his overall conceptual framework and research questions are not, by any means, uncontroversial. Michael Dummett and Crispin Wright are two of the most incisive critics of the philosophical ideas expounded above, and two of the most clearly focused on attacking what they consider Wittgenstein's dubious

view of mathematical objectivity. Since our present goal is to understand the philosophical meaning of mathematical proofs, we need to determine if Wittgenstein's views provide a satisfactory account of how proofs are objectively established.

1.3.1 Dummett on Wittgenstein's conventionalism

Dummett believes Wittgenstein's goal is to demonstrate that platonism tells a wrong account of mathematics, and a radical kind of constructivism tells the correct story. To clarify, for constructivism in general, Dummett reminds us, it is not the notions of truth and falsity which are central to determine the sense of mathematical statements, but rather the notion of proof, and the conditions under which we regard ourselves as justified in asserting a statement, i.e. when we are in possession of a method leading to the statement's proof (1959, p. 325). But, once again, Wittgenstein's views seem to be at the extreme. For even if we embraced constructivism and even if, with Wittgenstein, we could reject the idea that a mathematical statement is determinately either true or false, still, under a form of constructivism, we would still have no choice but to follow a proof and accept it as necessary *upon constructing it* (Dummett, 1959, p. 332), that is, because the proof imposes itself on us once it is constructed. Dummett considers Wittgenstein does not accept the latter claim, and so subscribes him to a 'full-blooded' constructivism, at the extreme of the constructivist spectrum, where the logical necessity of any statement is always the direct expression of a linguistic convention, of our choosing to treat it as necessary and unassailable (1959, p. 329), as it were, 'on the spot'.

Dummett affirms that Wittgenstein does not offer distinguishable criteria for accepting or rejecting a proof. He reads Wittgenstein's views as affirming that *if* we accept a proof then we have automatically conferred necessity on it, and since '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' he concludes that in Wittgenstein's rule-following account 'there is nothing which *forces* us to accept the proof.' (1959, p. 330) For Dummett, Wittgenstein's metaphor of placing a theorem 'in the archives' of mathematical knowledge is quite literal, and an arbitrary and new decision, detached from our previous engagements with other theorems and the logical steps involved in the derivation. His characterisation of Wittgenstein's stance paints a primitive picture of mathematics in which we're always deciding what to count as new theorems, ignoring the rigorous, correct, effective formalisation that mathematics has undeniably achieved to a great extent already.

Dummett seems to read Wittgenstein's remarks on the way we follow rules and perform mathematical practice as if such practice was limited to almost the proper physical actions of agents counting, proving, measuring, as well as teaching and learning to perform these activities. Evidence for such reductive view comes out in the way Dummett insists that the strict formalisation of arithmetic, the actual use of explicit formulations of rules with Arabic numbers and precise wordings of rules must warrant the correct application of rules (1959, p. 331), regardless of an agent's intentions, confusions or finite mind. Wittgenstein's account allegedly ignores all these formal elements and comes down to affirm that our recognition of logical necessity should become 'a particular case of our knowledge of our own intentions' (Dummett, 1959, p. 328), which of course is irrelevant to an account of the necessity of mathematical proofs.

To illustrate his point against Wittgenstein's radical position in rather basic terms, Dummett reminds us that if we count 5 boys and 7 girls in a room, and then proceed to count all the children and get 13, we would conclude we have made a mistake in our counting. This is of course because we add our first two counts and find the result discrepant with the third

count. Now, Dummett thinks Wittgenstein would explain our conclusion, that we have miscounted, rather by saying that we *choose* to adopt a new criterion for saying there are 12 children, which is different from the criterion of counting them all together, i.e. we now choose to add our boy-count to our girl-count instead of counting all the children. So when we use different calculating techniques (i.e. adding vs. counting) it seems we can get different, apparently equally valid results.

Dummett phrases it this way: if we have two criteria for *the same statement*, they may clash, but the necessity of operations such as ' $5 + 7 = 12$ ' consists precisely in that nothing clashes in our procedures (1959, p. 329). If all we have is a disconnected group of techniques, as Wittgenstein wants to say according to Dummett, then with our counting technique alone miscounts will often go undetected, hiding errors that *are* there and that our adding technique is ready to denounce. Our adding technique will point out *something* that the counting technique could not encompass, which seems to leave the truth of the statement ' $5 + 7 = 12$ ' somehow undecided and dependent on different criteria.

In learning a new criterion for miscounting, thanks to the incorporation of the adding technique to their mathematical skills, a person learns to objectify a symptom of their miscounting (Dummett, 1959, p. 334), they get a hint of evidence of a truth they missed. The person comes to understand that it cannot possibly be the case that whenever there are 5 boys and 7 girls in a room there are 13 children altogether. They would now know that that is utterly false because $5 + 7 = 12$. But, Dummett presses on, this person's acquirement of a new technique is not what makes their first miscounting necessarily false. Their first mistake, albeit undetected, was necessarily a miscount already. When we make a mathematical mistake, we must always make a sort of mistake, a particular mistake (*ibid.*), not one we could only conceive of after we acquire some technique.

Dummett's verdict is that 'Wittgenstein's main reason for denying the objectivity of mathematical truth is his denial of the objectivity of proof in mathematics' (1959, p. 346) and the latter is really the target of Dummett's criticism. For him, Wittgenstein believes 'a proof does not compel acceptance; and what fits this conception is obviously the picture of our constructing mathematics as we go along' (*ibid.*) as if we remained ever free to decide on a proof's correctness. Now, Dummett's paper in the end accepts that a good proof imposes itself upon us. Once we have accepted the proof, if we should later reject it this would be a criterion for determining we have not understood the terms in which it is expressed, and moreover no one could reject it without saying something which would have been recognised before the proof was given as going back on what they had previously agreed to, i.e. falling into logical contradiction, much like what was explained in the *reductio* example above on the proof of the irrationality of $\sqrt{2}$. Since Dummett interprets Wittgenstein as always allowing these retractions, or considering them harmless, he cannot let him have a solid account of mathematical objectivity and necessity.

Finally, and very importantly, Dummett highlights the difficulty of making sense of Wittgenstein's central claim that a mathematical proof is a conceptual change. We must admit that it is hard to accept the stance. Particularly, how could a proof bring about any progress in mathematics as a conceptual change? It seems we must either adopt the conceptual modification that the proof advances, and with that have all of our mathematics transform, and thereby have it not growing but rather modifying altogether (which is implausible enough); or, by rejecting the conceptual change, we would keep the mathematics we had, and no real progress would occur, things would stay the same. It looks as if either way there is not a proper growth of knowledge if we consider a proof a conceptual change. As Dummett notes, '[f]or Wittgenstein, accepting [a] theorem is adopting a new rule of language, and hence our

concepts cannot remain unchanged at the end of the proof. But we could have rejected the proof without doing any more violence to our concepts than is done by accepting it; in rejecting it we could have remained equally faithful to the concepts with which we started out.' (1959, p. 333) To spell this out properly, Wittgenstein allegedly considers that with a new proof mathematics does not grow, but changes, and it changes because we decide, without necessarily sticking to our rules of logical inference, to endow some arbitrary propositions with logical necessity. Even if we would come upon a theorem that is perfectly correctly derivable in a strong formal system in mathematics, Wittgenstein still would see it a viable option to adopt it or reject it, again, 'on the spot', once constructed. Now, it is fair to say that no fact of the matter binds us to follow a rule, that a rule does not correspond to any reality, Dummett continues, but we can remain critics of platonism and still accept that the force of the proof comes not from our recognition of it, but from a strict formalisation that is always available for us to check and which we are able to follow. This is his more moderate constructivist position coming to the fore, a view that sympathises with a critique of mathematical truth-conditions yet finds an alternative location for mathematical necessity in the compulsion we must feel *when and if* we formulate the assertibility conditions to construct a determinate proof.

Dummett remarks that the arbitrariness or downright lack of criteria for correctness in mathematics, the shunning away of formal systems available to check the correctness of a proof-derivation and the elusive notion of proofs as conceptual changes in Wittgenstein's account make a case for the latter to be completely incapable of supporting a cogent outlook on what makes mathematical proofs objective, what justifies them, constitutes them and constructs them.

1.3.2 Wright on unsubstantial rule-following

Wright takes a different critical stance. He subscribes Wittgenstein to an anti-realist conception not only of mathematics but also of understanding in general. First, Wright claims that Wittgenstein's account fails to explain how we are able to recognise a proof as such (1980, p. 50). This he draws from reading Wittgenstein's rejection of a mathematical proof as an authentic discovery. It would seem that when we discover something, by definition, we come upon a fact we formerly ignored. Wittgenstein's hostility to the existence of mathematical facts and rejection of content (the 'aboutness' of mathematics or, more specifically, the truth-aptness of mathematical expressions) in mathematical statements makes this ordinary notion of discovery inapplicable in mathematics. For Wright, as we derive a proof, we must know what we are looking for, and we set out to find it in a well-defined formal system, with specific axioms and rules, following which will lead us to it. We will go through the steps of the proof and know that the result is what we had to arrive at. The proof, in short, will lead us to a truth we can recognise from the outset. The proof's job is only to get us there. If we do not know what we are looking for, Wright's reasoning goes, we cannot pursue it. Formal systems in mathematics, like number theory or Euclidean geometry, provide decidable notions of the identity of statements within the system, that is, identity of statements that indicate where we start from and where we end up when deriving a proof. This is made available through the syntactic criteria of the system in question.

'We do not need to know exactly what a proof of a particular statement will be like to know that we shall recognise it if we see it; for the proof will be a consequence of the formulae of which the last is the statement in question and of which every element is either an admissible assumption or derived from such by specified rules of inference, correct application of each of which is an effectively decidable matter.' (Wright, 1980, p. 51)

So, in a related objection, Wright asks, if we have such clearly defined standards of mathematical discovery, whence the room for Wittgenstein's argument that we arbitrarily decide what to count as a proof? There is clearly no decision to be made. We arrive at the correct answer, we do not fabricate it at will.

Wright's second main objection brings up Wittgenstein's opposition to mathematical realism. It's worth to take note that there is great appeal in the philosophical views informed by mathematical realism in general. Realism provides straightforward answers about the nature and constitution of facts and objects, like mathematical ones, or at the very least entertains the possibility of their existence without many qualms. So long as we can define our object like, say, 'an odd perfect number', and we know what odd numbers and perfect numbers are, the quest to find it or to report that it does not exist is a substantial, valid one. With our limited, finite capacities, the fact that we do not know and might never know if there is an odd perfect number, a proof or disproof of its existence plays the role of an auxiliary method of investigation, an indirect access to mathematical truths. Wright gives an example of this view regarding the mathematical properties of the series of natural numbers. He affirms that open mathematical questions, like, say, Goldbach's Conjecture (henceforth GC), are not only perfectly intelligible but also, if proven, would be both an authentic discovery and an establishment of an objective feature of number theory (Wright, 1990, p. 82). GC states

All positive even integers ≥ 4 can be expressed as the sum of two primes

For Wright, open mathematical questions like these definitely have a solution, we just have not been able to craft them yet. Likewise, a rigorously defined structure such as the series of natural numbers has no room for indeterminacies: the correct mathematical answers to open

questions are determined, built into the identity of the mathematical facts and objects (1990, p. 83). Wright asks: 'how can the question whether *all* items of a certain kind have certain characteristic fail to have a determinate answer -even if we cannot know what the answer is- if the items in question are a sharply defined class and the characteristic in question is something which *each* of them determinately possesses or not?' (1990, p. 81) For instance, we know what even numbers are, so we could say we know the characteristics of all even numbers, and that they determinately have the property of being the sum of 2 primes or not. Thus, Wright affirms, contrary to what Wittgenstein argues and also drawing distance from Dummett, proofs have a sort of secondary role to play in mathematical research: they are means to get to ends, the ends being the mathematical truths themselves.

Wright confronts this against Wittgenstein's thought that our conceptual structures are not autonomous, that they are part of human practices and would not exist without us. For Wright that is enough to conclude that in Wittgenstein's view there is no external compulsion, no normative constraints upon us to ratify a proof. Wright thus defends the idea that we explore, rather than form, conceptual structures in mathematics (1980, p. 48).

Thirdly and with wider implications, Wright questions Wittgenstein's rejection of mathematical content also from another angle: he asks why we should have so many reservations about accepting that conjectures in mathematics have a clear content, although their truth has not been established, when elsewhere, like in the generalisations of natural science, we accept that scientific hypotheses indeed have content, without much objection (1980, p. 19). In other words, he asks why we accept so naturally that there is definitely an answer about whether, say, element x can or cannot be found in planet b , and that we just don't know it yet, (but perhaps in a near future we will send a scientific mission to go and find out); and yet we deny that the GC definitely refers to something, deny that there are two, and

only two, possible outcomes for it, either it is true or it is false, and we just don't know which one it is until either of the two proofs is articulated. If we cannot provide a satisfactory justification for the reason why we treat mathematical statements differently from empirical statements which are difficult to verify, that is, if we cannot justify why we treat some statements whose truth remains to be established as truth-apt and others as non-truth-apt, then we cannot arbitrarily deny that mathematical statements have a definite content.

Wright has a point. For how would we possibly understand the open question in the GC if we could not imagine it being true? How could I even write about it here? We don't need to wait for a conceptual change that accommodates the result in number theory, we already know, at least in a rough way, how either answer would look like. This objection will be more thoroughly addressed in Wittgenstein's distinction between proofs and experiments in the next chapter, but at this point it is enough to understand Wright's claim that mathematical problems are not solved by changing our conception of them, but that they are genuinely open questions that are answered through mathematical research, through obtaining more knowledge, and that once obtained, they will be genuine discoveries, analogous to those in physical sciences.

Wright reads, as Dummett also does, that Wittgenstein rejects any predetermination of the correct application of a mathematical concept, even if a proof 'is a mechanically decidable notion, that is, that we may programme a machine effectively to check any putative proof, [for Wittgenstein] there is somehow in reality no rigid, advance determination of those sentences which are theorems.' (1980, p. 21) In this sense he objects against Wittgenstein's comparison of mathematics to games like chess, against his highlighting only the practical nature of mathematics and the apparent flexibility of its rules, which ultimately makes them lack logical compulsion and determination. The alleged game of mathematics, Wright would argue,

depends significantly more on its formulation and rigour, not just on however we happen to learn and practice mathematics.

Drawing again some distance from Dummett, Wright also challenges intuitionistic or constructivist arguments, suggesting that some of Wittgenstein's arguments fit into that spirit, and claiming that the knowledge of the assertability conditions of unresolved mathematical statements cannot retain the classical view that mathematical proofs are binding and compelling (1980, p. 56). In other words, just knowing which deductive road to take to prove a statement does not do justice to the proof's logical necessity. For example, in Peano Arithmetic, the axioms and rules not only give us the correct line-up of steps in a proof of number theory, but also make us know *when* we've reached the desired theorem. Such identification is achieved syntactically.

'[T]he view that understanding a sentence is knowing the conditions of objective truth of what it expresses gives us some purchase on the identity of the mathematical statement independent of knowing when it would be proved. But on an anti-realist conception [...] the strategy of attempting to explain the proof-conditions of a statement by appealing to the idea of a valid chain of inferences culminating in the statement in question, falls foul of the fact that only to someone who is already aware of its proof-conditions is the identity of the desired conclusion intelligible.' (Wright, 1980, p. 52)

So within an anti-realist framework, knowing the proof-, or assertability-, conditions of a statement is not only insufficient to guarantee a correct definition of the identity of the proof, but these conditions are also impossible to foresee, unless we already had the proof, which is exactly what we are after. Wright accepts that we cannot foresee the techniques that might be used to, say, derive a proof of GC, but any technique, we can rest assured, will be answerable to previously accepted criteria (1980, p. 53), that is, it will conform to accepted axioms and rules. Again, calculating techniques by themselves are just our means of getting to prove the truth of mathematical statements.

Wright thoroughly explores Wittgenstein's rule-following account, and there is not enough space here to comment on his criticism. However, it is worth to pay attention, at least briefly, to his remarks on the lack of content in rules. For Wittgenstein, let us keep in mind, mathematical rules are less like informative, declarative, factual propositions and more like laws or imperatives. Still, as we have explained above, rules grow and change, are dynamic and do not seem to determine an expression's meaning 'once and for all'. Wright explains that this is an inaccurate account of mathematics, for we give a predicate like 'is the sum of 2 primes' a certain meaning by associating with it a certain criterion of application, which is determined in turn by the simpler expressions in it, like 'sum' and 'prime number'. So, he asks, how can there be any latitude, any indeterminacy regarding the application of such predicate? And he answers that, for Wittgenstein, such indeterminacy is due to the fact that he rejects that it is ever pre-determinate what counts as 'doing the same thing again' or 'applying a rule in the same way' (1980, p. 20), which, as the reader can probably tell at this point, will have major consequences not only in mathematics but in language and knowledge in general. Wright argues that understanding a rule, say, the rule of the expansion of an irrational number, must determine in advance what the n th place of the expansion should be (*ibid.*). This is telling of Wright's picture of mathematics, and why he thinks Wittgenstein's claims threaten 'not merely [...] the objectivity of truth in mathematics, in the distinctively platonist sense, but indeed the whole picture of pure mathematics as something conceptually stable, as something in which the primary objective and substantial task is not conceptual innovation but the tracing of the liaisons and connexions between concepts to which we are already committed.' (*ibid*) If we follow Wittgenstein, Wright explains, we would end up accepting that there is no substance to the idea of an expression being used in accordance with its meaning (1980, p. 21).

The final blow is thrown against the idea that proofs produce, or are better understood as, conceptual changes. Wright contends that, if by accepting a proof we change the sense of its conclusion, as Wittgenstein seems to suggest, and if in the end we somehow end up with a different concept of the proof we were working on, then there is no room for the orthodox idea that we must be faithful to the concepts as they were before the proof was accepted (1980, p. 41). But obviously we needed to conform to these concepts to carry out the task of proving in the first place, so are we now to turn our back on them? Are we not risking a self-imposed *reductio* here? For it seems as if the path that the open question created now turns back on itself. It changes, so we end up, in effect, with no question at all, no premisses, no intermediate steps, no conclusion, no proof. The intuitive idea is that we are able to recognise a proof because we understand the concepts involved in it and stick to their meaning. We keep, for example, uniform understandings of 'addition', 'prime number', etc. when we read and try to prove GC. How can accepting a conclusion *change* the conclusion, if we accept it precisely because it conforms to concepts we've been using constantly and consistently? And if the sense of the conclusion changes, 'then nothing in the way we understood it before can have required us to accept the proof; and similarly for our criteria for the correctness of the steps.' (*ibid.*)

Wittgenstein's account comes down to affirming there are no substantial facts about the content of rules and about what complies with or breaches them, it simply makes no sense to talk about the content of a rule (Wright, 1990, pp. 91-92), but then we end up being unable to talk about rules at all. Wittgenstein does not concede that to follow a rule we need to keep track of *something*, stick to a commitment, whose correct application is clearly dictated independently of anyone's judgement. What's worse, Wittgenstein leaves us in the ruins of his critique and gives no positive alternative account. It seems we have to either

admit that rules have a determinate content and explain that this is why we agree in our performances according to them, or rules will just remain unaccounted for.

Wright concludes that if we follow his reading of Wittgenstein as an anti-realist and believe there is no reality behind rules but rather that rules are somehow constitutionally responsive to our ongoing judgements and reactions, then we must accept that truth in general is constitutionally responsive in the same way, i.e. we can always justify our use of any rule.

1.4 Conclusions

This chapter has described some general philosophical questions surrounding the nature and the source of the necessity of mathematical proofs, briefly accounted for some of the major treatments and common lines of argument, and began to sketch Wittgenstein's position on the matter. It has acknowledged the latter's complexity and ambiguousness and admitted plenty of room for important critics to contribute to the discussion. This has served, nonetheless, to paint a more accurate picture of the scope and aims of Wittgenstein's remarks on proofs. Dialogue with his critics has helped to clarify philosophical claims on mathematical truth, what they amount to, which advantages and difficulties these claims present, as well as what issues alternative accounts would have to address.

We now turn to reply to such objections and examine if Wittgenstein's stance is defensible and philosophically relevant, particularly if it can provide us with a solid concept of mathematical objectivity, fundamental for a proper account of mathematical proofs.

Chapter II. Wittgenstein's account of the objectivity of mathematical proofs

The previous chapter introduced Wittgenstein's philosophical views on mathematical statements, proofs and rule-governed mathematical practice. It presented his criticism against the view that mathematical proofs lead us to discover mathematical truths, as well as his claims that such way of framing philosophical research on proofs would do no justice to a sound definition of mathematical necessity. Instead it highlighted the relevance and explanatory role that notions like 'form of life' and 'conceptual change' could play in our understanding of proofs, that is, that the use of communal, rule-governed mathematical techniques and practices are inextricably linked to the ways we conceive and deal with empirical reality, and they stand in need for philosophical analyses as much as formal systems and axioms. Under this view, in summary, we regard mathematical statements as norms, law-givers by which we abide, and without which we could not think and act as we do. This will be part of Wittgenstein's wider philosophical outlook, namely that mathematical proofs *transform* mathematics, rather than discover mathematical truths.

These views have received strong criticisms, particularly from philosophers that consider a working notion of mathematical truth indispensable to account for mathematical statements and proven propositions. Such philosophers are also sceptical of how much explanatory power Wittgenstein's rule-following considerations have in the case of mathematics, and fear relativism and conventionalism sneak in. The chapter paid special attention to such critical reviews and exposed them thoroughly, emphasising the need to explain more clearly how we are to understand mathematical objectivity if we are to follow Wittgenstein's reasoning. The present chapter serves as a reply to such criticisms and as a defence of Wittgenstein's account of the objective correctness of mathematical proofs. In order to effectively reply to Dummett's and Wright's objections raised in the previous chapter,

and besides engaging directly with them, this chapter will also provide a more solid account of mathematical practice, which is crucial to define the correction criteria needed to establish mathematical objectivity in a way that accords with Wittgenstein's views.

Following, up to a point, an argument developed by Steiner (2009), this chapter will also describe and defend the role that the notion of 'empirical regularities' plays in Wittgenstein's account of proofs. I will then argue why it is convenient to draw some critical distance from Steiner's conclusions. Finally, I put forward my own evaluation of Wittgenstein's idea of how empirical regularities are 'hardened' into mathematical rules, so that the source of mathematical objectivity has a direct, yet non-causal, relation with the empirical world, and avoids ontological commitments to abstract objects and realms. These arguments will show that mathematical proofs can be conceived not as tools for mathematical discovery, but as norms that produce new understanding, new possibilities for mathematical practices to develop.

2.1 Revisiting Wittgenstein's account

Let us recall that the target of Wittgenstein's criticism is not only platonism or, for that matter, any specific epistemological position within philosophy of mathematics, but rather the lines of thought which affirm there is some sort of substance or content behind mathematical statements. These lines are directly tied to the idea that mathematical objectivity, though it may be in a sense, and only secondarily, related to human practice, is ultimately quite independent from it. If Wittgenstein's account is to be successful, it must make a strong case that objectivity, practice and use in mathematics are more relevantly interconnected.

2.1.1 Reply to Dummett

Wittgenstein does not affirm, as Dummett suggests (1959, p. 334), that a new mathematical technique gets its whole being anew, that we just decide to fabricate it and decide on its correctness, when to apply it and when to cease doing so. Borrowing Dummett's example of the agent that can count but not add and makes a miscount, suppose Connor can count but not add and Aldo has the same counting and adding abilities as we do. On one occasion Connor counts 5 boys and 7 girls in a room, and wanting to know how many children there are, he counts them all and comes up with a total of 13. Connor's limited ability makes it impossible for him to detect a mistake. He is good at counting, so if he could have noticed, for example, that he counted a boy twice, he would presumably count again, but he did not notice anything to make him doubt that particular time. When Aldo teaches him how to add, Connor comes to realise how absurd his statement 'there are 5 boys, 7 girls, 13 children altogether here' was. But back then, before acquiring the technique, he was conceptually blind to the rules of addition he needed for the miscount to be salient to him. He could not yet grasp what adding discrepancies were. So far he likely just placed objects together and gave each a number according to the order of the natural number series.

Let us examine all the expressions involved here. We have,

- a) There are 5 boys
- b) There are 7 girls
- c) There are 12 children

And we have the miscount that Connor gets, let's call it

- c') There are 13 children

For Connor the relation $a + b = c$ (or a, b , therefore c) is not immediately clear with his counting technique. So for him to affirm a, b and c' does not involve falling into contradiction.

Now, even if I do not teach him yet how to add, I can order him 'Go to the drawer and bring

one lollipop for each child'. Remembering he counted 13 children, he will likely count and bring 13 lollipops and find out he has one left after all the children have already had their share, and this will presumably lead to his recount. But Dummett wants to press the point that the mistake is there regardless of Connor seeing it, either because he acknowledges it a second time around with his own counting technique, because I make him do an extra 'exercise' in bringing the lollipops or because I teach him to add. The problem with such point is that it entails that what makes $5 + 7 = 12$ is something like a feature of mathematical reality, so that when Connor says 'there are 5 boys, 7 girls, 13 children altogether here' he is uttering a falsehood, and that is the mistake. In Dummett's words,

'the effect of introducing [the person who can count but not add] to the concept of addition is not to be simply described as persuading him to adopt a new criterion for having miscounted; rather, he has been induced to recognize getting additively discordant results as a symptom of the presence of something he already accepted as a criterion for having miscounted. That is, learning about addition leads him to say, "I miscounted," in circumstances where he would not before have said it; but if, before he had learned, he had said, "I miscounted," in those circumstances, he would have been right by the criteria he then possessed.' (1959, p. 334)

Wittgenstein would explain the situation differently. For him mathematics forms concepts, concepts to deal with reality, and which condition our perceptions. Connor's very limited criterion for a miscount allows him to say 'There are 5 boys, 7 girls and 13 children altogether', and equally allows him to correct his mistake should he notice it. But he is plainly not conceiving the wrong addition that his counts entail. And, since on this occasion nothing particular happened that would lead him to think he miscounted, since it is only from our criterion for adding correctly that we become aware of the miscount, and since he cannot add, then he does not have, as it were, the relevant mathematical structures and operations to betray and fall into contradiction. He simply does not grasp them. The reasoning goes, if he is

not playing a certain game, then he cannot make a foul move in such.

We could be tempted to say that Aldo has Connor's criterion for miscounting, plus a complementary criterion given by the adding ability, but wouldn't it make more sense to say Aldo *can no longer* have Connor's limited technique, that we cannot put their counting abilities on a par? I say this because it seems Aldo cannot go back and *not* think of counting as, ultimately, working on a principle of accumulating (adding) units. If Aldo counted the boys in the room, getting a total of 5, and the girls in the room, getting a total of 7, he would probably not bother to count them all, for he can already work out ' $5 + 7 = 12$ '. Or, if he did count them all and got 13 as a total, he would likely judge that it is more probable that he got confused with all the children and ended up miscounting, rather than to have added two relatively small numbers and gotten the addition wrong. That is, he will stick to the criterion that the adding technique provides and judge it stronger than the counting one. It makes more sense to say that Aldo's arithmetical techniques changed, complementing each other and making him aware of a greater variety of situations.

To clarify, when Connor learns to add, he does not learn new truths, among which is the truth ' $5 + 7 = 12$ ', but perhaps something around the following lines: he learns that in counting he is grouping objects together, that here he has, let's say, a set of 5 kids, and here a set of other different 7 kids. Let's accompany Connor as he adds, let's make him scribble a stroke in a piece of paper for every boy he counts and surround all strokes in a circle, and then draw an asterisk for every girl he counts, and then encircle them as well. Then let's ask him to encircle both of these circles and see how many signs (strokes and asterisks) he has in total. Let's suppose he still thinks he got 13 children in total and he hasn't a clue what we're trying to show him. But then we ask him to count all the signs within the big circle, and being good at counting he will come up with 12. Now we can tell him that he actually just did with

strokes and asterisks what he did with boys and girls before. If he understands this and then we ask him how many children he thinks there are, he is entitled to be confused. Now we can explain him how we add, say, two small circles to make a big circle and get a grand total. Crudely put, if 5 is now understood as (I I I I I) and 7 as (* * * * * *) and if he counts them all, Connor will see he *must* get 12 and *cannot* get 13.

Learning to see more fully the implications of a calculating technique can thus modify the way we conceive it. Both criteria, counting and adding, do not clash at all, as Dummett fears, that is why Connor can i) learn how to add, ii) make sense of his first miscount *through* his already mastered counting abilities, and iii) learn to trust his adding technique more than his counting technique, for it is shorter and more perspicuous, as his correlating elements to signs on paper shows.

Let's look at it this way: Aldo and I can see the mistake right away when Connor gives us his counting results. Connor cannot see it until he is aided by his counting technique and, without looking for it, without even suspecting a mistake, coerced by us, finds a fault in his procedure; or until he is trained in the adding technique. Otherwise he will be justified to report his counting results. Keeping in mind Wittgenstein's notion of form of life here, if Connor belonged to a community that only counted but did not add and he was particularly prone to count people twice, his community would see such mistake and correct or, maybe, re-train him to distinguish better between individuals. But that is all they can do in a form of life that does not include addition within its conceptual framework. We can say that there is a mistake and there is a miscount, but only from our form of life, with addition playing the fundamental role it plays in our lives.

In short, two mathematical techniques do not clash because they are not referring to two different, mutually contradictory facts. Rather, a new technique changes our way of

understanding, in this case by meshing with the old one. Together they provide new mathematical norms. In a way, we can argue against Dummett, we fully realise what counting amounts to when counting meshes with adding. If it is unclear that we can separate both, that is a fault in Dummett's example, and an argument in favour of Wittgenstein's definition of mathematics as a network of norms and a spectrum of techniques. After we learn to add, the mistake of a miscount is not the same anymore. We see it anew. We are ready to be scandalised at the thought of someone seriously thinking that 5 boys and (+) 7 girls together are (=) 13 children, but only because we have already mastered adding. The fact that our boy-count and our girl-count must yield a certain number when we add them up to make our children-count is not enforced by a mathematical state of affairs, but it is a consequence of framing a mathematical question in a specific way and answering it following specific mathematical rules, in this case we do it all through the technique of addition. This is why when we are asked 'how many children are there?' we know we are, in a way, being asked to add our boy-count to our girl-count, to provide a sum total.

Wittgenstein never says that different criteria have to oppose or cannot complement each other. Different criteria of correction for mathematical techniques do not contain different truths which may contradict each other, they rather set the norms that govern different mathematical processes. Connor's correction criteria for counting did not agree or disagree with Aldo's. Rules extend, rather than delete, the network of old rules (RFM I §166), so Connor's rules for counting are extended once he learns to add, in a similar way as the concept of irrational numbers is extended with the proof that $\sqrt{2}$ is irrational. Before going through it, perhaps we merely knew that irrationals expressed a different type of quantity, one that cannot be captured in the concept of 'integers' or 'rationals'. The proof, then, may also help us understand other numbers, like π , and how they are used to find approximate values

of measurements.

Wittgenstein questions if we should say we have discovered a new kind of calculation 'if, having once learned to multiply, I am struck by multiplications with all the factors the same, as a special branch of these calculations, and so I introduce the notation " $a^n=...$ " [...] In exponentiation the essential thing is evidently that we look at the *number* of the factors. But who says we ever attended to the number of factors? It *need* not have struck us that there are products with 2, 3, 4 factors etc. although we have often worked out such products[...] For what purpose do I use what has struck me? -Well, first of all perhaps I put it down in a notation. Thus I write, e.g. " a^2 " instead of " $a \times a$ ". By this means I refer to the series of numbers (allude to it), which did not happen before. So I am surely setting up a new connexion! -A connexion- between what objects? *Between the technique of counting factors and the technique of multiplying.*' (RFM III §47, last emphasis mine).

The point Wittgenstein stresses is that, ultimately, it is in practice that we understand the necessity of mathematical procedures, because in practice they allow us to form new concepts and understand reality in new ways. In practice they fully strike us as necessary. By practice Wittgenstein does not refer to some arcane notion of mathematical understanding or intuition, but quite simply to our standard mathematical training, how we learn to count, to factorise and build polygons, using concrete examples and then abstracting from them to frame new concepts, wider, unforeseen situations.

Now what about the related criticism that Wittgenstein embraces a full-blooded conventionalism about mathematics? Dummett argues that one cannot make sense of the relation between an empirical regularity and the proof which induces us to 'put it in the archives'. He writes that

'for Wittgenstein an empirical regularity lies behind a mathematical law. The mathematical law does not *assert* that the regularity obtains, because we do not treat it as we treat an assertion of empirical fact,

but as a necessary statement; all the same, what leads us to treat it in this way is the empirical regularity, since it is only because the regularity obtains that the law has a useful application. What the relation is between the regularity and the proof which induces us to put the law in the archives Wittgenstein does not succeed in explaining.' (1959, p. 341)

Indeed this needs explanation. First, we must question if Wittgenstein really argues that mathematical objectivity depends on empirical laws in the way Dummett describes. It seems problematic to define mathematical laws as springing up from physical regularities. What we can read off from physical regularities usually translates rather into physical laws, which have a different necessity from mathematical necessity. We learn about electricity with different resources, aims and methods from those we use to understand the proof that $\sqrt{2}$ is irrational, for example. It is not enough either to say that we follow certain mathematical techniques because of regularities in our human practices, because 'it is simply what we do'. Needless to say, we are prone to make mistakes in measures and calculations and it actually takes a lot of effort to master mathematical operations. It is far from being something that 'comes natural' to us. At the very least there remains the question of why we choose one mathematical technique over another, why we happen to choose the correct one and how we correct mathematical mistakes. Why are some regularities 'hardened' into mathematical rules and others are not? How do we make the distinction? We now turn to construct a possible explanation of how a mathematical rule acquires its status.

It must be clarified from the outset that the analogy of placing a mathematical statement in the archives of language so that it becomes a standard to rule over the correct use of expressions is something Wittgenstein himself criticises. He admits that '[e]ven if [we] think of a proof as something deposited in the archives of language - who says *how* this

instrument is to be employed, what it is for?' (RFM III §29) This suggests that a full account of the necessity and workings of mathematical proofs must be backed up by a description of their use.

One way to interpret this extra need for an understanding of the use of mathematical expressions is to consider that mathematical theorems are rules supervenient on experience, yet independent of experience (Steiner, 2009, p. 10). Granted, a mathematical proof need not be causally connected with empirical regularities, but may have another type of connexion. Steiner phrases it thus:

'arithmetic cannot be in contradiction with empirical regularities because arithmetic rules are stipulated to be derived from these very facts. (Because they are stipulated, they are also necessary.) At the same time, arithmetic propositions provide the formal standard for what is called a fact. Conventionalist readings of Wittgenstein, at least those which imply that an alternative arithmetic to the standard one is an option, are radically mistaken: arithmetic and geometric theorems are indeed rules, but they are the only rules available.' (2009, p. 12)

Arithmetic rules are derived from empirical facts only in the sense that they are stipulated from the obtaining of regularities. We see results repeat themselves when we group objects, measure time or draw certain diagrams. Their regularity sets them as standards, necessary measuring rods to which facts must conform. These standards make up measuring systems, formulae, calculations. Using them we obtain the concepts for what we are going to call 'equal', 'same', 'correct', and all we need to conceive any form of fact, all we need to trace the limits of what is meaningful to express, what we are going to call 'true', 'false', 'possible' 'contradictory', etc. And for that matter, considering the ubiquitous character of mathematical discourse, as we philosophically define mathematical standards we also delimit intelligibility.

Yet we must be careful and take some distance from Steiner as he remarks that '[t]he only degree of freedom is to avoid laying down these rules, not to adopt alternative rules. It is

only in this sense that the mathematician is an inventor, not a discoverer.' (2009, p. 12) Here I want to say that we really do not have the choice of laying or not laying them down. Not to be confused with a radical conventionalist, Wittgenstein is very clear in explaining that mathematical proofs are not arbitrarily constructed as we go along, but that their necessity is perfectly defined; and yet he keeps our heads levelled by understanding the necessary character of mathematics as stemming from the equally non-arbitrary human activity and form of life. Distancing himself from behaviourism with equal strength, he declares: 'The proved proposition is not: that sequence of signs which the man who has received such-and-such schooling produces under such-and-such conditions.' (RFM VII §8) Such thought is too simplistic and ignores the fundamental character of a 'route' that a proof has, the route that guides us to a specific new understanding. We arrive at different results via different routes (RFM, VII, §3), that is why a proof can guide us, in a particular path, and also compel us to stick to our mathematical rules blindly, without providing the choice of 'not laying them down'.

Another, more subtle, point where to draw distance from Steiner's interpretation is when he remarks that Wittgenstein considers that the applicability of arithmetic depends on the existence of many sorts of stable objects, and the applicability of geometry depends on regularities of measuring locally Euclidean spaces (2009, p. 22). I merely want to avoid a misinterpretation: arithmetic and geometry do depend on these empirical features, yet the applicability of mathematics seems to be a much more complex notion. Even though features of the empirical world are certainly pivotal in the generation of mathematical thought, they are not needed for other practices of mathematics. That is, Wittgenstein's view does not force us to do mathematics exclusively with an aim of empirical application. There is nothing particularly obscure or illegitimate about, say, the concept of infinity, even though we do not 'experience' it as we experience instances of the commutative law of addition or how we see

a flying object draw a parabola. Basic and advanced mathematics remain linked to their origin in empirical regularities, and this agreement in action which starts in very basic activities like counting is carried further by the proofs (RFM IV §30). Proofs expand mathematical understanding to unforeseen techniques and conceptions. From counting and adding small quantities of medium-sized goods, proofs take mathematical reasoning to infinite extensions and numerical expansions, keeping each new development in logical order thanks to their rigorous derivations.

2.1.2 Reply to Wright

Wright argues above that if we should follow Wittgenstein's rule-following account, we would have to commit not only to the idea that there is no reality behind mathematical rules, but that truth in general would be constitutionally responsive in the same way, i.e. truth statements in general would not refer to anything but would be established according to our ongoing judgements and reactions, so that there would be no objective reality of any sort. I do see how an observation in mathematical necessity has repercussions in understanding in general, but it does not necessarily have to reject truth in general, or imply scepticism of the physical world or make any form of objective knowledge impossible. Wittgenstein actually argues that mathematics plays a particular role in understanding, one that impinges upon and involves changing the way we look at the world, not the world itself. Mathematics' role, in short, does not hang on correspondence with fact.

It may seem that the remarks that equate proofs with conceptual modifications, that affirm mathematics changes the way we look at the world, are nothing but very general observations of the profound implications that mathematical knowledge growth has in our lives. Certainly, mathematical progress changes our outlook on the world and very

fundamental concepts, the way we relate to our surroundings, to changes, the links we can make amongst disparate fields of research, the ways we can frame and transmit information. But if this is just a wide epistemological claim, then it needs to clarify why mathematical knowledge in particular works like that, i.e. through conceptual modifications, or if all knowledge works the same way. Moreover, these mathematical achievements, even if we agree to call them 'conceptual changes', do not explain much about how a proof is derived and what its result means, mathematically and philosophically. For, in terms of its logical structure, we are supposed to believe, Wright's argument goes, that in the course of a proof the premisses and rules of inference involved in its derivation are going to change, they will mean different things when we begin the proof and once we have proven the proposition in question. If this is what a proof is supposed to do, it is not hard to raise the objection that 'it would be as appropriate to urge that the effect of verification of a prediction furnished by a physical theory would be to modify the concepts on which it is based.' (Wright, 1980, p. 49) In other words, if we follow this account of knowledge growth as concept-modification, we could, say, predict that a certain comet will be visible in the Earth's Northern Hemisphere from 4:35 AM to 4:48 AM on 15 March 2021, *and we could make* the comet's passing by the Earth a fact regardless of what actually happens, with enough modifications on the concepts of 'comet', 'solar winds', and every astronomical notion and formula used to make the prediction. We can just say on the date that *now* we understand them differently, and even if the skies are completely clear all day we can change astronomy just enough to satisfy the prediction, and still call it legitimate astronomy. Of course, this is not how we make scientific observations and predictions, but so far no evidence has been raised that would push us to believe that scientific and mathematical knowledge are on a par in terms of their significance, methods, acquisition and growth.

Wright forgets the sharp distinction Wittgenstein makes between factual discourse and normative discourse, and the unique normative character he assigns to mathematical statements. Wittgenstein actually affirms that in many regions of discourse, particularly in the factual discourse of physical sciences, we use genuine propositions, and genuine propositions have the form 'This is how things are', not 'This is how things must be', the latter being the form of a normative statement, which pertains to logic and mathematics. Of course, scientific discourse follows its own governing, prescribing principles, but it is also largely constituted by descriptive statements and empirical generalisations. Mathematics, on the other hand, is constituted exclusively by rules and norms. We apply, as Wittgenstein says, the calculus of truth-functions to factual discourse (PI §136), that is, we say of factual propositions that they can be true or false, we operate with them to depict possible states of affairs. But such is not the task of mathematical propositions. Mathematical statements are measuring standards, not descriptions of things we measure (RFM III §75). A particular measurement can be true or false, but the measuring system itself does not 'engage' with truth, it is rather antecedent to truth and falsity claims. A mathematical proposition holds not because it depicts a state of affairs that obtains, but because of agreement, because of the regular working of measurements (*ibid.*) in a shared form of life. Measuring standards and measuring results together form a sound and objective measuring system.

Consider a group of people that have been trained in the same measuring system. They measure the height of a tower and all but one get the same result. We are entitled to suspect that the one person with a discrepant measure might have measured wrongly. Indeed, we make them all repeat the measurement a couple of times and, not surprisingly, by the last time everyone gets the same number. The hitherto dissident was in fact giving a false result, asserting 'the tower is n metres tall', a fact that did not obtain. But what was never put into

question while these people tried to solve the matter was the system of measurement, perhaps because they had all worked with it before and considered it a correct, effective standard. It is only because the system of measurement is taken as unassailable that they are able to correct the dissident and make the true result prevail. Likewise, if the dissident had been correct in their measurement, then the only way to make the rest of the group follow suit would be to refer to the technique, to tell them 'see, you misapplied the technique here' or 'you did not take such-and-such into consideration'. Now it becomes evident that we also need confirmation from empirical reality, the thing measured, to be sure that we are measuring correctly. Using mathematics is not a matter of convincing others that my calculations are correct, it is not the case that the majority will establish correctness at will. If we didn't compare and apply results we could just as well measure however we liked, change the system as often as we liked. This dependence on communal agreement is an important and often muddled idea that Wittgenstein painstakingly advances to define his notion of mathematical objectivity.

There is indeed a way of reading the above argument that leads to conventionalism, but Wittgenstein is not driving his point this way. He wants to keep the objectivity of mathematical results without arguing that such objectivity rests upon the existence of mathematical facts, properties or relations. To argue this he draws heavily from his observations on how linguistic expressions are meaningful and how there are correct and incorrect ways of using them. For language to work there must be 'agreement not only in definitions but also (queer as this may sound) in judgements. [...] It is one thing to obtain and state results of measurement. But what we call "measuring" is partly determined by a certain constancy in results of measurement.' (PI §242) This way, Wittgenstein makes it clear that we do not agree on what is true and false in the sense that we just decide on it, but rather,

because we share a language and a form of life, we share our measuring systems (cf. PI §241), and thus we can determine if statements like 'the tower is n metres tall' is true or false, regardless of who considers it either way. In other words, we share mathematics and this means a correct use of mathematics, which allows us to deal with the empirical world through procedures which are objectively correct.

Still, even if we accept that conceptual modification applies only to the mathematical case, Wright remarks that if we could 'discern an alteration in our concept of, for example, the pattern of application of a particular rule of inference, brought about by the application of it made in the proof, then the proof would fall short of complete cogency precisely at the point where that rule is applied.' (1980, p. 43) So, in one sense, our notions of logical inference and of all the intermediate steps we take while deriving a proof, would always be changing, under Wittgenstein's view. Wright adds that, in another plausible interpretation, Wittgenstein's account would actually turn on itself and not allow conceptual changes at all, for such account has no objective criteria from which we can detect two states in our knowledge, a 'before' and 'after' the proof, if you will. The picture evoked is something like playing a game we always alter, we would not know when we started playing with new rules if the rules are always changing. If we would make a new move, we would change the whole of the game, so that it would make no sense to ask if the move is allowed or not, there is just no objective account of how the game is supposed to go. If everything changes in mathematics then, so to speak, it all remains the same: we cannot account for new developments if we do not have a base from which they stem, if it is all in permanent motion.

'Unless one's understanding of an expression may be thought to have a determinate character, it seems to make no sense to speak of a modification in it; but if it may be allowed to have a determinate character, it would seem that it would at least have to make sense that certain linguistic moves made

with it should accord with that character. How, then, are we to reconcile Wittgenstein's sloganising about concept modification with his repudiation of the idea that our understanding of expressions reaches ahead of us to so far unconsidered situations in a predeterminate way?' (Wright, 1980, pp. 47-48)

Let's clarify the last lines: Wright refers to Wittgenstein's rejection that there is some substantial content to rules, that rules are 'about' something, as we have examined in the previous chapter. So Wright is demanding either an acceptance of the determinate characteristics of mathematical statements, that is, to accept that we do not construct mathematical meaning, but that such is established from the beginning, and proofs are just auxiliaries that point us in the direction of mathematical truth; or a rejection of conceptual modification as a genuine mathematical advance. If mathematical expressions have no established meanings, then what is going to change once they are proved?

The problem with Wright's argument is that it still understands mathematical statements as contentful (descriptive) when Wittgenstein has been arguing to accept them as rules (prescriptive). Let us grant, for the sake of argument, that a mathematical proof indeed represents a conceptual change in mathematics. Wright detects this problem: after the change of the concept of mathematical statement S , with the advent of its proof, what account can be given of what S used to mean? (1980, p. 43) We can reformulate the question in Wittgenstein's terminology, to be fair: after the change of the concept of mathematical rule S , with the advent of its proof, what account can be given of what S used to prescribe? Framed in the latter way, we can see that, to begin with, S would not be a rule without a proof that would 'put it in the archives'. So it seems Wright is separating a mathematical statement and its proof in a questionable way. He asks, what happens to S when we have the proof of S ? Well, obviously, it is proved. We can tell what Fermat's Last Theorem means when it is only stated as the conjecture written in the margin of a book, but it will be more intelligible to us

once we learn some algebra and higher mathematics. Yet we did not have the proof of it for a while. Even now, many of us know it is proven, yet are not fully capable of reading and following the entire proof. If I learn about elliptic curves, perhaps the theorem will reveal itself in its full meaning, and that means, using Wittgenstein's terminology, that I will know how to use it, I will be mathematically competent to employ the concept. This is the kind of change brought about by mathematics, a change in our abilities.

But Wright's objection is more complex. Let us suppose that statement T expresses after the proof³ what S used to express before the proof.

Perhaps we can put it this way

$$T = S \wedge Sp$$

Where T is a proved statement, S is the statement that got proven and Sp is whatever set of statements, rules and theorems we need to derive in order to get to T . In other words, roughly put, Sp is the proof of S , that is, the very mathematical insights, the new understandings in algebra or topology or group theory that allow us to put S in the archives of mathematics, right in the place where it belongs.

It is clear from Wright's argument that we cannot have

$$T = S$$

for the whole point is to distinguish them. What marks the difference is precisely Sp . So Wright has tacitly accepted this formulation: $T = S \wedge Sp$. This respects Wright's idea: T expresses what S used to express before the proof (1980, p. 44). Now, he goes on to ask,

³ It is worth noting that in this example Wright provides the variable T for the proven statement, and S for the statement before the proof, but he does not assign a value, or at least attach some significance, to the proof process itself. He leaves it out of the account.

should T be regarded as proved by the proof of S ? He concludes that Wittgenstein has no correct way to answer this.

For, if T is not proved by the proof of S , then it is wrong to accept T as a proof of S (that is, wrong to accept it as Sp). But if T is proved by S 's proof, then what can T express other than what S expresses? If there was a concept modification in S , so that S expresses what it now expresses, how can we avoid allowing that accepting the proof as a proof of T modifies the sense of T ? So, 'there cannot in principle be, even by outright stipulation, any way of expressing what was formerly meant by a statement whose meaning changes as a result of its receiving a proof which conforms with its meaning.' (*ibid.*)

Let us reformulate that more clearly. The argument distinguishes a proposition and a proven proposition. S and T are to be regarded as synonyms, so that a proof proves S if and only if it proves T . But the analogy, I want to argue, does not stand because $Sp \neq S$. That is the whole point. A proof is not a reaffirmation of the conjecture, it is a complex account and tracing of the place where the statement belongs. Proving is selecting, accommodating. By understanding where it is in the mathematical network and what role it plays, our use of the statement is upgraded, in a manner of speaking. Such is the conceptual modification.

Let's say S states GC, to remind the reader:

GC: All positive even integers ≥ 4 can be expressed as the sum of two primes

T would have to establish the above statement beyond doubt. Sp would provide the tools to do so. Yet, just as we right now can only say, or rather speculate, that every even number greater than or equal to 4 can be read as the sum of 2 primes, the proven statement would presumably amount to much more information. Here Wittgenstein suggests we rather think

that the proof would change our understanding of the mathematical question. Think of all the understanding of elliptic curves that was involved in the proof of Fermat's Last Theorem, for instance. Theorems are not isolated truths standing on their own, but normative, rule-like propositions that belong to, and transform within, mathematical techniques. *T* has to account for all the mathematical development that the proof of *S* brings, it cannot just stand for a final statement, a truth of some sort. So 'what *S* used to mean' or 'what *S* used to express' turns out to be a not entirely cogent concept.

Wittgenstein argues that a proof derives a proposition which serves as a rule by which we will abide, yet not only does it tell us 'this is how you must now proceed', it also show us *how* we are to follow it. The proof leads to a specific mathematical application.

'The proof constructs a proposition; but the point is *how* it constructs it. Sometimes, for example, it first constructs a *number* and then comes the proposition that there is such a number. When we say that the construction must *convince* us of the proposition, that means that it must lead us to apply this proposition in such-and-such a way. That it must determine us to accept this as sense, that not.' (RFM III §28).

Wittgenstein insists that, because of this prescriptive role, it is not accurate to classify mathematical knowledge, or to use his term, mathematical understanding, as advancing very sophisticated assortments of truths, in principle very much similar to those we obtain from sciences, and more so the more theoretic the latter get. This last type of reasoning goes something like this: if we can buy the existence of positrons, of which we have no direct sensory evidence, then why doubt the existence of, say, the real numbers? They seem similarly intractable, after all. For many philosophers, the comparison is sound enough to trace parallels between mathematical and scientific statements. But Wittgenstein denounces this as a counterproductive, dogmatic way of thinking. For him it is a procrustean effort to

make mathematics fit a model of knowledge that deals with truths.

Now, Wittgenstein does not forbid making descriptions of what rules are about, so long as we understand that no fact pins them down. To elaborate, no isolated fact could be what a rule refers to, but a rule-governed series of facts could. Ultimately the emphasis lies in that no sort of fact could be claimed to correspond to a mathematical proof. We do not discover a fact in the proof, the proof is not pointing towards a fact that laid there independently of it, so that the proof is merely our way of getting to it. But a series of facts that follows a pattern and has a constancy, better defined as a regular practice (see section 2.2.1 below), sanctioned by communities that share a form of life, can more accurately reflect what a proof achieves. 'I want to say that what we call mathematics [...] hangs together with the special position that we assign to the activity of calculating. Or, the special position that the calculation...has in our life, in the rest of our activities.' (RFM VII §24) That special position is the origin and support of the inexorability of mathematics.

But then how does Wittgenstein propose to establish the standards for correctness of mathematical practice through mathematical practice itself? He calls our attention to the fact that measuring and calculating are not learned through definitions. The meaning of the word 'length' is learnt by learning, among other things, what it is to determine length (PI II, xi, p. 225), that is, by learning how to perform the action of measuring in a community of fellow measurers and understanding when it is done correctly and when it is not, to be able to spot errors and correct them. A similar, apparently less controversial, phenomenon occurs in scientific communities. Note that, for instance, when we explain the reliability of physicists' beliefs we make use of physical modes of justification (Linnebo, 2006, p. 563), including descriptions of how, for example, our senses receive the information from physical phenomena. If we can accept physical explanations about physical sciences, then why cannot

we do the same with mathematical practice? That is, why can't we employ sorts of justification that belong to mathematical techniques to explain the nature and necessity of said techniques?

To say that we have the mathematics we have because we have been trained in a certain way is not, however, intended as a final philosophical answer to explain the normativity of mathematics. Wittgenstein acknowledges the difference between saying that in following a rule we go by a signpost, and explaining what this 'going-by-the-signpost' consists in (PI §198). The first is just to highlight a causal connexion. But by describing in detail the practices, customs and regularities behind such training and following of mathematical rules as signposts, we come upon key concepts of correctness, agreement, rule-following and practice, which surround the notion of mathematical proof. Similarly, Linnebo distinguishes an explanation that ensures that a process is reliable from an explanation of what makes it the case that the process is reliable (2006, p. 563), which is a more external explanation, draws more distance between what is assumed and what is explained. Even if it assumes that certain claims and methods are reliable, it still sets out to determine why they are so, and why such methods are conducive to finding out the truth of such claims (Linnebo, 2006, pp. 564-565). How do we know our mathematical methods touch upon an objective correctness instead of just inventing one that is prone to fall into error? After all, we could be in what Linnebo calls a 'lucky fool' scenario (2006, p. 549), namely that, upon being questioned about a mathematical proof's correctness, a person or a community may just luckily happen to get the answer right (by tossing a coin and deciding on its outcome, or by any other haphazard method, for that matter). The mathematically competent community differs from the lucky fool in that the former has followed theories, moreover in that they have followed theories *for a reason*, which is somehow connected to the theories' being true (Linnebo, 2006, p. 556). Wittgenstein must explain how this happens, why it is not an accident that we use the correct

mathematical theories. For Linnebo, this is actually a question of why mathematicians only accept true sentences as axioms (2006, p. 561), but we can read it also as a question that Wittgenstein's rule-governed community can and must answer. Linnebo proposes an answer that closely resembles Wittgenstein's view, arguing that 'mathematicians' tendency to accept as axioms only true sentences is adequately explained by pointing out that the historical process that led to the acceptance of these axioms is a justifiable one according to the standards of justification implicit in the mathematical and scientific community.' (2006, p. 561)

The problems with this explanation are that it comes from within the science that is being questioned, and also does not account for how the knower gets to know truths. In a way, this account assumes that the practitioners of the discipline in question are justified in practising it, and *uses this justification* to account for their axioms and mathematics and their reliability. This is why Linnebo calls it an 'internal explanation', since it intrinsically sets out from an alleged connexion between the practitioners' beliefs and the subject matter of these beliefs (Linnebo, 2006, p. 562). But is there any way we can actually explain a branch of science's reliability without employing the sort of justification that is peculiar to the science in question? Wittgenstein and Linnebo would agree in that there isn't, but would also point out that even more 'external' explanations would ultimately rely on internal elements of the theory in question. We can, and do, examine scientists' methods and claims with contemporary science, and find explanations of how our perception works

'having to do with light being reflected from our physical surroundings and impinging on our retinas, and this information's being interpreted by our visual system. As this example shows, external explanations too have to rely on claims from the contested discipline. To explain the reliability of our perceptual beliefs, we have to appeal to our knowledge of light and of the workings of our perceptual system. And this knowledge is ultimately based on the verdicts of our senses.' (Linnebo, 2006, p. 564)

Thus we can account for mathematical objectivity and reliability without commitment to mathematical content or truth and rather attending to our mathematical practices within our form of life.

2.2 Proofs and experiments

Wittgenstein's distinction between proofs and experiments is exhaustive and resonates in his philosophy as a whole. To examine it carefully, let us begin by distinguishing simpler calculations from experiments and then proceed to account for more complex mathematical statements, like those found in proofs. We see that a calculation has an internal, unbreakable relation with its result, expressed in the '='. ' $2 + 2 = 4$ ' reveals the interchangeability of elements on either side of the '='. The result is taken as the criterion for one's having gone by the rule (RFM VI §16), that is, if we do not obtain the sum result '4', then we simply have not added ' $2 + 2$ '. Contrary to this, the conditions of the experiment do not include the result (LFM X, p. 97). An experiment consists precisely in searching for results, so there are no 'wrong' experiments (LFM X, p. 94), in the sense that the results in empirical reality do not have to conform to anything. The burden is rather on us, we must register the findings faithfully and make sense of them in our scientific theories.

In scientific research, discoveries add to our knowledge of the world. Such research deals with problematic cases in a particular way. For example, if we find an organism that cannot be classified in standard taxonomy, perhaps a new phylum must be created to explain what we found and fit it in with the rest of biological theories. We cannot ignore the organism, we have to explain its existence. On the other hand, in mathematical proofs' derivation, it

seems the proof alters our understanding, rather than provide us with new facts. The answers the proof provides seem to open the scope of what we can conceive, instead of giving us more constituents of reality.

Wittgenstein elaborates on the distinction: 'In a most crude way—the crudest way possible—if I wanted to give the roughest hint to someone of the difference between an experiential proposition and a mathematical proposition which looks exactly like it, I'd say that we can always affix to the mathematical proposition a formula like "*by definition*".' (LFM XII, p. 111). He is analysing the uses, the *grammar* that rules over the word 'proof' (MS 122, 74r, in Mülhölzer, 2005, p. 69), for it is a grammatical distinction what tells experiments from proofs, namely the different role they play in language and the different uses we give them. The premiss is that both types of statements, those used in calculations and those in experiments, are often presented as propositions that look very much alike. In both cases, we seem to be talking about the nature of things, pointing at their characteristics, describing their relations.

We use expressions such as

'7 and 3 make 10'

'Red blood cells and plasma make blood'

'Force equals mass times acceleration'

Here we need to make sense of the predicates 'make', 'equals', 'is' and what they are about.

We must remember that in the mathematical case, the sentence is not signalling a fact, but stating a mathematical norm. This is, I take it, what Wittgenstein means when he stresses that a mathematical statement is withdrawn from experience and taken 'to the archives': it is consolidated as a standard. That is why we check the correctness of our multiplications with the multiplication tables. We trust them as standards.

But we trust many different types of standards, for instance very general empirical ones,

like physical laws and constants. Why are even the most general natural laws not quite as certain as mathematical axioms? Why are the principles of Newtonian mechanics not regarded as mathematical? Wittgenstein replies that an axiom is such '*not* because we accept it as extremely probable, nay certain, but because we assign it a particular function, and one that conflicts with that of an empirical proposition.' (RFM IV §5) Mathematical postulates about, say, spaces and planes are not general truths about geometry, but norms that regiment our concept of space. Their necessity is at an altogether different level from general laws, for '[a] proposition which it is supposed to be impossible to imagine as other than true has a different function from one for which this does not hold.' (RFM IV §4) If we can imagine the opposite of what a proposition holds, or if we can imagine the fact it describes not obtaining, then it is not a mathematical proposition.

An experiment has an open outcome. There are no wrong results in an experiment⁴, we just set out to see what we find. In an experiment we perform certain actions, like make chemicals react or release objects in a vacuum, and see what results from it. We register the facts and try to make sense of them, why they happened and so on, but we are genuinely awaiting an unknown outcome, even if we have an idea, sometimes a very precise idea, of what we should expect. Mathematical statements, on the other hand, are characterised by certainty. The very criterion for performing a mathematical operation correctly is that we get a

⁴Up to a point, of course. We must consider at least 3 provisions. First, we do not proceed entirely 'blindly' in empirical research, but with certain methodologies, expectations, instruments of research and previous results which guide our way. Secondly, there are also, for example, demonstrations in lecture rooms that are supposed to conduct an experiment in a determinate way to teach students about certain phenomena, so in a strict sense they do not produce discoveries, but then again they are 'fixed' experiments, predetermined for teaching purposes. Finally, there are also empirical results that an expert on the subject would immediately recognise as mistakes produced by some wrong procedure during the experimental process, outcomes that simply could not have occurred without some tampering on the experiment. Apart from these types of cases, which are rightly acknowledged by Wittgenstein (cf. LFM X, p. 98), we want to say, the experiment is an instrument that leads to discovery, a procedure in which we set out to find new truths. An experiment differs from a proof in the sense that, even though both have theories that back up their procedures, in experiments we must also accommodate results so that they are faithful to reality. We discuss below if there are similar 'surprising' results in mathematical proofs.

certain result from it (RFM VI §22), whereas the result of an experiment is left undetermined, it is an authentic discovery.

We can begin to see more clearly the motivation of Wittgenstein's hostility against the idea of the content of mathematical proofs, of them signalling facts. Such is 'not an objection to the term "fact" itself, but to the illegitimate use of the term outside the "language game" in which alone it has meaning' (Steiner, 2009, p. 14, fn. 18), that is, outside empirical discourse or the discourse of physical sciences. According to Wright (cf. 1.3.2 above), Wittgenstein's view leads to the conclusion that we ultimately decide what is true, in any matter whatsoever, as we go along. But arguing against mathematical truth, Wittgenstein is simply pointing out an illegitimate use of terms which 'leads to the universalization of the term "fact", and the appearance of pseudo explanatory theories using "fact" as a technical term of academic philosophy. One of these is the "theory" that every true proposition is true "in virtue of" the fact that it expresses, which then requires "mathematical facts" à la Hardy.' (Steiner, 2009, p. 14, fn. 18)

We cannot conceive mathematics as depicting facts because mathematics does not share the bivalence, the possibility of being true or false, of empirical statements. It does not share it because we cannot *fully* imagine a well-established mathematical result to be different from what it is. We may consider, off-handedly, that we can surely utter and understand ' $2 + 2 = 5$ ' as a statement of arithmetic, without grave consequences, except that we would be making a false statement. The problem is that an incorrect mathematical statement is not false, but stops being mathematics altogether. The full implications of coming to terms with ' $2 + 2 = 5$ ' surpass our imagination⁵. By coming to terms I mean to actually use

⁵Mathematicians like Tim Gowers (2009) sympathise with Wittgenstein's treatment of statements like ' $2 + 2 = 5$ '

' $2 + 2 = 5$ ' in our mathematical operations, consider it unassailable like multiplication tables, make it a part of our understanding of the world, of how we join and separate objects, etc. This is what must be understood correctly: such non-feasibility is at the same time a logical impossibility and a restriction placed by our form of life. The statement ' $2 + 2 = 5$ ' is utterly incompatible with what we have established as 'unit', 'quantity', 'number' in our counting and adding techniques.

A factual proposition has content, it pictures a possible state of affairs. We could say that the content of a rule is a set of instructions. A proposition provides information, a rule prescribes instructions. But isn't an instruction information after all, i.e. information about 'what must be the case'? It is, in a way, yet it does not have all the properties of empirical information. For instance, when we have a mistaken belief about an empirical phenomenon, we picture a fact that does not actually obtain. We believe a false proposition. When we are mistaken in mathematics or in another rule-governed activity, we picture something more difficult to characterise. What if I imagine a possible move in chess? I am in the middle of playing a game and, assessing the situation, I think I will deliver check-mate in 3 movements. I imagine how the game would continue, how my opponent will move and the spaces be uncovered. But then when the game progresses I realise it is impossible to do checkmate in 3 moves. What happened? It will be difficult to say at first glance, but suppose I took a record of the game and all its moves. At some point I would go back and study the game in question and find which rules I did not apply correctly or which blocking move was available to my opponent when I thought it was not. Were these errors already contained in the original plan

in considering they should not be treated as falsehoods, but rather as statements that involve thinking, or trying to think, of a form of life, or at least a significant number of activities, incommensurable with ours, where, for instance, counting with fingers highlights different things (the gaps between the fingers instead of the fingers), and containers change the number of objects we put in them, to entertain some examples.

of attack? It seems they are not contained in the first move, say. So where are they? Are they in the second one? But could I have arrived at the second one without the first? Presumably, I could at some point in the match see that I am heading for a bad move and change strategy. But it is only if I carry out the attack completely, considering of course my opponent's moves, that its fault will be revealed. Again, I would have to look at the whole technique I am employing, not isolated movements. Let us put someone else in this situation, in terms of what pieces remain in the board and who's next to move. Maybe they have a different plan, maybe they also make my first move, but planning to do other things. And, if they don't make the mistake I did, they might succeed delivering check-mate in 3 moves even if we both started with the same movement and I failed. So was the mistake in my first move but not in theirs? It seems the mistake does not follow from a particular move, but from the activity of playing in certain specific directions, different 'routes' if you will. A mathematical proof is analogously a specific route that provides a particular insight. Let us remember that for Wittgenstein '[I]anguage is a labyrinth of paths. You approach from *one* side and know your way about; you approach the same place from another side and no longer know your way about.' (PI §203) I believe something similar can be accounted for in the construction of a mathematical proof. For example different contributions made by new mathematical proofs of the same theorem, we could say, are different routes that provide different understandings.

Now, the concept that the proof forms, whatever it might be, brings with it at the same time the compulsion to accept it, that is, that 'this is how it must be', e.g. a geometrical construction must be so, a mathematical series must extend to infinity, etc. The proof-construction convinces me that proceeding according to the same rules produces the same result. What it convinces me of is expressed in the proved proposition, which tells me, compels me, to proceed in a certain way (RFM VII §72), to follow certain mathematical rules

and calculations. Conviction comes with the formation of the new concept. We are convinced that the same configuration will follow in all cases. By 'same' here, Wittgenstein means the ordinary concept of sameness, the one we have acquired through our linguistic training, and that follows the ordinary rules of comparison and copying (RFM III §72).

In one sense, the mathematical statement is an objective 'fact' of arithmetical techniques, but in any case it is a fact only within calculating. Note that Wittgenstein does not write 'within calculus' or 'within arithmetic' or 'within number theory', but 'within *calculating*', as an activity (RFM III §58). So if we learn facts within our arithmetical education, these are not about properties of numbers or something of the sort. What we learn is to unfold and develop the capacities of a calculating technique until we master it, like a language-game.

Wittgenstein uses an example of mathematical induction to distinguish mathematics from empirical statements. Suppose we are working with an Archimedes Spiral, in which a function determines how tight the spiral is. The spiral intersects points with a constant separation distance.

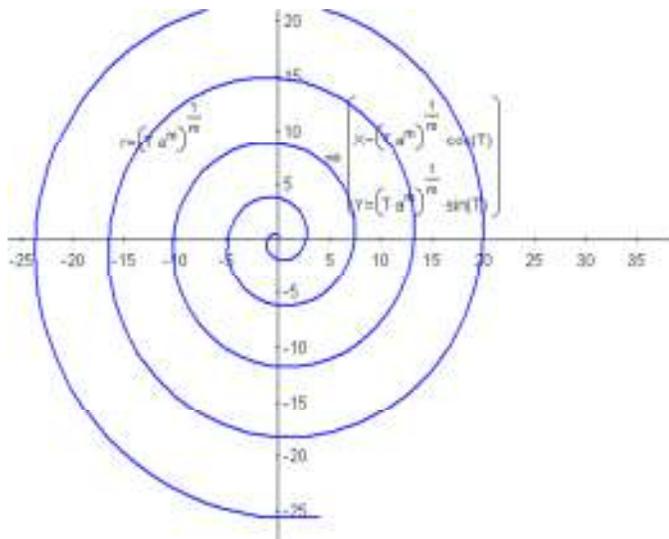


Fig. 1 Archimedes Spiral

We can extend the line and, making some calculations, using the formula, figure out the exact point where the 3000th cut will be made. How are we so certain of this? How can we just neglect so many steps? (LFM XXXI, p. 288). If the spirals are drawn correctly, then what we obtain in the formula *must* be the cutting point. 'This is a declaration of geometry-[and tells us nothing about the world]. It is a new rule. If we continued the spirals with a gauge and it didn't come right, we might say that the spiral wasn't Archimedean or that something had happened to our gauge.' (LFM XXXI, pp. 288-289, Wittgenstein's own brackets). Mathematical statements, then, do not tell us something about how the world is, but give us standards to which the world must conform. On the basis of them we judge if something *must* happen (LFM XXXI p. 289). 'That our 3000 steps [with the spiral and the gauge] will produce this result is an experiential proposition. But that this result is correct is a *rule*. We don't allow any experiential process either to refute or to establish a rule.' (LFM, XXXI, pp. 289-290, original brackets)

Mathematical propositions are grammatical (RFM III, §26); experiential ones are factual. The conviction a proof provides comes from its being grammatical, being a rule or an instruction we accept and by which we proceed, just as grammar governs over the formulation of correct expressions. If there is something that mathematical proofs are about, then we could say they contain instructions. 'The mathematical proposition says to me: Proceed like this!' (RFM VII §72), 'I act according to the proposition that got proved.' (RFM VII §74) In a proof it becomes clear what its mathematical application is, its use is revealed and now we can accommodate it with the rest of mathematics. It comes as a package: the proof-derivation, its certainty, its compelling result, its conceptual change. A mathematical proof should not display or spot a mathematical feature. It is not an arrow that lands on a mathematical fact. The arrow, as it were, is the very proof, its trajectory constitutes the

instruction, the recipe, as Gowers puts it (2009, p. 193) to make mathematical constructions. The mathematical proof constructs a sign that has, we can say, its compulsion *built into it* (RFM III §29), it does not depend on mathematical facts. In other words, proofs do not acquire the status of norms because they describe how mathematical objects or structures stand. They are certainly derived from other statements and axioms, but they do not arise as descriptions of any sort of reality. This directly opposes Frege's idea that 'the normative force of the laws of logic is due to the fact that they are true descriptions of the general features of concepts and objects' (Friedrich, p. 10).

Consider also Wittgenstein's discussion of the parallel postulate (henceforth PP, defined above, fn. 3). When we first learn about the postulate, experience definitely plays a part in our understanding of it, but not in the sense that experience shows it to us. '[W]e haven't made experiments and found that in reality only one straight line through a given point fails to cut another.' (RFM IV §2) Imagination tells us it. Before the proposition, the concept is still pliable. Once the concept is fixed in the proof, a restricted use has been specified for it. The restricted use is illustrated, for instance in that, convincing as PP is, working with hyperbolic geometry it does not hold. This is what Wittgenstein has in mind about a proof bringing a conceptual change, in this case changing our conception of space. Postulates do not present us with new chunks of space to discover and analyse, but provide new possibilities of navigating through it, as it were.

If someone does not acknowledge a proof result as correct, and instead endorses one that is blatantly incompatible with the axioms with which we started to derive, and to which this person assented, then they are better described as proposing a different axiomatic system or conceptual framework than as erring about a certain subject matter (Friedrich, p.

14). This is what a *reductio* in a proof, as the one above about the irrationality of $\sqrt{2}$, amounts to: it is not a falsehood to consider $\sqrt{2}$ is rational, but a misplacing, a confusion.

2.2.1 Empirical regularities and the salience, or 'hardening', of mathematical rules

Wittgenstein makes a distinction between empirical regularities and empirical facts (RFM IV §50), and points out the very different roles they play in language. He writes that there are some propositions which, though they are empirical, we cannot doubt, if making judgements is to be possible at all (OC §308). The propositions he is talking about are sanctioned experiences, or 'meant experiences' like seeing a pattern, always linked to a process and a result. To speak about such empirical regularities we need a common backdrop that decides what is regular and what is exceptional. And to understand sameness and difference in the first place we must inhabit a language and a mathematics that teaches us these concepts. Thanks to the interaction of regularities in human behaviour and regularities in nature, all within our form of life, we get the necessary conditions for the entire institution of rule-following (Steiner, 2009, p. 5), for the establishment of meaningful expressions and delimitation of what makes sense in language and mathematics.

With regularities like these we begin our logical reasoning:

'Someone who hears a bit of logic for the first time at school is straightway convinced when he is told that a proposition implies itself, or when he hears the law of contradiction, or of excluded middle. -Why is he immediately convinced? Well, these laws fit entirely into the use of language he is so familiar with. [...] The proof convinces him that he must hold fast to the proposition, to the technique that it prescribed; but it also shews him how he can hold fast to the proposition without running any risk of getting into conflict with experience.' (RFM VII §73)

Once again, in examples like these Wittgenstein explains logical compulsion as the

compulsion of following rules in a technique, not as a consequence of a discovery of a mathematical feature.

Only because we know our way through the assigned, calculated steps of a mathematical technique can we make predictions of the results we will get in a mathematical operation, and of course predictions applied to empirical reality. '[W]e should not call something "calculating" if we could not make such a prophecy with certainty. This really means: calculating is a technique.' (RFM III §66) We can make predictions because techniques conform with regularities, otherwise the technique would just give out, cease to be effective. Wittgenstein clearly states that language is based on regularity, on agreement in action (RFM VI §39). It is in the salience of certain regular patterns that we begin to accommodate a mathematical insight.

Wittgenstein wants to challenge the idea that mathematical proofs reveal truths about facts that are 'already there', already built into abstract mathematical reality. Under such view, proofs are the roads that connect us from mathematical truths to more mathematical truths, and these (proof-) routes are already traced, even if no one ever happens to go through them. This can be eloquently summed up in Frege's idea that the Pythagorean theorem is 'timelessly true, true independently of whether anyone takes it to be true. It needs no bearer. It is not true for the first time when it is discovered, but is like a planet which, already before anyone has seen it, has been in interaction with other planets.' (quoted in Brown, 1999, p. 136) In a similar vein, for Hardy, a proof is, taken to an extreme, merely accessory indications of a more direct discovery in a perfectly identifiable realm (1929, p. 18). He did not downplay a proof's importance but presented a proof as if it ultimately served to communicate results, somewhat indirectly at that. So it seems that, at least in principle, the proof is separable from

the knowledge obtained by it. This is precisely what Wittgenstein wants to oppose. Yet, he also aims to keep a strong notion of mathematical objectivity, that is, that the correctness of mathematical proofs is established solidly by independent criteria, independent of arbitrary individual or collective decisions. These criteria lie on the very practice of mathematics. To understand this, however, one must stop thinking of mathematical practice as a collection of isolated facts and see them within a technique, as we explained at the beginning of this section.

The 'hardening' of a regularity into a rule does not depend exclusively on our recognition of the regularity, or in a communal decision to treat it as necessary. So what does it depend on? How is the common use of mathematical operations tied to the necessity of such operations? Again, let us remember that a mathematical rule 'doesn't express an empirical connexion but we make it because there is an empirical connexion.' (LFM XXXI, pp. 291-292) Such connexion lies in the empirical world, outside the proof. The regularity hardened into a rule becomes a new kind of judgement in this sense: when we master a calculating technique, we know how we need to compute the relevant operations to obtain correct results. In this sense there are no surprises in the derived results. Consider a more simple example of what Wittgenstein is trying to do here: we don't happen to apply an operation we know well, like '+1' in the cardinal numbers series, and get surprised at the result obtained each time we count one more. Rather, we know how to recognise that the operation has been applied correctly, and so we can judge future results, we have 'a paradigm with which experience is compared and judged.' (RFM VI §22) As Wittgenstein points out, we can now say that a person worked on a calculation and got x *instead of* y , because we know y is what must result if the technique is applied correctly.

We learn, for example, that if one set of objects has been arranged in the form of a

(usually-shaped) human hand, so that to each finger corresponds an object; and another set has been arranged so that an object corresponds to each of the angles of a pentacle, we say the two sets are equal in number (RFM I §30). We incorporate this image into our actions, so that if we group objects using both arrangements and don't get an equal number, we check what we did again to see if we maybe omitted something or forgot to count an object or an angle. 'And if it were not like this the ground would be cut away from under the whole proof. For we decide to use the proof-picture instead of correlating the groups; we do not correlate them, but instead compare the groups with those of the proof' (RFM I §31), so that the proof becomes our standard measure, a paradigm of language (RFM I §32). We refuse to take any other path except the one the proof directs us to (RFM I §34) and consider it the essence of figures or groups of objects that they conform to such proof-picture.

Steiner notes that we can explain the notion of hardened regularities if we understand that mathematical statements are rules which are supervenient on experience, yet independent of experience (2009, p. 10). Let's say a group of people gathers apples and puts them in a special container. The first time they do this they manage to fit 400 of them. The next day they happen to fit 400 again. This keeps happening for some time. At some point, they firmly and surely report that, say, the company that bought 11 containers will have 4400 apples to deal with. Now they are using a mathematical operation. They will trust more in their calculations than, say, reports of individuals of how many apples have been packed, how many have they sold, etc. We could say, they trust them because the statements are no longer contingent, they are normative, they are about numbers rather than about fruit. Yet if we leave it at that, distinguishing that one kind of statement talks about numbers and another about apples, this confuses again the sphere of mathematics and that of scientific, contingent statements. Wittgenstein suggests we rather consider the numerical case as the use of a

mathematical technique, which undoubtedly makes us see the adding of our apples in a new way, we understand why our groupings of apples yield certain quantities, and why it must be like that.

Likewise, we are not uttering an empirical statement when we say which number follows when we are counting. 'That is not the empirical proposition that we come from 449 to 450 when it strikes us that we have applied the operation $+1$ to 449. Rather is it a stipulation that only when the result is 450 have we applied this operation. It is as if we had hardened the empirical proposition into a rule. And now we have, not an hypothesis that gets tested by experience, but a paradigm with which experience is compared and judged.' (RFM VI §22) I believe here he has a correct, yet underdeveloped idea of how it is a regularity, not an isolated fact of grouping objects together once or twice, what convinces us of the necessity of the techniques. For example, the operation ' $200 + 200 = 400$ ' is not a model for the fact that 200 things plus 200 things yield 400 things. When we group 200 objects with another 200, of course we can say they actually, in fact, yield 400 as a total. Yet if we take ' $200 + 200 = 400$ ' as a criterion of correctness for the addition of these numbers, then the statement announces an arithmetical rule, it becomes normative. It is not an empirical generalisation. Only when we've learned to add do we develop expectations about outcomes, and look at the operation of grouping things together differently. We know something is 'going to happen', we know to expect a result, and only one, with complete certainty. So a categorial change occurs when we 'elevate' an experimental use of an expression to a mathematical use (LFM XII, p. 112). Steiner reads a proof-derivation similarly, as a change of a propositional status. A proof is a specific mathematical route that makes ('hardens') propositions expressing empirical regularities into propositions expressing rules (2009, p. 2). So it seems the proof, like mathematical techniques, shows why a regularity must obtain, why its pattern must be the

case, but also *how*, specifically, through a specific route, the regularity and the proved propositions are *internally* related.

Wittgenstein is not undermining the necessity of mathematical proofs by arguing that no matter how well we understand what is going on in a proof-derivation we are never completely constrained to assent to it, as Wright seems to suggest (1990, p. 94), an idea which becomes even more confusing when, as Wright also notices (1990, p. 95), Wittgenstein writes that every reproduction of a proof must also reproduce the compulsion to obtain it (RFM III §55). We are entitled to ask how a proof can merely *guide* us towards a conception of things and at the same time *compel* us to accept it (Wright, 1990, p. 95). Wittgenstein carefully explains what he means by this apparently double standard:

“The rules compel me to...”—this can be said if only for the reason that it is not all a matter of my own will what seems to me to agree with the rule. And that is why it can even happen that I memorize the rules of a board-game and subsequently find out that in this game whoever starts *must* win. And it is something like this, when I discover that the rules lead to a contradiction. I am now compelled to acknowledge that this is not a proper game. [...] What is it that compels me?—the expression of the rule?—Yes, once I have been educated in this way. But can I say it compels me to follow it? Yes: if here one thinks of the rule, not as a line that I trace, but rather as a spell that holds us in thrall.’ (RFM VII §27)

It is blatantly obvious why his critics are impatient with his explanations. What could it possibly mean to understand that mathematics holds us in thrall? Well, we can say that Wittgenstein is simply insisting that the compulsion to accept a proof comes within a specific training, not as a raw, brute force of logic. We always get compelled in a certain way, in a certain path. '*What* is unshakably certain about what is proved? To accept a proposition as unshakably certain -I want to say- means to use it as a grammatical rule: this removes uncertainty from it.' (RFM III §39) What is proven is a guideline of how to use mathematical operations, a guideline we follow without hesitation and independently of our will. This automatic surrendering and

acceptance to the mathematical result is the only thing Wittgenstein wants to point out when he says a 'spell holds us in thrall'. We are simply obeying blindly mathematical compulsion, which we have learned to detect thanks to the techniques with which we live. An outsider, a dissident calculating agent, is immune to such spell, also completely foreign to our forms of understanding, thinking and doing mathematics.

Just to leave an example of how necessary it still is in philosophy to dispel ontological commitments in mathematics, Wittgenstein highlights that we often, mistakenly, think in mathematics as if standards were independent of our practices. We do this at very basic levels, thinking of lines and points, and something like the 'substance' of measuring standards, fixated on the individuation of '*the* standard metre'. But it is unthinkable that the standard metre in Paris, or to be more contemporary, that the definition adopted by the International Committee of Weights and Measures⁶, would exist without the institution of measuring in the metric system. We can put the metre in the archives of the Committee but that is not what gives it its character of a standard. It has rather been our constant practice, checking against reality, trial and error, the way it has paid off to use such system, and the fact that we keep using it, what make it a standard. For someone who has never measured, the standard metre in Paris or the agreed definition means nothing. What would it mean for a large community to suddenly agree that the metre in Paris was a wrong measure and declare it was only 7/8 of the 'real' metre? Doesn't it look more like we cannot talk about a 'real' metre, but more of a current, widely used standard measure?

Mathematical definitions and proofs, again, are norms in the sense that they provide a

⁶The actual standard metre, made out of a platinum and iridium alloy, was still problematic and imprecise. Other definitions of the metre, like 'the distance light travels in a vacuum in 1/299792458 of a second' have been included to reduce uncertainty. These ongoing efforts to reduce the uncertainty in our measurements, and the importance we give to them, exemplify what Wittgenstein emphasises regarding the fundamental role mathematics plays in our lives.

standard of what counts as a correct deployment of mathematical concepts (Friedrich, 2011, p. 9). A dissident, someone who does not derive, add, count, etc. like the rest of us, does not believe mathematical falsehoods, but, more accurately, fails to participate in the language-game of mathematics (*ibid.*), parts company with us from the outset. 'The concept of measurement, like all concepts, is thus also grounded on regularities of behavior—but it is grounded as well in objective regularities of nature.' (Steiner, 2009, p. 5) The concept responds to a regularly executed technique, which in turn responds to the characteristics of the physical world. This backs the argument that our given form of life is an objective standard for mathematical correctness. It may seem trivial and even lenient to say that dissidents simply 'part company with us'. The logician is perhaps ready to be scandalised at the dissident's mistakes, but Wittgenstein reminds us the dimension of what 'parting company with us' consists in: it means to be ousted from any meaningful activity and discourse. It would mean to be banished to unintelligibility. As difficult as this is to imagine, that is how necessary and normative our mathematics are.

What a proof achieves is not a symbolic formulation or translation, but a sort of upgrade in a technique, to achieve which one needs to travel the road of practice. What happens with proofs in higher mathematics? Which regularity is hardened there? Maybe here we are still thinking within the scientific paradigm of 'higher' and 'lower' theoretical knowledge. Proofs expand and modify concepts which allow us to deal with empirical reality. Proving is primordially an activity that changes our understanding. Mathematics does not cease to be an activity at higher or lower levels. So long as we can frame a mathematical statement as a rule, part of a technique, we can keep Wittgenstein's notion of proofs as conceptual changes, as advances in the possibility of use of mathematical operations, at any theoretical level.

The motivation for treating mathematics as a network of interrelated rules is ultimately to preserve an accurate account of the normative character of mathematical proofs. A proof should be necessary in the sense that we cannot think of its derivation being different. This amounts to it being impossible to imagine the proof's result as 'being false', or even that we cannot conceive of the mathematical fact it depicts as 'not obtaining'. Let us remember that '[a] proposition which it is supposed to be impossible to imagine as other than true has a different function from one for which this does not hold.' (RFM IV §4) As Gowers (2009) argues, we can try to imagine a world in which it would be natural to think that $2 + 2 = 5$. But perhaps the sheer impossibility of making our way through imagining that $2 + 2 = 5$ is the more natural thought. Wittgenstein and Gowers invite us to perform an exercise that is not feasible, and its non-feasibility illustrates how mathematical proofs set the standards of what is intelligible, what is thinkable.

Gowers mentions that there are mathematical terms for which, though they have a formal definition, it remains difficult to see what they define, what kind of objects they describe. This difficulty to see what is defined, what kind of object or fact, could be a favourable argument for Wittgenstein's stance and an effective, practice-based attack on the tenability of mathematical platonism. Then let us ask the mathematician: can we treat mathematics as a describer of truths or should we conceive it differently? Let us remember that Wittgenstein considers that to understand an expression is to know how to use it in the language where it belongs. So to understand a mathematical concept is to know how to use it. How can we be sure that we understand it correctly? Again, Gowers suggests a very philosophical way to find out: if we can transmit the mathematical understanding to a computer, then we can say we master it. He suggests we

'think what you would have to program into a computer if you wanted it to handle a mathematical concept correctly. If the concept was an ordered pair, then it would be ridiculous to tell your computer to convert the ordered pair (x,y) into the set $\{\{x\},\{x,y\}\}$ every time it came across it. Far more sensible, for almost all mathematical contexts, would be to tell it the axiom for equality of ordered pairs. And if it used that axiom without a fuss, we would be inclined to judge that it understood the concept of ordered pairs, at least if we had a reasonably non-metaphysical idea of understanding - something like Wittgenstein's, for example.' (2009, p. 192)

Avigad makes a related remark about faults in proof assistants, software used as aid to derive mathematical proofs. Regardless of all the data and instructions fed into programmes, these are still a long distance away from supporting, for instance, algebraic reasoning. This is telling of our own lack of full understanding of algebraic inferences and reasoning, even in very simple cases (2008, p. 345). Whatever efforts we direct towards a better understanding of such reasoning, it is useful to have as a goal the possibility to programme such knowledge into a computer. Wittgenstein's rule-following approach seems to lend itself for these fruitful purposes.

2.3 Conclusions

This chapter exposed and defended the cogency and relevance of Wittgenstein's account of mathematical proofs, clarifying some of its problematic claims and dispelling subtle, well-engrained misrepresentations of his unorthodox remarks. This exposition hinged on a vindication of Wittgenstein's concepts of mathematical practice and techniques, and their importance not only in pragmatic matters but also in the very way we think about mathematics, in the way logical compulsion permeates proof-derivation and in the way mathematical objectivity is established and sustained.

Through Wittgenstein's distinction between experiments and proofs, we arrived at a clearer difference between descriptions and norms, empirical falsity and mathematical misconception, factual discourse and normative discourse. The examples and arguments supported the claims that analysing mathematical proofs as pointing towards mathematical facts, or relating proofs to any sense of mathematical content whatsoever, is problematic and unnecessary. In the particular case of proofs, it was clarified that the progress a mathematical proof brings consists in recasting the identity of a mathematical statement: to prove a statement is to identify it as necessary. The proof makes this transition, taking us in a particular deductive route from what is the case to what *must* be the case (RFM IV §29). This draws distance from empirical procedures, where we rather report what is the case, setting out in any methodological route that faithfully depicts our findings in empirical reality. The mathematical 'must' does not express a mathematical truth, but rather our inability to depart from the concept introduced by a proof (RFM IV §30), for such concept is necessary as part of a mathematical framework that delineates all meaningful thought and action.

We see it proves quite useful to draw from Wittgenstein's observations in language when it comes to define the necessity of mathematical statements. Just as he writes in PI §329: 'When I think in language, there aren't "meanings" going through my mind in addition to the verbal expressions: the language itself is the vehicle of thought', he affirms an analogous thing about proofs, namely that there aren't any 'meanings' going through our minds in addition to the mathematical calculations: the calculation or proof itself is the vehicle which takes us to a new concept, not from contentful mathematical assertions to new mathematical assertions.

Wittgenstein has the merit of linking pre-theoretical linguistic practice with the

theoretical network of mathematics, an often overlooked source of philosophical insights. Analysing such practice, we can draw a solid notion of mathematical objectivity based on the concepts of sameness, difference, correctness and all the notions needed to establish rule-following in language. With these, we can argue that the reasons why we accept a proof as necessary are external to the proof (RFM III §41), referring to the objectivity of empirical and cultural regularities that engage in language and that establish the correction criteria for proof-derivation, yet also affirming that it is not something behind the proof, but the proof, that proves (RFM I §42), referring to the specific derivation that a proof constructs, and also pointing out that a proof is not an assertion that refers to a mathematical fact which, in principle, could have been reached otherwise.

Nonetheless, Wittgenstein recognises the lack of a sharp boundary between propositions of logic and empirical propositions, and considers this reflects the lack of a sharp boundary between rule and empirical proposition (OC §319), considering how close empirical regularities are both from empirical and normative discourse. He is not one to give the final answer that settles the sharp boundary, but reports this as a result of his philosophical analysis, and it may be a starting-point for further research into the parallels and differences between mathematical and scientific claims, which may in turn help us decipher the nature of proofs.

Chapter III. Seeing new physiognomies

This final section recapitulates what can be extracted as Wittgenstein's philosophical contribution to the understanding of mathematical proofs, its insights and limits, and leaves some open questions that may guide us to further analyse if these are relevant to contemporary debates in philosophy of mathematics. Regardless of what the answer might be, this paper has presented a less simplistic account of Wittgenstein's views on mathematical proof, providing a less dismissive and deeper analysis of his approach. It has addressed important criticisms and given space for the reader to carefully examine the dialogue between Wittgenstein and some of his more critical interpreters. We have argued why his remarks on proofs cannot be incorporated to the ranks of conventionalism or intuitionism, and have justified his objections to the notions of mathematical content and truth. The rest of the paper makes some final remarks about what we can understand from a proof's result bringing about a conceptual change, and if such change really entails mathematical progress. We end by suggesting that Wittgenstein's account makes important methodological observations, namely about the similarities between the research methods of philosophy and mathematics, how a mathematical proof and a philosophical remark do not follow scientific methods, but rather disentangle a confusing situation and bring it into clarity.

3.1 Surveyability of proofs and the philosophical 'synoptic vision'

One of Wittgenstein's central aims has been to point out that a mathematical proof achieves something quite difficult to define, but equally unique in its normativity: a conceptual change. This notion is pivotal in his philosophy, it is necessary to understand his distinction between normative and factual discourse, his opposition to mathematical truth, his definition of mathematics as a rule-governed activity. He spends a lot of time suggesting how to make

sense of a proof producing a conceptual change in mathematics, and providing examples to give an idea of its significance. He faces a difficult task, for if there is no body of mathematical truths to back up objectivity, it would seem we cannot have non-arbitrary criteria to distinguish between correct and incorrect mathematical calculations. But Wittgenstein argues that this philosophical call for mathematical facts to sustain mathematical objectivity is not legitimate, but comes from 'habits' in our linguistic practice. The thing is we have a very useful method for problem-resolution in empirical sciences. It is undoubtedly effective, and Wittgenstein recognises this, never puts it or scientific objectivity in question. What he points out is that this methodological habit can be harmful when we use an experimental approach to solve a mathematical question. Problem and method then simply pass by one another (PI II xiv) and no real progress is made, because we are not posing the right questions in the first place. In philosophy, and in philosophy of mathematics in particular, we often want the answers to fit a certain model, instead of looking at what we are trying to analyse, ignoring the benefits of looking at what the expressions and concepts mean in our daily activities and uses. We presuppose that the work to be done resembles digging deep for more basic components or layers of mathematical truths. Wittgenstein's philosophy is important in that it denounces the poorness of our results when we follow such methods, at least in the sense that we are not achieving what we set out to do, i.e. we have not set unquestionable foundations for mathematics, and we have not fully explained whence comes the compulsion in deriving a proof correctly. Mühlhölzer remarks that Wittgenstein takes an entirely different philosophical approach by calling us back to see the *grammar* of the word proof (2005, p. 70), that is, the linguistic standards that govern the use of proof in daily mathematical practices, and also by reminding us of our familiar mathematical practice through exhaustive examples and descriptions (2005, p. 76), illustrating the normativity of mathematical techniques in multiple

scenarios. Wittgenstein thus advances methodological claims: he believes the way philosophy has traditionally defined necessary truths in mathematics has little to do with the actual source and the actual way of recognising necessity. Such misguidance, he goes on to say, reflects a fault in our philosophical practices themselves. All this goes hand in hand with Wittgenstein's insistence that mathematical and philosophical analyses share a common feature: they both transform our understanding, rather than accumulate knowledge. 'I want to say: "We don't command a clear view of what we have done, and that is why it strikes us as mysterious.'" (RFM I App. II §8) That recommendation goes both for mathematics and philosophy. Our grammar often lacks perspicuity to clearly see the correct use of words. 'A perspicuous representation produces just that understanding which consists in "seeing connexions".' (PI §122) Philosophy must come up with intermediate cases, diverse cases of applications and language-games to illustrate confusions and misuses of expressions, and so guide us back to their correct use. He proposes we apply a similar philosophical treatment for mathematics.

The tricky aspect is that mathematics is part of this law-giving grammar. As grammatical rules, the function of mathematical propositions is not to assert that such-and-such fact obtains, but to supply a framework in which it is possible to make accurate assertions and descriptions (RFM VII §2). 'A "series" in the mathematical sense is a *method of construction* for series of linguistic expressions.' (RFM II §38, emphasis mine) This is directly related to rule-following: we can get an instruction to, say, write the corresponding word beside a given word in a list. We see the first three elements 'free-independent; cold-chill; difficult-hard'. We are told 'go on in the same way', and we understand that we are to write a synonym of the word we get. Similarly, a series like the natural number series prescribes how we are to construct certain numbers following the application of the operator '+1'. In both

cases, we build expressions, those to express synonymy and succession in the case of the examples, and learn to construct them for future uses. A derived proof is similar to such cases in the sense that it is a rule for description, it modifies what can be expressed mathematically. Consider, for example, Euclid's Theorem of the Infinitude of Primes. Briefly put, suppose that there is a finite number of primes, so there would be one largest prime, let's call it N . Now, we should be able to account for what the number $N! + 1$ is, i.e. the number we get by multiplying N by each of its predecessors and adding 1 to that total. The resulting number cannot be prime, for N is already the largest prime, following our hypothesis. So $N! + 1$ must be composite. If it is a composite, then it can be expressed as a multiplication of prime factors. But these prime factors *must* be greater than N , otherwise they could not divide $N! + 1$ without remainder, they would all leave remainder 1, considering how we constructed $N! + 1$ in the first place. Bottom line, we *need* primes greater than N to be able to factorise $N! + 1$. This logical, faultless reasoning runs against our hypothesis that there is one greatest prime and no more primes after it. We *must* reject it and affirm that, contrary to what we supposed, the sequence of primes goes on endlessly. We can say that this theorem gives us a 'recipe' for extending any finite list of primes (Gowers, 2009, p. 193), a justification and an endless permission to do so. This recipe-yielding mathematics echoes some points of agreement between Wittgenstein and formalism that cannot be fully explored here.

When Wittgenstein criticises ways of thinking like that of regarding number theory as the 'mineralogy of numbers', he reminds us of his pronouncement against 'the bewitchment of our intelligence by means of language' (PI §109), and urges we return to the spatiotemporal phenomenon of language (PI §108). Wittgenstein does not oppose or criticise pure mathematics nor favours exclusively applied mathematics, but strongly rejects conceptions that view philosophy of mathematics as accounting for 'the natural history of mathematical

objects' (RFM II §42), as if philosophy was supposed to unearth how mathematical statements and proofs direct us through to discover the details of a mathematical world. The details we do discover are those of the empirical world, and we describe them well enough with empirical propositions and methods, but it is mathematics that guides us in shaping the form of these descriptions, i.e. determining what is the correct use of expressions, establishing syntactic rules, creating a framework in which we can talk about correctness, sameness, truth, etc. I believe there is a way of keeping the full strength of mathematical compulsion, the one any mathematician would recognise in their daily work as pertaining to a proof, and at the same time reject that the route of the proof has already been traced, or as Wittgenstein puts it, to reasonably doubt that the mathematical rules already lead in a certain way, even if no one went it (RFM IV §48).

Wittgenstein insists a proof must be surveyable. This requisite has often been interpreted as evidence for Wittgenstein's rejection of very long proofs, proofs we cannot take in, or concepts like infinity. Such interpretation is partially correct. By surveyable, I believe Wittgenstein means something like 'ready for use'. Even a very elaborate tool remains useful if we properly know how to employ it, if we can use it for various purposes, if we command it. If a proof is surveyable it means that it is usable as a guideline in judging other mathematical statements (RFM III §22). The Hubble telescope may be a very complex instrument to use, yet if we master it and can use it for research then it becomes a basis with which to judge many astronomical discoveries. Wittgenstein compares mathematical proofs to cinematographic pictures, (*ibid.*) perhaps having in mind an instructional film, where the mechanisms are recorded once and for all and serve as a model for mathematical practices. This is how Wittgenstein wants to regard proofs: not as columns of symbols and derivations, but as graspable guidelines of how to operate in mathematics. The proven proposition shows

how something has to be, how some operation must be carried out, how some geometrical figure is to be constructed, to list some examples, and this transmission of its necessity depends on it being presented in a memorable, surveyable, configuration. Such configuration becomes impressed in our minds not because it is easily taken in, but because we now recognise it as a paradigm of identity (RFM III §9), a way in which we must proceed if we are to do mathematics at all. And yet, Wittgenstein does admit that a hugely long proof would be similar to our trying to understand '1000' in the form '1+1+1+1...' (RFM III §10). Such shape is not memorable, in the sense that if we see the long string of '+1s' we do not take in that they stand for '1000'. In this sense, a long proof would not be considered a proof in Wittgenstein's account, but not simply because of our human incapability of taking it in, but rather because we could not use it or apply it. Copying a proof does not make the mathematician understand it, just as a machine that checks the correctness of a long proof is not quite a mathematical agent like us (RFM V §2). Consider the Classification Theorem in group theory. Aschbacher (2005) challenges Wittgenstein's link between surveyability and usability in his remarks on the Classification Theorem by explaining that it is basically impossible, given the current available techniques, to write an error-free proof of such theorem, which looks more like a highly complex theory of physics. Aschbacher recognises that mathematicians take an idealised notion of proof as a model for which to strive but which does not necessarily occur every time (2005, p. 2402), and that does not mean the theorems are any less operational or useful. Does this undermine Wittgenstein's requirement for surveyability?

Not necessarily, I want to say. We could phrase Wittgenstein's requirement in Poincaré's terms: we do not only want to know whether all the syllogisms of a demonstration are correct, but why they are linked together in one way *rather* than another (quoted in Avigad, 2008, p. 319). One of Wittgenstein's important insights is his equating of mathematical

understanding as having the relevant mathematical abilities. To understand a proof, for instance, means to be able to explain how we derive it, why we should choose its chain of derivations instead of any other. This is part of having a surveyable representation of such proof. It is, indeed, questionable whether Wittgenstein would consider the Classification Theorem as an authentic proof, since it does have uses, but it is available to comparatively few mathematicians and in very specific scenarios that necessarily involve higher mathematics whose use is difficult to pin down as straightforwardly as Wittgenstein would like. One must make a deeper analysis of how the Classification Theorem and other similar theorems are actually implemented in mathematical practice, what such practice consists in, how it connects to other areas of mathematics, in order to give a more accurate evaluation of Wittgenstein's views on surveyability. I believe this is a promissory field of philosophical research into which I cannot venture in this paper.

What Wittgenstein does affirm is that '[w]hen we say in a proof: "This *must* come out"- then this is not for reasons we do not see. It is not our getting this result, but its being the end of this route, that makes us accept it. What convinces us- that is the proof: a configuration that does not convince us is not the proof, even when it can be shewn to exemplify the proved proposition.' (RFM III §39) Wittgenstein's goal is to show that no reduction to simple logical components will show what makes the proof necessary. The correct derivation and presentation of the logical signs is a necessary, yet not a sufficient condition for a proof to have the character of being inexorably certain. Even if we spell out the axioms under which a formal system works, there would remain to explain what makes these axioms necessary, and why are we compelled to follow them, in an explanation that should draw some distance from the formal system's own principles. What convinces us is perhaps the link we make with other proofs, the insight that the proof unveils, which is unequivocally a practical insight: we see a

new possibility, a new operation, we see the technique has grown and we can perform new tasks with it.

Wittgenstein is not alone at this position. Avigad notes that there is a tendency in philosophy to take understanding to be the "possession" of a meaning that somehow "determines" the appropriate usage' (2008, p. 324) of expressions. Sometimes the object of understanding can be neatly packed in, say, an algebraic formula, but then this only delays the question about understanding momentarily, for we can still ask what it means to understand that formula, what it implies, when do we really grasp it. Again, beyond formal systems and placing statements in the archives, the relevant question in the end will still be if we know how to use a proof.

Due to mathematics' intertwining with language, as explained above, a proof ends up affecting what we can see and conceive very radically. A proof makes us see a new physiognomy, perhaps in a similar way as what we see when we understand why we perceive an optical illusion: we see something we already saw, but anew, with different salient features, new things to notice. If there is something to be found through a proof, we can say we find physiognomies, patterns, logical arrangements. We authentically find them, yet not in the ways of scientific discovery, but through use, like when we make different figures or planes coincide, and some new shape authentically emerges. When we understand the proof that an angle cannot be trisected using exclusively ruler and compass, it is *in the course* of such proof that we form a way of looking at the trisection of the angle, 'our way of seeing is changed -and it does not detract from this that it is connected with experience. Our way of seeing is remodelled.' (RFM IV §30) This is an interesting insight yet it can also be limiting for Wittgenstein's account, for it relies heavily on geometrical observations and does not do justice to the reasoning that occurs in other branches of mathematics, even at very basic

levels. Wittgenstein's remarks have to go beyond visual metaphors to become powerful tools for philosophical analysis of mathematics.

3.2 Open questions

As we saw above, Wright notes that Wittgenstein's arguments on mathematical proofs mix, or rather confuse, philosophical remarks on mathematics with remarks on understanding in general (1980, p. 49). I have argued that indeed Wittgenstein's observations on proofs echo wider ideas on the nature of understanding, but that they do so not necessarily in a confusing or distorted way. On the contrary, I believe we have seen there is merit in analysing mathematics not as a separate area of discourse, but as a network of norms (RFM VII §67) that impinges upon any and all meaningful expressions.

Wittgenstein has extensively argued that mathematical proofs change mathematics, yet it is still not clear how they change it. Wright explains one of the problems with Wittgenstein's conception of mathematical objectivity this way: if we say that accepting a proof of a statement changes its meaning, then it ought to be possible, after we have accepted the proof, satisfactorily to convey what our understanding of the statement used to be. But if we follow Wittgenstein's argument, he continues, it does not seem possible to give an account of how certain concepts were modified. Roughly put, there would be no stages we could identify as 'before' and 'after' the proof. It rather seems that it is typically in virtue of the understanding we already have (and maintain) of expressions and rules involved in our mathematical discourse that we are capable to accept new proofs. Correct proofs agree with the way the concepts involved in them are understood already. We even take the capacity to recognise a good proof as a criterion for having a proper grasp of the concepts involved.

Moreover, how can Wittgenstein's remarks guarantee that the proof we derive, whether

we understand it as a conceptual change or not, is derived correctly? Should we prove that it is so, maybe in another system? Would we then have to prove that we have proved the first result, falling into an infinite regress of proofs? Wittgenstein suggests an alternative: let us construct a proof in a way that can be taken in, with which we easily recognise the necessity of the result and are absolutely compelled by it once we go through it (RFM III §13), and by absolutely compelled here he means that we would follow it blindly, because it would be unthinkable for it to be otherwise, for us to use it in a different way and act (whether performing mathematical operations or in general performing our daily activities) as if the result were different. Here we can see that the source of compulsion to follow proof-derivations is only fully accounted for in human action, in what we can and cannot perform otherwise.

I want to point out a mathematical reflection which, if tenable, could reconcile or at least shed some light on Wittgenstein's regard of the establishment of a proof as being somewhat of a decision and yet also as a result we are compelled to derive. It comes from Poincaré and reads: '[Mathematical] [d]iscovery consists precisely in not constructing useless combinations, but in constructing those that are useful, which are an infinitely small minority. Discovery is discernment, selection.' (*Science et Méthode*, quoted in Avigad, 2008, p. 320) If we are well-trained in a mathematical technique, then we know how to use it as a search tool, to search for solutions to new problems that may emerge. Mathematical understanding allows us to navigate this mathematical network of roads, or of norms, as Wittgenstein would put it (Avigad, 2008, p. 320).

Wittgenstein admits this seemingly contradictory idea: at the end of a proof one does not rightly know what one has proved by the old criteria (RFM III §14). To try to understand

this, let us go back to the miscount example above. When Connor learned to add he did not lose his counting abilities, but rather modified them. Now that he masters addition, he will count differently, make less mistakes, etc. We need new criteria to get out of the muddle and command a clear view of the proof, for example, by making a hitherto unsurveyable proof surveyable. Making it perspicuous is exactly the advance. Think of an example Wittgenstein uses to illustrate the rules we use for counting. We learn to count not only by adding one unit successively, we also learn 'shortcuts' depending on what we want to count. If we want to count all the months in 148 years, we don't have to count unit by unit, from 1 to 1776. We can just count '12+12+12...', presuming we still can't figure out that it's easier to multiply 148 times 12. Taking 12 steps at once consists, for Wittgenstein, in regarding not the unit step but a different step as decisive. In the case of a proof, derived in a mathematical system we know quite well, we are guided by the formal rules step by step, yet at the end it is a different step that is counted as decisive (RFM III §20). Thus, in Connor's case, we have introduced a new rule for counting, in which the old rule of counting unit by unit remains, and yet, has been developed in a new way.

In sum, if one reads the argument that mathematics forms and modifies concepts as if every new proof transformed the whole of mathematics, then of course it is an absurd claim to make. Wittgenstein argues that a conceptual change produces a new mathematics but not a new set of truths that is separate and unrelated to the 'former'. His idea is more akin to thinking that mathematics develops by transforming, expanding and connecting the uses of our many mathematical techniques. With a new proof, one does not end up with a new piece of mathematics, but with a new use. Understanding that the primes run on endlessly, as we explained above, gives us a better grip on mathematics, and multiplies the tasks we can perform with the new theorem. A new use means a different, better equipped mathematics,

not the destruction of a system. A proper mathematical change occurs when it affects the application of the technique we were using (RFM III §47), not by invalidating its previous use, but by coming to understand where it stands in the bigger mathematical picture.

I want to finish by challenging this dissertation, or rather by acknowledging that there are important points that Wittgenstein's philosophy of proofs must account for. One of the most crucial ones is to make the conceptual-modification account of proofs explain how mathematical techniques have unforeseen applications. This is not the place to make a more elaborate argument, but it is worth pointing out Steiner's observation that Wittgenstein's account does not explain how mathematical techniques can have 'unforeseen consequences', that is, that one such technique may serve us well in its original purpose, yet happen to work with other techniques, much like different scientific doctrines together give us a picture of the universe. After developing an argument that links mathematical techniques used in kinematics, topology and molecular physics, Steiner (2009, pp. 24-26) concludes that mathematics simply does not ever render superfluous information, that is, that what we may consider a useless feature of a technique will end up serving a purpose. Steiner writes that

'Wittgenstein's account of mathematical application was seriously lacking. He apparently thought that the use of mathematical concepts for unintended purposes is of no philosophical importance, like the use of a knife to turn a screw. [...] [M]athematical concepts often contain within them the germ of further applications—to see this, however, one must go far beyond the elementary examples that Wittgenstein worked with.' (Steiner, 2009, p. 26)

But this, again, returns to the view that mathematics describes aspects of reality, so that nothing is unnecessary in a mathematical technique, for we will find its meaning sooner or later. I want to end this note proposing that it may be possible to read Steiner's remark as a

confirmation of the view Wittgenstein has been insisting upon. That is, perhaps Steiner's observation points to two relevant arguments in Wittgenstein's philosophy of mathematical proofs: one, that there is a fundamental difference between mathematics and science, and that this lies in that the former does not describe facts nor, consequently, report truths about (some) reality; and two, that mathematics is first and foremost an activity, so that the proofs developed within it are expected to have multiple applications, regardless of what we first used them for. Just as, say, the 8 queens puzzle⁷ was not foreseen when chess was invented (and I believe it is safe to say chess was not invented to play the 8 queen puzzle game) so is the case with unforeseen applications. When people invented it, they imagined the whole game of chess, just as we imagine a mathematical technique. That is, we do not cumulatively build up a technique, just as we did not develop chess by first dividing a board, then having a 'king', then 'bishops' and waited to see what happened with them. We do not say that '[s]o far that's all we know about the game; but that's always something.- And perhaps more will be discovered.' (RFM II §39)

Ultimately, the upshot of regarding a proof as a conceptual change, a change in what we can conceive and do, what we can perform in our techniques, as Wittgenstein suggests, is that then we pay attention to often overlooked aspects of mathematical practice and how they affect mathematics and knowledge in general. Wright is correct, Wittgenstein's remarks on mathematics spread unto understanding in general, but this might happen in a non-vicious, philosophically insightful way.

⁷The puzzle consists in placing 8 queens on a chessboard in positions where they will not attack each other, following the rules for the movement of the queen in chess.

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