Università Ca' Foscari Venezia

DOTTORATO DI RICERCA IN FILOSOFIA, XXIII CICLO
Scuola di Dottorato in Scienze Umanistiche

Settore scientifico disciplinare di appartenenza: M-FIL/05

Reconsidering Wittgenstein's Philosophy of Mathematics

Tutore del Dottorando:
Prof. Luigi Perissinotto

Tesi di Dottorato di:
Matteo Plebani
Matricola 955448

Direttore della Scuola di Dottorato:
Prof. Carlo Natali

A.A. 2009-2010

Contents

Acknowledgements

I am very thankful to Prof. Victor Rodych for having discussed with me some of the issues addressed in Chapter Four. I am very grateful Prof. Pasquale Frascolla for having provided a lot of precious advice and constant encouragement. It was a pleasure to have Prof. Luigi Perissinotto as a tutor. Thanks to Ivan Kasa: discussing with him I learned a lot about the Neo-Fregean program and I shaped my ideas about epistemological objections to Platonism. Thanks to David Liggins, who read part of Chapter Three, providing very useful comments. Thanks to Vittorio Morato, Beppe Spolaore and Tommaso Gordini for LaTeX assistance. Thanks to Maria Baetzing for linguistic advice, and much more. Thanks to my beloved flatmates as well.

Abbreviations

AWL Ambrose, A. (ed.) *Wittgenstein's Lectures, Cambridge 1932-35*, Blackwell, Oxford, 1979.

LB *The Blue and Brown Books: Preliminary Studies for the 'Philosophical Investigations'*, ed. Rush Rees, Blackwell, Oxford, 1958.

LFM Diamond C.(ed.) *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, Cornell University Press, Itacha, N.Y., 1976.

PG *Philosophical Grammar*, ed. R. Rhees, trans. A. J. P. Kenny, Blackwell, Oxford, 1974.

PI *Philosophical Investigations*, ed. G. E. M. Anscombe and R. Rhees, trans. G. E. M. Anscombe, 2nd edn, Blackwell, Oxford, 1958.

PR *Philosophical Remarks*, ed. R. Rhees, 2nd edn, trans. R. Hargreaves and R. White, Blackwell, Oxford, 1958

RFM *Remarks on the Foundations of Mathematics*, ed. G.H. von Wright, R. Rhees and G. E. M. Anscombe, trans. G. E. M. Anscombe, 3rd edn, Blackwell, Oxford, 1978.

T *Tractatus Logico-Philosophicus*, trans. D. F. Pears and B. F. McGuinness, Routledge & Kegan Paul, London, 1961.

WVC McGuinness, B. F. (ed.) *Wittgenstein and the Vienna Circle: Conversations recorded by Friedrich Waismann*, trans. J. Schulte and B. F. McGuinness, Blackwell, Oxford, 1979.

Z *Zettel*, ed. G. E. M. Anscombe and G.H. von Wright, trans. G. E. M. Anscombe, 2nd edn, Blackwell, Oxford, 1981.

Overview

The aim of the present work is to investigate whether Wittgenstein's Philosophy of Mathematics turns out to be of some interest for the contemporary research in this field. Chapter 1 exposes Wittgenstein's pivotal theses in the Philosophy of Mathematics and provides a brief summary of their possible relevance for the most global matters discussed by contemporary philosophers of Mathematics. The problems of mathematical knowledge, the ontological status of mathematical entities and the question about the Objectivity of Mathematics are analyzed in depth in the three following chapters.

Chapter 1

Wittgenstein's Philosophy of Mathematics in a Nutshell

> For we undoubtedly incline to think of the Peano axioms, informally expressed as above, and their logical consequences, however that class is to be characterised, as being truths of some sort; so there has to be a philosophical question how we ought to conceive of the *nature* of the facts which make those statements true, and of how they fall within the human epistemological compass. At least, there have to be such questions if we may presuppose that it is philosophically respectable to think of those statements as truths of any sort. This presupposition has, of course, been questioned. It was questioned by the Formalists of Frege's day [...] And it was questioned for quite different, and much more profound, reasons by the later Wittgenstein.
>
> [Wright, 1983, p. xiv]

This is (W)right, to my mind: one of the key features of Wittgenstein's account of Mathematics is a strong opposition to the idea that in Mathematics we are dealing with genuine truths. Given that propositions are usually taken to be truth-bearers, the same claim can be expressed by saying that there are no mathematical propositions. After a reconstruction of Wittgenstein's motivation for this claim (paragraph 1.1) my main concern will be to point out how much Wittgenstein's position is radical, comparing his mature position with a more traditional form of anti-realism he embraced during the intermediate phase of his reflections.

The profound reason Wright mentions is in my opinion the difficulty that any Platonists account of necessity must face and which the normative conception of mathematical statements should solve.

My idea is to summarize Wittgenstein's position in two pivotal theses: that mathematical sentences do not depict facts (they do not express propositions), and that this is due to the fact that they are grammatical rules in disguise.

Apart from the stress on the problem of necessity as the original source of Wittgenstein's reflections, the key interpretative thesis of this section borrow heavily form [Rodych, 2008b], [Rodych, 1997] and [Frascolla, 1994].

1.1 The Negative Claim: There are no mathematical propositions

1.1.1 The Claim

> Might we not do arithmetic without having the idea of uttering arithmetical propositions, and without ever being struck by the similarity between a multiplication and a proposition?
>
> Should we not shake our heads, though, when someone showed us a multiplication done wrong, as we do when someone tells us it is raining, if it is not raining? Yes; and here is a point of connexion. But we also make gestures to stop our dog, e.g., when he behaves as we do not wish.
>
> We are used to saying "2 times 2 is 4", and the verb 'is' makes this into a proposition, and apparently establishes a close kinship with everything that we call a proposition. Whereas it is a matter only of a very superficial relationship. [RFM App. III 4]

This is Wittgenstein in his mature phase and it is clear that in that context Wittgenstein's answer to the rhetorical question raised in the first paragraph is "Yes". The interesting fact is that the same conclusion was reached by Wittgenstein in the Tractatus. This provides ground for taking the idea that there are no mathematical propositions as a cornerstone of Wittgenstein's overall philosophical reflection on Mathematics. As Frascolla summarized:

> A mathematics without propositions is a mathematics without statements and without truth-values: thus, so many years later and in a so greatly changed overall theoretical context, the radical claim of the Tractatus that a mathematical proposition does not express a thought definitely prevails.
>
> [Frascolla, 1994, p. 127]

At this point, though, we need to get clear about the sense of the claim: what does it mean to suggest that there are no mathematical proposition? First, we need an account of what a proposition is.[1] One way to find such an account in Wittgenstein's writings goes back to the Tractatus, where Wittgenstein is concerned with the problem of finding out "the essence of the proposition" (see [NB, p. 39]). Let's start from the beginning: it is worth spending some time to outline Tractatus' framework and the account of mathematics that can be found there.

According to the Tractatus, a proposition is "an image of reality" [T 4.01]. This means that a proposition depicts a certain state of affairs in virtue of the relation between its constituents and those of the state of affair depicted: a proposition is made up of names and state of affairs are made up of objects;

[1] Just one caveat: Wittgenstein's german word for proposition is "Satz", which has a notorious double meaning: proposition or sentence. It is plausible to argue that in a Wittgensteinian framework there would be no place for the notion of proposition, conceived as an abstract object, so my discussion seems pointless: given that there are no propositions at all, it's trivial that there are no mathematical propositions. This is not the case, because the suggestion we are considering, as I will show, is a suggestion about the notion of truth and, given that propositions are taken as truth-bearers, it is relevant for the issue about the nature of propositions. If you like, you can take proposition here to mean your favourite truth bearer. Alternatively, you can take the claim to be conditional: even if the were propositions, there still would be no mathematical propositions.

names stand for objects and the relations between names represent relation between objects; the proposition is true if the objects stand in the configuration that is represented by the configuration of the names in the proposition; if this happens, the state of affairs (= configuration of objects) depicted obtains.

A key feature of state of affairs, according to the Tractatus, is their contingency: the idea of necessary facts is a contradiction-in-terms in the framework of Wittgenstein's first masterpiece.

My conjecture is that this is the route of the idea that there are no mathematical propositions.

In fact, it is possible to reconstruct a Tractarian argument for the conclusion that there are no mathematical propositions along these lines:

1. The general form of a proposition is: such and such is the case [T 4.5]
2. All happening and being so is accidental [6.41] (happening and being the case can be taken as synonymous).[2]

   from 1 and 2 follows that
3. If mathematical sentences expressed propositions, they could not express something necessary

   but of course:
4. Mathematical sentences express something necessary [T. 6.2331][3]

---

[2]In this dissertation I will be concerned with defending the idea that there are no mathematical facts. This can be seen (and was seen by Wittgenstein) as part of a case against the idea that there are necessary facts, be they mathematical or of another kind. Unfortunately, even if it was possible to show that there are no peculiar mathematical facts, the way towards a refutation of the claim that there are necessary facts would still be quite long.

In the contemporary scene, the most prominent cases for the claim that there are de re necessities are the cases of a posteriori (not a priori) necessity made famous by the work of Saul Kripke [1980]. The fact the water is H2O seems to require empirical enquiries in order to be discovered; at the same time, according to Kripke, the chemical composition of a liquid is an essential component of it, so that it is impossible for some liquid to be the same liquid as that we refer to with the world "water" and not be made of H2O. This means that it is necessary for water to be H2O. This necessity, though, cannot be accounted for a priori: it is not by investigating our conceptual apparatus that we come to discover but by engaging in an empirical investigation of the natural world. This kind of consideration can be used (and has been used) to argue that it makes perfect good sense to speak of cases of de re necessity: the source of this necessity is the way the world is, as opposed to the way our conceptual schemes are.

Kripkean examples just cited seems to be about the natural world. They seem to contradict the idea that the only necessity is logical necessity. The problem, though, seems to run deeper. A case has been made not to regard the idea of necessary facts as nonsense. Once the idea of necessary statements about the world is accepted, it is not so surprising to think that not just a posteriori necessity statements are about the world, but logical and mathematical as well. The problem is that "water is H2O" seems to be fact-stating, but "pi is the ratio of the circle" seems to be fact-stating too [Yablo, 1982, p. 879]. Moreover, Timothy Williamson has recently argued [Williamson, 2009] that a statement like "Mars has always been dry or not dry" should be taken at face value, as a statement about Mars, so that even an instance of the law of the excluded middle should be taken as expressing a feature of the world, though a very general one. Logical and mathematical statements, even if a priori, thus become substantial, in the sense of increasing our knowledge of the world. This shows that a complete defense of the claim that there are no necessary facts should take the kripkean counterexamples in serious consideration. Marconi [2010] explores the issue and I have nothing substantial to add to his analysis.

[3]In the constext of the Tractatus, to say that mathematical sentences "express" something necessary means that they try to describe what can only be shown, namely necessary features of the language and the world

From 3 and 4 it follows by modus tollens that:

5. Mathematical sentences do not express propositions

The contrast between the "propositions" of mathematics and logic on one side and those of science on the other is very sharp and can be summarized in this list:

Science

1. Reign of Truth
2. Proposition which express thoughts
3. Truth is a matter of comparison between proposition and reality [T 2.223]
4. Discoveries are possible
5. They say something
6. The signs denote
7. No A priori [T 2.223-2.225]

Logic and Mathematics

1. Reign of Necessity
2. Pseudopropositions that do not express a thought [T. 6.124], [T. 6.21]
3. Truth comes from the symbols alone
4. No real discoveries
5. Try to say what can only be shown
6. The signs do not denote
7. A priori [T 6.2331]

If it is true that both Logic and Mathematical propositions are very different from empirical one, the contrast in the latter case is even more radical: tautologies and contradictions are expressed by propositional signs that are at least syntactically well formed (see [T. 6.2] and [T.6.21]).

Now, the idea that mathematical discourse should be interpreted as stating the property and relations of some abstract entities seems prima facie very appealing.

It seems sensible, at this point, to ask what rationale Wittgenstein had to question this conception of the nature of mathematics. My conjecture is that Wittgenstein thought there to be a tension between the notion of Truth and Necessity.

1.1.2 Platonism and Necessity

I think that Wittgenstein was ruminating about a problem that any platonistic account of mathematical necessity must face. According to Platonism, Mathematics is the science that describes the mathematical world, like physics does with the physical one.

The analogy with physics is usually challenged by pointing out that the mathematical world, as conceived by the Platonist, is too different from the physical world: being abstract, it can't have causal relations with us, so that it is not clear how we can get knowledge about it and how mathematical truths apply to the physical world [Benacerraf, 1973]. Here Wittgenstein is making a different point, namely that the mathematical world of the Platonistic picture is too similar to the physical world. Given that the truths about the physical world are usually taken to be contingent, the realist cannot account for the source of their necessity. In Wright's words [Wright, 1980, p. 3]: "What sort of explanation are we going to be able to give of the necessity of pure mathematical truths, if it is merely a reflection of a feature of the domain which pure mathematics allegedly describes?".

Priest made the same point:

> [the necessity of mathematical statements] is a particularly difficult point for realism to cope with. Empirical statements which state the relationships between physical objects are contingent. But if realism is correct, mathematical assertions also state de facto relationships between real objects. Whence then derives their necessity? Of course it could claimed that necessity is sui generis to relations between abstract objects. However, this just labels the problem rather than explaining it.
> [Priest, 1983, p. 54]

In other words there is a tension between the alleged descriptive nature of mathematical sentences and their being necessary.

To put the matter slightly differently, consider that according to the TLP the meaning of a proposition is given by its truth conditions: given that, if mathematical sentences expressed propositions, they would express necessary propositions, this would mean that all mathematical sentences have the same truth conditions, therefore the same meaning, but this is really an implausible result (a problem familiar from the times of Frege).

Of course, even Wittgenstein is not completely insensitive to epistemological qualms about Platonism: in [RFM I 106-112] he discusses at length the difference between a sentence like "I believe that it will rain tomorrow" and "I believe that 13x13= 169"; and he certainly thought that his account of mathematics as a collection of rules fitted very well with the outcome of his reflection on certainty.

The important point is that Wittgenstein conceives a new line of attack to platonism when he stresses the fact that the conception of mathematics as a science describing a realm of abstract objects does not account for the necessity of its (alleged) truths. This is a point that Wittgenstein already made at the times of the Tractatus:

> The Theory of classes is completely superfluous in mathematics. This is connected with the fact that the generality required in mathematics is not an accidental generality [T 6.031]

And repeatedly stated:

> And if there is an infinite reality, then there is also contingency in the infinite. And so, for instance, also an infinite decimal that isn't given by a law [PR 143]

In his mature phase Wittgenstein thought to have found the solution of the problem of mathematical necessity in his conception of Normativity and the contrast between grammatical and empirical uses of sentences. Before turning to this, it is interesting to give a close look at his position during the intermediate years of his reflection

1.1.3 Two Puzzles about Necessity

The core point of the last section can also be seen in another perspective, as the claim that traditional forms of Platonism are not able to deal with two classical puzzles about Necessity.

The first puzzle has already been noticed: there seems to be a difference between claiming merely that something is so and so on the one hand and claiming that it must be so and so on the other hand. But what does entitle us to claim that something must be so and so and not merely that it is so and so? In the words of Sider:

> I can see that this colored thing is extended, and indeed that all colored things I have examined are extended, but where is the necessity, that colored things must be extended? Part of the puzzlement here is of course epistemic, and epistemic reasons for reductionism have already been mentioned. But there is a particularly metaphysical puzzlement here as well. In metaphysics one seeks an account of the world in intelligible terms, and there is something elusive about modal notions. Whether something is a certain way seems unproblematic, but that things might be otherwise, or must be as they are, seems to call out for explanation.
> [Sider, 2003, p. 198]

Platonism, prima facie, has difficulties answering this request for explanation: the picture of ideal world of abstract entities as truth-makers of our mathematical statements does not help to understand why the facts in this reign should be necessary. It seems that the Math world is just an hardened version of the empirical one. In many places Wittgenstein suggests that this is a misleading analogy:

> If we talk of a logical machinery, we are using the idea of a machinery to explain a certain thing happening in time. When we think of a logical machinery explaining logical necessity, then we have a peculiar idea of the parts of the logical machinery- an idea which makes logical necessity much more necessary than other kinds of necessity. If we were comparing the logical machinery with the machinery of a watch, one might say that the logical machinery is made of parts which cannot be bent. They are made of infinitely hard material-and so one gets an infinitely hard necessity. How can we justify this sort of idea?[LFM, p. 196]

> Similarly, if I say that there is no such thing as the super-rigidity of logic, the real point is to explain where this idea of super-rigidity comes from- to

> show that the idea of Super-rigidity doe snot come from the same source which the idea of rigidity comes from. The idea of rigidity comes from comparing things like butter and elastic with things like iron and steel. But the idea of super-rigidity comes from the interference of two pictures- like the idea of the super-inexorability of the law. First we have: "The law condemns", "The judge condemns". Then we are led by the parallel use of the pictures to a point where we are inclined to use a superlative. We have then to show the sources of this superlative, and that it doesn't come from the source the ordinary idea comes from. [LFM, p. 199]

A hint about how the puzzle should be solved, according to Wittgenstein, comes from this passage:

> Aren't you confusing the hardness of a rule with the hardness of a material?[RFM III 87]

The hardness of a rule is to be found in its normative role, which has nothing to do with the fact-stating role of empirical proposition. A rule doesn't depict any ideal state of affairs, but is part of the conceptual apparatus by which we describe the only kind of state of affairs, i.e. empirical states of affairs:

> What I am saying comes to this, that mathematics is normative. But 'norm' does not mean the same thing as 'ideal'. [RFM V 40]

Another typical puzzle has to do with the problem of necessary existence. There is a quite widespread intuition that logic should be completely free of any existential assumption. Another way to put the matter is to say that logically speaking an empty world is possible. That many philosophers are inclined to think like that is witnessed by the fact that they consider the question "Why is there something instead of nothing?" as one of the most interesting ones.

Kant tried to justify this intuition with an argument that aims to show that the denial of the existence of something could never turn out to be contradictory, since, in order to contradict herself, one has to say something which entails that an entity does fall under a concept and at the same time does not fall under it; but how could this be the case, if the only claim made is that an entity of some sort does not exist? In this case one does not predicate anything of anything, so, how could she contradict herself?

This is the reason why, according to some logicians, even classical first order logic, where the existence of at least an individual is assumed, would lack the purity required to a perfect logic. Wittgenstein seems to have shared this position, when, at the times of the Tractatus, he complained that the axiom of infinity contained in the Principia Mahtematica was not a logical one, given that an empty world is logically possible.

Everyone who accepts the conception of logic as free of any existential assumption will be presented with a dilemma when confronted with the apparent abundance of existence claims made in mathematical theories (like "There are infinitely many prime numbers"): either these claims do not really entail the existence of abstract entities, or mathematics cannot be reduced to logic.

One could think that for a friend of Wittgenstein's view both horns could be acceptable. But there is a problem with this proposal: Wittgenstein always maintained that the necessity of mathematical and logical statements was to be explained in a similar fashion. This is certainly the case in the framework of the Tractatus, where he held that "mathematics is a method of logic"[6.234] and that "there is only logical necessity"[6.375], so that the idea of attributing to mathematical statements some sort of metaphysical necessity, not reducible to the logical one is ruled out.

There has been some controversy among Wittgenstein's scholars about the issue whether he was a logicist at the times of the Tractatus (see Rodych [2008b] for references): it seems to me that the issue is in some sense irrelevant. If the main problem for Wittgenstein was to account for the problem of necessity, his alleged logicism can be seen as the thesis the both in mathematical and in logical proposition we are not dealing with the description of facts, but with the misleading and mislead attempt to show what cannot be expressed (the structure of the world, which is not a fact). Something very close to this holds for the later Wittgenstein as well: the common feature of mathematics and logic lies in their showing grammatical rules of our language, the features of our descriptive apparatus. The later Wittgenstein will make the point in this way:

> So much is true when it's said that mathematics is logic: its moves are from rules of our language to other rules of our language. And this gives its peculiar solidity, its unassailable position, set apart. (Mathematics deposited among the standard measures) [RFM I 165]

Closely connected with the puzzle of the necessary existence of mathematical objects there is a problem about the nature of this objects: not only they do necessarily exist, but they also possess all their properties necessary (essentially). This is very puzzling, given that with concrete objects usually it is the other way round: very few properties are essential to them. As Stephen Yablo put it:

> Not a whole lot is essential to me: my identity, my kind, my origins, consequences of these, and that is pretty much it. Of my intrinsic properties, it seems arguable that none are essential, or at least none specific enough to distinguish me from others of my kind. And, without getting into the question of whether existence is a property, it is certainly no part of my essence to exist. I have by contrast huge numbers of accidental properties, both intrinsic and extrinsic. Almost any property one would ordinarily think of is a property I could have existed without. So, if you are looking for an example of a thing whose "essence"(properties had essentially) is dwarfed by its "accense"(properties had accidentally), you couldn't do much better than me. Of course, you couldn't easily do much worse than me, either. Accense dwarfs essence for just about any old object you care to mention: mountain, donkey, cell phone, or what have you. Any old concrete object, I mean. Abstract objects, especially pure abstracta like 11 and the empty set, are a different story. I do not know what the intrinsic properties of the empty set are, but odds are that they are mostly essential. Pure sets are not the kind of thing we expect to go through intrinsic change between one world and another. [...] The pattern repeats itself when we turn to relational properties.
> [Yablo, 2002, p. 220]

Wittgenstein's account of Mathematics can be used to vindicate the intuition that mathematical statements, being necessary, are free from any ontological commitment. As I will show in Chapter 3, Wittgenstein's view of Mathematics can be usefully compared with some fictionalists approaches to mathematics, as they both maintain that our acceptance of mathematical claims does not commit us to an abstract ontology.

1.1.4 The Shift From the Tractatus to the Intermediate Phase: mathematical propositions vindicated

The idea that Necessity does not fit with Truth is in the mood of the Tractatus: Mathematical equations are attempts to express formal (i.e. necessary) features of the language (and reality), but they do not (cannot) succeed in doing so, because what is necessary can only be shown and not said.

During the intermediate phase of his career (1929-1934) Wittgenstein had a flirt with Verificationism and tried to make sense of the notion of "Mathematical Proposition".

He thought that the only meaningful mathematical sentences were the decidable ones. Call a sentence *decidable* if we have at hand a decision procedure for it, in the broad sense of a method that allows us to settle the truth value of the proposition in question. Consider for instance "58+67=125": the decision procedure is the usual mechanical technique to compute sums

His idea was to identify the sense of a proposition with the method to verify it. The sense of the proposition "58 + 67 = 125" (the way to verify it) is then definable in this way: apply the general procedure denoted by "+" to the numerals "58" and "67" and check the result.

According to this view, mathematical propositions are just one kind of propositions with some peculiar method of verification:

> "Every proposition says what is the case if it is true" [...] And with a mathematical proposition this "what is the case" must refer to the way in which it is to be proved [PR, 148]

This is an application to the case of mathematics of the Tractarian account of propositions:

> The proposition shows its sense. The propositions shows how things stand if it is true. And it says, that they do so stand [T 4.022]

According to this view, there is a gap between our understanding of the sense of the sentence and our knowledge of the truth value of the sentence: we can understand the sense of the sentence without knowing whether it is true.[4]

1.1.5 The Rule following considerations and the idea of Mathematical sentences as Rules of Grammar

But then, the rule following consideration came. The point of the rule-following consideration is that the mentioned way of conceiving the sense of a mathematical sentence is untenable.

To say it very roughly: there is no unique concept of what has to count as the correct application of a rule in a particular case, before our decision of taking a result as the correct one. There is a logical gap between a rule (a procedure) and its new applications: no matter how clearly the rule is formulated, no matter what was its past use, it is always compatible with different interpretations that yield different results (see Kripke [1982]).

Wittgenstein mature view was that every single computation is a new rule of grammar in disguise. The rule fixes what has to count as the correct application of a rule in a particular case:

[4] Wittgenstein stresses the importance of this point the Tractatus

> To say if one did anything other than write 110 after 100 one would not be following the rule is itself a rule. It is to say "This rule demands that one write 110". And this is a rule for the application of the general rule in the particular case [AWL, p. 133]

The point is that adopting "58+67=125" as a rule of grammar is a free decision from the logical point of view: in no way the meaning of our words (the sign "+" and the numerals), nor the speaker's intention, nor our past use, forced us to take it. In this case "it *looks* as if a ground for a decision were already there; and it has yet to be invented" [RFM V 9].[5]

> How is it decided what is the right step to take at any particular stage? The right step is the one that accords with the order "as it was meant". So when you gave the order "+2" you meant that he was to write 1002 after 1000? and did you also mean that he should write 1868 after 1866, and 1000036 after 100034, and so on, an infinite number of such propositions? No: what I meant was, that he should write the next but one number after every number that he wrote; and from this all those propositions follow in turn. But that is just what is in question: what, at any stage, does follow from that sentence. Or, again, what, at any stage we are to call "being in accord" with that sentence (and with the meaning you then put into the sentence?whatever that may have consisted in). It would almost be more correct to say, not that an intuition was needed at every stage, but that a new decision was needed at every stage. [PI 186]

> 'I have a particular concept of the rule. If in this sense one follows it, then from that number one can only arrive at this one'. This is a spontaneous decision [RFM VI 24]

> "But am I not compelled, then, to go the way I do in a chain of inferences?"- Compelled? After all I can presumably go as I choose! - "But if you want to remain in accord with the rules you must go this way"- Not at all, I call this "accord". "Then you have changed the meaning of the word 'accord', or the meaning of the rule." - No; - who says what "change" and "remaining the same" mean here? [RFM I 113]

> And *like this* (in "go on like this") is signified by a number, a value. For at *this* level the expression of the rule is explained by the value, not the value by the rule. For just where one says "But don't you see. . . ?", the rule is no use, it is what is explained, not what does the explaining [Z 301-2]

This is at odds with what we may call the "phenomenology" of proofs: the fact that we perceive proofs as binding. We always feel that we have no real choice in performing, for instance, a calculation. Wittgenstein replied that this feeling of compulsion has only anthropological and biological grounds: as a matter of facts we almost always yield the same results calculating, we feel that only one way of continuing a series, applying a rule, etc..., is the natural one. But that's simply a feature of how we are (of our form of life): a brute fact without logical explanations (see Frascolla [1994, pp. 140-141]).

[5]See [Dummett, 1959, p. 171] "There is nothing in our formulation of the axioms and of the rules of inferences, and nothing in our minds when we accepted these before the proof was given, which of itself shows whether we shall accept the proof or not; and hence there is nothing which forces us to accept the proof".

This is linked with Wittgenstein's conception of mathematical proofs: proofs are sequences of applications of rules of inference to theorems, but, according to the rule following considerations, every step, every new theorem, is the adoption of a new rule of grammar. A proof is the process by which a new rule is adopted in our language. The theorem proved didn't have a sense prior to its proof: it acquires a sense trough the proof. The proof convinces us to call something "the correct result". Acquiring a sense only by our decision to confer it a normative role (to treat it as a rule, see next paragraph), a mathematical sentence doesn't depict any state of affairs, not even the outcome of our calculation, given that there is no fact of the matter about what count as the correct application of the rules in our calculation.

In this sense, Wittgenstein eventually returned to the view expressed in the Tractatus, that the very idea of mathematical propositions should be called into question.

1.1.6 Dummett's full blooded Conventionalism and its critics

The conclusions about Wittgenstein's Rule Following Considerations we reached so far have much in common with the interpretation of Wittgenstein's Philosophy of Mathematics suggested by Michael Dummett in [Dummett, 1959] and still maintained in his most recent statements about this issue [Dummett, 1994].

This interpretation has been subject of two sort of criticism: one is that the view Dummett attributes to Wittgenstein is not the one he held and the second is that Wittgenstein did not hold this view because it is indefensible.

The point is that Dummett's Wittgenstein claims that at every step of a calculation (or of a proof) we are free to proceed in many different and incompatible ways. As we saw before, the obvious rejoinder to this view is that this goes completely against the way we proceed in calculating or deriving and that it does not account for our need of proof: if accepting or rejecting something as a theorem is just a matter of decision, why do we engage in this complex activity of proving theorems [Kreisel, 1958]?

Compelling as they may be, these objections do not show immediately that the position of the radical conventionalist is hopeless: the fact that we are free from a logical point of view of proceeding in many different ways without entering in conflict with our past usages, intentions, formulations of the rules etc..., does not mean that in performing a calculation we do not see ourselves as compelled, that there is more then one natural answer. The natural answer is the correct one and we do engage in a process of convincing the other members of our community that it is so, but in no way the rule pre-determined what the correct answer is.

It is worth to discuss this point at some length. The overarching problem can be briefly put like this:

> The problem of rule following may be formulated thus: what makes 1002 the result of the correct application of the rule "Add 2" to 1000? Wittgenstein's reply is that it is only our spontaneous decision, which is agreed upon by all those who have had a certain training and possess certain linguistic inclinations, that establishes it.
> [Frascolla, 1994, p. 135]

One could try to speak against this reading like this: true, according to Wittgenstein, a rule does not pre-determine all its (correct) applications to particular cases. But this does not mean that, confronted with the application of the rule to a particular case, there are different results which are compatible with the rule, so that it is up to us to decide which to choose as the right one. There are passages where Wittgenstein himself tries to maintain a part of the commonsensical way of looking at rules, namely that it is not up to us do decide which is the correct result of the application of the rule. Consider these passages (quoted also in [Rodych, 2000b, pp. 273 and following]):

> Then according to you everybody could continue the series as he like; and so infer anyhow!? In that case we shan't call it "continuing the series" [RFM I 116]
>
> "The rules compels 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. [RFM VII 27]

This kind of material should be handled with care. We must distinguish some claims actually made by Wittgenstein.

1. Between the rule and its applications, Wittgenstein repeatedly stresses, there is an internal relation: if everything could count as the correct result of the application of a rule, then there would be no rule [PI 202]. It is essential for a rule that, in any case in point, it can be applied correctly in just one way. At any stage, there is only one way we can act, if we want to respect the rule. This is not a fatal objection to the version of the community view we have presented. It is no part of this view to say that different result are compatible with correct following the rule. It is just the interpretation of the rule that is compatible with different results: from this fact, Wittgenstein draws the conclusion that following the rule does not consist in interpreting it.

    > "But how can a rule shew me what I have to do at this point? Whatever I do is, on some interpretation, in accord with the rule."- That is not what we ought to say, but rather: any interpretation still hangs in the air along with what it interprets, and cannot give it any support. Interpretations by themselves do not determine meaning. [PI 198]
    >
    > This was our paradox: no course of action could be determined by a rule, because every course of action can be made out to accord with the rule. The answer was: if everything can be made out to accord with the rule, then it can also be made out to conflict with it. And so there would be neither accord nor conflict here.
    >
    > It can be seen that there is a misunderstanding here from the mere fact that in the course of our argument we give one interpretation after another; as if each one contented us at least for a moment, until we thought of yet another standing behind it. What this shews is that there is a way of grasping a rule which is not an interpretation, but which is exhibited in what we call "obeying the rule" and "going against it" in actual cases. [PI 202]

This shows that nothing logically compels us to follow the rule as we do. There is no sense in which, if the community was to follow the rule differently, one could say that they are not being faithful to the meanings of their words. It is the communal practice that fixes the meaning of their words and what counts as a correct application of their rules.[6]

2. Another sort of criticism is that the view that collective agreement is the source of necessity is inherently implausible. Wittgenstein himself seems to be have been of this advice:

> "So you are saying that human agreement decides what is true and what is false?" It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life. [PI 124]
>
> And does this mean e.g. that the definition of "same" would be this: same is what all or most human beings with one voice take for the same? Of course not.
> For of course I don't make use of the agreement of human beings to affirm identity. [RFM VII 40]

I find the claim that these passages refute the community view contentious. It is true that Wittgenstein refuses the idea of taking the shared human opinion as a criterion for correctness, but he still claims that it is shared human practice, or shared form of life that determines what count as correct application of the rule in any given case:

> it has often been put in the form of an assertion that the truths of logic are determined by a consensus of opinions. Is this what I am saying? No. There is no opinion at all; it is not a question of opinion. They are determined by a consensus of action: a consensus of doing the same thing, reacting in the same way. There is a consensus but it is not a consensus of opinion. We all act the same way, walk the same way, count the same way. In counting we do not express opinions at all. [LFM 184]

3. There is still a problem with the communitarian view, at least for those, like Wittgenstein, who want take seriously some things we say in order to explain what a rule is (the grammar of the word "rule"). It seems to be part of our concept of rules to allow the possibility that a whole community, for instance, agrees on taking something as the correct result of an addition and still is mistaken, and can be corrected. Even a whole community can go wrong in applying a rule. I think that a friend of the community view can reply even to this charge. It is true that we allow for the possibility of correcting a mistake which passed unnoticed, so that what was universally acknowledged as the correct result now reveals not to be so. But the point is that this "revelation", according to Wittgenstein, would not count as

[6] See [Frascolla, 1994, p. 128]: "To Wittgenstein, the acknowledgment of necessary connections is a sort of creation, subject to conditioning of a pragmatic, or even biological, nature, but without constraints of a logical nature" and p. 120: "This picture, in which a total uniformity of behaviour from the anthropological point of view is a pendant of an absolute freedom from the logical point of view, is, in my opinion, the first component of Wittgenstein's conception of rules"

such if it were not ratified by the whole community. One can speak of there being a mistake only in so far as the mistake is acknowledged as such.

> "But mathematical truth is independent of whether human beings know it or not" Certainly, the propositions "Human beings believe that twice two is four" and "Twice two is four" do not mean the same.[. . .] the two propositions have entirely different uses.But what would this mean: "Even though everybody believed that twice two was five it would still be four"? For what would it be like for everybody to believe that?- Well, I could imagine, for instance, that people had a different calculus, or a technique which we should not call "calculating". But would it be wrong? (Is a coronation wrong? To beings different from ourselves it might look extremely odd.) Of course, in one sense mathematics is a branch of knowledge,but still it is also an activity. And 'false moves' can only exist as the exception. For if what we now call by that name became the rule, the game in which they were false moves would have been abrogated. [PI, Ilxi 227]

4. That said, it is true that there is a quietistic element in Wittgenstein's writings which consists in the refuse to give any answer (even the communitarian one) to the kind of question which are usually labeled as *constitutive questions*: "what makes 1002 the result of the correct application of the rule "Add 2" to 1000?", "what is the criterion we use to affirm that a rule has been correctly followed?"

   > For of course I don't make use of the agreement of human beings to affirm identity. What criterion do you use, then? None at all.
   > To use the word without a justification does not mean to use it wrongfully. [RFM VII 40]

   > Our mistake is to look for an explanation where we ought to look at what happens as a 'proto-phenomenon'. That is, where we ought to have said: this language-game is played [PI 654].

   The idea here is to conceive the task of Philosophy as that of resisting the temptation to try answer some kind of questions, which simply are not legitimate questions (they are not genuine problems). One way in which this noble task can be accomplished it so show that all purported answers to such questions (like the mythology of rules as rails to infinity) run into hard difficulties. The result of this therapeutic practice would substitute the insane attempt to answer the constitutive questions with that of describing our practice of rule following for what it is. To put it in a different way, the point of the rule following considerations would be that rule following cannot be reduced to interpreting or to grasping some mental content or whatever. As Wittgenstein was fond of quoting bishop Butler "Everything is what it is, and not another thing". I won't discuss whether this perspective is really tenable (see Wright [2007] about this issue), but for sure it is not an easy task to really understand it. Consider this example: according to Victor Rodych [2000b] Wittgenstein maintained both that:

   (a) The rule does not determine what the correct result of a new calculation is. Before we yield something as the correct result of a calculation, there is no correct result of that calculation

(b) We were not free of taking another result as the correct one: if we did elsewhere, we would have come into conflict with the rules

In a sense, it is certainly true that Wittgenstein would have subscribed even the latter claim: once the new rule constituted by the adoption of a result of a calculation as the correct one is in force, it gives a new criterion to check whether someone does correctly understand the general rule that is being applied in the new case. Once "125" has become (it wasn't before, as Rodych recognizes) the correct result of "58+67" we have a new way to see whether a pupil knows how to calculate, i.e. ask him to calculate this addition. If the pupil answer "129", he definitely comes into conflict with the rule. But this is just because the new calculation expands the rule: when we apply a rule, according to Wittgenstein, we also extend and explain it (see Pagin [1987] and Pagin [2002]).

But look at the situation before the new calculation has been made: the rule does not determine what is the correct result of the calculation (nor do our intension, mental states, verbal formulations ecc..); we are to determine what the correct result of the calculation is. But if our actions have the only effect of determining something (the correct result) that the rule does not determine, how could our actions come in conflict with the Rule?

I will return to discuss the rule following consideration in the final chapter, where the issue will be linked with the problem of the objectivity of mathematical claims.

1.2 The positive Claim: Mathematical sentences are Rules of Grammar

1.2.1 What is a rule of Grammar?

Wittgenstein's alternative picture is that mathematical sentences are rules of grammar in disguise. If you like labels, we can call it a normative conception of mathematics.

Roughly, this means that their function is to govern the use of mathematical notions in empirical propositions: to rule out some combinations of signs as meaningless and license some inferences between propositions. The meaning of a theorem like "The sum of the angles of a triangle is 180°" is to exclude as meaningless empirical sentences like: "I have correctly measured this triangle's angles and their sum turned out to be 179°". Geometrical theorems are not about an abstract ideal triangle; their role is rather to fix the meaning of our words, to show what count for us as a triangle. This explain why we can say that the sum of a triangle's angles must be 180°: we simply could not understand anyone claiming otherwise, because is part of the concept of triangle to have the sum of his angles equal to 180°.

To better understand the Claim, let's start with something that we are undoubtedly inclined to call a rule of grammar and then move to more controversial cases:

GR 1 In German the verb takes always the second position

GR 2 Every bachelor is an unmarried man?

GR 3 The chess-king moves of only one square per time??

GR 4 Nothing is darker than black???

GR 5 The sum of a triangle's internal angles is 180° ????

GR 6 18+37= 55 ???????

There is a point of connection: grammatical rules rule out as incorrect some combinations of signs:

- GR 1 rules out as incorrect a sentence like "Das ich verstehe"
- GR 2 rules out as senseless an empirical sentence like "I met a bachelor and his wife in Willy Brandt Platz"
- GR 6 "rules out as senseless any assertion that an individual X, by correctly applying the usual algorithm for addition to the two numerals '37' and '18', has got any numeral other than '55'. The obtainment of that numeral is entrusted with the role of a criterion for the correct performance of the relevant sign transformations: as a consequence, of any person who made the addition of 37 and 18 and got a result other than 55, we are authorized to say that he must have miscalculated" [Frascolla, 2004, p. 5]

The pivotal point is that in calling mathematical sentences grammatical rules, Wittgenstein wanted to stress:

1. Their normative role: they tell us what must be the case
2. Give a purely linguistic account of normativity (the old necessity): roughly, that it is a matter of the convention ruling our use of words

> "the sum of the angles of a triangle is 180 degrees" means that if it doesn't appear to be 180 degrees when they are measured, I will assume there has been a mistake in the measurement. So the proposition is a postulate about the method of describing facts, and therefore a postulate of syntax [PG, p. 320]
>
> For the proposition whose truth, as I say, is proved here, is a geometrical proposition - a proposition of grammar concerning the transformations of signs. It might for example be said: it is proved that it makes sense to say that someone has got the signs . . . according to these rules from . . . and . . .; but no sense etc. etc. [RFM III 38]

1.2.2 From Necessity to normativity

Wittgenstein's idea was that when we realize that mathematical sentences lack any descriptive content, the fact that mathematical sentences strikes us as something that could not be false (the problem of necessity), is no more a problem.

The only way to make sense of the notion of necessity is to interpret it as normativity: rules of grammar are standard to evaluate the competence of other speakers and correct them.

This allows us to account for the hardness of the 'must' or 'can't' involved in allegedly necessary statement, without committing us to the obscure notion of necessary facts. When I say that "Nothing can be darker than black", I'm just pointing your attention to a grammatical rule, not to some sort of hidden fact. They have a distinctive role, different form that of the empirical propositions:

> If it is not supposed to be an empirical proposition that the rule leads from 4 to 5, then this, the result, must be taken as the criterion for one's having gone by the rule. Thus the truth of the proposition that 4 + 1 makes 5 is, so to speak, overdetermined. Overdetermined by this, that the result of the operation is defined to be the criterion that this operation has been carried out. The proposition rests on one too many feet to be an empirical proposition. It will be used as a determination of the concept "applying the operation + 1 to 4". For we now have a new way of judging whether someone has followed the rule: Hence 4 + 1 = 5 is now itself a rule, by which we judge proceedings. [RFM VI 16]

1.2.3 Wittgenstein and Formalism

If Formalism is the view that Mathematics is not a science describing a domain of abstract or mental objects, but just a rule governed manipulation of signs, Wittgenstein is definitely a formalist, despite his own view about this issue: when Wittgenstein denies to be a formalist, he does so just because he has a very strange notion of formalism in mind.

According to Wittgenstein:

> Frege was right in this criticism [of formalism]. Only he did not see the other, justified side of formalism, that the symbols of mathematics, although they are not signs, lack a meaning. For Frege the alternative was this: either we deal with strokes of ink on paper or these strokes of ink are signs of something and their meaning is what they go proxy for. The game of chess shows that these alternatives are wrongly conceived? although it is not the wooden chessmen we are dealing with, these figures do not go proxy for anything, they have no meaning in Frege's sense. There is still a third possibility, the signs can be used the way they are in the game. [WVC p. 105]

The problem with Wittgenstein's interpretation of formalism is that a formalist is not necessarily committed to the view that Mathematics consist in describing the features of some concrete tokens, nor that it is about the structure of a system of types as in Hilbert's conception of Metamathematics; instead, the conception of Mathematics Wittgenstein endorses in this passage, according to which Mathematics is a rule governed sign game that can be aptly described by the analogy with chess, is much in the spirit of traditional formalism. I agree with [Rodych, 1997, pp. 198] that Wittgenstein's third way is just a version of formalism. So, we can label Wittgenstein a quasi-formalist, as Frascolla [1994] does, if we are interested in distinguishing his position from that of Hilbert, but probably the best strategy to get clear about this (partly terminological) issue is to follow [Rodych, 1997, pp. 196-97] in distinguishing various kind of formalism and understand the Wittgenstein's relation to them:

> Strong Formalism (SF): A mathematical calculus is defined by its accepted or stipulated propositions (e.g., axioms) and rules of operation. Mathematics is syntactical, not semantical: the meaningfulness of propositions within

> a calculus is an entirely intrasystemic matter. A mathematical calculus may be invented as an uninterpreted formalism, or it may result from the axiomatization of a meaningful language. If, however, a mathematical calculus has a semantic interpretation or an extrasystemic application, it is inessential, for a calculus is essentially a sign-game, its signs and propositions do not refer to or designate extramathematical objects or truths.
>
> Weak Formalism (WF): A mathematical calculus is a formal calculus in the sense of SF, but a formal calculus is a mathematical calculus only if it has been given an extrasystemic application to a real world domain.

Rodych [1997] has an interesting story to tell about how Wittgenstein's oscillation between the Weak Formalism of the Tractatus and RFM and the Strong Formalism of his middle years' writings is related to the his views about the importance of extra mathematical application of mathematics.

Before turning to it, let me note another telling feature of Wittgenstein's formalism: traditional forms of formalism do not have a lot to say about the relation of logical (in the sense of proof-theoretical, syntactical) consequence. In Hilbert's case, it is clear that once the axioms and the rules of inference of a formal system have been settled, the set of the theorems, i.e. of the provable sentences of the system, is completely determined. This is why it makes always sense, according to this view, to ask whether a system is contradictory or not, i.e. if there is a contradiction belonging to the set of the system's theorems. This position is incompatible with Wittgenstein's position: as it is noted in [Rodych, 2000b, p. 274] according to Wittgenstein an unmade calculation, an unproved theorem, etc..., simply do not exist, and this is due to the fact that rules do not determine their correct application.[7]

Finally, it's worth noting a point of connection between Wittgenstein position and Hilbert's Formalism: the idea that mathematical sentence are rules of grammar, that they contribute in defining the meaning of the concept involved in their formulation is reminiscent of (and a radicalization of) Hilbert's view of axioms as implicit definitions of primitive concept.[8]

1.2.4 The role of applications

In the Tractatus, the importance of the applicability of mathematics outside pure formal systems is clearly stated:

> Indeed in real life a mathematical proposition is never what we want. Rather, we make use of mathematical propositions only in inferences from propositions that do not belong to mathematics to others that likewise do not belong to mathematics. (In philosophy the question "What do we actually use this word for?" repeatedly leads to valuable insights.)
> [T 6.221]

During the intermediate phase, Wittgenstein partially changed his mind on this point, endorsing a stronger version of formalism, according to which Mathematics is the purely intra-systemical activity of sign transformations starting from the axioms of a system and following the rules of inference:

[7] See also Dummett [1959, p. 168] on this point
[8] Shanker [1988] makes the same point.

> One always has an aversion to giving arithmetic a foundation by saying something about its application. It appears firmly enough grounded on itself. And that of course derives from the fact that arithmetic is its own application. Every mathematical calculation is an application of itself and only as such does it have a sense. [PR, p. 109]

> It seems to me that you can develop arithmetic completely autonomously and its application takes care of itself since wherever it?s applicable we may also apply it. [PR, p. 109]

This position has a very counter-intuitive consequence: for instance, that it is not possible to prove the same theorem in different systems, given that the meaning of any mathematical proposition depends on which system it belongs.

> There cannot be two independent proofs of one mathematical proposition [PR, p. 184]

In the later phase, this aspect is present in Wittgenstein's discussion of Gödel's Theorem: the idea that a sentence expressible in the language of PM (Principia Mathematica, also called by Wittgenstein "Russell's System") and not provable could nonetheless be shown to be true is an anathema for Wittgenstein, because a sentence has a meaning just as long as it belongs to a system and if a string of symbols that it is not provable in one system is provable in another one, it simply has two different meanings in the two cases

> "But may there not be true propositions which are written in this symbolism, but are not provable in Russell's system?" 'true propositions', hence propositions which are true in another system, i.e. can rightly be asserted in another game. [...] a proposition which cannot be proved in Russell's system is "true" or "false" in a different sense from a proposition of *Principia Mathematica* [RFM App.III 7]

Things changed in the later years, for quite an ironic reason: Wittgenstein was repelled by Set Theory, according to him one of the most relevant sources of philosophical confusion and particularly misleading in overlooking the importance of the applications of a mathematical statement for its *meaningfulness*:

> These considerations may lead us to say that [Cantor's Theorem] That is to say: we can make the considerations lead us to that. Or: we can say this and give this as our reason. But if we do say it "what are we to do next?" In what practice is this proposition anchored? It is for the time being a piece of mathematical architecture which hangs in the air, and looks as if it were, let us say, an architrave, but not supported by anything and supporting nothing. ([19], 2:35)

The same theme appears in Wittgenstein's critical remarks about Goedel's Theorem:

> The non-philosophical aspect in Goedel's paper lies in this, that he does not see the relation between Mathematics and its application. In this respect, he has the sticky concepts of most mathematicians. [MS 124, p. 115]

> Here one needs to remember that the propositions of logic are so constructed as to have no application as information in practice. So it could very well

> be said that they were not propositions at all; and one's writing them down at all stands in need of justification. Now if we append to these "propositions" a further sentence-like structure of another kind, then we are all the more in the dark about what kind of application this system of sign- combinations is supposed to have; for the mere ring of a sentence is not enough to give these connexions of signs any meaning. [RFM, III 20]

But at the same time he realized that as a formal calculus Set Theory was perfectly legitimate according to his own standard of the time. The solution to this tension was found in the idea that extra-mathematical application of a calculus represent a necessary condition for its meaningfulness.

> concepts which occur in 'necessary' propositions must also occur and have a meaning in non necessary ones [RFM V 42]

> I want to say: it is essential to mathematics that its signs are also employed in mufti. It is the use outside mathematics, and so the meaning of the signs, that makes the sign-game into mathematics. Just as it is not logical inference either, for me to make a change from one formation to another (say from some arrangement of chairs to another) if these arrangements have not a linguistic function apart from this transformation. [RFM V 2]

The fact that applications provide a criterion to establish the meaning of the sentence allow for the possibility of different proofs in different theorems of the very same proposition, thus resolving another tension emerged in the intermediate phase.

> Of course it would be nonsense to say that one proposition cannot have two proofs- for we do say just that [RFM III 58]
> It all depends what settles the sense of a proposition, what we chose to say settles its sense. The use of the signs must settle it; but what do we count as the use? That these proofs prove the same proposition means, e.g.: both demonstrate it as a suitable instrument for the same purpose. And the purpose is an allusion to something outside mathematics. [RFM VII 10]

There is still one problem left, that has to do with Frege's reason of dissatisfaction with formalism: if mathematics is just a collection of arbitrary rules to manipulate signs, if it is not a science, that means a true description of something, how could it be so useful in describing the physical world?

I think that one very interesting reply to this challenge is that of Yablo's figuralism: even if mathematical statements are not literally true, they increase our expressive powers, so that thanks to them we are able to state truths that would have been difficult, if not impossible, to express otherwise. In the second part, after presenting Yablo's views, I will discuss the issue of how much of this position could have been endorsed by Wittgenstein.

Before turning to a comparison between Wittgenstein and the contemporary debate in the Philosophy of Mathematics, I think it could be useful to give a summary of Wittgenstein's position with respect to the most global issues of the most recent debates in this field.

1.3 Classical Problems dissolved?

1.3.1 Existence of mathematical object vs. Objectivity of Mathematics

In the contemporary debate about the Philosophy of Mathematics, two issues are candidates for the title of central problem of the discipline. The first is the ontological issue whether numbers and other mathematical objects do really exist. The other is the so-called issue of the “Objectivity of Mathematics”, where the problem is to make sense of the idea that the correctness of mathematical statements is (or is not) an objective matter.

The distinction between the two issues is traditionally (see Shapiro [2005b, p. 6] and Linnebo [2009]) spelled out along these lines:

THE QUESTION OF ONTOLOGICAL REALISM Do Mathematical objects REALLY (mind- language-culture-independently) exist?

THE QUESTION OF SEMANTICAL REALISM (OBJECTIVITY) Are our mathematical statements true or false independently of our capacity to decide which is the case?

Realism in ontology and realism in truth value are the view according to which the correct answer to the first and the second question respectively is “yes” (anti-realism in ontology and in truth value are defined in analogous fashion).

Given that, after Frege, numbers are usually taken to be abstract objects, the ontological question is usually seen more as the problem of finding out whether a certain kind of objects exist (Do numbers, functions, and other abstract mathematical entities exist?) than as the problem of finding out which is the nature of some kind of object, whose existence is taken for granted (what are the numbers, assuming that they exist?). This question is usually seen as a chapter of analytical ontology and, after Quine, the problem is usually formulated in terms of ONTOLOGICAL COMMITMENT: does our scientific practice require quantification over mathematical entities? (That means: must entities of this kind exist in order for our scientific theories to be true?).

It is quite natural to expect Wittgenstein to be suspicious towards question of such a kind. To a certain cast of mind, these questions seem to arise from conceptual confusions in the use of phrases like “existence”, “requires”, “entities”. Despite that, the approach of the Neo-Fregean school (see Wright and Hale [2001], MacBride 2003) finds in Wittgenstein’s writing the indications to solve the existential problem (or to understand and assess it better).

In addition, despite his life long idea that in Philosophy one should not argue for a controversial thesis but rather collect reminders which can help in not forgetting the obvious, in many places Wittgenstein seems to be denying the existence of mathematical entities, or to regard it as a completely unnecessary assumption: this is precisely the goal of contemporary nominalist programs in the Philosophy of Mathematics, so that a comparison with these approaches seems worth to be considered.

The issue about the objectivity of mathematics is closer to a classical theme quite familiar to Wittgenstein, i.e. the opposition between those who see mathematics as an act of invention and those who see it as an act of discovery.[9]

[9]This is true even if one regards the contemporary formulation as an improvement, as

The idea of Semantical Realism is that questions like whether the Continuum Hypothesis is true or false have a determinate answer, despite the fact that it is independent from our theory of Sets, makes sense only if one thinks that mathematics is the attempt to correctly describe facts of a certain kind: it does not matter if the relevant facts are taken to be about some abstract entities or about modal notions.

Moreover, the problem of objectivity is sometimes put in terms of finding a CRITERION of CORRECTNESS for mathematical statements, so that the question becomes: What does it mean for a mathematical statement to be correct?(see Chapter Four). For instance, one classical question, which belongs to this problem, is whether correctness could be identified with formal derivability, so that the set of correct claims expressible in the language of some theory (e.g. in the case of Arithmetic, the language of PA) could be identified with just the theorems of some formal theory (e.g. PA or an extension of it).

This formulation touches a problem that it is central for Wittgenstein as well: as we have seen a central problem for Wittgenstein was to make sense of our tendency to speak, in the empirical as well as in the mathematical case, of TRUE PROPOSITIONS. He saw a possible solution [Rodych, 2008a] to this problem in the idea that the only similarity between the two lies in the fact that in both cases we have at hand criteria to judge the correctness of the statements (which means, the correctness of a certain action), but that the criteria are deeply different.

> Let us suppose to have initially stated: "Mathematical propositions can be true or false". The only clear thing of this claim is that we assert some mathematical propositions and deny other. (LFM, beginning of lesson XXV)

In the empirical case, correctness is being judged by comparison with reality, whereas in the mathematical case there is no reality to compare the statement with (the one and only reality being empirical), so that the issue of correctness receives a completely different meaning in this case. The only way, according to Wittgenstein, to make sense of this is look at the mathematical practice, which should show that to assert something correctly (= being justified?) means just to state an axiom or to give a proof of it. This seems to be the moral of [RFM, App. III, paragraph 4], which, tellingly, begins a discussion of the issue of undecidable sentences. Chapter four is a critical examination of this position.

1.3.2 Epistemology

If mathematical sentences do not express propositions, there is no problem about how we can know mathematical truths. Mathematical sentences are rules of grammar, invented by human beings, not truths about an abstract world as in the Platonist picture.

Consider Hartry Field's [1989] epistemological charge to Platonism (probably the strongest argument of this kind present in literature): it claims that Platonism is not able to account for the reliability of our mathematical knowledge (see Liggins [2006] for a defence of Field's argument and Linnebo [2006] for criticism).

Wittgenstein's standpoint offers a pleasant solution to these problems: far from being something in need to explanation, the reliability of mathematics is a

Wright [1980, Chapter 1] argues.

precondition of its practice. In order to achieve the rank of rules, of paradigm of correct application of the mathematical concepts, proofs have to meet the requirements of being perspicuous and surveyable. (see Frascolla [1994, Chapter 3].

1.3.3 Applicability

Discussing the issue whether Arithmetic should be better conceived as a game or as a science, Frege [Geach and Black, 1960] argued for the latter option, on the ground that otherwise it would be impossible to account for the applicability of Arithmetic. Were Arithmetic just a game, with arbitrary rules and with no ambition to depict the true nature of anything, how could it turn out to be so useful in natural sciences? Arithmetic must then be a science and its claims are the most general truths about the world. This sounds a little bit puzzling, as Stephen Yablo remarked:

> The surprising thing is that the same phenomenon of applicability that Frege cites in support of a scientific interpretation has also been seen as the primary obstacle to such an interpretation. Arithmetic qua science is a deductively organized description of sui generis objects with no connection to the natural world. Why should objects like that be so useful in natural science = the theory of the natural world? This is an instance of what Eugene Wigner famously called "the unreasonable effectiveness of mathematics". Applicability thus plays a curious double role in debates about the status of arithmetic, and indeed mathematics more generally. Sometimes it appears as a datum, and then the question is, what lessons are to be drawn from it? Other times it appears as a puzzle,and the question is, what explains it, how does it work?
>
> Yablo [2005, p. 89]

What's the link between the truth of "2+2=4" and "if you eat 2 oranges and two apples you eat 4 fruits" ? As in the previous case, here Wittgenstein is again 'turning the examination round'. Arithmetical sentences are not about some sort of abstract objects, rather they are rules of grammar and an essential feature of them is to fix the use of concepts employed in empirical sentences. In Chapter 3 I will connect this idea with some central themes from Stephen's Yablo version of Fictionalism.

Chapter 2

Wittgenstein and Mathematical Knowledge

In this Chapter I discuss the relevance of some remarks by Wittgenstein for the contemporary debate about the Epistemology of Mathematics.

2.1 The Problem

One of the most classical challenges to mathematical platonism has been epistemological in character. Mathematical objects are intangible, unobservable, in general causally isolated from us and causally inert: they do not bear any causal relation to us, neither active nor passive. Moreover, they do not leave any traces of their existence, contrary to what happens with other unobservables like theoretical entities of physics.

This has lead many people to worry about the problem of how it is possible for us to obtain any knowledge of such a kind of ghostly objects. The underlying thought is that mathematical objects, even if they existed, could play no role in any explanation of our mathematical knowledge. But if it necessary (and hence possible) to account for mathematical knowledge without postulating mathematical objects, then there is no reason to believe to their existence and it is therefore reasonable to renounce to an ontology of abstracts objects and embrace nominalism.

In contemporary philosophy of mathematics, there have been many attempts to use epistemological considerations for making a case for nominalism. Benacerraf [1973] is usually taken to have been the first place where considerations of such a kind were developed, despite the fact that Benacerraf is explicitly non concerned, in that paper, with making a case for nominalism, but rather with presenting a problem for the mathematical platonist.

2.1.1 Benacerraf's Challenge

Benacerraf sketches an argument against platonism of this kind:

P1 In order to know propositions about some kind of entities, we need to be in causal contact with them[1]

P2 We cannot be in causal contact with any mathematical entity

C We cannot know propositions about mathematical entities

The structure of the argument is clearly this (see Liggins [2006] and [2010]):

P1 In order to have knowledge about X, condition C must apply

P2 Condition C does not apply in the case of mathematical entities, therefore:

C we have no knowledge about mathematical entities

[P1] states a necessary condition for knowledge and in this sense can be seen as a part of a theory of knowledge. More specifically, [P1] is the core of the so called causal theories of knowledge, quite popular a the times of Benacerraf. Nowadays epistemologists are much more suspicious about such theories. One reason for this skepticism is that there seems to be counterexamples to the theories, like our knowledge of the future (causation does not proceed backwards) or of universal truths (truths concerning regions of space time inaccessible to us). It can be contented that whether this examples really succeed in refuting any theory of knowledge that could deserve to be called causal in a broad sense, but in any case they pose serious problems.[2]

Another reason for skepticism is more structural. It has to do with the very form of the argument, not just with the particular choice of one kind of causal theory or another:

> That form of argument requires very strong premises about the sort of entity that can be known about, or that can plausibly exist; and these premises can always be exposed to ridicule by proposing the numbers themselves as paradigm-case counterexamples Yablo [2001, p. 87][3]

2.1.2 Field's Challenge

The fact that Benacerraf's argument hinges upon a causal theory of knowledge of course does not suffice to show that every epistemological argument against platonism needs to rest on such a theory. Recently, Harty Field [1989] has put forward an argument which, according to him, "does not depend on any theory

[1]"For X to know that S is true requires some causal relation to obtain between X and the referents of the names, predicates, and quantifiers of S" Benacerraf [1973, p. 22]

[2]For a discussion about the tenability of causal theories of knowledge see Liggins [2010]. Cheyne [2001] propose a modified theory of knowledge in which causal contact is required only for knowledge about the existence of something; in order to know that X exist one needs only to be in contact which some events to which they participate. Colyvan [2001] presents counterexamples to these modified versions as well. Another way to weaken the requirement (Goldman [1967]) is to say that what is required is just an appropriate causal connection between the knower and the object of knowledge: for instance, according to Goldman it is enough to share a common cause with entities featuring in future events in order to have knowledge about them. Or one could just require as a necessary condition for knowledge about some entities to be in principle possible for us to have causal contact with them (as Ivan Kasa once suggested me).

[3]One further reason for dissatisfaction with causal theories of knowledge has to do with the fact that they seem not to be able to solve the problem they were designed to solve, namely the so called Gettier problem, see Burgess and Rosen [1997, p. 36-37]

of knowledge in the sense in which the causal theory is a theory of knowledge: That is, it does not depend on any assumption about necessary and sufficient conditions for knowledge" [Field, 1989, pp. 232-33]; moreover, the argument can even "be put without use of the term of art know' " [Field, 1989, p. 230].

The argument appeals to a rather different principle: that "we should view with suspicion any claim to know facts about a certain domain if we believe it impossible to explain the reliability of our beliefs about that domain"[Field, 1989, pp. 232-233].

The structure of Field's argument is something like that [Hale, 1994]:

1. Our mathematical beliefs are reliable
2. We should account for Reliability
3. Platonism is unable to account for Reliability

C Platonism is an untenable position

The phenomenon of reliability to which Fields refers here is the fact that mathematician tend to have true mathematical beliefs in a systematic way: even if sometimes they do make mistake, most of times this can be recognized when a purported proof is checked by the community. What needs to be accounted for is the fact that, for mathematical sentences p:

Reliability I If mathematicians accept p, then p [Field, 1989, p. 230]

Field of course recognizes that the platonist has some story to tell about the way in which mathematicians acquire their mathematical beliefs: every mathematical theorem can be considered as a logical consequence of a very limited number of axioms, either the axioms of the relevant theory or, given the widespread conviction that almost all mathematical theories can be reduced to set theory, directly the axioms of set theory. Field admits that the logical reliability of the mathematician is not at stake here, so we can assume that if a mathematician believes that p, than p follows from the axioms of set theory (or from the axiom of the relevant mathematical theory for the sentence in question). Even though Field grants this, he still thinks it is not enough to account for Reliability tout court. For this story has nothing to say about the mathematician's reliability in choosing the axioms of set theory (or the relevant theories). We still need an account of the fact that:

Reliability II If mathematicians accept p as an axiom, then p

Field does even accept that the logical competence of the mathematician can account for the fact that if they accept a set of axioms, the resulting theory is consistent. But this, according to him, does not account for Reliability two yet. We want an account that is able to explain the correlation between the mathematician's belief and their being true, not just the correlation between the mathematician's beliefs and their consistency. Field reasons by analogy: it would never suffice to show the reliability of our physical beliefs to point out that they are consistent.

> There is a gap between the consistency of an axiomatic theory and its truth. In the case of Physics we can presumably fill the gap at least in

> sketch: we can sketch the route whereby the assumed properties of, say, the electromagnetic field lead to various observable physical phenomena, and thereby affect our perceptual beliefs, and thereby affect our beliefs about the electromagnetic field. But nothing remotely analogous to this seems possible in the case of mathematics. Field [1989].

This may look like unfair to the platonist: why should one suppose that the fact that there is no causal account of reliability precludes there being a non causal account? After all, this is what any platonist would have reasonably expected. Field does not barrel the possibility of a non-causal account of knowledge, but rather express a kind of skepticism about it:

> There seems prima facie to be a difficulty in principle in explaining the regularity. The problem arises in part from the fact that mathematical entities as the [platonic realist] conceives them, do not causally interact with mathematicians, or indeed with anything else. This means we cannot explain the mathematicians beliefs and utterances on the basis of the mathematical facts being causally involved in the production of those beliefs and utterances; or on the basis of the beliefs or utterances causally producing the mathematical facts; or on the basis of some common cause producing both.Perhaps then some sort of non-causal explanation of the correlation is possible? Perhaps; but it is very hard to see what this supposed non-causal explanation could be. Recall that on the usual platonist picture [i.e. platonic realism], mathematical objects are supposed to be mind- and language-independent; they are supposed to bear no spatiotemporal relations to anything, etc. The problem is that the claims that the [platonic realist] makes about mathematical objects appears to rule out any reasonable strategy for explaining the systematic correlation in question. Field [1989, pp. 230-232]

It is worth pointing out, following Linnebo [2006] and [2010] that the spirit in which Field makes his considerations about the epistemological problems of Platonism is much more that of a challenge than that of a knock down argument against Platonism. If one looks at the reconstruction just sketched of the argument, one sees that it essentially relies on a premise (3) that is bound to be controversial. How can Field justify the claim that an account of reliability is in principle unavailable to platonists?

Divers and Miller [1999, pp. 278-79] present and attempt to interpret Field as reasoning like that:

1. Platonism is committed to the acausality and mind-independence of mathematical objects

2. Any causal explanation of reliability is incompatible with the a-causality of mathematical objects

3. Any non-causal explanation of reliability is incompatible with the mind-independence of mathematical objects

4. Any explanation of reliability must be causal or non-causal

   therefore:

5. There is no explanation of reliability that is compatible with both the a-causality and mind-independence of mathematical objects

Therefore

C There is no explanation of reliability that is compatible with Platonism[4]

But I think that the most plausible way to read the passage from Field just quoted is that he is simply pessimistic about the possibility of a non-causal account, even in he does not consider the possibility of such an account forbidden. As a matter of fact, Field paid a lot of attention to recent attempt from Neo-Fregeans to meet the epistemological challenge and didn't simply rule them out invoking some sort of trascendental argument against Platonist epistemology. Field point is simply a request for an account of a certain kind of systematic correlation, that between mathematician's belief and mathematical facts, combined with a certain scepticism (pessimism) about the possibility for Platonist to provide one.[5] I think that Field is right in claiming that the phenomenon of reliability is "so striking to demand explanation" and for very commonsensical reasons.[6]

The most reasonable thing to do, in order to evaluate the challenge, is to analyse the attempts that have been made to actually meet it.

2.1.3 Replies to Field (I)

One way to answer Field's challenge is to try to show that the kind of request he is advancing is in some sense too demanding, and so misguided. The point of this strategy is not to take too seriously Field's challenge, or alternatively to argue that there is a very trivial explanation of the phenomenon of reliability at hand. Consider this hilarious passage from David Lewis "Credo"[7]:

> If there are no classes [or numbers], then our mathematics textbooks are works of fiction, full of false 'theorems'. Renouncing classes [or numbers] means rejecting mathematics. That will not do. Mathematics is an established, going concern. Philosophy is as shaky as can be. To reject mathematics for philosophical reasons would be absurd. If we philosophers are sorely puzzled by the [entities] that constitute mathematical reality, that's our problem. We shouldn't expect mathematics to go away to make our life easier. Even if we reject mathematics gently, explaining how it can be a most useful fiction, 'good without being true" we still reject it, and that's still absurd. Even if we hold onto some mutilated fragments of

[4]"Field's point is not simply, echoing Benacerraf, that no causal account of reliability will be available to the platonist. Rather, Field conceives what is potentially a far more powerful anti- platonist dialectic when he suggests that not only has the platonist no resource to any account of reliability that is causal in character, but that she has no resource to explanation that is non-causal in character either"Divers and Miller [1999, p. 278]

[5]David Liggins has suggested to me the possibility of seeing Field's argument as relying on a sort of pessimistic induction: all the attempts made by platonist to account for reliability have failed, so it is reasonable to think future ones will fail too.Kasa [2010] argues that the force of Field's argument does not hinge upon the choice to read it as a challenge and not as a knock down argument.

[6]Field [2001, pp. 324-25] illustrates the principle at work here with the following example: suppose that John and Judy run into each other for 52 consecutive Sundays in various locations, without there being any explanation of this (they don't know each other, nobody sistematically plans them to meet, etc.). Wouldn't this strike us as odd? For ananlogous reasons, to postulate reliability without accounting for it would strike us as weird.

[7]See Linnebo [2006] for more on the boring explanation.

> mathematics that can be reconstructed without classes, if we reject the bulk of mathematics that's still absurd.
>
> That's not an argument, I know. But I laugh to think how presumptuous it would be to reject mathematics for philosophical reasons. How would you like to go and tell the mathematicians that they must change their ways, and abjure countless errors, now that philosophy has discovered that there are no classes? Will you tell them, with a straight face, to follow philosophical argument wherever it leads? If they challenge your credentials, will you boast of philosophy's other great discoveries: that motion is impossible, that a being than which no greater can be conceived cannot be conceived not to exist, that it is unthinkable that anything exists outside the mind, that time is unreal, that no theory has ever been made at all probable by evidence (but on the other hand that an empirically ideal theory can't possibly be false), that it is a wide-open scientific question whether anyone has ever believed anything, and so on, and on ad nauseam? Not me!
>
> Lewis [1993, pp. 14-15]

Here we can find the root of one naturalist argument against Field's objection of this form:

1. mathematical beliefs are justified according to scientific standards
2. Philosophical arguments are not powerful enough to overrule scientific standards of justification (due to different credentials of the two disciplines, which is made quite clear by looking at the track record of success of them)

C Field's argument does not undermine the justification of our mathematical beliefs

Burgess and Rosen's anti-nominalist argument[2005] can be seen as an instance of an argument of such a form. According to them, Field's argument is just a form of anti-naturalistic skepticism. It is a form of skepticism because it casts doubts on the justification of our (mathematical beliefs) and it is anti-naturalistic because, according to current scientific standards, those beliefs are justified.

The problem with those readings, as Liggins [2006] and [2010] has pointed out, is that Field's target is a philosophical and not a scientific theory. Field's argument, per se, just calls into question an account of mathematical knowledge which is not able to explain the reliability of our mathematical beliefs.[8] This falls short of disbelieving this mathematical beliefs. It is not prima facie contradictory to maintain that almost all the mathematician's assertions are true and at the same time reject Platonism.[9]

[8] As Liggins [2006, footnote 5] notes, the fact that Field actually claims that we should disbelieve mathematical claims is not relevant for the present point. He holds this for reasons which are quite independent from his epistemological argument against platonism.

[9] It is worth mentioning another point borrowed from Liggins [2006, last section]. According to Burgess and Rosen, Field's argument would run like that: "Mathematicians (to whom empirical scientists defer in these matters) generally obtain their existence theorems by deduction from previously established results, which ultimately depend on existence axioms. But such a deduction provides a justification of the theorems only if the axioms are themselves justified. (This is not a "theory" of justification, but just a platitude.) Now has anyone shown that the kind of process by the axioms were arrived at is a reliable one, tending to lead to true axioms? Have the axioms been justified?" [Burgess and Rosen, 2005, p. 522]. At this point, they compare it with a parallel argument against perception: "Scientists generally

Another kind of objection to Field's argument goes back to Lewis [1986, pp. 111-112]. According to Lewis, to ask for an account of the reliability of our mathematical beliefs is to ask for something in principle impossible. To account for the reliability of a class of beliefs about entities of the kind F is just to ask which kind of beliefs we would have had, if relevant fact about the F were different. Thus, for instance, it is possible to account for the reliability of our beliefs about electrons because it makes sense to wonder wether our beliefs would have been the same, had the electrons behaved differently. But it makes no sense to ask whether our beliefs about the number 17 would have been different, had the number 17 not existed, because the existence of number 17 is a matter of necessity, and nothing can depend counterfactually upon something which is necessarily true.

Apart of some doubts about the claim that we cannot judge in an interesting sense claims of the form "If a certain mathematical proposition P were false, then..."[10] the main problem of this objection is that the request for the account of reliability can be freed from any formulation using modal idioms: the point is just to account for an actual correlation between the mathematician's beliefs and what is mathematically true, not to judge whether it would be maintained in counterfactual situations.

2.1.4 Replies to Field (II)

For those contemporary Philosophers who do not simply dismiss Field's challenge as misguided, two main strategies have been pursued: an appeal to intuition or a broadly holistic stance in which confirmation of mathematical theories is seen as continuous with the confirmation of empirical theories in which they are embedded.

The appeal to intuition is notoriously associated with the name of Kurt Gödel and a most quoted passage about this topic is the following:

> The objects of transfinite set theory ... clearly do not belong to the physical world .... But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e. in mathematical intuition, than in sense perception, which

derive their results from ordinary perceptual judgments (about their observational instruments and experimental apparatus). But such a derivation provides a justification only if ordinary perceptual judgments are themselves justified. (This is not a "theory" of justification, but just a platitude.) Now has anyone shown that the kind of process by ordinary perceptual judgments are arrived at is a reliable one, tending to lead to true judgments? Have ordinary perceptual judgments been justified?"

Liggins [2006, last section] points out the parallel is misleading: a better one would be with an argument that proposes to reject a certain Theory of the formation of perceptual beliefs on the ground that this theory would not be able to account for the reliability of our mathematical beliefs.

[10] "It is doubtless true that nothing sensible can be said about how things would be different if there were no number 17; that is largely because the antecedent of this counterfactual gives us no hints as to what alternative mathematics is to be regarded as true in the counterfactual situation in question. If one changes the example to "Nothing sensible can be said about how things would be different if the axiom of choice were false", it seems wrong: if the axiom of choice were false, the cardinals wouldn't be linearly ordered, the Banach-Tarski theorem would fail and so forth." Field [1989, pp. 237-38]. See Berto [2009a] for more on how to meaningfully interpret conditional with an impossible antecedent.

> induces us to build up physical theories and to expect that future sense perceptions will agree with them. Gödel [1983, pp. 483-84]

The classical charge to this proposal is that of guilt by association: believers in numbers are asked how they can have knowledge of such mysterious entities, and they appeal to the faculty of mathematical intuition; what if believers in paranormal phenomena, challenged to provide evidence of the existence of such events, were to reply that the evidence is given by their extra-sensorial perception? There have been in recent times serious attempt to vindicate the idea that mathematical intuitions play a prominent role in mathematical knowledge (see Maddy [1990] and Parsons [1980]), but the consensus seems to be that they are still unsatisfactory (see Hale and Wright [2002].

The strategy which have been mostly pursued, in order to reply to epistemological challenges to platonism, is that of Quine: the idea is that we know a posteriori that there are mathematical entities, because of the success of our scientific theories in which the existence of numbers is assumed. The success of a theory is considered by Quine a "pragmatic" criterion for its truth, and his holistic conception of meaning views the confirmation of a theory as a global matter: if mathematical physics deals quite good with experience, this is evidence for the truth of both his mathematical and non-mathematical assumptions (for more on Quine's argument for Platonism, see Chapter 3).

> Existence statements in this philosophical vein do admit of evidence, in the sense that we can have reasons, and essentially scientific reasons, for including numbers ... in the range of the values of our variables [i.e. for asserting things which entail the existence of numbers or classes]. ... Numbers and classes are favoured by the power and facility which they contribute to theoretical physics and other systematic discourse about nature.
>
> Quine [1969, pp. 97-8]

Both strategies would have been subjected to strong criticism by Wittgenstein. In particular it is very telling to look closely to the material one can find in Wittgenstein for a critic of Quine's idea of a posteriori knowledge of mathematical truths, and how this is linked with Wittgenstein's positive solution of the epistemological puzzle we are confronting with.

2.2 And Wittgenstein

Wittgenstein was notoriously against the idea that appeals to intuition could be a viable solution to the epistemological problems raised by mathematical Platonism:

> When it is held that logic is true, it is always held at the same time that it is not an experiential science: the propositions of logic are not in agreement or disagreement with particular experiences. But although everyone agrees that the propositions of logic are not verified in a laboratory, or by the five senses, people say that they are recognized by the intellect to be true. This is the idea that the intellect is some sort of sense, in the same way that seeing or hearing is a sense; it is the idea that by means of our intellect we look into a certain realm, and there see the propositions of logic to be true. (Frege talked of a realm of reality which does not act on the

> senses.) This makes logic into the physics of the intellectual realm. In philosophical discussions, you continually get someone saying, “I see this directly by inspection” No one knows what to say in reply. But if you have a nose at all, you will smell that there is something queer about saying you recognize truth by inspection. What is the answer if someone says, “I see immediately that (say) 2 +2 =4?” O r that he is immediately aware of the truth of the law of contradiction? What should we say? Are w e to take it lying down? It seems unanswerable; for how can you contradict such a person without calling him a liar? It is as if you asked him what colour he sees and he said “I see yellow."- What can you say?[LFM, pp. 172-173]

His opposition to this idea seems to have been grounded in the fact that intuition is actually immaterial for the way in which we judge whether someone masters the relevant mathematical concepts. This can be seen as an argument parallel to the (alleged) argument against the conceivability of a private language:

> Saying of logic that it is self-evident, meaning it makes a particular impression, doesn't help us at all. For one might reply, “If it is self-evident to you, perhaps it's not self-evident to someone else"-thus suggesting that his statement is a psychological one. Or we might ask, “What's interesting about your statement?"-thus suggesting the same thing. So if we want to see in what sense the propositions of logic are true, what should we look for?-Ask what sort of application they have, how they are used. [LFM, p. 174]

> What one means by “intuition” is that one knows something immediately which other only know after long experience or after calculation [...] And a man knows anatomy by intuition if he can pass the exam without studying-If we all knew by intuition and by intuition alone, this isn't what we could possibly call intuition. [...] The real point is that whether he knows it or not is simply a question of whether he does it as we taught him; it's not a question of intuition at all. [LFM, p. 30]

At the same time, the idea of an empirical justification of mathematical claims seemed to it as much misguided as the appeal to intuition. Wittgenstein's reason of dissatisfaction with Quine's Holist theory of justification resemble in part a well-known criticism of Quine's position by Charles Parsons [1980]. According to Parsons, what is lost in Quine's account is the obviousness of mathematical knowledge: how it is possible to maintain that the justification we have to accept a mathematical claim like “2+2=4” depends on the success of our whole scientific enterprise? There seems to be a big difference between the way we test mathematical claims and the way we test highly controversial physical hypothesis. And it is certainly telling that usually mathematics is a background theory for all competing physical theories and that there was no case in which the failure of a theory is imputed to some of the mathematical assumptions it incorporates.[11]

[11] Of course Quine could try to account for this by pointing to the fact that his holism is compatible with idea that some parts of our web of beliefs are harder to be given up than others. Still this is not the way the situation prima facie strikes us “. . . . there is the apparent contrast between logico-mathematical truths and others that the former are a priori, the latter a posteriori; the former have “the character of an inward necessity” in Kant's phrase, the latter do not. Viewed behavioristically and without reference to a metaphysical system, this contrast retains reality as a contrast between more or less firmly accepted statements; and it

It is worth highlighting that in the realist framework this difference has been called into question. Gödel famously argued for the adoption of something like the method of inference to the best explanation in mathematics:

> Secondly, however, even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its "success". Success here means fruitfulness in consequences, in particular "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs. [...] There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems [...] that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory.
>
> Gödel [1983, p. 477]

Wittgenstein's criticism to that view is twofolded. For the one side, he doubts that it is even intelligible to hold that a mathematical proposition is true without being presented with a proof of it. When one speaks, as Field does, about mathematical beliefs, Wittgenstein would have been much concerned with pointing out the differences between those beliefs and ordinary empirical beliefs.

> Professor Hardy in an article on 'Mathematical Proof" in Mind says that one can believe a mathematical proposition, as opposed to knowing it. The point is: what is the relation between that unfounded belief and the proved proposition that it must recur?[LFM, p. 123]
>
> For I want to say: "One can only see that 13X13=169, and even that one can't believe. And one can -more or less blindly- accept a rule". And what am I doing if I do this? I am drawing a line between the calculation with its result [...] and an experiment with its outcome [RFM I 109]

This is another way of stating the difference between genuine propositions and mathematical ones. From the times of the Tractatus, the hallmark of genuine propositions is the possibility of separating their sense and their truth value, in the sense that one can understand the sense of a proposition without knowing its truth value. With mathematical propositions this feature breaks down. (See Rodych [2008a])

On the other side, the difference between believing in a mathematical proposition and an empirical one provides also the materials for a reply to Field request to account for the reliability of our beliefs about mathematical axioms would have been that:

> Something is an axiom 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 III 5]

obtains antecedently to any post facto fashioning of conventions. There are statements which we choose to surrender last, if at all, in the course of revamping our sciences in the face of new discoveries; and among these there are some which we will not surrender at all, so basic are they to our whole conceptual scheme. Among the latter are to be counted the so-called truths of logic and mathematics, regardless of what further we may have to say about their status in the course of a subsequent philosophy." Quine [1936, p.26]

The certainty we have of mathematical claims is rooted in their normative role, and in this sense it has no need of justification. They are part of the conceptual scheme by which we depict reality and for this reason they are not subjected to test against reality. A rule needs no confirmation, not in the sense that it is "obviously true" (see the critique of mathematical intuition), but in the sense that the question of its validity, as long as it works as a rule, does not arise:

> If you say: "I believe that castling takes place in such and such a way", then you are not believing a rule of chess, but believing e.g. that a rule of chess runs like that [RFM I 3]
> What I am saying comes to this, that mathematics is normative. But 'norm' does not mean the same thing as 'ideal'. [RFM V 40]

Another approach that can be interestingly contrasted with that of Wittgenstein is Mark Balaguer's [1995, 1998] full blooded Platonism. This version of Platonism is explicitly designed to furnish a solution to epistemological worries of the kind advanced by Field. The ground idea of this view is that, for mathematical theories, consistency suffices for truth, so that the reliability of our mathematical beliefs can be completely explained in terms of our reliability concerning logical matters, like the ability to recognize the consistency of some set of axioms.[12]

The idea that consistency is not a sufficient condition for truth is based on misleading analogy between mathematical and physical theories. The reason why two different mathematical theories, both internally consistent but not compatible between them (because one entails some claim A, where the other entails the negation of A), cannot be both true about the physical world is simply that there is just one empirical world, which cannot render true both theories. With mathematical theories the situation is wholly different, according to Balaguer's full blooded platonism: for each consistent mathematical theory there is a mathematical universe which satisfies the axioms of the theory and if two consistent theories are incompatible (take ZFC + the Continuum Hypothesis and ZFC + the negation of CH), this just means that they are true of different universes.

Balaguer's proposal has met some serious criticism (see Restall [2003]), but its interest lies in the fact that it is a modern version of a quite old idea: that of viewing the axioms of a mathematical theory as implicit definitions of the primitives terms of the language of the theory. The view is typically associated with the name of Hilbert, but is quite in spirit with Wittgenstein geometrical conception of the axioms (see Chapter 1) and with his holistic ideas about the meaning of mathematical terms (that the meaning of mathematical terms is their role in the proof of the sentences in which they figure). Probably Wittgenstein

[12] Field illustrated his challenge with the this example: consider the case of some Napalese village, which is completely remote to us, but such that we still have reliable beliefs about its daily happenings. Would this not look rather mysterious? Balaguer notes that this is so just because not all possible Napalese villages do in fact exist, so that it is not enough, in order to show that our beliefs about them to be reliable, to show that they are consistent (possible, for Balaguer, means logically possible, i.e. consistently describable). But "[I]f all possible Nepalese villages existed, then I could have knowledge of these villages, even without any access to them. To attain such knowledge, I would merely have to dream up a possible Nepalese village. For on the assumption that all possible Nepalese villages exist, it would follow that the village I have imagined exists and that my beliefs about this village correspond to the facts about it." Balaguer [1998, p. 49]

would just have noted that the ontological counterpart of mathematical theories postulated by . Using the analogy with chess, one could view Balaguer's view as the attempt to interpret chess pieces as standing for some kind of entities, which have a structurally identical behaviour as that of chess figure: of course one can engage in such an activity, but it is difficult to see the point in it.

> it is not possible to appeal to the meaning [Bedeutung] of the signs in mathematics,? because it is only mathematics that gives them their meaning [Bedeutung] [RFM V 16]

2.3 The problem of consistency

The idea that mathematical propositions are rules of grammar seems incomplete at least in one respect. In order to be useful, it seems that mathematical rules must be consistent, because from contradictory rules everything would follow. This means that even in Wittgenstein's framework a a proof of consistency of our mathematical systems would be needed. This causes two problem:

1. The emergence of a foundational worry of the kind that Wittgenstein's philosophy of mathematics was supposed to dismiss.

2. The alleged informative nature of the consistency claim. We do not want to just rule out contradiction by fiat. We want a proof that they do not logically follow.

It seems vital to Wittgenstein's proposal to find a way to discharge this objection. This shows a connection between Wittgenstein's view about contradictions and what we have taken to be his pivotal thesis about the nature of mathematics, namely the grammatical view of mathematical propositions.

From an historical point of view it is interesting to note that a critique quite similar to the one just sketched against Wittgenstein grammatical view was developed by Kurt Gödel against Carnap conventional view of mathematical statements [Gödel, 1995]. According to Gödel's reconstruction, Carnap held, with many positivist (among which figures Wittgenstein) that mathematics is just "syntax of language", i.e. a set of conventions regulating the use of our (mathematical) sings. What Gödel suggests is that a proof that this set of rules are consistent is needed, otherwise we would not have any warrant that mathematics is devoid of empirical content.

A contradictory theory is not devoided of empirical content, according to Gödel, because, given the law of ex falso quodlibet, everything would follow from a contradiction, in the sense that every well formed formula belonging to the language of the theory would be a theorem of the theory. As long as a mathematical contradictory theory is embedded in a more comprehensive theory, whose language contains also predicate for concrete objects, this in turn means that a contradictory physical-mathematical theory would have empirical claims as consequences. This is quite in line with a problem Alan Turing pointed out to Wittgenstein, according to the reconstruction of LFM[13] :

[13]The objection has been pressed by Charles Chihara in more recent times, see Chihara [1977])

> Turing: The sort of case which I had in mind was the case where you have a logical system, a system of calculations, which you use in order to build bridges. You give this system to your clerks and they build a bridge with it and the bridge falls down. You then find a contradiction in the system.-Or suppose that one had two systems, one of which has always in the past been used satisfactorily for building bridges. Then the other system is used and the bridge falls down. When the two systems are then compared, it is found that the results which they give do not agree. [LFM, p. 212]

It has long been pointed out (Marconi [1984], Priest [2004], Tennant [2008], Berto [2009b]) that the rule of ex falso quodlibet, which is valid in both classical and intuitionistic logics, doesn't hold in some deviant logics called paraconsistent logic, in which inconsistency (the derivability of a sentence of the form A & not A) does not imply triviality (the derivability of every formula). Some observation made by Wittgenstein seems certainly to anticipate the spirit of paraconsistent logic, particularly their philosophy of not making a drama of contradictions ("The superstitious dread and veneraition of mathematicians in face of contradictions"[RFM, III 17]).

Still, the lack of interest displayed by Wittgenstein for reforming logic is in my view telling. Instead of trying to changed the logic in order to avoid ex falso quodlibet, he just limited to point out that we are not forced do draw any claim from a contradiction (it is up to us). It is also interesting that Wittgenstein seems to come close to accept Gödel's point that a proof of consistency, as far as it is just a theorem of some mathematical theory, cannot dispel the skeptical fears which leads people to look for it. It would a piece of mathematics like many others, and in this sense it could ground nothing. This is part and parcel with the idea that Metamathematic is just more mathematics. The first phenomenon can be explained by recollecting one of the feature of Wittgenstein's reflection about the meaningfulness of mathematical problems from the middle period: algorithmic decidability as a test for meaningfulness. This criterion is applied to the problem of consistency as well: as long as "hidden contradictions" cannot be detected the question whether they are there simply lacks a sense (lacking a criterion of decision). At the same time, Wittgenstein's polemic with the mechanical conception of rule following comes to play here as well (see for instance [RFM III 83, 87] and [RFM VII 11]). The consequences of our theory are not already there before somebody draws them. Logic doesn't force us and this is the reason, in Wittgenstein's perspective, not to fear contradiction:

> One may say, "From a contradiction everything would follow." The reply to that is: Well then, don't draw any conclusions from a contradiction; make that a rule. You might put it: There is always time to deal with a contradiction when we get to it. When we get to it, shouldn't we simply say, "This is no use-and we won't draw any conclusions from it"? [LFM, p. 209]

If Wittgenstein's position on meaningfulness, rule following and modality are adopted, undesired derivations of empirical results from contradictions would then become just errors of calculation, something from which there is no assurance.

> "Up to now a good angel has preserved us from going this way" Well, what more do you want? One might say, I believe: a good angel will always be necessary, whatever you do. [RFM VII 16]

Chapter 3

Wittgenstein on the existence of Mathematical Objects

The Ontological Question in the Philosophy of Mathematics ("Are there abstract objects as numbers, sets, functions, etc.?") belongs to a family of questions whose philosophical reputation has been seriously called into question. As Stpehen Yablo [1998, p. 230] noted, question like whether there really are "such entities as the number nineteen, the property of roundness, the chance that it will rain, the month of April, the city of Chicago, and the Language Spanish" strike people with a certain cast of mind as "naïve to the point of comicality". Yablo [1998, p. 231]calls these people *Quizzical*: their attitude about ontological questions is to "doubt there is anything to find" and to be 'inclined to shrug the question off".

Wittgenstein, of course, seems to share this attitude, but only in part: his point seems to be to try to SHOW "how little sense we have succeeded in giving" [Folyd and Putnam, 2000, p. 624]to those questions. This is a good thing, because it is by no means clear that ontological question cannot be taken seriously. In fact, they have been taken seriously: the party of the *Curious*, using Yablo's phrase, is anxious to find an answer to the ontological question, which means of course that they think they attach a determinate sense to it; and this is the dominant party in contemporary Analytic Philosophy, so that one cannot ignore the reason that led to make this approach so widespread. In the following I will explore those reasons, showing some subtleties of the approach made famous by Quine to the ontological problems, in particular his capacity to respond of some superficial reason for skepticism about it that seems to have been shared by Wittgenstein. After this, I will comment on some of the problems that the quinean approach does in any case gives raise to and in the second part I will try to read some indications of Wittgenstein as a proposal for the solution of those problems.

3.1 Language and Reality

The ontological question about Mathematics is supposed to be a question about the furniture of Reality. Wittgenstein's main concern, instead, seems to have always been more about the correct understanding of how our language works and his interest in the Philosophy of Mathematics seems to be triggered by the fact that mathematical discourse seems prone to generate misunderstandings. Is there an overlap between these two kinds of investigation? On one side (Ontology) we have Reality, on the other (Wittgenstein) we have Language.The problem, then, becomes to find it out whether there is a link between Language and Reality, between what we say and what there is. These questions seem certainly to be relevant for the ontological problem in the Philosophy of Mathematics, because the problem is precisely to understand whether, in order to make sense of our mathematical discourses, we have to postulate the existence of some kind of abstract entities.

3.1.1 Meaning and Referring

If one is interested in this kind of question, the beginning of The PHILOSOPHICAL INVESTIGATIONS is the natural point to start with. After a famous quote from Augustine, Wittgenstein makes some interesting remarks:

> These words, it seems to me, give a particular picture of the essence of human language. It is this: the individual words in language name objects-sentences are combinations of such names. — In this picture of language we find the roots of the following idea: Every word has a meaning. This meaning is correlated with the word. It is the object for which the word stands. Augustine does not speak of there being any difference between kinds of words. If you describe the learning of the language in this way, you are, I believe, thinking primarily of nouns like 'table', 'chair', 'bread', and of people's names, and secondarily of the names of certain actions and properties; and of the remaining kind of words as something that will take care of itself.

"The remaining kind of words" includes naturally number-words like the numerals and mathematical terms in general. This hints at a possible relevance of Wittgenstein's ideas about language (his rejection of certain picture of meaning, namely the REFERENTIAL picture of meaning just sketched) and his reflection on Mathematics. In a discussion in the Lectures on The Foundations of Mathematics, Wittgenstein says something that seems to go in this direction, linking the confusions that arise in the Philosophy of Mathematics to the referential picture of meaning and its tendency to reduce all semantics relation to just one, denotation:

> What kind of misunderstandings am I talking about? They arise from our tendency to assimilate to each other expressions which have very different functions in the language. We use the word 'number' in all sorts of different cases, guided by a certain analogy. We try to talk of very different thing by means of the same schema. This is partly a matter of economy; and, like primitive people, we are much more inclined to say, "all these things, though looking different, are really the same" than we are to say, "all these things, though looking the same, are really different". Hence I will have to stress the differences between things, where ordinarily the similarities are stressed, though this, too, can lead to misunderstandings. [LFM, p. 15]

Later on, Wittgenstein seems to give materials to draw a connection between his critique of the REFERENTIAL picture of meaning, and his rejection of a the ontological side of Platonism, i.e. the idea that mathematics describes a realm of abstract objects.

> Suppose we said, "A reality corresponds to the word 'two'." –Should we say this or not? It might mean almost everything. "A reality corresponds to the word 'perhaps'."–Does one, or not? You MIGHT say so; but nobody would. – Or to "or", or to "and". It is unclear what reality we should say corresponds here? We have certain words such that are such that if we were asked, "What is the reality which corresponds?", we should all point to the same thing- for example, "sofa", "green", etc. But "perhaps", "and", "or", "two", "plus" are quite different. [LFM, pp. 248-49]

The connection is this: in order for words to have meaning they need not refer to something, so it is possible for our mathematical terms to have meaning without referring to something. The idea that Meaningfulness has much more to do with usage than with denotation is explicitly connected with the rejection of abstract objects:

> The point is this. We *can* explain the *use* of the words "two", "three" and so on. But if we were asked to explain what the reality is which corresponds to "two", we should not know what to say.

Wittgenstein repeatedly stresses that the idea that a word can have a meaning only by referring to something is at the root of many philosophical confusion:

> The questions "What is a length?", "What is meaning?", "What is the number one?" etc., produce in us a mental cramp. We feel that we can't point to anything in reply to them and yet ought to point to something. (We are up against one of the great sources of philosophical bewilderment: a substantive makes us look for a thing that corresponds to it). [LB, p. 1]

> The meaning of a name is not the thing we point to when we give an ostensive definition of the name; that is, it is not the bearer of the name - The expression "The bearer of the name N' " is synonymous with the name "N". The expression can be used in place of the name. "The bearer of the name 'N' is sick" means "N is sick". We don't say: The meaning of 'N' is sick. The name doesn't lose its meaning if its bearer ceases to exist (if he dies, say). [PG, pp. 63-64]

So far, so good: but we haven't yet reached even the sketch of an argument against Platonism. What we do have at hand, instead, is a refutation of an argument FOR Platonism, albeit quite a silly one: Number-words have a meaning, so they refer to something, say the numbers; therefore, there is something to which number-words refer, namely the numbers, therefore numbers exist. Of course, this is a bad argument FOR Platonism, and Wittgenstein's considerations point out why it is a bad argument, but the road to a cogent argument AGAINST Platonism is still quite long.

3.1.2 Wittgenstein and Quine on Language and Reality

To get an idea of why these (very plausible) considerations of Wittgenstein about the relation between 'meaning' and 'reference' do not provide by themselves the materials for an argument against Platonism, it is instructive to recall that considerations very close to that of Wittgenstein have been made by the most famous contemporary advocate of Platonism, and they are placed in a paper in which he sketches the route to makes sense of the ontological questions in general, and even outlines the bases to construct an argument FOR Platonism.

The Philosopher in question is Quine, who in his seminal paper "On What there is" [Quine, 1980b] notes that a word can be meaningful without naming something, because "there is a gulf between MEANING and NAMING". In the course of a discussion on nominalism about universals, i.e. the position according to which there are no universal properties as the redness , but just individuals as this red house or this red pullover, Quine notes (p.11) that the opponent of this position

> cannot argue that predicates such as 'red' or 'is red', which we all concur in using, must be regarded as names each of a single universal entity in order they to be meaningful at all. For we have seen that being a name of something is a much more special feature than being meaningful.

Quine [1976a, p.204] makes the same point and almost seems to echo Wittgenstein:

> Why should 'fish', or 'aquatic' or 'similar' be put on a par with names such as 'Chicago', and 'Truman' and 'Partheon'? Many words are admissible in significant sentences without claiming to name; witness 'the' and 'of' and 'sake' and 'kilter'. Why not 'fish' and 'aquatic' ad 'similar'?

The problem is that, after recognizing that the connection between language and reality cannot be drawn rigidly, resting on the false idea that a word must refer to something in order to be meaningful, Quine makes a further step. He still recognizes that there is a connection between what we say and what there is, in the sense that there is a criterion to establish what must be there in order for what we say to be true:

> Up to now I have argued that we can use singular terms significantly in sentences without presupposing that there are the entities which those terms purport to name. I have argued further that we can use general terms, for example, predicates, without conceding them to be names of abstract entities. [..] Does nothing we may say commit us to the assumption of universals or other entities which we may find unwelcome? I have already suggested a negative answer to this question, in speaking of bound variables, or variables of quantification, in connection with Russell's theory of descriptions. We can very easily involve ourselves in ontological commitments by saying, for example, that there is something (bound variable) which red houses and sunsets have in common; or that there is something which is a prime number larger than a million. But, this is, essentially, the only way we can involve ourselves in ontological commitments: by our use of bound variables. To be assumed as an entity is, purely and simply, to be reckoned as the value of a variable. In terms of the categories of traditional grammar, this amounts roughly to saying that to be is to be in the range of reference of a pronoun. Pronouns are the basic media of reference; nouns might better have been named propronouns. The variables of quantification, 'something','nothing', 'everything', range over our whole ontology, whatever it may be; and we are convicted of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations true.
>
> Quine [1980b, p. 12]

Here Quine has simply defined what it means for somebody to commit herself to entities of some kind: to quantify over those entities. This does not mean that ontological commitments can be read off from the surface of what we say: matters are more complicated, as we shall see discussing the issue of paraphrase in the next section. In any case, there is another element we need to consider. The ontological issue we are considering is whether numbers really exist or not. We are told by Quine that to quantify over numbers it to acknowledge or presuppose the existence of numbers: this simply doesn't seem to answer our question. One thing is our ontological commitments, another is what there really is: what is the connection?

3.1.3 Quine's strategy (outline)

Quine seems to recognize that to keep track of our ontological commitment is just the first step in the process of attempting to solve an ontological controversy:

> Now how are we to adjudicate among rival ontologies? Certainly the answer is not provided by the semantical formula 'To be is to be the value of a variable'; this formula serves rather, conversely, in testing the conformity of a given remark or doctrine to a prior ontological standard. We look to bound variables in connection with ontology not in order to know what there is, but in order to know what a given remark or doctrine, ours or someone else's, says there is; and this much is quite properly a problem involving language. But what there is is another question.
>
> Quine [1980b, pp. 15-16]

Soon after acknowledging this, he makes this remark:

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from the considerations which determine a reasonable construction of the whole.
>
> Quine [1980b, pp. 16-17]

The idea is that science is an attempt to make sense of our past sense experiences and predict and control future ones. In order to do that, we elaborate some theories, which means, for Quine, sets of assertions. Between these assertions figure naturally also statements, like "there are some particles smaller than the atoms" or "there are prime numbers greater than a million", which seem natural to translate in formal language with a sentence in which a variable is bound by an existential quantifier. These assertions marks the ontological commitments, or ontological presuppositions, of the theory in question. They tell us what must be there in order for the theory to be true. Quine's idea is that as soon as we can choose between rival theories applying scientific standards, we can also choose between rival ontologies, applying to the very same standard: indeed, the only way to choose whether to affirm that there are thing of the kind F or to deny it is to check whether our best scientific theory is committed to the entities in question. If the problem is to establish whether numbers exist, we already have the elements to see what reply Quine could give: if "we commit ourselves to numbers saying that there are prime numbers greater than one million", then, given that we just say it, we are committed to numbers and, if we are not thinking to be smarter than scientists, we cannot give up the commitments. Quine's strategy can is well summarized in [Liggins, 2008, p. 117] in this way:

> When we know how to read off the existential implications of sentences in canonical notation, we have a naturalistic way of answering ontological questions, giving entities a place in our ontology for "essentially scientific reasons" Quine [1969, p. 97]. We paraphrase our scientific theories into canonical notation; then we look to see what the paraphrases quantify over. Quine assumes that we should believe the paraphrases of well confirmed empirical theories, and everything that they entail.

The important thing to note, as Liggins points out, is that before looking for the ontological commitments of our theories, we must formulate them in what Quine calls "canonical notation", which is essentially the language of first-order logic. This of course has to be done in order to permit ontological scrutiny, that means for a strictly philosophical reason. It is better to spend some words to make clear why the passage of paraphrasing our theories into canonical notation is so important.

3.1.4 The problem of Paraphrase

It is important to stress that the question whether something has to be acknowledge as one of the theory's ontological presuppositions is a complex one. In "On What there is" Quine tells us that the use of variables bound by an existential quantifier is the mark of ontological commitment. Variables and existential quantifiers, though, belongs to formal languages (Quine has first-order languages in mind). The problem is that our theories are not originally formulated in this idiom, but rather in NATURAL language. Already in "On What there is", Quine notices that the use in *natural* language of expressions which could be rendered in formal language by the use of the existential quantification is not a decisive sign of ontological commitment:

> On the other hand, when we say that some zoological species are cross-fertile we are committing ourselves to recognizing as entities the several species themselves, abstract though they are. We remain so committed at least *until we devise some way of so paraphrasing the statement as to show that the seeming reference to species on the part of our bound variable was an avoidable manner of speaking.*
>
> Quine [1980b, p. 13](italics mine)

In later writings he stresses the point repeatedly:

> My point is that everyday language is slipshod, slipshod though it be. We must recognize [...] that a fenced ontology is just not implicit in ordinary language.

> The common man's ontology is vague and untidy in two ways. It takes in many purported objects that are vaguely or inadequately defined. But also, what is more significant, it is vague in its scope; we cannot even tell in general which of these vague things to ascribe to a man's ontology at all, which things to count him as assuming.
>
> The idea of a boundary between being and non being is a philosophical idea, an idea of technical science in a broad sense. Scientists and philosophers seek a comprehensive system of the world, and one that is oriented to reference even more squarely and utterly than ordinary language. Ontological concern is not a correction of a lay thought and practice; it is foreign to the lay culture, though an outgrowth of it.
>
> Quine [1981, p. 9-11]

The point of introducing canonical notation (i.e. the language of first order logic) is precisely to draw attention to the ontological commitment of what one is saying:

> We can draw explicit ontological lines when desired. We can regiment our notation, admitting only general and singular terms, singular and plural predication, truth functions, and the machinery of relative clauses[...] then [...] we can say that the objects assumed are the values of the variables, or of the pronouns. Various turn of phrase in ordinary language that seemed to invoke novel sorts of objects may disappear under such regimentation. At other points new ontic commitments may emerge.
>
> Quine [1981, p. 9-10]

In dealing with sentences in the language of first-order predicate calculus we do have at hand a precise criterion to keep track of the ontological commitments: "Existence is what existential quantification expresses. There are things of kind F if and only if $\exists xFx$"Quine [1969, p. 97]. From this follows that:

(Canon) If a sentence s of canonical notation entails a sentence of the form "$\exists xFx$" s entails that there are Fs

Given that a sentence s is ontologically committed to Fs iff s entails that there exists an F (cfr. Quine 1969a: 93), this mean that:

(CanNot) If a sentence s of canonical notation entails a sentence of the form "$\exists Fx$", s is Ontologically committed to F's. (If a sentence s in canonical notation implies a sentence of the form "$\exists x(x = a)$", s is ontologically committed to the entity a).[1]

Given that the ontological commitments of the sentences form our everyday language are not clearly shaped and those of the paraphrases with which we substitute them are clearly shaped, this can only mean that in the process of paraphrasing "there is room for choice" [Quine, 1981, p. 9-10]. The passage to the canonical notation does not REVEAL previously hidden ontological commitments but force one to get clear about his own Van Inwagen [1998, Section 4].

The fact that there is more than one way to translate in canonical notation the sentences of our theories does not mean that there is no space to argue that

[1] See Liggins [2008] for similar formulations.

one paraphrase is superior to another. After acknowledging that in the process of translation into canonical notation "there is room for choice", Quine also notes that "one chooses with a view to simplicity in one's overall system of the world" [Quine, 1981, p. 9-10]. For instance, as Van Inwagen [1998, pp. 247-48] points out, in simple cases the refusal to introduce the existential quantifier in the translation of the sentence in question is very difficult to understand. Van Inwagen's example is that it is very weird to refuse to translate a sentence like "some metaphysical sentences are meaningful" as "$\exists x$(x is a sentence & x is metaphysical & x is meaningful)", which clearly implies "$\exists x$ (x is a sentence)". Given this, it seem almost inevitable that if one claims that some metaphysical sentences are meaningful, she commits herself to the existence of sentences. This explains why Quine repeatedly stress that we commit ourselves to the existence of the numbers when we say that some prime numbers are greater than a million.

Another point is that relations of logical consequence which seems clearly to hold between sentences of the natural language should be conserved in the translation: "there are more books in the shell than holes in my piece of cheese" seems to follow from "there are three holes in my piece of cheese" and "there are four books in the shell" and so the translation of into canonical notation of the first sentence should be derivable from the translation of the others; if one refuses to quantify over holes, this is not a trivial task to accomplish and one can call into question the devices used to have an alternative translation [Van Inwagen, 1998, p. 249].

3.1.5 Quine's Strategy (Complete)

We are now in a position to fully understand Quine's scientific method in ontology. In order to argue that things of the kind F exist, one has to proceed this way (see Liggins [2008, p. 117]):

1. Show that our best theories, once paraphrased, are ontologically committed to Fs

2. Appeal to the fact that as far as we should believe our best theories, we should believe the paraphrases of our best theories as well (which is connected to what Quine calls "Naturalism in Philosophy").

Conclude that:

C We should believe that there are Fs (or: believe that a exists)

3.1.6 Quine's official menu

A consequence of Quine's rules is clearly stated in the beginning of Yablo [2001]: if one does not want to admit the existence some sort of entities, but she says something that on the surface seems to commit herself to them, then Quine's official menu offers the following alternatives:

1. Stop uttering the problematical statements

2. Find a paraphrase that avoids the commitment

3. Give up one's resistance to accept the commitment

Quine's argument can be seen as a case to rule out the first two alternatives when the problematic statements in question are the existential theorems of a mathematical theory.

3.1.7 Quine's argument for Platonism

Quine's argument for Platonism is best seen as an application to the case of numbers of his general strategy to settle ontological controversies. Following Liggins [2008, p. 125], we consider a sentence like:

(S) The surface area of Saturn is $1.08 \cdot 1012km^2$

According to Quine (1960, p. 245) the paraphrases of sentences of such a kind must be something like

(S*) $\exists x$(x= the area in Km2 of Saturn & x= 1.08 · 1012)

By (CanNot), S* is ontologically committed to 1.08 · 1012, so it is committed to the existence a number. Applying to this case the strategy explained in the previous section, we have that:

1. we should believe the translation into canonical notation of the statements of our well-confirmed theories,
2. S is part of one well-confirmed theory

   From (1)& (2) follows that:
3. We should believe S

   But:
4. S* seems to be the only possible translation into canonical notation of S

   From (1), (3) &(4) follows that:
5. We should believe S*

   But:
6. S* implies the existence of a number

   So:
7. We should believe the existence of numbers

3.1.8 Wittgenstein vs. Quine

Quine has a criterion to distinguish between those usages of language that commits us to the existence of some sort of entities and those which are ontologically neutral: existential quantification. Moreover he has a criterion to establish which of those usages we must retain: accept the ontological commitments of the paraphrases into canonical notation of the sentences which form our best scientific theories. Wittgenstein has no such criterion, and seems simply to appeal to some sort of skepticism about the idea that ontological question can make sense:

> "A reality corresponds to the word 'two'." -Should we say this or not? It might mean almost everything. [LFM, p. 248]

This is not a viable response to Quine, because he has argued that a precise sense can be given to the question whether "A reality corresponds to the word 'two' ": to say that 2 exist is simply to say that there is something which is identical to two ($\exists x \quad x = 2$) or that 2 must be included between the values of our variables. This seems to be a trivial consequence of many sentences of pure mathematics that we accept ("$\exists x$ x is a prime number & x is even & x= 2") as well as of sentences of applied mathematics like the one considered in the previous paragraph (e.g. "2= the mass in Kg of this piece of bread").

3.1.9 Way out (1)

Even if Quine's approach is much more systematic than Wittgenstein's, there is still a feeling of strangeness in it. On one side, consider an argument like this (see Yablo[2008]):

(α) The number of my sheep is identical to the number of you degrees

(Ω) There are numbers

Even if we recognize that:

1. (α)is quite uncontroversial and
2. (Ω) logically follows from (α), and in a quite trivial way, given that we find no better way of paraphrasing (α) than to introduce numerical entities over which we quantify and to which we refer by numerical singular terms like "The number of..."

we still find (Ω) controversial. This is what Stephen Yablo[2008] calls CARNAP'S PARADOX: a controversial statement seems not controversially to be a logical consequence of an uncontroversial statement. The paradox arises because we expected that the property of being not controversial to be closed under logical consequence and the example seems to provide a counterexample. The problems seems not to be due (or seems not entirely to be due) to Quine's idea that the mark of ontological commitment is existential quantification. "There are numbers" seems to be very apt to express the position according to which there are numbers, and this is the reason why (Ω) sounds metaphysical and controversial. The problem seems to be that the CONTENT of the two statements seems to be very different: in (Ω) the existence of the numbers is explicitly asserted, whereas it seems to play a different role in (α). One could say that (α) implies (Ω) only if we take (α) AT FACE VALUE, but that we need not to do so. This is what Yablo [2001] calls the fourth way that FICTIONALIST approaches to mathematical discourse add to Quine's official menu. The idea is that we can preserve us from undesired ontological commitments by refusing to believe the literal content of our statements. What would have Quine had to object this strategy?

3.1.10 The problem of Applications

Instead of arguing for Platonism starting from a sentence like S, one could to start with an existential theorem from pure mathematics like "There are prime numbers greater than one billion" and argue in a similar vein. For instance, Burgess and Rosen [2005, p. 251] present an argument for Platonism that seems to be inspired by Quine's methodology, but which does not discriminate between statements taken from pure and applied mathematics :

1. Standard mathematics, pure and applied, abounds in "existence theorems" that appear to assert the existence of mathematical objects, and to be true only if such objects exist; which is to say, to be true only if nominalism is false. Such, for instance, are:

   - There are infinitely many prime numbers.
   - There are exactly two abstract groups of order four.
   - Some solutions to the field equations of general relativity contain closed timelike curves.

2. Well-informed scientists and mathematician (the "experts") accept these existence theorems in the sense both that they assent verbally to them without conscious silent reservations, and that they rely on them in both theoretical and practical contexts. They use them as premises in demonstrations intended to convince other experts of novel claims, and together with other assumptions as premises in arguments intended to persuade others to some course of action.
3. The existence theorems are not merely accepted by mathematicians, but are acceptable by mathematical standards. They, or at any rate the great majority of them, are supplied with proofs; and while the mathematical disciplines recognize a range of grounds for criticizing purported proofs, and while it occasionally happens that a widely accepted proof is undermined by criticism on one or another of these grounds, nonetheless the proofs of the existence theorems, or at any rate the great majority of them, are not susceptible to this kind of internal mathematical criticism.
4. The existence theorems really do assert and imply just what they appear to: that there are such mathematical objects as prime numbers greater than 1000, abstract groups of various orders, solutions of various equations of mathematical physics with various properties, and so on.
5. To accept a claim in the sense of assenting verbally to it without conscious silent reservations, of relying on it in theoretical demonstrations and practical deliberations, and so on, just is to believe what it says, to believe that it is true.
6. The existence theorems are not merely acceptable by specifically mathematical standards, but are acceptable by more general scientific standards. Not only do empirical scientists in practice generally defer to the mathematicians on mathematical questions, existence questions included; they are by scientific standards right to do so. There is no empirical scientific argument against standard mathematical theorems, existence theorems included.

7. There is no philosophical argument powerful enough to override or overrule mathematical and scientific standards of acceptability in the present instance.

   From (1), (2), (4), and (5) there follows an intermediate conclusion:

8. Competent mathematicians and scientists believe in prime numbers greater than 1000; abstract groups of various orders, solutions of various equations of mathematical physics with various properties, and so on. Hence, if nominalism is true, ex- pert opinion is systematically mistaken.

   From (8) together with (3), (6), and (7) there follows the ultimate anti-nominalism conclusion:

9. We are justified in believing (to some high degree) in prime numbers greater than 1000, abstract groups of various orders, solutions of various equations of mathematical physics with various properties, and so on, which is to say we are justified in disbelieving (to the same high degree) nominalism.

Letting for the moment the details aside, what matters here is that the argument has certainly a similarity with that of Quine, but the choice of pure mathematics together with mathematical physics is an important difference, as Maddy [2005] notes. It is quite clear that Quine attaches the greatest weight to the applications of mathematics to the physical science, and so the choice of an example like S is appropriate. He maintained that mathematics is good only as far as it is part of this business. Quine [1986, p. 400] says that he accepts "higher set theory" and "indenumerable infinities only because they are forced on [him] by the simplest known systematizations of more welcome matters", between those play a prominent role those part "oriented to application in empirical science"; but the more speculative branches of Set Theory are dismissed as "mathematical recreation".

3.1.11 Naturalism, Indispensability and Inference to the best explanation

So it looks like Quine is arguing in a different vain, claiming that the reason why we should accept the ontological commitments of mathematics lies in its indispensability for our physical science. Something like this:

> So far I have been developing an argument for realism along roughly the following lines: quantification over mathematical entities is indispensable for science, both formal and physical; therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes
>
> [Putnam, 1971, p. 347]

This is the LOCUS CLASSICUS for the formulation of the so called "Indispensabiltiy Argument" for Mathematical Platonism, usually attributed to Putnam and Quine (the attribution is highly controversial: see Colyvan[2008] and Liggins [2008]). Colyvan [2001] and [2008] states it in this explicit form:

(P1) We ought to have ontological commitment to all and only the entities that are indispensable to our best scientific theories.

(P2) Mathematical entities are indispensable to our best scientific theories.

(C) We ought to have ontological commitment to mathematical entities.

The first problem with this kind of argument is to understand what "indispensable" means here. One suggestion [Field, 1989, Introduction] is to compare indispensability arguments with arguments to the best explanations. An argument to the best explanations seeks to establish the truth of a certain hypothesis from the fact that this hypothesis offers the best explanation of a phenomenon we want to explain. This methodology is typically used to argue for the existence of the posits of theoretical Physics such as atoms, electrons, etc. : we are legitimate to postulate the existence of these entities, even if we cannot observe them directly, because their existence helps us to explain observable phenomena (the existence of unobservable entities such as atoms explains why the water boils in certain condition and not in others). The interesting fact is that the same physical theory that postulate the existence of atoms seems also to postulate the existence of functions, real numbers and other mathematical entities; if the argument for the existence of atoms is that the explanatory power of the theory is a good reason to believe it true, this means that this argument is an argument for the existence of mathematical entities as well, because the very same theory is committed to these abstract entities as well.

Quine stresses repeatedly the importance in the construction of a scientific theory of postulating the existence of posits which are not given in our immediate experience (see Quine [1955]) and he seems to accept inference to the best explanation. Still, there is something odd in claiming that it is necessary to postulate the existence of numbers in order to explain the behaviour of physical objects, because it is not clear how abstract objects could help in explaining this in the first place. Being abstract objects, numbers are supposed to be causally inert, which means that they do not cause any change in the empirical reality and nothing that happens in the physical world could make them change. Apart from being the source of epistemological quarrels against Platonism, this means that the behaviour of physical object would be exactly the same even if there were no numbers.

At least in our reconstruction of Quine's argument, there is no mention of indispensability, nor of inference to the best explanation. One could say that indispensability comes into play because one has to accept an ontological commitment only if there is no way to paraphrase it away, as Quine's menu suggests. But, in the case of mathematical entities, his point seems rather that 1) we should accept the ontological commitments of the paraphrase our scientific theories, because there is no philosophical argument powerful enough to call into question a statement that is justified according to scientific standards; 2) the choice of how to paraphrase a theory should be conducted along the usual scientific standard of simplicity, expressive power, familiarity of principle, etc...(see Burgess and Rosen [1997]). It is quite plausible to read Quine as arguing that mathematical Physics is our BEST scientific theory, according to scientific (as opposed to philosophical) standards, not that it is our ONLY possible theory. Even if Nominalization projects were to succeed, it would still remain an open question whether the new theories are superior to the old ones: after all, it seems that in scientific practice ontological economy (the so called "Ockam Razor") is not the only factor which is taken into account in deciding how to choose between competing theories (see Burgess and Rosen [1997], Burgess [1998]).

The point seems to be that quantification over mathematical entities such as numbers does not increase the explanatory power of a theory, but rather its descriptive power. Numbers & co. work, in Stephen Yablo's expression, as DESCRIPTIVE AIDS. Of course, one could say that this was just the point of the indispensability argument: if we have to quantify over numbers in order to say something we want to say, then we have to accept the existence of numbers. This seems to be the way in which one of the most prominent anti-Platonists of our days understands Quine's challenge (of course, he thinks the challenge can be met):

> For instance, there may be observations that we want to formulate that we don't see how to formulate without reference to numbers, or there may be explanations that we want to state that we can't see how to state without reference to numbers...if such circumstances do arise, then we will have to genuinely accept numerical theory if we are not to reduce our ability to formulate our observations or our explanations.
>
> Field [1989, pp. 161-162]

One could still wonder whether indispensability is really what is at issue here. The problem is this: does the use of abstract entity as representational aids really commit one to their existence? If the answer is yes, then we do already have an argument for Platonism: abstract objects are used as representational aid in (the canonical formulation of) our scientific theories; if this use is ontologically committing, then our scientific theories are committed to these entities and there is no naturalistic reason to give the commitment up. If the answer is no, then the use of number talk is not ontologically committing, being it indispensable or not, so the argument for Platonism does not even take off the ground.

3.1.12 The Irrelevance Argument

But why should the existence of something matters for its capacity of working as a representational aids? If there were no numbers, would this really in any way weaken their expressive power? To see the point, consider, as Yablo [2005] suggest, the scenario described in the opening of Burgess and Rosen [1997]:

> Finally, after years of waiting, it is your turn to put a question to the Oracle of Philosophy...you humbly approach and ask the question that has been consuming you for as long as you can remember: 'Tell me, O Oracle, what there is. What sorts of things exist?'To this the Oracle responds: 'What? You want the whole list? ...I will tell you this: everything there is is concrete; nothing there is is abstract....'
>
> Burgess and Rosen [1997, p. 3]

What would one do if such were the case? Would he go to the physical and mathematical department of her University and ask everybody to stop talking about the problematic entities? Would not this sound silly? It would sound silly, and quite independently fro the question whether one chooses the pure or the applied Mathematics department as the first camp to spread her nominalistic gospel. It seems that the philosophical question about the existence of abstract entities is just irrelevant for the scientific practice. The problem, then, becomes how it is possible: aren't mathematician, pure and applied, talking about numbers and other mathematical entities? If it is so, how it is possible for what they say to still have a point even if the entities which they are allegedly talking about do not really exist?

So the idea is to mount an argument with this form:

P The existence of Mathematical entities is irrelevant for the content we want to communicate in mathematical discourse

C The real content of our mathematical statements is not committed to the existence of mathematical entities (it would still be true even if there were no such entities)

Another way to make the point Yablo [2002, pp. 84] is to call the attention on the fact that an ontological disagreement between two mathematicians would be a barrier for their communication:

> How is it that mathematicians can happily communicate despite having different views of the nature, and even the existence, of mathematical objects? How can the ontological questions that philosophers sweat over be so irrelevant to actual practice?

Moreover Yablo [2002, pp. 89]:

> People making statements purporting to be about numbers are strangely indifferent to the question of their existence. Suppose that you as a math teacher tell little Fred that what 2 and 3 add up to is 5. And suppose some meddler points out that according to the Oracle (which let us assume we all trust), everything is concrete and so not a number. Instead of calling Fred in to confess your mistake, you tell the meddler to bug off. This makes sense if the meddler's information is irrelevant to what you were really saying - as indeed it is if your message was that it is five things (not six as Fred had supposed) that two things and three other things amount to. (Compare being rebuked for saying that Gandhi had a mind of his own on the basis that Gandhi is wholly physical.)

In Yablo's writings there is also much stress on another version of the irrelevance argument, which is more directly concerned with the problem of the relevance of the ontological questions for the application of mathematics. Curiously enough, very similar considerations are developed by Hilary Putnam [1996, p. 247] in the course of a discussion of Wittgenstein's position towards ontological questions in the philosophy of Mathematics:

> wouldn't mathematics have worked exactly as well even if the 'intangible objects' didn't exist? For, since the supposed mathematical objects are causally inert, the ordinary empirical objects would have behaved just as they do, and our applied mathematics would have succeeded and failed just when it does succeed and fail. But doesn't this already show that postulating immaterial objects to account dot the success of mathematics is a useless shuffle.

The question whether Wittgenstein held something close to the irrelevance argument is very interesting for the purposes of the present work. Putnam refers to a version version of the argument in the passage quoted above, which is part of a discussion of Wittgenstein's Philosophy of Mathematics, but he does not give any precise reference. Priest [1987, p. 190] attributes to Wittgenstein the following argument:

> Abstract mathematical objects, even if there were any, would be completely irrelevant to mathematical activity, and hence to the meaning and truth of mathematical assertions. Suppose, for example, that we got rid of all abstract objects by burning them in a Platonic (or Meinongian) incinerator. Would this destroy the meaning of '3'? Would it make '1+2=3' false? Not as long as we continue to compute in the same way

Priest points to Klenk [1976, pp.8-18] for references to Wittgenstein's texts and discussion. If we look at the passage reported by Klenk, we do find something interesting. In fact, we have already encountered those passages.

> Isn't it over-hasty to apply a proposition that one has tested on sticks and stones, to wavelengths of light? I mean: that 2 X 5000= 10000. Does one actually count on it that what has proved true in so many cases must hold for these too? Or is it not rather that with the arithmetical assumption we have not committed ourselves at all? [RFM III 10]

> Imagine the following queer possibility: we have always gone wrong up to now in multiplying 12 X 12. True, it is unintelligible how this can have happened, but it has happened. So everything worked out in this way is wrong! - But what does it matter? It does not matter at all! - And in that case there must be something wrong in our idea of the truth or falsity of arithmetical propositions. [RFM I 134]

Wittgenstein's seems to be much more concerned with the problem of our certainty of mathematical statements that with the problem of communication. His problem is that mathematical certainty does not square with the idea of mathematical knowledge as ascertainment of facts, because this ascertainment would always be fallible. Still, there is a partial overlap with Yablo's problem: the normative role of mathematical propositions is what makes empirical facts irrelevant for their adoption. Given that for Wittgenstein, as we saw in the previous chapter, existence of any entity is always an empirical matter, this means that the existence of mathematical entities is irrelevant for the adoption of grammatical rules. Given that mathematical statements are grammatical rules in disguise, the existence of mathematical object is irrelevant for our willingness to utter mathematical statements.

3.1.13 Aboutness , Applications and the problem of the content

The central problem seems to be that of content: what do we really claim when we make a (pure or applied) mathematical statement? This is a semantical problem, so a problem about language and thus we have come in full circle: the problem of finding an account for our mathematical discourse is central for the problem of the reality of mathematical objects. It could be useful to summarize the points reached so far:

1. It is quite sensible to take seriously Quine's criterion for ontological commitment: if a sentence in canonical notation implies that $\exists x$ (x is a number), then this sentence is ontologically committed to numbers;

2. (The paraphrase of) some mathematical statements, taken at face value, commits us to the existence of mathematical entities;

3. The existence of mathematical entities seems to be irrelevant for the point of our mathematical discourse: if there were no number, we could still go on talking as we always did;

4. The problem of Aboutness and that of Application seem to be linked, in the sense if, in applied mathematics, we are not talking about the existence of numbers when we use number-talk, this could explain why numbers, which are abstract, are so useful to describe physical objects, which are not abstract. The explanation would go something like this: the existence of numbers is irrelevant for the success of our scientific practice because numbers are not what we talk about when we do empirical science; their role is not to be the subject of our discourse, but rather representational aids, something TROUGH which communicate ABOUT concrete things (see Yablo [2001]); in order to be useful in this way they do not need to exist;

5. If this were the case, we would have an advantage over approaches like that of Field, which try to explain how mathematics could be dispensable, but do not explain how present day, abstract talk-loaded mathematics could be so useful (see Yablo [2005]). If the use of numbers as descriptive aids is not ontologically committing, then there is no problem even if it is not dispensable. So the question becomes whether Wittgenstein's idea that mathematical sentences are rules of grammar can help in the enterprise of accounting for the Application of Mathematics and the Aboutness of the mathematical statements in the way requested.

3.2 Wittgenstein, Fictionalism and the problem of Aboutness

Recently Kahalt [2008] has attacked Wittgenstein's idea that mathematical statements are rules of grammar on the ground that this claim commits to the implausible thesis that they are not about numbers. This is interesting, because we have seen that the problem of the Aboutness is interestingly linked with that of the existence of the Mathematical entities. I reply to this charge by showing how it can be maintained that mathematical sentences are grammatical rules and that are about numbers as well. My account is based on ideas drawn from the kind of Fictionalism about mathematical entities (Figrualism) advocated by Stephen Yablo. At the end of the day, I discuss the issue whether Wittgenstein really held something close to the position defended here.

3.2.1 The Problem

It is customary attributed to Wittgenstein the view that mathematical statements are rules of grammar (see for instance Frascolla [1994, Chapter 3], Steiner [2009, p. 15], Glock [1996, p. 234] and [RFM III 38]). As we have seen in the first part, the attribution is debatable and, more importantly, even if it is correct, it is not completely clear how to understand the claim that mathematical sentences are rules of grammar.[2]

For the purposes of this paper I will stick to simplest way to understand the doctrine: mathematical statements are grammatical rules in the sense that "what we call 'statements about numbers' have the role of rules for the use of number-words or numerals" Baker and Haker [1985, p. 281].

The problem is that this claim raises an obvious worry concerning the aboutness of mathematical sentences:

> for Wittgenstein, a proposition like "2 + 2 = 4" is about numbers, not in the sense that it tells us a fact about them, but in the sense that it lays down a rule for the correct use of the numerals "2" and "4". But thus construed, the proposition is about numerals, not about numbers.
>
> Kahalt [208, p. 10]

It is worth noting that Wittgenstein presented himself with the problem in almost the same terms of Kahalt:

> queer trouble: one asks such a thing as what mathematic is about- and someone replies it is about numbers. Then someone comes along ad says that it is not about numbers but about numerals; for numbers seem very mysterious things. And then it seems that mathematical propositions are about scratches on the blackboard. That must seem ridiculous even to those who hold it. [LFM, p. 112]

[2] See for instance [LFM, p. 55]. Still, this shows that the Austrian philosopher always thought that the analogy between mathematical sentences and rules is an important one. I am content to endorse a moderate proposal made by Steiner [2009, p. 10, p. 15]: it can be said that mathematical proposition are rules of grammar, even though, during the years Wittgenstein came to broaden his conception of what a rule of grammar is.

The purpose of this paper is to explore a strategy to reply to this objection, showing a way to make Wittgenstein's claim compatible with the (commonsensical) idea that numerical statements are about numbers.

3.2.2 Some Replies

Some friends of Wittgenstein are content to reply to Kahalt that that "mathematical sentences are not about anything [in the descriptive sense]" Shanker [1988, p. 211], see also Rodych [2006, p. 69] There is plenty of textual evidence in support of such an interpretation:

> In mathematics everything is algorithm and nothing is meaning; even when it doesn't look like that because we seem to be using words to talk about mathematical things?[PG 468]
>
> Arithmetic doesn?t talk about numbers, it works with numbers [PR 109] It is already mathematical alchemy, that mathematical proposition are regarded as statements about mathematical objects, - and mathematics as the exploration of these objects? [RFM V 16]

I have three qualms about this interpretation. One is scholarly in nature: it is true that there is a formalistic strand in Wittgenstein's philosophy ("mathematics is about nothing"), but there is also a quietist strand (see [PI 124] and [LFM, lecture 26] for some -debatable- evidence of this attitude when discussing the point at issue here): to be faithful to it, Wittgenstein should be reluctant to take a stance that is at odd with the common sense view that numerical sentences are about numbers. [3]

The other problems are more theoretical: first of all, claiming that mathematics is about nothing seems not be a fair reply to Kahalt: he was complaining that if mathematical sentences are rules of grammar, they must be about the numerals, so that they cannot be about the numbers; the reply that mathematical sentences are about nothing faces a similar objection: if mathematical sentences are rules of grammar, they seem to be about something, i.e. the numerals. Maybe at this point one could appeal to Shanker's gloss "in the descriptive sense": mathematical sentences do not depict numerals' behaviour, they rule it, so the sense in which they are about the numerals is not the one in which empirical sentences are about empirical objects. To this one could add the fact that Wittgenstein had quite a peculiar view about the way to understand the distinction between numerals and numbers. According to Rodych [1997, p. 197]:

> As Wittgenstein says at (WVC 34, Ft. #1), "[n]umbers are not represented by proxies; numbers are there." This means not only that numbers are there in the use, it means that the numerals are the numbers, for "arithmetic doesn't talk about numbers, it works with numbers"[PR 109]

And Wittgenstein adds (and Rodych quotes):

[3] Wittgenstein there says that "2+2=4" is not about the number two, but "There are three windows in this rooms" is about the number three: this sounds quite weird, given that the latter proposition seems to be more about the windows than about the numbers. More interestingly, at some point (lecture XXVI) Wittgenstein denies to be claiming that it is wrong to say that numerical sentences are about the numbers "I am not claiming that is wrong to say that mathematical propositions are about the numbers".

> What arithmetic is concerned with is the schema | | | |. “But does arithmetic talk about the lines I draw with pencil on paper?” Arithmetic doesn’t talk about the lines, it operates with them. [PG 333]

Putting all this together, one could try to say: according to Wittgenstein numerals are numbers, and mathematical sentences are just strings of signs (among which numerals figure): this configurations of signs do not depict anything, like configurations of chess pieces on the board. Of course, the correct performance of some signs constructions is a criterion to judge the mastery of the relevant sign-game: to obtain “125” as result of adding “58” and “67” is a rule for the correct usage of the numerals and the algorithm for addition, and in these sense “58 +67=125” is about the numerals (number), more or less as “the chess-king moves one square at a time” is a rule of chess and about the chess king. This is almost the position taken by Glock in his reply to Kahalt, with a telling difference:

> Statement (2) [The chess-king moves one square at a time] is both about the chess-king and a rule for the use of the chess-king. Kalhat might question the analogy. For in the chess case, the rule guides the use of what it is about. In the linguistic case, by contrast, the rule guides words, yet it is supposed to be about what those words denote. Wittgenstein would respond, however, that the sense in which numerals like “2” denote numbers is a special one. Numbers are what numerals denote or signify, but the meaning of numerals is given not by a mental pointing at entities beyond space and time, but by specifying the rules for their use[...]
>
> Glock [2008, p.32]

Glock is not disposed to identify numerals and numbers and it is easy to understand why: the most usual objection to term formalism is precisely that we do not usually identify the word we use for the numbers (=numerals) with numbers. The objection is far from being conclusive, but still show the identification of numerals with numbers is not very in tune with the quitestic attitude present in Wittgenstein. More importantly, there are other reasons of dissatisfaction with a straightforward denial of any aboutness to mathematical sentences. Traditional formalism is quite silent on a point where some story should be told (and where the analogy with the game of chess seems to break down): why mathematical language looks like referential? Why it is so easy to take a mathematical theorem like “there are infinitely many prime numbers” at face value, as implying the existence of some abstract object called “numbers” and so difficult to take a configuration of chess pieces on a board as asserting anything? Why chess has no application to the physical world and mathematics so many?

Still, I see two major problems in Glock’s account: first, it does not tell us what is the “special sense” in which numerals denote numbers. This is closely related to another worry: many Philosopher of Mathematics nowadays are attracted by what is called Nominalism: the view that there are no abstract object as numbers (see Burgess and Rosen [1997]). Even Wittgenstein is commonly taken to have some sympathy for this view; but how is this possible to leave open the possibility that there are no numbers, if numerals denote numbers?

3.2.3 The project

The problems deepen, because it seems that, if we want to save the core of Wittgenstein's account ant still don't want to reject nominalism, we are bound to claim something like that :

1. Arithmetical sentences are grammatical rules
2. Arithmetical sentences are about numbers
3. Our willingness to utter arithmetical statements does not carry a commitment to abstract objects as numbers

So, instead of one problem, we now have two: (1) seems to be inconsistent with (2) and (2) seems to be inconsistent with (3). But (2) and (3) are claims which are not peculiar to Wittgenstein: to solve the inconsistency, then, it seems sensible to look for an account that can accommodate for (2) and (3) as well and then see whether (1) can be made to fit into this account.

3.2.4 Figuralism

Fortunately, there is such an account: Yablo's Figuralism (his version of fictionalism) is the view that our number talk is non committal about the existence of number and is still, in a sense, about numbers. The basic idea of Fictionalism is that some sentences, numerical sentences for instance, are put forward in a fictional or make-believe spirit [Yablo, 2001, p. 74]. Take for instance an (applied) arithmetical statement like:

(AM) The number of the Moon of Mars is two

When we utter (AM), we are pretending that there are numbers in order to communicate something about the moon of Mars (i.e. something about the concrete world), as when we say that Italy is a boot and Crotone is at the arch of the boot, we are pretending that Italy is a boot in order to communicate something about the geography of that country.

Understood in this way, mathematical talk becomes a case of metaphorical talk and this account for (3): when we say that the number of the Martians moon is two, we should not be taken at face value, no more that when we speak of Italy in terms of a boot. At the same time, this account for (2) as well: if we take (AM) at face value, it is about the number two.

The idea is that pretence are representational devices and that they succeed in doing so thanks to what Walton[1993] called generation principles, i.e. principles which tell us under which real world condition a sentence is true according to the fiction (fictional). Examples of generation principles in children's make-believe games are:

- "x is the King of France" is true according to our pretence iff x is wearing a hat
- "x is injured" is true according to our pretence iff x is lying on the floor

According to Yablo, number talk can be interpreted in a similar fashion (see for instance Yablo [200] and [2002]):

- "The number of G's = n" is true according to the fiction of arithmetic iff there are n G's .
- "the # of Fs=the# of Gs" is true in the Arithmetical fiction iff there are as many Fs as Gs

The same holds for other cases of abstract object. If lines count as concrete entities, the Fregean direction principle tell us how to relate the abstract direction-talk to talk of concrete line talk:

DIR "The Direction of line a = the Direction of line b" is true in the Direction Theory iff line a is parallel to line b.

3.2.5 Aboutness

It seems to me that generation principles are good candidates for the role of wittgensteinian Rules of grammar. After all, a generation principle tell us how to USE mathematical terms, and in this sense gives them a meaning:

> it is not possible to appeal to the meaning [Bedeutung] of the signs in mathematics, because it is only mathematics that gives them their meaning [Bedeutung][RFM V 16]

Reflecting on the way Generation principles work we can also understand how the problem of the aboutness can be solved. One form of fictionalism criticized by Yablo is what he calls Meta-Fictionalism Yablo [2001, p. 75]. Meta-Fictionalism claims that the real content of an arithmetical sentence like "2 +3= 5" is "according to standard Math, 2+3=5". This raises many problems: one is that it seems a matter of necessity (as well as something a priori knowable) that 2+3=5; instead, it is not necessary and not a priori knowable what standard math claims (standard math could have been different). Moreover, consider:

(AM2) The number of starving people is increasing

Meta-Fictionalism reads it as equivalent to "According to standard (applied) maths, the number of starving people is increasing"; but when we utter this sentence we seem to be more concerned with the destiny of the starving people than with the content of a certain (applied) mathematical theory. Meta-fictionalism is wrong in taking number talk to be about the fiction of arithmetic. As Yablo explains,Yablo [2001, p. 76]:

> I am certainly relying on the rules of English, when I utter the words "snow is white." It is those rules that make my utterance a way of saying that snow is white. It is just that relying on rules is one thing; talking about those rules is another. Likewise, when the words "the number of apostles is twelve" come out of my mouth, I am relying on the number-fiction. It is thanks in part to that fiction that my utterance is a way of saying that there are twelve apostles. Again, though, relying on a fiction is one thing; talking about it is another. The fiction (like the rules of English) functions as medium and not message. The rules tell us which sentences are true under which worldly conditions.

Does Yablo's idea that "the fiction functions as the medium and not message" not resemble Wittgenstein's idea that "Arithmetic doesn't talk about the lines, it operates with them" [PG 333]. We are in a position to accommodate for (2): taken at face value, mathematical discourse really is about mathematical entities, but there is no reason to take it at face value. Number talk is adopted to communicate something that does not hinge upon the existence of mathematical entities. How is this possible? Because some mathematical proposition have a role akin to that of generation principles: they set a function from what is fictional (true according to the fiction) and what is real, so that one "sees through" the fiction a content which has nothing to do with the fiction (one sees trough the applied mathematical claim that the number of my sheep is equal to that of yours the fact that is really depicted, namely that I have as much sheep as you). The analogy I am relying on is this:

Rules Mathematical Sentences ≅ Generation principle ≅ Rules of English (which of course are Rules of Grammar)

Generation principles are rules of grammar and they are not about numbers, in the sense that, like the rules of English, they are conventions trough which number sentences receive their meaning and the adoption of this convention does not hinge upon the result of metaphysical disputes about the existence of numbers. This should account for (1).

Arithmetical sentences like (AM1) and (AM2) taken at face value are about some fictional characters called "numbers", but number talk is just a (an essential?) representational aid for describing the concrete world. In order to fulfil this function, the objects pretended to be there need not really be there. This should account for (3) In other words, we are in a position to account for the intuition that:

> mathematics appears to us now as the natural history of the domain of numbers, now again as a collection of rules [RFM IV 3]

The intuition is correct, because mathematics is both.

3.2.6 Mathematics and Generation Principles

I have argued so far that we can account for:

1. Mathematical statements, taken at face value, are about numbers: both from "The Number of Apostles=12" and "2+2=4" follows that there are numbers;

2. Fortunately, there is no need to take these statements at face value;

3. Mathematical statements are akin to Generation Principles and in this sense to Grammatical Rules

One naturally wonders if the Mathematical statements to which is alluded to in (1) are the same as those mentioned in (3). In part, the answer is in the negative. Examples of typical mathematical statements from applied and pure mathematics are (AM1) or arithmetical equations like "3+5=8" etc? The comparison with generation principles is NOT made considering THESE sentences, but rather something other that mathematician claim, to which usually one refers to as arithmetical (or geometrical, or whatever) principles, like Hume's Principle:

HP #F = #G iff F≈G

Consider the latter case. The most prominent friends of this principle, the Neo-Fregeans, say about it that it introduces a new concept ("Number"), and somebody denying this principle simply does not grasp the concept of number (see Hale and Wright [2001]). It is considered just a definition, a stipulation: something quite close to "The chess king moves one square at time". All these are typical features (and problems) of Grammatical Rules (see RFM IV 29, VI 10). Still it seems to be a mathematical statement at least as far as it can be used to derive from it other mathematical statements: from HP one can derive all the Peano Axioms, as Frege's Theorem [Boolos 1990] shows. So, as far as (HP) works in this way, it is still true that, taken at face value, (HP) is about the numbers, whereas it really works as a generation principle. The same, though, could not be said about (AM1) which in no way works as a generation principle, and which is about numbers taken at face value, and really about the moon of Mars.

So far, I have accounted for sentences of Applied Mathematics like (AM1) and Principle like (HP)What about "3+5=8"? Taken at face value it is about numbers, but what is it really about? And is it a rule of grammar or not? Before answering this question, it is worth to spend some more time highlighting the centrality of the problem of application both for Wittgenstein's view and for the Figuralist account.

3.2.7 The problem of applicability

> The difficulty in looking at Mathematics as we do is to make a one particular section- to cut pure mathematics off from its application. It is particularly difficult to know where to make this cut because certain branches of mathematics have been developed in which the charm consist in the fact that pure mathematics looked as though it were applied mathematics-applied to itself. And so we have the business of a mathematical realm. [LFM, 15]

Here Wittgenstein is thinking primarily about Set-Theory, which he considered a major source of confusion in the Philosophy of Mathematics and a driving force in the emergence of Platonism (find sources). What is interesting about Wittgenstein's critique of Set Theory is that, ironically, at a certain point it seemed to be unjustified even accordingly to Wittgenstein's own standards (see Rodych [1997]), and this caused a tension during the mature phase of his reflection. Wittgenstein sought a solution to this tension in the central role played by applications for the meaningfulness of a Mathematical Theory. The interesting fact is that Yablo's account provides exactly what Wittgenstein requested. The central claim of this section is this:

Appl Principles of Generations share a feature of Wittgensteinian conception of mathematical proposition as rules of grammar, namely their being essentially connected with the application to the non-mathematical propositions

As we already saw in the First Part, the problem for Wittgenstein was that in his mature phase he wanted to criticize Set Theory as pointless and misleading, but in order to do that he could not maintain the strong formalism he embraced during his intermediate phase. According to this doctrine, every mathematical calculus is on a par: in the end, they are nothing but rule-governed activities of manipulating sing, and one system of rules and axioms is just as good as another. This clearly implies that as long as Set Theory is a formal theory, it is as good as every other mathematical theory. Moreover, against this position, Frege's critique seems to have a point: if arithmetic were just a game based on a set of arbitrary rules, would not its applicability to the physical world look like quite mysterious?

Anyway, in his mature phase, Wittgenstein returned to his position a the time of the Tractatus, according to which the applicability of mathematical concept and results in empirical science was the very reason for being of the mathematical systems:

> I want to say: it is essential to mathematics that its signs are also employed in mufti. It is the use outside mathematics, and so the meaning of the signs, that makes the sign-game into mathematics. Just as it is not logical inference either, for me to make a change from one formation to another (say from some arrangement of chairs to another) if these arrangements have not a linguistic function apart from this transformation. [RFM, V, 2]
>
> concepts which occur in 'necessary' propositions must also occur and have a meaning in non necessary ones [RFM V 42]

The importance of the latter point is also stressed very clearly by Yablo in passages of such a kind, where he does not only endorse Wittgenstein's latter claim, but also gives an explanation of why it is so:

> all of the statements [generation principles, note mine] employ a distinctive vocabulary: "number," "butterflies," "x|Fx," "market penetration" – a vocabulary that can also be used to talk about concrete objects and their contingent properties. One says "the number of English Kings is growing," "her marital status is constantly changing," and so on. Third, its suitability for making contingent claims about concrete reality is the vocabulary's reason for being. .... Our interest in 11 has less to do with its relations to 7 than with whether, say, the eggs in a carton have 11 as their number, and what that means about the carton's relation to other cartons whose eggs have a different number.Yablo [2002, pp. 228-229]

The explanation is this: numbers are representational aids, their utility in application is no more a mystery, but an obvious consequence of their nature.

> All abstract objects yet discovered have "turned out" to come in handy as representational aids. How is this interesting coincidence to be explained? Why have numbers, sets, properties, and so on all turned out to be liable to the same sort of use? This should remind us (says the figuralist) of Wittgenstein's fable in which we first invent clocks, and only later realize that they could be used to tell time. It is no big surprise if things with representing as their reason for "being" show a consistent aptitude for the task. Yablo [2001, p. 89-90]

As we saw in the First Part, both traditional Platonism and Traditional Formalism have some problem in accounting for the applicability of Mathematics to the physical world: they should explain why the description of some abstract objects or a mere sign game are so helpful in the natural sciences (which deals with the concrete world).

This point is conceded in some degree even by modern nominalist. For instance, Hartry Field [Field, 1989], [Field, 1980], has repeatedly stressed the fact that a necessary condition for a formal calculus N to be a suitable instrument in the natural sciences is to be CONSERVATIVE, which means that, if we have an assertion A of a theory F, formulated in a purely physical ("nominalistic") language (i.e., where the variables range over concrete objects), then A is derivable by some set of premisses in N, then A is derivable also by these premisses alone. The role of N, then, is just to shorten derivations, which, in every case, could be carried out also in a purely nominalistic theory. Conservativeness is a even a stronger condition then consistency and this tells us that Field is NOT a strong formalist in the sense specified in the first part.[4]

Figuralism shares with traditional Formalism the idea that mathematics is ontologically neutral, but it has a nicer explanation of the phenomenon of the applicability of mathematics to the physical world. That mathematics is so useful in describing the empirical reality should be no surprise: the reason why we adopted abstract-talk was precisely that it permits us, via the generation principles, to express some truths about the concrete world. We saw that highlighting the importance of extra-mathematical application of mathematical concepts is one of the most telling features of Wittgenstein's later philosophy of Mathematics and that his reflection on this point created some tension with the radical formalist position of the middle years.

The analogy on which I am relying is that between Wittgensteinian Mathematical Propositions as Rules of Grammar and Yablesque (and Waltonian) principles of generation. As long as the "reason for being" of both kind of proposition lies in their application to extra-mathematical contexts, we have a very telling coincidence. It is not easy to understand what Wittgenstein's account of mathematical proposition was: at least, the Figuralist's position satisfies what Wittgenstein considered a fundamental desideratum of every tenable account of Mathematics.

3.2.8 Pure Mathematics

In accounting for the meaning of Mathematical propositions, I have stressed, following Yablo (2005), the problem of Applicability. The idea is that, at the level of literal content (AM1) is about numbers and astronomical bodies, whereas at the level of the real content it is just about astronomical bodies. In the case of "3+5=8" part of the account runs the same way: the sentence is about numbers, at the level of the literal content. What about the level of the real content? In the case at hand, it possible to appeal to a suggestion due to Frege: to consider the content of an arithmetical statement as a logical truth: in the case at hand,

[4]Strong Formalism (SF): A mathematical calculus is defined by its accepted or stipulated propositions (e.g., axioms) and rules of operation. Mathematics is syntactical, not semantical: the meaningfulness of propositions within a calculus is an entirely intrasystemic matter. A mathematical calculus may be invented as an uninterpreted formalism, or it may result from the axiomatization of a "meaningful language". If, however, a mathematical calculus has a semantic interpretation or an extrasystemic application, it is inessential, for a calculus is essentially a "sign-game"?its signs and propositions do not refer to or designate extramathematical objects or truths. Rodych [1997, pp. 196-197] For an example of a consistent system which is not conservative, see Field [1989].

'3+5=8' express the fact that if there are exactly three things of the kind F, exactly five things of the kind G and there is nothing which is both F and G, then there are exactly eight things which are F or G.

This is a form of non Platonist logicism. Wittgenstein seems to allude to something like this when he says in a passage like the following (even if he is probably ruminating on something quite different):

> So much is true when it is said that mathematics is logic: its moves are from rules of our language to other rules of our language [RFM I 165]

In any case, this works only for arithmetical equations, so we still lack a general account for pure mathematics, for instance the claim that "the number of natural numbers is Aleph null".

This is a crucial problem, given that, according to Wittgenstein, it is crucial to give an account of this pure mathematical statements (particularly those from Set-Theory) because they give rise to the false platonistic picture of mathematics as a description of a real of sui-generis objects:

> "Fractions cannot be arranged in order of magnitude"- First and foremost, this sounds extremely interesting and remarkable. It sounds interesting in a quite different way from, say, a proposition of the differential calculus. The difference, I think, resides in the fact that such a proposition is easily associated with an application to physics, whereas this proposition belongs simply and solely to mathematics, seems to concern as it were the natural history of mathematical objects themselves. One would like to say of it e.g.: it introduces us to the mysteries of the mathematical world. This is the aspect against which I want to give a warning. [RFM II 40]

One suggestion comes from Yablo [2005, p. 110]: numbers Co. "start life as representational aids", but the system is quite complex, so that at a certain point there is the need to describe not only the real world, but also our system of representational aids. To this end, the very same representational aids are used, like in "the number of even prime is two" or new ones are introduced, like in "the number of natural numbers is aleph null".

> Numbers start life as representational aids. But then, on a second go-round, they come to be treated as a subject-matter in their own right (like Italy or the thundercloud). Just as representational aids are brought in to help us describe other subject-matters , they are brought in to help us describe the numbers. Numbers thus come to play a double role, functioning both as representational aids and things-represented.
>
> Yablo [2005, p. 110]

This seems to be very close to the picture of "mathematics self-applied" to which Set-Theory gives rise and which Wittgenstein criticized (see above), but the tension is only superficial: at the bottom level, the link with the application to empirical science is the source of mathematical meaningfulness, as requested by Wittgenstein. The fact that we can use representational aids like numbers also to represent themselves should not be confused with platonism: the subject matter of Pure Mathematics is not a realm of existent abstract object, but just a system of tools; not "the numbers as they are", but "the numbers as they are imagined to be" Yablo [2001, p. 85].

In the end, we are really confronted with "the mathematical object themselves",

> The bulk of pure mathematics is probably best served by interpretation (M). This is the interpretation that applies when we are trying to come up with autonomous descriptions of this or that imagined domain. Our ultimate interest may still be in describing the natural world; our secondary interest may still be in describing and consolidating the games we use for that purpose. But in most of pure mathematics, world and game have been left far behind, and we confront the numbers, sets, and so on, in full solitary glory.
>
> Yablo [2005, p. 110]

but this has no ontological import: we are just confronting with a system of descriptive aids, which were invented for their utility in the applications to the real world.

It could be of interest to note a passage of Wittgenstein which comes quite close to what Yablo says about the development of pure mathematics out of applied mathematics, which is rooted in practical needs:

> Well, I could say: a mathematician is always inventing new forms of descriptions. Some, stipulated by practical needs, others, from aesthetic needs, and yet others in a variety of ways. And here imagine a landscape gardener designing paths for the layout of a gardner; it may well be that he draws them on a drawing board merely as ornaments strips without the slightest thought of someone's sometime walking on them. [RFM I 166]

3.2.9 Conclusions

My proposal is that a figuralist position is compatible with both the thesis that some mathematical sentences(= generation principles) are rules of grammar and with the stress on the importance of applications to the physical world, and I take this to be two pivotal thesis of Wittgenstein's reflection.

Figuralism also has a story to tell to explain the interplay between the literal content of other sentences (e.g. arithmetical sentences) and their real content: literal content is about the numbers and so the truth of the literal content hinges upon the existence of numbers; but the truth of the real content does not hinge upon metaphysical issue and in this sense endorsing the real content of arithmetical statements does not commit to the existence of abstract objects.

Interpreting Wittgenstein as a figuralist, then, does not only provide a reply to the aboutness objection that is superior, in my view, to that of Glock, but it also accounts for one of the most important shift in his views (the stress on the role of applications) and has some nice outcomes (it makes Wittgenstein's position compatible with Nominalism). This should provide sufficient grounds for taking this interpretative conjecture in consideration.

3.3 Two conceptions of Ontology

3.3.1 The Neo-Fregeans' Strategy

The idea of looking at sentences containing (alleged) singular terms, in order to find out what there is, is one of the polemical targets of Quine[1980b]:

> Up to now I have argued that we can use singular terms significantly in sentences without presupposing that there are the entities which those terms purport to name
>
> Names are, in fact, altogether immaterial to the ontological issue, for I have shown, in connection with "Pegasus" and "pegasize", that names can be converted to descriptions, and Russell has shown that descriptions can be eliminated.
>
> Whatever we say with the help of names can be said in a language which shuns names altogether

Stephen Yablo illustrates the point with a hilarious comparison:

> There is a cartoon about the last worker on the Sara Lee assembly line: She sits by the conveyor belt asking herself of each passing pie, "Would I be proud to serve this to my family?" Replace the pies with noun phrases and you have Quine's picture of the traditional ontologist: "Am I content to think this refers to a bone fide entity?"
>
> Yablo [1996, note 33]

Quine's proposal is to substitute disputes about the referential properties of bits of language with an analysis of our use of quantified expressions:

> Quine thought he had made sufficiently clear about half-a-century ago[Yablo refers to On What there is], one makes little progress on matters of ontological commitment by staring at controversial chunks of language waiting for them to yield up the secret of whether they are really referential or not. [...] The true and proper test of ontological commitment is quantification into the position a given chunk of language occupies.
>
> Yablo [1996, p. 266]

So the prospects of mounting an argument for the existence of mathematical object which starts from the ascertainment that we use singular terms to denote numbers don't look so good in the quininean tradition. But if we look a the Fregean tradition, things are quite different. One of the major advocates of this strategy, Crispin Wright, seems to suggest that the link between the use of singular terms and the ontological questions is tighter than Quine thought:

> Frege's belief that numbers are objects is not to be dismissed as a technicality. It is the belief that numbers are objects in what is (or ought to be) the ordinary understanding of the term, and it is the product of a deceptively simple train of thought. Object are what singular terms, in their most basic use, are apt to stand for. And they succeed in doing so when, so used, they feature in true statements. Certain sorts of expression, for instance the standard decimal numerals, and expression formed by applying the numerical operator, "the number of", to a predicate, are used as singular terms in the pure and applied arithmetical statements of identity and predication in which they feature. Many such statements are true. So such terms do have reference, and their reference is to objects.
>
> Hale and Wright [2001, p. 253]

What Wright is suggesting here is an argument for Platonism which starts from the premise:

1. If a certain linguistic expression e is a singular term and e figures in true statements, then e must refer to an object. As an instantiation of (1), we have that:

2. If a numeral like ”4” is a singular term and it figures in true statements, then ”4” refers to an object So, if one adds that:

3. A numeral like ”4” is a singular term

4. A numeral like ”4” figures in true statements, like “2+2=4” It follows by modus pones from 2 and the conjunction of 3) and 4) that:

5. “4” refers to an object

This object is of course a number, so 5) entails the Platonist conclusion that there are numbers (to which the numeral refers).

The argument seems to be valid, which means that one needs to reject one of the premises 1, 3, 4 in order to reject the conclusion. This has in fact been done. One can of course have doubts about how to understand 1. For instance, one could think that the neo-fregeans definition is someway circular: objects are defined as the references of singular terms and singular terms are defined as those linguistic entities whose semantical function is to refer to objects. But then, how could we ever manage to ascertain whether an expression is a singular term BEFORE having established whether it refers to something, which means before having resolved the problem from which we started? Neo-Fregeans are quite aware of this problem (see [Wright and Hale 2005]) and this is why an important step in their strategy is to show that singular terms can be identified by syntactical criteria, usually the fact that they figure in grammatically correct identity statements and that they have the inferential property of permitting an existential generalizations. If one accepts the idea of a syntactic characterization of singular terms, there is still much room left for rejecting the argument. One can in some sense deny 1 by maintaining that the function of singular term in mathematical language is not to denote objects, in the sense that the truth-conditions of sentences containing them can be given, for instance, in modal terms, like in Hellman [1989] (see Chapter 4). This can be maybe better seen as a rejection of 2 as well, in the sense that, for instance, a modal structuralist, refusing to take mathematical language at face value, refuses to consider a term like “4” a genuinely singular term. But there is another strategy, typically associated in the contemporary literature with the name of Hartry Field [1980, 1989]. The basic point of this strategy is simply to deny that there is any compelling argument which forces us to consider mathematical statements like that figuring in 4 literally true. Mathematics, according to one famous motto by Field, needs not to be true to be good. It is in replying to such an argument that the Neo-Fregean strategy becomes very interesting from our perspective.

Neo-Fregeans are asked by Field to show a reason to take numerical statements as true, a reason to believe in mathematical statements, which in turn means an account of how it is possible for human beings to know that these statements are true, so that his complaint with a premise like 4 can be seen as connected with the epistemological challenges to Platonism (see Chapter 2).

3.3.2 Wright on Frege's use of the context principle

Wright's idea is that the correct way to reply to this kind of epistemological worries is to understand the way in which numerical terms are introduced. Wright finds the key point for his reply to this objection in the way Frege [1884, §62] addressed the epistemological challenge to platonism:

> How, then, should a number be given to us, if we can have no idea or intuition of it? Only in the context of a proposition do words refer to something. It is therefore essential to define the sense in which a number word occurs.

A hint about how to understand the appeal contained in this passage to the famous "context principle" is given soon after in the same paragraph, where Frege goes on arguing that

> we must define the sense of a proposition
> "the number which belongs to the concept F is the same as that which belongs to the concept G"
> without using the expression 'the number which belongs to the concept F'.

Frege introduces a new terminology in order to make it clear what he is looking for: "a general criterion of identity for numbers". If a sign a is supposed to designate an object to us, we must have a criterion that decides generally whether b is the same as a, even if it is not in our power to apply this criterion. Dummett [1991, p.111] defines this "the most pregnant philosophical paragraph ever written" and connects it in an interesting way with what he takes to be the central lesson of the linguistic turn: conceive philosophical problem as problems about language. In the case at hand, the philosophical problem is that of the existence of mathematical objects and of our knowledge of them, which linguistically re-formulated becomes the problem of understanding whether mathematical language contains singular terms and which are the truth-conditions of the sentences involving such singular terms, in particular whether these truth-conditions are such that we can ascertain whether they obtain.[5] Frege makes a suggestion, focusing on the case of direction-terms in geometry:

> The judgement "line a is parallel to line b", or, using symbols, a//b can be taken as an identity. If we do this, we obtain the concept of direction, and say: "the direction of line a is identical with the direction of line b". Thus we replace the symbol // by the more generic symbol =, through removing what is specific in the content of the former and dividing it between a and b. We carve up the content in a way different from the original way, and this yields us a new concept

One natural idea is to extend such a treatment to the case of numbers by substituting (DIR) with what is called "Hume's Principle". Hume's principle generalize the simple observation that if (and only if) there were no polygamy or polyandry we could conclude that the number of wife is the same as the number of husband, which means that is a necessary and sufficient condition for the number of wife to be equal to that of husbands that every husband has a wife and only one and that there is no wife which doesn't have an husband.

[5]Dummett [1991, p.111]: "an epistemological problem, with ontological overtones, is [...] converted into one about the meanings of sentences".

HP The number of Fs = the number of Gs iff there is a 1-1 correspondence between the Fs and the Gs

Frege goes on dismissing this idea, for reasons we don't discuss here, but the point is that Neo-Fregeans thinks that he was wrong in this.[6] Abstract terms are introduced by means of generation principles, which are bi-conditionals of this form:

AP $\forall\alpha\forall\beta(\S\alpha = \S\beta \leftrightarrow \alpha \approx \beta)$

where § denotes a one place function from objects to objects and $\approx$ an equivalence relation. The point of the neo-Fregeans is that our knowledge of mathematical objects is yield by reflecting on abstraction principles and basic facts relevant for the right hand side of those conditionals. For instance, we can conclude that directions exists by simply drawing a line a and reflecting on DIR. Let's call the following line a:

a ————————————————————————————-

From the fact that a//a and that according to DIR a//a if and only if Dir(a)=Dir(a), we can conclude that Dir(a)=Dir(a), which, by existential generalization, yields that $\exists x$(x=Dir(a)). In the case of Numbers, the story is more complicated. Roughly stated, the Neo-Fregeans a result known as Frege's Theorem to argue that our knowledge of the existence of mathematical entities and their properties is logical in nature, thus vindicating Frege's Logicism. What Frege's Theorem shows is that a version of the Peano Axioms (and thus a version of Arithmetic) can be derived in Frege Arithmetic (FA), which is full second order logic plus a formulation in this language of Hume?s Principle. Many things are to be discussed about the claim that Frege's Theorem can be used to vindicate Logicism. It must be discussed whether the Neo-Fregean strategy can be extended beyond Arithmetic. Of course it can be contended that the ontological commitment of Second Order Logic are too heavy to let it count as Logic [Quine, 1970] and more importantly, that even if Second Order Logic counts as Logic, knowledge of some principles of Full-Second order Logic requires a prior knowledge of mathematical principles, so that it counts as Mathematics as well, and thus founding mathematical knowledge on Knowledge of Second Order Logic is just founding mathematical knowledge on Mathematical knowledge [Macbride, 2003]. Moreover, one can have doubts about the impredicative nature of the Full Comprehension Schema used in Full Second Order Logic and if one adopts a weaker version of Second Order Logic, a lot of interesting technical questions arise [Burgess, 2005]. It can also be questioned whether the fact the Arithmetic can be interpreted in FA is enough to make FA a foundation of Arithmetic, or even just that it should be so interpreted. But apart from this kinds of problems, there is still a basic worry left about the ontology of Abstraction Principles. The question is whether it is legitimate to use Abstraction Principles to prove the existence of various kind of abstract (mathematical) entities. Rosen [1993] considers this issue orthogonal to the question of how we should number theoretic statements. I don't think this is the way Neo-fregeans see the problem. As the previous quotations and the one at the beginning of Part One show, the central problem for Wright is that of accounting for our mathematical knowledge, in the

[6]They have to do with the so called "Cesar Problem".

sense of answering the question 'How do we know that number theoretic claims are true?". Neo-Logicism is supposed to answer this question: we know that by reflection on logic and abstraction principles. But even if the problems are connected for the Neo-fregeans, I agree with Rosen that they can be taken to be independent. For instance, even setting apart questions about how to interpret our mathematical theories, one can still wonder whether the argument sketched before to prove the existence of directions is sound or not . So, I propose to concentrate on the question whether reflection on Abstract Principles can be used to prove the existence of abstract objects.

3.3.3 Objection to Neo-Fregeanism and Wittgenstein

The neo-fregean claim that we obtain mathematical knowledge via the reflection about Abstraction principles invites the question of how it is possible to know that abstraction principles are true. Wright and Hale answer seems to be connected to their ideas about the nature of abstraction principles. Their main point about this seems to be that abstraction principles are stipulations by which new concepts are introduced and in this sense they are analytically true of those concepts and to know them means just to master a concept.

One way to reply to the neo-fregeans is to deny that the two sides of the biconditional of an abstraction principle could be seriously taken to be equivalent. The qualification "seriously" is clearly necessary, given that there is an obvious sense in which a new string of signs can be considered equivalent to an old one just by stipulation. If "P" is an uninterpreted letter we can always add it to our language and let is stand for whatever we want, including the fact that the line a is parallel to the line b. The same of course goes if instead of "P" we consider a (rather more complex) string of sings like "Dir(a)=Dir(b)". Of course it is not what Neo-Fregeans want to claim. Their claim is that the superficial syntax of direction statements should be taken seriously, which of course presupposes that Direction sentences do have a syntactic form, namely that of identity statements. So, according to neo-fregeans, directions statements do have a structure and cannot be considered as a mere concatention of sings. The reductionist objection to neo-fregeanism (see Wright (1983) and MacBride (2003)) presses them to show why should this be the case. How can we know that the terms figuring in the right hand-side of the biconditional are genuinely referential? Which guarantee do we have that they succeed in referring to something? asks the reductionist. Indeed there is a critique to Neo-Fregeans approaches to ontology which is parallel to that of reductionists. Roughly, the idea is that Hume's principle cannot be viewed as an innocent stipulation, as its status of analytical principle requires, because it has a huge ontological import. This critique is know under the label of rejectionism (see MacBride(2003)). Wright's reply to this charge is based on a complex view about metaphysical problems which brings in elements of Frege's context principle, a certain understanding of the linguistic turn, and Wittgenstein's critique to the so-called "Augustinian picture of Language". To be a singular term is to be used as a singular term. Linguistic categories are prior to ontological ones, our understanding of the notion of "object" is based on our understanding of the notion of singular term. The meaning of a singular term has to be explained in terms of its contribution to the meaning of the sentences in which it figures. It is in replying to the reductionist that Wittgenstein takes centre stage:

> the prime spur towards the "naturalist" tendency which finds abstract objects per se problematical is the idea, at the heart of the Augustinian conception, that some, however primitive, form of conscious acquaintance and hence, when acquaintance is naturalistically construed, some kind of causal relationship must lie at the roots of all intelligible thought of, and hence reference to objects of a particular kind. While this is not the place to enlarge upon the relevant points in detail, it is by no means an unusual reading of Philosophical Investigations to believe that the book as a whole accomplishes a compelling critique of this idea.

Wright's reply seems to be that once it has been shown that an expression exhibits the syntactical pattern of singular terms, there is no longer room for doubt that it is a singular term. And once the truth-condition for the sentences involving such an expression are fixed by laying down abstraction principles, there is a very uncontroversial way to ascertain that these condition hold and hence that the expression has a reference. If one has the impression that something else must be warranted in order to show that the expression has a reference, he is under the delusion of a misleading conception of reference, the Augustinian conception. The heart of Wittgenstein's critique to the Augustinian picture of language lies in the observation that naming cannot be the source of linguistic meaning because in order to name something we must already have assigned to it a place in our linguistic practices.

Wittgenstein criticizes the idea that naming is the source of meaning by criticizing the idea that a meaning can be attached to previously meaningless terms by ostensive definitions. The key point here is that, in order to identify the referent of a singular term, one must already master some kinds of concepts by which the referent is to be identified. In order to see this, Wittgenstein's consider the way in which an ostensive definition is given: we point to something and say "This is an X". If we do not specify what is the kind of thing we are pointing to, this practice cannot succeed in specifying the referent of X.

One could be pointing to a color, a shape, an object. The mere action of pointing in one direction leaves this completely underdetermined. In order to fix the reference, we must specify our definition adding something like that: "This color/shape/object is X". We need to know what kind of thing the referent is: this means we must know the grammar of the term we are defining, given that "Grammar tells what kind of object something is"(PI 373). This means we must already master (speak and understand) a meaningful language in order to engage in the practice of giving and receiving ostensive definition.

> Let us explain the word "tove" by pointing to a pencil and saying[...] "this is called 'tove' "[...] The definition can be interpreted to mean: "This is a pencil"
> "This is round"
> "This is one"
> "This is hard", etc. etc. [BL, p. 2]

> The ostensive definition explains the use- the meaning- of the word when the overall role of the word in language is clear. [...] One has already to know (or be able to do) something in order to be capable of asking a thing's name. [PI, 30]

> a great deal of stage-setting is presupposed if the mere act of naming is to make sense [PI 257]

It would be very interesting to contrast this remark of wittgenstein with the metaphysical problem of the nature of mathematical objects. The problem this time is not that of finding out whether numbers do exist, but what they are. Wright tries to argue from this that the demand for a criterion to identify the referent of a singular term as something given prior to the criterion to understand the meaning of the sentences in which it figures is something absurd . The critique to the Augustinian conception of reference is linked with the critique of what MacBride[2003, p. 127] calls a crystalline conception of ontology.

> The disagreement between the global rejectionist and the neo-logicist thus appears to come down to this. The rejectionist assumes that the structure of state of affairs is crystalline-fixed quite independently of language. By contrast, the neo-logicist assumes that states of affairs lack an independent structure, that state of affairs are somehow plastic and have structure imposed upon them by language.

Wright and Hale express this point in a slightly different way

> The metaphysics of abstractionism must rather be a form of quietism about the realist/idealist alternative that the objector here seeks to impose. We therefore reject the idea, implicit in Tractarian realism, that truth involves a transcendental fit between the structure of reality and the structure of our forms of thought; but equally we reject the idea that we structure an otherwise amorphous world by the categories in which we think about it. Rather we can merely acquiesce in the conception of the general kinds of things the world contains which informs the way we think and talk and is disclosed in our best efforts to judge the truth. This is all we can justifiably do. For we have no means of independent assessment of the issue of fit; and that unavoidable lack is no reason for idealism. Holding a pack of cards in one's hand really is holding four (token) suits and fifty-two token playing cards. These descriptions are not in competition for true reflection of the structure of the world. But to recognize that is hardly to fall into idealism. So with the parallelism of a pair of lines and the identity of their directions.

The difference is noted also by Macbride. Soon after the passage quoted above, he makes a very interesting suggestion. The real concern of the Neologicist, he suggests, should not be seen as taking position in the dispute between two competing metaphysical position, the crystalline and the plastic one. Rather, he could deny that it really makes sense to ask whether there is a correspondence between the categories of our language (e.g. names) and those of reality (objects).

3.3.4 Quine and the Crystalline conception of Ontology

The position envisaged in the end of the following paragraph looks much like a form of quietism. The interesting fact is that such a form of quietism can be attributed to the most famous modern advocate of a serious approach to Ontology, Quine. That Quine takes ontological question seriously is witnessed by the fact that he is explicit in rejecting the idea that they are question merely about the way we speak. True, ontological questions are, according to Quine, questions about which sentences of a certain form (existentially quantified statements) we should accept. This means that the ontological issue is framed in semantical terms. This does not mean, though, that for Quine it is a question about language and not about reality:

> It is no wonder, then, that ontological controversy should tend into controversy over language. But we must not jump to the conclusion that what there is depends on words. Translatability of a question into semantical terms is no indication that the question is linguistic. To see Naples is to bear a name which, when prefixed to the words "sees Naples", yields a true sentence; still there is nothing linguistic about seeing Naples.
>
> [Quine, 1980b, p. 16]

But now, consider a passage like this:

> The fundamental-seeming philosophical question, How much of our science is merely contributed by language and how much is a genuine reflection of reality? is perhaps a spurious question which itself arises wholly from a certain particular type of language.
>
> Quine [1953, p. 78]

Quine can support this position in a number of different ways. For instance, his attack to the factual/conceptual distinction parallels that between analytic/synthetic truths.

> My present suggestion is that it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement

An interesting fact is that he points out to a superficially different kind of argument when he writes that:

> Certainly we are in a predicament if we try to answer the [ontological] question; for to answer the question we must talk about the world as well as about the language, and to talk about the world we must already impose upon the world some conceptual scheme peculiar to our own special language.
>
> Quine [1950, pp. 632]

This resembles an idea of Wittgenstein:

> The limit of the language is shown by its being impossible to describe the fact which corresponds to (is the translation of) a sentence, without simply repeating the sentence. (This has to do with the Kantian solution of the problem of Philosophy) [CV, p. 10e]

To understand how it is possible to reconcile the view expressed by Quine in the two previous quotations, it is convenient to give a look at the recent exposition (and defense) of Quine's position by John Burgess in Burgess [2008]. According to Burgess, Quine maintains that we should accept our best scientific theories, and given that he maintains that they are ontologically committed to abstract object, we should accept this commitment (see section 6). A qualification, though, should be added in order to correctly understand this claim, according to Burgess. The fact that we accept science together with its ontological commitments doesn't reveal us the ultimate structure of reality, features of reality prior and independent of how we conceptualize it; rather it reveals reality as it presents itself to creatures like us, equipped with a conceptual apparatus which is the way it is in large part due to our biological evolution. Burgess draws a contraposition between Quine's position, which he calls naturalist realism and metaphysical realism. The contrast seems to resemble quite closely that between a crystalline and a quitestic conception of ontology sketched in the previous section:

> Metaphysical realists suppose, like Galileo and Kepler and Descartes and other seventeenth-century worthies, that it is possible to get behind all human representations to a God's-eye view of ultimate reality as it is in itself. When they affirm that mathematical objects transcending space and time and causality exist, and mathematical truths transcending human verification obtain, they are affirming that such objects exist and such truths obtain as part of ultimate metaphysical reality (whatever that means). Naturalist realists, by contrast, affirm only (what even some self-described anti-realists concede), that the existence of such objects and obtaining of such truths is an implication or presupposition of science and scientifically-informed common sense, while denying that philosophy has any access to exterior, ulterior, and superior sources of knowledge from which to 'correct' science and scientifically-informed common sense. The naturalized philosopher, in contrast to alienated philosopher, is one who takes a stand as a citizen of the scientific community, and not a foreigner to it, and hence is prepared to reaffirm while doing philosophy whatever was affirmed while doing science, and to acknowledge its evident implications and presuppositions; but only the metaphysical philosopher takes the status of what is affirmed while doing philosophy to be a revelation of an ultimate metaphysical reality, rather than a human representation that is the way it is in part because a reality outside us is the way it is, and in part because we are the way we are.
>
> Burgess [2008, pp. 1-2]

The label naturalist' alludes to the technical sense in which Quine employs this word[7]. According to him,

> ....naturalism: the abandonment of the goal of first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method.
>
> Quine [1981, p. 72]
>
> naturalism: the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described
>
> Quine [1981, p. 72]

The adoption of naturalism forces the quinean to be a realist in the sense of accepting the existence assertions implied by our scientific theories and refusing to take seriously the question "apart from what science and common sense claim to exist, what does really exist?".(See Burgess [2008, p. 2])

3.3.5 Carnap and the internal/external distinction

In the quotation of the previous section, Wright and Hale suggest that the only thing we can reasonably do, if we are trying to find out what exists, is to

[7] On Quine's and related forms of Naturalism see Maddy[2005] and Resnik[2005]

"acquiesce in the conception of the general kinds of things the world contains which informs the way we think and talk and is disclosed in our best efforts to judge the truth". But what is a "conception of general kinds of things the word contains"?

An answer goes back to Carnap's famous essay "Empiricism, semantic and Ontology"[Carnap, 1951]: this general conceptions are linguistic frameworks, i.e. rule-governed languages; an ontological question is internal if it can be answered following the rules of the framework, otherwise it is external; external questions are not meaningful and this is the source of the troubles in philosophy; the only way to make sense of an external question is to interpret it as a practical question about the opportunity to adopt a framework.

It can be useful to have a look at the test. Carnap's concern is to "show that that using such [platonic] language does not imply embracing a Platonic ontology but is perfectly compatible with empiricism and strictly scientific thinking"[[Carnap, 1951, p. 206]. He confronts two kinds of questions[8]

Class A : Are there material objects? Are there numbers?Do and insects share important anathomical properties?

Class B : Is there a piece of paper on my desk? Is there a prime number greater then one million? Are there properties?

Class A statements have some peculiar feature: they never question the reality of a whole system of entities, but only of a subclass of it. They seem to take for granted that there are numbers and material objects, and try to find how things stand relative to those department of the being.[9]

These kinds of question don't look like particularly philosophical, nor controversial: they can be answered by ordinary means, empirical analysis in one case and mathematical proof in the other. Carnap is very clear in explaining the difference between the two kind of questions.

> If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a linguistic framework for the new entities in question. And now we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind within the framework; we call them internal questions; and second, questions concerning the existence or reality of the system of entities as a whole, called external questions. Internal questions and possible answers to them are formulated with the help of the new forms of expressions. The answers may be found either by purely logical methods or by empirical methods, depending upon whether the framework is a logical or a factual one. An external question is of a problematic character which is in need of closer examination.
>
> Let us consider as an example the simplest kind of entities dealt with in the everyday language: the spatio-temporally ordered system of observable things and events. Once we have accepted the thing language with its

[8] The example are borrowed from Carnap's text, except the one about properties that is from Peter Van Inwagen

[9] In other words, the existence of numbers seems to be presupposed in those statements, not asserted. It is not by chance that the theme of presupposition is at core of an approach like that of Stephen Yablo, which is clearly inspired by Carnap's position

> framework for things, we can raise and answer internal questions, e.g., "Is there a white piece of paper on my desk?" "Did King Arthur actually live?", "Are unicorns and centaurs real or merely imaginary?" and the like. These questions are to be answered by empirical investigations.
>
> From these questions we must distinguish the external question of the reality of the thing world itself. In contrast to the former questions, this question is raised neither by the man in the street nor by scientists, but only by philosophers. Realists give an affirmative answer, subjective idealists a negative one, and the controversy goes on for centuries without ever being solved. And it cannot be solved because it is framed in a wrong way. To be real in the scientific sense means to be an element of the system; hence this concept cannot be meaningfully applied to the system itself. Those who raise the question of the reality of the thing world itself have perhaps in mind not a theoretical question as their formulation seems to suggest, but rather a practical question, a matter of a practical decision concerning the structure of our language. We have to make the choice whether or not to accept and use the forms of expression in the framework in question.
>
> If someone decides to accept the thing language, there is no objection against saying that he has accepted the world of things. But this must not be interpreted as if it meant his acceptance of a belief in the reality of the thing world; there is no such belief or assertion or assumption, because it is not a theoretical question. To accept the thing world means nothing more than to accept a certain form of language, in other words, to accept rules for forming statements and for testing accepting or rejecting them. The acceptance of the thing language leads on the basis of observations made, also to the acceptance, belief, and assertion of certain statements. But the thesis of the reality of the thing world cannot be among these statements, because it cannot be formulated in the thing language or, it seems, in any other theoretical language.
>
> Carnap [1951, pp. 206-208]

The acceptance of a framework consists, according to Carnap in a two-step procedure:

> First, the introduction of a general term, a predicate of higher level, for the new kind of entities, permitting us to say for any particular entity that it belongs to this kind (e.g., "Red is a property," "Five is a number"). Second, the introduction of variables of the new type. The new entities are values of these variables; the constants (and the closed compound expressions, if any) are substitutable for the variables.3 With the help of the variables, general sentences concerning the new entities can be formulated.
>
> Carnap [1951, pp. 213-214]

Carnap thinks he has found a vindicating explanation of this phenomenon: questions internal to a linguistic framework can be settled following the frameworks rule, and thus are not up to be particularly controversial. No wonder they do not regard the whole entity system: if one adopts a system, it must be non-empty, otherwise it would be pointless to use it. One can ask in an internal spirit "Are there numbers?", but this sounds much like "Assuming that there are numbers, are there numbers?" [Yablo 2008].

Questions from the Class B, then, force a non-internal reading.

> What is the nature of the philosophical question concerning the existence or reality of numbers? To begin with, there is the internal question, which, together with the affirmative answer, can be formulated in the new terms, say, by "There are numbers" or, more explicitly "There is an n such that n is a number". This statement [...] is rather trivial, because it does not say more than that the new system is not empty; but this is immediately seen [...] . Therefore nobody who meant the question "Are there numbers?" in the internal sense would either assert or even seriously consider a negative answer. This make it plausible to assume that those philosophers who treat the question of numbers as a serious philosophical problem and offer lengthy argument on either side, do not have in mind the internal question.
>
> Carnap [1951, p. 209]

What they mean, instead, is "a question prior to the acceptance of the new framework". What they are asking has nothing to do with our acceptance of the framework, but with the ultimate furniture of reality: it has to do with the world, not with the language. But Carnap goes on regretting that no precise sense has been given to this kind of question, which cannot be formulated in scientific language, giving that in order to speak a scientific language one needs to adopt a framework. Give up the frameworks, and there will be no rules according to which one can take a stance about a statement.[10]

Carnap's position is of course a kind of relativism, in the sense that existence questions make sense only relative to the background of a linguistic framework. This fits well with the suggestion by Carnap that ontological questions asked in an external fashion can become meaningful if they are intended as a practical questions about our decision to adopt a framework. Of course, nobody can force someone to adopt a framework, as nobody can force someone to adopt a concept.[11] This Carnapian point is fully in line with the strategy of Neo-fregans. Replying to Field, Wright makes clear that:

> you cannot force someone to accept a concept. What Field needs to deny is that it is possible to establish concepts with the characteristics - a priori truth of the relevant equivalences - which the platonist wants. Such a denial can only be based on disclosure of something unintelligible on the route.
>
> Hale and Wright [2001, p. 168]

3.3.6 Context Principle, Meaning as use and the Irrelevance argument again

The idea that the one should look to the way language is used in order to find the source of the meaning of the words is in some place employed by Wittgenstein with a certain amount of rhetoric:

[10]There seems to be a link here with Carnap's verificationism: external questions are meaningless because they lack a criterion of verification. Connect this with Wittgenstein's verificationism during the middle phase

[11]This is connected with Carnap's Tolerance principle, according to which "in logic there are no morals".

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

Passages like this are misguided as far as they suggest that all the efforts made by people who engage in ontological disputes arise only due to a lack of understanding that the meaning (the "life") of a sign should not be confused with its referent. That this is misleading is shown by the fact that Quine understood perfectly well the fact that "there is a gulf between meaning and naming" (see above)[12]. Quine thinks that serious ontological disputes have nothing to do with bizzare semantical thesis. They have to do with our USE of the language instead, as Wittgenstein recommends, and precisely with our willingness tho express sentences which, when paraphrased in the notation of first order logic, have a certain (existential) quantificational form.

At the same time, Neo-Fregeans also agree with Wittgenstein's idea that it is in the use of our language that the answer ontological disputes must be looked for. They disagree with Wittgenstein about the fact that:

> "A reality corresponds to the word 'two' " - [...] might mean almost everything. [LFM, p. 248]

because they think that "to be an object is to be the referent of a (possible) singular term" "reference is imposed on singular terms by their figuring in true statements of a given kind"[Wright and Hale 2001]. The impression that ontological question are irremediably confused arises only if one adopts a bad picture of what is at issue in debates about the existence of some kinds of entities, a picture which Neo-Fregeans are happy to give up:

> the possibility of some sort of independent, language-unblinkered inspection of the content of the world, of which the outcome might come to reveal that there was nothing there capable of serving as the referents of...numerical singular terms
>
> Wright [1983, pp. 13-14]

We have seen that in order to sustain this claim they make appeal to the Fregan view (shared by Wittgenstein) of the Context principle and to Wittgenstein's own critique of the Augustinian picture of Language.

[12]Quine is not just opposed to the idea that singular words must refer in order to have meaning. He also challenges the view according to which sentences need to express propositions in order to be meaningful- this latter idea seems to be the polemical target of the remark from Wittgenstein just quoted

Still there is one sense in which consideration about meaning as use can be used to argue that ontological disputes are in some sense moot. [13]This is strictly connected with the problem of the irrelevance argument.

Yablo (2009) use something very close to the considerations which are at the core of the irrelevance argument to argue for the claim that ontological questions such as that about the existence of numbers are objectively moot. This means that there is not a correct answer to those questions, in the same sense in which there is no correct answer to the question whether Tom Cruise is shortYablo [2009].

Yablo's argument can be presented in this way:

1. The only thing that is relevant for the referential capacities of mathematical expressions is their contribution to the meaning of the sentences in which their figures. We can take this to mean that the property of an expression to refer to some entity depends on the distribution of the truth-values among the sentences in which the expression in question figures

2. The distribution of truth-values would be the same in the case there were no numbers and in the case there were numbers Therefore:

3. The existence (or non-existence) of mathematical entities cannot be decided on the ground of our use of the language

C The question whether there are numbers is objectively moot

Premiss (1) is clearly inspired by the Context principle and much in the spirit of the Neo-Fregean view of the relations between language and reality discussed in the previous sections. Premiss (2) is based on Yablo's idea that the "real" content of our mathematical statements would be true even if the were no abstract entities ("The numbers of the martian moon = 4" would still be true even if there were no number 4, because it would still be true that there are four martian's moon). The passage between (2) and (3) and (3) and (4) can be justified by invoking a certain view of the relation between meaning and use, which can be seen as broadly Wittgensteinian.

Eventually, then, we must conclude that it is true that the idea that meaning of our words must somehow be displayed in the use we made of this word has a strong impact on the way we conceive ontological questions.[14]

3.3.7 Wittgenstein the meta-ontologist and the vacuity of Platonism

David Manley in [Manely, 2009, pp. 4-5] collects three ways to give shape to the vague idea that the mainstream approach to ontological disputes is somehow misguided:

[13]This is true even if Wittgenstein did not simply equate meaning and use.

[14]It would be very interesting to contrast this remark of wittgenstein with the metaphysical problem of the nature of mathematical objects. The problem this time is not that of finding out whether numbers do exist, but what they are. This problem is become prominent in contemporary philosophy of Mathematics from Benacerraf [1965]. The fact that it does not matter which kind of entities numbers are, as long as they have some structural features, could be explained by their being representational aids. Consider also Wittgenstein's dictum that "Grammar tells what kind of object something is" [PI 373]

1. The dispute is 'merely verbal' - somehow due to differences in the way the disputans are using certain terms.
2. Neither side succeeds in making a claim with determinate truth-value.
3. The right answer is much harder or easier to reach than the disputans realize, and as a result, the way in which they attempt to reach it is misguided.

What is interesting is that ll this views can somehow be attributed to Wittgenstein. Wittgenstein's claim that statements like "Number two exists" can mean "almost everything" seems to be quite in tune with the spirit of theme one. The idea discussed in the previous section, that ontological questions are objectively moot, is a variation on theme 2. Finally, it is worth noting that at a certain point Wittgenstein says that Platonism can be interpreted as a "mere truism"(LFM 239). This is quite in line with the spirit of Neo-Fregeans Platonism. According to [Burgess 2008] Crispin Wright once exposed his argument for the existence of numbers like this: if one posses the concept of "number" it is a triviality for him that if I have two hands, then the number of my hands is two, which in turn entails that there is something which is the number of my hands, which in turns means that there is something that is a number and hence that numbers exist.

This means that Wittgensteins considerations about the link between meaning and use are at the intersection of many important issues from which the meaning of ontological disputes depends.

Chapter 4

Ontology vs. Modality

According to some nominalists, a mathematical claim such as "There are infinitely many prime numbers" (Euclid's Theorem) should not be read as stating the actual existence of an infinity of abstract entities like numbers, but as claiming the possible existence of infinitely many concrete entities like the numerals (which are intended by nominalists as concrete tokens, inscriptions of chalk on the blackboard or of ink on the paper).

According to this strategy, Euclid theorem should not be read as:

1. For all number m, there is a number n greater than m such that n is prime but as:

2. Necessarily, for all numerals m, there could be a numeral n . . . , such that n is . . . (where ". . . " is replaced with an appropriate translation of the predicates "greater" and "prime" when applied to numerals - in the first case, "greater" would probably mean something like "longer")

This is an example of what in the terminology made famous by Quine is called trading ideology for ontology: one uses linguistic resources stronger than the usual ones (modal operators, in this case) in order to avoid quantification on dubious entities. It is also interesting to note that Quine's advice was definitely against the trade.

> long ago, Goodman and I got what we could in the way of mathematics [. . .] on the basis of a nominalist ontology and without assuming an infinite universe. We could not get enough to satisfy us. But we would not for a moment have considered enlisting the aid of modalities. The cure would have been far worse than the disease [Quine, 1986, p. 397]

The reason for this choice, as he explains, is that his "extensionalist scruples outweight the nominalistic ones"[Quine, 1986, p. 397], which means that he considered modal talk as irremediably obscure, whereas abstract entity like sets have precise identity criteria (provided in this case by the axiom of extensionality). Quine's position seems to have become the received view, as least as far as the language of modern mathematics is concerned. As Burgess and Rosen explain [Burgess and Rosen, 1997, p. 125] where "the ancient practice was to speak of 'what is possible to produce' ", the modern one is "to speak of 'what exists' ", so that instead of saying (as it is done up today in informal talk) that "an integer or equation or function or space or formula [...] as being one that can be divided or solved or differentiade or metrized or proved" in the "formal definition" one says of such an integer, equation, function, space, formula, "that it is one for which there exist a divisor or solution or derivative or metric or proof".

But of course, modern nominalists would just say that the real obscurity is to try to make sense of the possible existence of some system of concrete entities by postulating the actual existence of abstract entities thus committing a typical flaw of reification. How could the existence of another world (to which, by the way, we have no epistemic access) explain what is possible in the actual world? Given that modal claims should be accounted for and that they cannot be accounted for by invoking abstract entities, should we not be better to renounce to this entities altogether?

Within the perspective of the present work, it is quite natural to wonder what Wittgenstein would have thought about this kind of debate and the natural supposition is of course to align him with the ontologically parsimonious nominalists. There are passages where he considers, and eventually rejects, the modern substitution of the language of possibility of some mathematical construction with that of actual existence of some kinds of abstract objects:

> Frege, who was a great thinker, said that although it is said in Euclid that a straight line can be drawn between any two points, in fact the line already exists even if no one has drawn it. The idea is that there is a realm of geometry in which the geometrical entities exist. What in the ordinary world we call a possibility is in the geometrical world a reality. In Euclidean heaven two points are already connected. This is a most important idea: the idea of possibility as a different kind of reality; and we might call it a shadow of reality. [...] there is something fishy [about this argument] [LFM, 144-145]

Another hint about a possible overlap with modal nominalists is that a recurrent theme in Wittgenstein's critique of Set Theory is his polemics with the conception of mathematical infinity as actual infinity, where an infinite set is conceived as a completed totality of objects, whose member all exist (at the same time, or, more properly, out of time). Wittgenstein's agrees with the nominalist that the only clear thing, when speaking of the infinite number of some mathematical entities, is that we posses a linguistic technique (rule) to generate the (names of) ever new numbers:

> If one were to justify a finitist posit in mathematics, one should say just that in mathematics 'infinite' does not mean anything huge. To say 'There's nothing infinite' is in a sense nonsensical and ridiculous. But it does make sense to say we are not talking of anything huge here [LFM, 255]

> The infinite number series is only the infinite possibility of the series of numbers [PR 144]

> the concepts of infinite decimals in mathematical propositions are not concepts of series, but of the unlimited technique of expansions of series [RFM V 19]

Despite this similarities, Wittgenstein does not coincide with that of modern nominalist, and for interesting reasons. Victor Rodych, in Rodych [2000b, p. 249] calls the choice between Platonism and Modal Nominalism "Quine's dilemma" and argues that Wittgenstein's position avoids both horns of the dilemma. I agree with his diagnosis, but I think that some terminological clarification would allow us to understand why Wittgenstein would have rejected both, the contemporary version of modal nominalism as well as classical Platonism. The problem lies in the fact that beyond the disagreement between these two positions about the ontology of Mathematics, there is a fundamental consensus on the issue of the objectivity of mathematics. In the next section, I will give a precise meaning to this two labels.

4.1 The Problem Of The Objectivity of Mathematics In The Contemporary Debate

4.1.1 The Standard Analysis

We already encountered in Chapter 2 the distinction between the two issues (see for instance Shapiro [2005a, p.6], and Linnebo [2009]:

THE QUESTION OF ONTOLOGICAL REALISM Do Mathematical objects REALLY (mind- language-culture-independently) exist?

THE QUESTION OF SEMANTICAL REALISM (OBJECTIVITY, OR REALISM IN TRUTH VALUE)] Are our mathematical statements true or false independently of our capacity to decide which is the case?

Realism in ontology and realism in truth value are the view according to which the correct answer to the first and the second question respectively is "yes" (anti-realism in ontology and in truth value are defined correspondently).

Usually the term *Platonism* is used to denote a combination of realism in ontology and realism in truth value and *Nominalism* is simply defined as the (ontological) view according to which there are no abstract objects and therefore no mathematical objects, given that usually mathematical objects are taken to be abstract.

The important point is that the semantical and the ontological issue are held to be independent. One simple way to see this is given by a consideration made by Stephen Yablo [2001, pp. 88-89]. Whether there are numbers or not seems to be quite immaterial for the problem of deciding whether our CONCEPTION of the mathematical reality is determinate or not, in the sense of making all the relevant statements true or false. In one direction, if there where no numbers, but we did have a conception of the structure of the natural numbers such that in every number-like system of objects every numerical claim has the same truth-value, we could maintain realism in truth-value without embracing ontological realism.

In any case, a look to the literature suffices to prove that one can be a semantical realist without embracing Platonism: according to the modal Nominalism of Hellman [1989] mathematical statements can be analyzed as modal statements, which have a determinate truth value even if they lack ontological import.

In the other direction, it is not so easy to find a case of someone embracing ontological realism and rejecting semantical realism. Such a position seems puzzling, because there is a PRIMA FACIE argument for the conclusion that ontological realism implies semantical realism. If mathematics is the description of mind-independent objects, as ontological realism maintains, then the truth-value of mathematical statements depends uniquely upon the properties and relations of these objects and not upon our ability to determine which are these. This seems to be confirmed by the fact that as paradigmatic example of advocate of semantical anti-realism it is often cited the case of intuitionists, according to which mathematics is the product of human mental activity and for a mathematical theorem to be true or false it needs to have been experienced as true or false, so that there cannot be "unknown truth[s]" in mathematics [Brouwer, 1948, p. 90]. Most of the versions of Intuitionism are presented as version of mathematical idealism, so that the typical intuitionist is an anti-realist in ontology as well (numbers are mind-dependent entities).

Despite this, there seems to be someone [Tennant, 1997] holding a position that can be qualified as ontological realism and semantical anti-realism and such a position is certainly in principle conceivable. To see this, it is useful to follow a suggestion from Wright [1983, Introduction]. Considering Dummett's case for intuitionism [Dummett, 1975], one notices that it is based on an argument (the so called "manifestation argument", which aims to show that the meaning of a sentence should be identified with its assertibility conditions, instead as with its truth-conditions) that applies in the mathematical as well in the non-mathematical case. If this is correct, a dummettian should be a semantical anti-realist also concerning our discourse about material objects, but this of course would not preclude him to hold that material objects really exist. As far as the mathematical and the non-mathematical case are parallel, this seems to show that an intuitionist of this fashion could hold that mathematical object exist as objectively as physical ones, but in both cases for a statement to be true it must be possible to know in principle how to verify it.

But even in this case, there is a simple consideration to show that the assumption of the existence of mathematical objects isn't enough to guarantee mathematical objectivity. The passage from Yablo [2001]quoted above goes on by asking what would happen if our conception of the natural numbers where not determinate, in the sense that for some claim S, in some number-like structures it is the case that S and in some others it is the case that not S. The only way such a claim S could receive a determinate truth value non arbitrary would be to say that just one of the structures answering to our conception exists, so that S receives the truth value it has in this structures. But this would make the truth of mathematical statements dependent on what exists in an unacceptable way.

> This, however, makes it a (conceptually) contingent matter which arithmetical claims are correct. There will be arithmetical claims S that are true in our mouths, but false in the mouths of our intrinsic duplicates, false in the mouths of (conceptually)possible people just like us internally but who live in a universe with undetectably different numbers. Arithmetical concepts are not supposed to be externalist in this way. It should not be that although I am right when I say that there are infinitely many primes differing by two, my doppelganger on Twin-Prime Earth is wrong when he says the same thing. If there are infinitely many twin primes, the reason should not be that such and such are the number-like entities that happen to exist.
>
> [Yablo, 2001, p. 88]

The point is that even if there were structures of abstract entities, as long as it were indeterminate to which structures we are referring when doing mathematics, the truth value of our mathematical claims would be indeterminate as well.

> So the number-hypothesis, conceived as objectivity-bolstering, is faced with a dilemma. If we are clear enough about what we mean by it, then the hypothesis is not needed for objectivity. And if we are not clear what we mean, then it is not going to help. It is not even going to be tolerable, because arithmetical truth is going to blow with the ontological winds in a way that nobody wants.
>
> [Yablo, 2001, p. 89]

Similar considerations are at the heart of a very interesting example of a Platonist position compatible with anti-realism in truth values presented by MR, which also lead to a reformulation of the question of the objectivity of Mathematics in different terms, as witnessed, for instance by Field [2001, essays 11 and 12].

4.1.2 Field on Objectivity and Putnam's Model-Theoretic Argument

Both in the classical and in the Dummettian version, intuitionism requires a revision of our logic and an approach to semantics alternative to the truth-conditional one, at least in the mathematical case. One could wonder whether it is really necessary to take such a radical position in order to be a semantical anti-realist in mathematics. Hartry Field [2001, essays 11 and 12] presents a version of anti-objectivism that is compatible with classical reasoning and that is quite independent from any issue linked with intuitionism. The first step is a reformulation of the objectivity issue in terms of question about the STATUS of undecidable sentences of a mathematical theory. In the jargon of Mathematical Logic, a sentence S is called provable iff it is possible to derive S from the axioms of the System by a sequence of application of the rules of inference. On the other side, S is called refutable iff the negation of S is provable. An undecidable sentence is one which is neither provable nor refutable. The central question for Field [2001, p. 336] becomes:

DTV Which undecidable sentences do have a determinate truth-value?

Field goes on defining an objectivist (what we previously called a semantical realist) as one who would answer "all" to this question, whereas anti-objectivist could be of a moderate or extreme sort, depending on his choice to answer "only some" or "none".

This is quite in line with the spirit of the previous definition of the issue of objectivity: if someone is inclined to think that grasping the meaning of mathematical sentences is a matter of understanding their truth-conditions, which are quite independent from our ability to recognize when they obtain, she should not have any problem in accepting the idea that a sentence can have a truth value quite independently from the possibility to decide by formal means which is that.

The interesting fact about this way of formulating the problem of objectivity is that it helps to make clear that the anti-objectivist position could be maintained even from an ontologically realist point of view.

The argument for this conclusion goes back to Putnam [1980]: even if we assume the point of view of classical model-theory, according to which the truth of a given formula in a model depends only upon the features of the model in question, there is still room to claim that the truth value of a formula is an open question, as long as it is an open question in which model the sentence has to be valued. If a sentence is undecidable, there must be different kind of models that are such that they are all compatible with our practice (all these models satisfy the axioms of our mathematical theory); nonetheless, in some of this models the sentence in question is true and in some other is false. As far as they are all compatible with our mathematical practice, there is no fact of the matter as to which kind of model is the intended one, so there is no fact of the matter as to whether we should identify the truth of the mathematical sentence as truth in one kind of model or in the other, and so there is no fact of the matter as to whether the sentence is true or not. This is the so called MODEL-THEORETIC argument attributed to Putnam, which is based on the thought that as long as the MEANING of our mathematical vocabulary is indeterminate, TRUTH is indeterminate as well, given that the truth of a sentence always depends on the way a sentence is interpreted. This yields the conclusion that undecidable sentences lack a truth-value, given that there are different interpretations, all compatible with our practice, which assign different truth-condition to them.

It is possible to read some passages from Wittgenstein as quite in line with the spirit of Putnam's argument:

> We multiply 25 X 25 and get 625. But in the mathematical realm 25 X 25 is already 625. - The immediate [objection] is: then it's also 624, or 623, or any damn thing- for any mathematical system you like.- If there is a line drawn there between two points, there are 1000 lines between the points - because in a different geometry it would be different. I'm not saying anything against that picture. We don't yet know how to apply it. There would be an infinity of shadowy worlds. Then the whole utility of this breaks down, because we don't know which of them we're talking about. As long as there is only one, we know where to go to find out what we want. I am to make an expedition into one- but which? You might say, "I want to go into a world where a straight line really does connect two points."-Yes, but there is an infinity of those. And an infinity of consequences follow, etc. You never get beyond what you've decided yourself; you can always go on in innumerable different ways. The whole thing crumbles because you are always making the assumption that once you are in the right world you'll find out ? You want to make an investigation, but no investigation will do, because there is always freedom to go into another world. [LFM 145]

What is interesting is that the idea that there is a connection between the problem of the meaning of mathematical statements and the question of decidability is a constant thought of Wittgenstein; moreover, there seem to be materials that enable us to attribute an anti-objective position (in the sense made precise in the previous lines) to Wittgenstein, a position which can be interestingly compared with the one of an advocate of Putnam's model-theoretic argument.

4.1.3 A problem for extreme anti-objectivism

Wittgenstein, as it is made clear from his discussion of Gödel's (first) incompleteness theorem, was an extreme anti-objectivist. There is an oscillation in his writings between the thesis that calling a mathematical sentence true could only mean that it is provable, and that it could only mean that it is proved. But setting aside the problem of the possible relevance of this oscillation (see section 6), this position in any case implies extreme anti-objectivism.

One way to make extreme anti-objectivism look plausible is to appeal to the fact that it seems a mere platitude to say Mathematical activity consist in the purely syntactical activity of deriving theorems from a theory. We assert a statement when we are presented with a proof of it, and in this sense we can call it true; when we are presented with a reductio ad absurdum of the theorem we deny it. Why should one suppose that anything more than this is going on?

> a cogent disproof of [the picture of mathematics as a purely symbolical activity] would seem to be up against the insurmountable task of denying that the mathematical formalisms we have created, the formalisms with which we work, are not exclusively systems of symbols and syntactical rules. Certainly, they are this, but how, one wonders, can one cogently prove that mathematics is more than these systems? Given that we cannot exhibit Platonistic mathematical objects and we cannot exhibit a human access to a Platonistic realm (e.g., one available to anyone with the requisite training), it seems that the would-be critic must somehow show that a purely syntactical conception of mathematics cannot do something that it must be able to do (e.g., reconstruct all of transfinite mathematics, or provide finitistic absolute consistency proofs of elementary mathematical calculi).
>
> Rodych [2001, p. 532]

The kind of problems I would like to discuss have to do with a possible candidate of "thing" that a purely syntactical conception of mathematical truth "cannot do". What it cannot do is to account for our tendency to think that, in the case of some undecidable number theoretical statements, the thesis that they do not have any determinate truth-value has undesirable consequences.

Consider [Field, 2001, p. 318] the case of a sentence S of PA of the form 'for every n, B(n)" where "B(x)" is a decidable predicate, that means that for every numeral n, we have at hand a method for deriving in PA "B(n)" or its negation. Suppose that for some n, the application of the method to n yielded "not B(n)": from this we could easily refute S (being B(n) an instance of S). If S is undecidable, S is not refutable, which means that there is no numeral n such that we prove in PA not B(n); but then, being "B(x)" a decidable predicate, it must be the case that for every numeral n, B(n) is provable in PA. The situation seems to be this: S is a generalization such that all its instances are provable, hence true. Why should not we conclude that S is true?

It is useful to compare different replies to this argument. One way is to question that every number must be named by some numeral, but one could just define the truth clause for "for all x, A(x)" as "for every numeral n, A(n)"(see Smullyan [1992, Chapter I] for an example).[1]The model-theoretic way of Putnam is more radical: roughly, it says that it is not clear what we mean with "there is

[1]Moreover, in some systems it can be provable that every natural number is "denoted" by

at least one numeral". If S is undecidable, there are different interpretations of the expression "there is at least one numeral such that?" that are all compatible with our practice (they all satisfy the axioms of our arithmetical theory), but in some of these interpretations S turns out true, in some other false. As long as both interpretations satisfy the axioms, there is no fact of the matter as to which one is the intended one, so there is no fact of the matter as to whether S is true or not.

One problem with this approach which has been stressed in the literature has to do with the fact that there seems to be a way to discern the 'intended" models of arithmetic by appeal to our grasp of the notion of "finiteness". Consider [Field, 2001, pp. 337-338] a theory N+ which is just like a normal formalization of Arithmetic (say first order Peano Arithmetic), but to which we add as an axiom the claim that every natural number as only finitely many predecessors. One can do it by introducing a quantifier "F" to be read as "there are only finitely many", or alternatively defining a "finite" predicate, which can be done if we allow our theory to contain some set-theoretic machinery. The logic of the quantifier "F" is not formalizable, in the sense that there are models of N+ in which a sentence of the form "Fx A(x)" is true even if there are infinitely many items in the models which satisfy "A(x)". Still, one would like to rule out these models as "unintended", in virtue of our alleged grasp of the notion of "finiteness": if these models are left out, the remaining ones are all isomorphic with each other, which means that either a sentence is true in all of these models, or it is false in all of them; so, even undecidable sentences could receive a determinate truth value, depending on their being true or false in all the intended models of N+.

One could try to respond to this reasoning by applying the basic strategy of Putnam's model-theoretic argument to the present case: given that the are models of N+ which satisfy the axioms, but which assign incompatible extensions to the predicate "finite", there is no determinate notion of finiteness. Still, this is quite an unwelcome conclusion. The reason why it is so has been clearly stated by Hartry Field [Field, 2001, p. 338]:

> the notion of finiteness is after all a central key ingredient in many key notions, such as that of sentence in a given language (sentences being certain finite strings of symbols meeting certain conditions) and that of a proof in a given system (proof being certain finite strings of sentences). Any indeterminacy in the notion of finiteness would doubtless infect the notions of sentence and proof, and of logical consequence and logical consistency (this is so even if consequence and consistency are not defined in terms of proof - or in terms of model either-, but are related to proof and model indirectly by a "squeezing argument": see Field[1991]).

The fact that such a conclusion would be a hard pillow to shallow by itself provide no conclusive argument for rejecting the idea of an indeterminacy in the notion of finiteness, but it still points out that extreme anti-objectivism looks unattractive for a "commonsensical guy" [Field, 2001, p.]. Not so for Wittgenstein, who would have accepted the idea that, in some sense, our notion of proof in a given system and the cognate notion of consistency are in some sense relative. In order to see this, we need to give a closer look to some aspects, already mentioned in the first part, of his position.

---

some numeral: of course, if the model is non standard, there will be non-standard numerals as well as non-standard natural numbers.

4.2 Wittgenstein's position

Wittgenstein's reply to the problem for anti-objectivism just considered is more complex and involves some of the pivotal theses of his view of mathematics. In order to understand them, it could by useful to briefly compare Wittgenstein's position with that of another anti-objectivist position, the Intuitionist one.

Intuitionists hold that for a statement to be true it must have been proved to be true and for a statement to be false it must have been shown to be contradictory; they do not claim that for a mathematical statement to be meaningful we must have AT HAND a decision procedure for it, which means an effective method for settling the question whether the statement or its negation are correct, a method for proving or refuting it. By contrast, this seems to have been Wittgenstein's position, at least in the middle period of his carrier (1932-39). The central idea in this phase is a connection between the issue of decidability and that of MEANINGFULLNESS [Rodych, 1997]: the meaning of a sentence is just its use and we know how to use a mathematical statement only if we know how to decide it, so that a sentence has meaning just as long as there is a KNOWN decision procedure associated to it. This has to do with Wittgenstein's idea that mathematics is purely syntactical in nature, just "a calculating machine" [WVC 106], so that the meaning of a sentence lies in its computation, in what Wittgenstein calls its "proof", which can be a formal derivation or just a daily-life computation (the usual calculation of "23+2= 25" counts as a proof in Wittgenstein's jargon).

There are some oscillations in Wittgenstein's view, at least during the Middle Period. It is possible to ascribe to him the position according to which a mathematical proposition has a sense iff we have an applicable decision procedure for it and the sense of the proposition lies in the decision procedure associated to it. According to Frascolla 1994, this is Wittgenstein's position in his intermediate phase. Even Rodych [2008b] seems to subscribe to such an interpretation, when he writes that according to the intermediate Wittgenstein "an expression is a meaningful (genuine) proposition of a mathematical calculus iff we know of a proof, a refutation, or an applicable decision procedure" and the textual evidence he refers to ([PR 151], [PG 452], [PG 366], [AWL 199-200]) seems to confirm it. (I am resting on the equivalence "being meaningful= having a meaning = having a sense").

It is also possible to read Wittgenstein [Rodych, 2008a] as arguing that a certain concatenation of symbols is a mathematical proposition iff we have at hand a decision procedure for it, but that this does not imply that such a concatenation of signs has already a sense BEFORE its proof has been carried out; rather, it receives a sense from its proof. Moreover, the sense of the mathematical proposition is its proof, which implies that a proposition that is not proved does not have a sense and this in turns implies, al least as far as only consistent systems are taken into account, that refuted mathematical proposition do not have a sense. On this account there would be a difference between (a) string of symbols that are mathematical propositions but which lack sense, because they have been refuted and (b) strings of symbols that are not even mathematical propositions, because we don't have associated any decision procedure to it. An incorrect arithmetical sum as "23+2=43" belongs to the first category, a mathematical problem like Goldbach's Conjecture (henceforward GC) to the second. It is useful to recall a summary of this reading, borrowed from Rodych

[2008a, p. 86](“Csign” abbreviates “concatenation of signs”):

A1 Mathematical Proposition: A Csign is a mathematical proposition o Calculus gamma iff it is algorithmically decidable in calculus gamma and we know this to be the case.

B1 Having Mathematical Sense. Only primitive propositions (e.g. axioms) and proved propositions of calculus Gamma have mathematical sense in calculus Gamma.

C1 The sense of a Mathematical Proposition of Calculus Gamma is its syntactical position in the syntactical structure that is calculus Gamma.

(But compare with the fact that appealing to the extra-systemic applications Wittgenstein says that there CAN be two proofs of the same proposition. Possible reconciliation: the same proposition can have different syntactical sense, but it is still the same proposition because it has the same extra-systemic applications).

A consequence of this view is that mathematical statements are always intra-.systemic: a proof is system-relative and given that the sense of a mathematical proposition is its proof, the proposition is system-relative as well. Moreover, given that a proof is a sequence of well formed formulas obtained from some axioms applying some rules, the sense of a mathematical proposition depends essentially form its inferential links, i.e. from its positions in the derivations of the system.

In any case, decidability in Wittgenstein's sense is at least a necessary condition for being a meaningful mathematical proposition. A meaningful question in mathematics MUST be one that is possible to settle: “We may only put a question in mathematics (or make a conjecture) where the answer runs: “I must work it out” [PR 151]” . It must be stressed the EPISTEMOLOGICAL constrain imposed by Wittgenstein about what counts as a decision procedure: for a sentence to have meaning, we must KNOW a method for constructing a proof of the sentence or of its negation. To see the point, consider the case of Golbach's Conjecture:

GC Every even number greater then 2 is the sum of two prime numbers

As far as we know, there is no *finite* procedure that can answer the question whether (GC) is true or not. One can look for counter-examples, but the search cannot be exhausted, being infinitely many cases to be checked. Of course, one can try to find *a reductio ad absurdum* of GC, or try to prove it by induction. Wittgenstein's point is that even if one succeeds in that, it would only by a bit of genius or luck, not the product of a systematic search (there is none) and this makes a big deal of difference: if one proves GC, she does not show that a meaningful statement is true, but confers a sense to a previously meaningless string of symbols. This is true even if the proof is carried out using just the usual rules of a system like Peano Arithmetic (henceforward PA). It is a pivotal claim of the later Wittgenstein that a proof are invented and not discovered, so that a proof doesn't exist before it is laid down and it makes no sense to say that a proof of GC in PA founded today shows that GC was already provable (therefore, decidable) in PA yesterday: “the later Wittgenstein agrees with the intermediate Wittgenstein that the only sense in which an undecided mathematical proposition

[RFM VII 40] can be decidable is in the sense that we know how to decide it by means of an applicable decision procedure" [Rodych, 2008b].

Consider now the position of two version of intuitionism on the problem of mathematical conjectures (i.e. propositions for which we have no decision procedure at hand). The point of contrast is just the same: according to intuitionists, these claims have a precise sense, whereas for Wittgenstein they don't.

According to Brouwer, a mathematical claim can be meaningful even if we don't know how to prove or refute it: the fact that we lack of a decision procedure for it implies only that it lacks a truth-value. The problem can be seen in this light: according to Brouwer and classical intuitionism, the sense of a mathematical claim is quite independent from its truth-value; on the other side, for Wittgenstein there is no gap between understanding of the meaning of a mathematical sentence and knowing its truth value, and this marks the difference between empirical and mathematical propositions (see Frascolla [2004] for a similar formulation).

This of course has to do with the different conception of MEANING of the two. For Brouwer, a mathematical claim express the request for a certain mental construction and to understand the sense of a mathematical proposition is to understand what construction is demanded. This implies that for Brouwer GC is claim about all the natural numbers, whereas for Wittgenstein is not even this: it simply lacks any meaning. This has to do with Wittgenstein's view about mathematical quantification, which is linked with his general conception of the meaning of mathematical proposition as completely non extensional and non referential: we do mathematics bit-by-bit and as long as the mathematical entities are created by such a process, there is no more to them then what we have proved about them. One could even say that mathematical sentences are ABOUT mathematical objects, but this must be read in line with the considerations of the previous chapter: not the numbers (and sets, etc?) as they are, but the numbers (and sets, etc?) as they are imagined to be are the subject matter of pure mathematics, and Wittgenstein clearly thinks that there is no more to our conception of mathematical entities than what we have proved about them.

> it is not possible to appeal to the meaning [Bedeutung] of the signs in mathematics, because it is only mathematics that gives them their meaning [Bedeutung] [RFM V 16]

Another way to detach the knowledge of the meaning of a mathematical proposition from the knowledge of its truth value is to identify the meaning of a mathematical proposition with the logical structure of his proof, as suggested by Michael Dummett's version of intuitionism (see for instance Dummett 1997). Against this conception Wittgenstein could point out that the "logical structure" of a statement is just the net of inferential links between it and other sentences of the calculus to which it belongs. So, once again, to know the meaning of a sentence is to know its proof.[2]

[2] On this point see Wright [1980].

4.3 Logical Objectivity and the Rule Following Considerations

The extreme anti-objectivist, as defined in section 1.2, doesn't hold such an extreme position, contrary to what the name suggests. Certainly he is not required to be an anti-objectivist about logic: he can maintain that the question whether, for any mathematical claims P and Q, Q logically follows from P has a perfectly determinate answer. Stated otherwise, he could be an objectivist about logical consequence. This means that one could think that even if there are no mathematical entities, there still are logical facts, in the sense that every question about logical consequence admits only one correct answer.

One could maintain, for instance, that, at the ontological level, all the mathematical objects that exist are the signs we use (conceived as tokens, concrete inscriptions made of chalk or ink, or sequences sounds) plus maybe something like our psychological states (possibly conceived as brain states). Still, one could think, when the axioms and rules of inferences of our mathematical calculi are laid down, there is a fact of the matter about what we can derive in the system and what we cannot derive. Even if there are no "unmade calculation" and "unproved theorems"[Rodych, 2000b, p. 274], this is compatible with holding that even before any application of the rules of inferences to the axioms, it is perfectly determinate whether we could get a certain formula as a theorem, whether it is possible to derive it. Moreover, it seem to be clearly a determinate fact whether it is possible to obtain a certain result applying a rule of inference to some set of sentences. The matter here is not what there is, but whether we have a standard for the correctness for modal claims of a certain sort, namely claims about the derivability of a conclusion from some premises.

Not so with Wittgenstein, and in two different senses: the first has to do with Wittgenstein's choice of decidability (in his sense) as a test for meaningfulness. Let us call a logical question a question of the form: "Is Q a logical consequence of P1, P2,. . . , Pn" The problem is this:

DTV2 Which undecidable (in Wittgenstein's sense) logical questions do have a determinate answer?

Wittgenstein's answer to this question is none, for the reasons exposed in the previous sections. Given that there is no general effective test for the relation of logical consequence, logical questions, posed without restrictions, are simply meaningless, according to Wittgenstein, just like it is meaningless to ask whether it is possible to obtain a sequence of four 7 in the decimal expansion of phi, without further specification. Once again, this is well reflected in Wittgenstein positions about the issue of contradictions. Wittgenstein denied that there is anything like "hidden contradictions"(see for instance [RFM III 78]). Once more, not just because the only sentences that exist are the one we have actually written. The point is that for there to be an hidden contradiction there must be a formula of a certain kind which is: 1) derivable from a system, but 2) such that we have no test which enables us to recognize whether it is derivable from the axioms of the system. Wittgenstein simply denies that there is something like that because, as long as the predicate "derivable from the axioms" is not decidable, the question whether the a formula is derivable in a system lacks meaning, exactly as the question whether a certain sequence of numbers is present in the infinite expansion of an irrational number lacks a meaning. The further expansion of an axiomatic system by deriving a new theorem is like the "further expansion of an irrational number" by calculating its digits: it is a further expansion of mathematics, in which new problems acquire meaning and answers. Take the case of Russell's paradox: points Wittgenstein claims that until it wasn't derived by Frege's Basic Law V, Frege's system was not inconsistent. It became inconsistent by Russell's reasoning, but was not discovered to be so. But Wittgenstein's perspective is even more radical than this. Consider an axiomatic System S which is syntactically complete in the logician sense: applying the rule of inferences to the axioms in a systematic way, one eventually obtain, for every well formed formula A, either A or not A as a theorem. The notion of logical consequence of the axioms for a system like that can be taken to be a purely formal one, i.e. derivability from the axioms. Moreover, applying the rules of inferences, we have, at least in principle, a mechanical test for the problem whether A is logical consequence of the axioms of S. Let us ask now to Wittgenstein: once the axioms and rules of inferences are laid down, but before any proof is actually carried out, does the question whether A is a logical consequence of S have a determinate answer? The problem becomes:

DTV3 Which undecided logical problems have a determinate truth-value?

The problem, of course, has to do with the much celebrated Rule Following considerations. The central issue there is the relation between a general Rule and its applications. The problem, as emerges from the work of Saul Kripke [1982] and Michael Dummett, is that Wittgenstein seems to challenge an assumption usually made about this relation. The assumption is that once a rule is formulated, it is determined which is the correct application of this rule to every particular case, even before the application is actually made.

One way to see the importance of this point, is to consider an objection made by Quine to the idea that logical claims are made true by convention. The second point made by Quine is that even if mathematical axioms could turn out to be conventionally true in some sense, logic cannot be made true by convention simply because logical truths are infinite whereas we can only stipulate a finite number of conventions.

Even if we regard logical axioms as true by convention, this still does not fix the extension of logical truth, given that logical truth, in every formal system are all the logical consequences of the axioms of the system.

Either we define logical consequence model-theoretically, as truth in all the models in which premises are true or we do it proof-theoretically, as the set of sentences provable from the axioms. In the latter case, of course, the definition is elliptical: something is provable from a set of axioms only relative to some set of rules of inference. It is here that the Quine's point comes into play. How are we to regard the rules of inference? A famous consideration put forward by Lewis Carroll seems to show that the role of inference in a formal system cannot be reduced to that of the axioms.

Take the case of Modus Ponens (MP):

$$\text{(MP)}\,\frac{\vdash P, \vdash P \to Q}{\vdash Q}$$

What does it mean to accept MP? It cannot be simply a matter of accepting a new axiom. If you just add to a formal system a new axiom P&(P→ Q)→ Q you simply get:

$$(\text{MP*})\frac{\vdash P, \vdash P \to Q \vdash P\&P \to Q}{\cdots}$$

This is simply a set of statements, where is the element that warrants the passage between these statements and another statements, namely Q?

The moral of the Lewis Carroll Paradox seems to be that Rules of inferences cannot be reduced to axioms; one could try do use this to show, contra conventionalism, that the notion of logical consequence cannot be reduced to a matter of accepting a convention. To accept a rule is a matter of convention, but Rules are not merely accepted or rejected, they must be applied and whether in a given case they are applied correctly or not is not a matter of convention. Or so it seems.

The problem is that there is a temptation to read Wittgenstein as rejecting precisely the contrast between the choice of a rule, a matter of convention, and the application of a rule, a non-conventional matter. This is precisely Dummett's interpretation of Wittgenstein's rule following remarks. According to Dummett [1994, p. 49] Wittgenstein thinks that what is necessary is simply what is "treated as being necessarily true" . The route of our necessity would be our decision to consider something necessary. Wittgenstein's claim that "the proof makes new connections, it does not show that they were already there" [RFM III 31 would mean, according to this reading, that before we derive a theorem, it was not a logical consequence of our axioms and our rule of inferences. It became a consequence of them due to our decision of treating it as such.

Before we accept something as the correct result of the application of a general rule to some particular case, there is no fact of the matter about which is the correct result of applying the rule to that particular case. Take the case of a calculation, for instance an addition, that we have not made an we will never make. According to Dummetts interpretation, Wittgenstein would have denied that there is a fact of the matter about which is the correct result to obtain. Not even God knows what we should obtain if we were to apply the rules of the addition to that particular case. If we don't perform the calculation, there would be nothing for God to know, because "it is only our doing the calculation and 'putting it in the archives' that constitutes its result as being that obtained by doing it correctly"[Dummett, 1994, p. 63].

We have seen in the first part the problems of this interpretation. Without taking a stance on this quite intricate problem, I will be content to highlight two points relevant in this context. First, there is a strong tendency by the friends of Wittgenstein to try to make his conclusions look much less radical than that attributed by Dummett. This attempts should be regarded with some caution. For instance, it has been argued that, when claiming that "the proof makes new connections, it does not show that they were already there" [RFM III 31], Wittgenstein is simply denying that "an unmade calculation, an intended action, and an unproved theorem exist" [Rodych, 2000b, p. 274]. Put in this way, one could even think that Wittgenstein is once again simply opposing realism in ontology and not the objectivity of mathematics.

> There is absolutely nothing logically incoherent about maintaining that our rules (e.g., modus ponens), as we have created them and understand them, compel or require (or permit) us to derive q rather than ¬ q whenever we have before us p→ q and p but this does not mean that q existed as a theorem of our system prior to our deriving it. [Rodych, 2001, p. 5]

One could add: the only answers that exist are those we have actually given, and of course there are no entities like truth-values, so unanswered question and unresolved problems lack truth-values, solutions and answers because there are no such entities. But the point is more radical: as we saw before, it is perfectly legitimate to be an anti realist about ontology and a semantical realist. One needs just to hold that it is established whether some logical claims are correct or not even if we don't know how to settle the question. When Wittgenstein denies that mathematical conjectures like GC have any truth-value, he holds this not because of ontological parsimony about entities like truth-values or answers, but because of his doctrine that undecidable problems lack a meaning. This is enough for being an anti-objectivist about logic. The point is that, in his later phase, he came to hold that even undecided problems lack a meaning.

Consider a subjunctive conditionals of the form, "if we correctly applied the rule R to the case a, we would obtain ...", where the rule R has not been applied to the case at hand yet. According to Wittgenstein, statements of this form simply lack any truth value, not just because there is no result of an unmade calculation, but because they even lack a meaning: in cases like that, the meaning of the phrase "correctly applying the rule R to the case a" is not determined. To understand this point it is worth recalling one of the key points of our exegesis of Wittgenstein, namely his stressing the difference between empirical and mathematical statements and his rejection of the notion of mathematical propositions. One of the key points about this issue is the fact that, according to Wittgenstein, the hallmark of genuine propositions is that one can understand their sense quite independently of knowing its truth value. In his intermediate phase Wittgenstein thought that this could apply to (decidable, in his sense) mathematical sentences as well, by identifying the sense of the mathematical proposition in question with its method of verification. It is exactly this assumption that seems to be challenged by the rule following considerations. The extension of the concept "correct application of the rule R" is "its ratified extension, [...] the class of actions which are acknowledged as being with R in the internal relation expressed by the formal dyadic predicate 'being a correct application of' or 'being in accord with' "[Frascolla, 1994, p. 122].

This brings us to the second point. According to Dummett, Wittgenstein's anti-objectivism about logical consequence is due to the assumption that "there is no truth beyond our aknowledgement of truth" Dummett [1994, p. 63]. If this were the case, we should conclude that if no one judges the position of the door, there is no fact of the matter about the door being closed or not Dummett [1994, p. 64]: admittedly, an implausible conclusion, so implausible that it constitutes for Dummett a *reductio ad absurdum* of the Rule Following Consideration. Dummett's example of the door is telling, because it involves clearly an empirical claim. Here lies the main problem with its interpretation: if Wittgenstein's motivation for anti-objectivisim about logic and mathematic lead also to an anti-objectivism about empirical statements, this would not fit very well with his insistence on the difference between empirical and logic-mathematical sentences. The crucial difference is that between empirical and normative sentences: the first state facts, the latter determine concepts. Of course, we do have empirical concepts beyond the mathematical ones; moreover, the reason why mathematical concepts are introduced is just to apply them to the empirical ones, so that mathematical statements are in the last analysis rules for the use of empirical concepts, just like all the other grammatical rules. The real contrast is between proposition whose correctness depends on how the world is done and proposition whose correctness depends on the way we shape the conceptual apparatus trough which we depict the world. Mathematical and logical statements belong to the latter type, and here lies the ultimate reason for Wittgenstein's anti-realism:

> If it is not supposed to be an empirical proposition that the rule leads from 4 to 5, then this, the result, must be taken as the criterion for one's having gone by the rule. Thus the truth of the proposition that $4 + 1$ makes 5 is, so to speak, overdetermined. Overdetermined by this, that the result of the operation is defined to be the criterion that this operation has been carried out. The proposition rests on one too many feet to be an empirical proposition. It will be used as a determination of the concept "applying the operation $+ 1$ to 4". For we now have a new way of judging whether someone has followed the rule: Hence $4 + 1 = 5$ is now itself a rule, by which we judge proceedings [RFM, VI 16]

Bibliography

Baker, G. and Haker, P. M. S. (1985). *Rules, Grammar and Necessity.* Blackwell, Oxford.

Balaguer, M. (1995). A Platonist Epistemology. *Synthese*, **103**, 303–325.

Balaguer, M. (1998). *Platonism and Anti-Platonism in Mathematics.* Oxford University Press, Oxford.

Benacerraf, P. (1965). What Numbers Could Not Be. In Benacerraf and Putnam [1983], pp. 272-296.

Benacerraf, P. (1973). Mathematical Truth. In P. Benacerraf and H. Putnam, editors, *Philosophy of Mathematics*, pages 403–420. Cambridge University Press.

Benacerraf, P. and Putnam, H., editors (1983). *Philosophy of Mathematics.* Cambridge University Press, Cambridge, 2nd. edition.

Berto, F. (2009a). Impossible worlds. In E. N. Zalta, editor, *The Stanford Encyclopedia of Philosophy.* Fall 2009 edition. `http://plato.stanford.edu/archives/fall2009/entries/impossible-worlds/`.

Berto, F. (2009b). The Gödel Paradox and Wittgenstein's Reasons. *Philosophia Mathematica*, **17**, 208–19.

Boolos, G. (1990). The Standard of Equality of Numbers. In G. Boolos, editor, *Meaning and Method: Essays in Honor of Hilary Putnam*, pages 255–274. Cambridge University Press, Cambridge.

Brouwer, L. (1948). Consciousness, Philosophy and Mathematics. In Benacerraf and Putnam [1983], pp. 90-96.

Burgess, J. P. (1983). Why I am not a nominalist. *Notre Dame Journal of Formal Logic*, **24**, 93–105.

Burgess, J. P. (1998). Occam's Razor and Scientific Method. In M. Schirn, editor, *The Philosophy of Mathematics Today*, pages 195–214. Clarendon Press, Oxford, New York.

Burgess, J. P. (2005). *Fixing Frege.* Princeton University Press, Princeton.

Burgess, J. P. (2008). *Mathematics, Models, and Modality: Selected Philosophical Essays.* Cambridge University Press, Cambridge.

Burgess, J. P. and Rosen, G. (1997). *A Subject with No Object: Strategies for Nominalistic Interpretation of Mathematics*. Clarendon Press, Oxford.

Burgess, J. P. and Rosen, G. (2005). Nominalism reconsidered. In S. Shapiro, editor, *The Oxford Handbook of Philosophy of Mathematics and Logic*, pages 515–535. Oxford University Press, Oxford.

Carnap, R. (1951). Empiricism, Semantics and Ontology. In Carnap [1956], pp. 205-221.

Carnap, R. (1956). *Meaning and Necessity*. The University of Chicago Press, Chicago.

Cheyne, C. (2001). *Knowledge, Cause, and Abstract Objects: Causal Objections to Platonism*. Kluwer, Dordrecht.

Chihara, C. (1977). Wittgenstein's Analysis of the Paradoxes in His Lectures on the Foundations of Mathematics. *Philosophical Review*, **86**(3), 365–381.

Colyvan, M. (2001). *The Indispensability of Mathematics*. Oxford University Press, New York.

Colyvan, M. (2008). Indispensability arguments in the philosophy of mathematics. In E. N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Fall 2008 edition. `http://plato.stanford.edu/archives/fall2008/entries/mathphil-indis/`.

Divers, J. and Miller, A. (1999). Arithmetical platonism: reliability and judgement-dependence. *Philosophical Studies*, **95**, 277–310.

Dummett, M. (1959). Wittgenstenstein's Philosophy of Mathematics. *The Philosophical Review*, **68**, 324–48. Reprinted in [Dummett, 1978], pp. 166-185.

Dummett, M. (1975). The Philosophical Basisi of Intuitionistic Logic. In Dummett [1978].

Dummett, M. (1978). *Truth and Other Enigmas*. Duckworth, London.

Dummett, M. (1991). *Frege: Philosophy of Mathematics*. Duckworth, London.

Dummett, M. (1994). Wittgenstein on Necessity: Some Reflections. In P. Clark and B. Hale, editors, *Reading Putnam*, pages 49–65. Blackwell, Cambridge, MA.

Field, H. (1980). *Science without Numbers: a defense of nominalism*. Blackwell, Oxford.

Field, H. (1989). *Realism, Mathematics and Modality*. Blackwell, Oxford.

Field, H. (1991). Metalogic and Modality. *Philosophical Studies*, **62**, 1–22.

Field, H. (2001). *Truth and the absecnce of fact*. Oxford University Press.

Folyd, J. and Putnam, H. (2000). A note on Wittgesntein "Notorius Paragraph" about the Gödel Theorem. *Journal of Philosophy*, **XCVII**(11), 624–632.

Frascolla, P. (1994). *Wittgenstein's Philosophy of Mathematics*. Routledge, New York.

Frascolla, P. (2004). Wittgenstein on Mathematical Proof. In A. Coliva and E. Picardi, editors, *Wittgenstein Today*, pages 167–184. Il Poligrafo, Padova.

Frege, G. (1884). The Foundations of Arithmetic. A Logico-mathematical Enquiry into the Concept of Number. trans. by J.L. Austin.

Geach, P. and Black, M. (1960). *Translations from the Philosophical Writings of Gottlob Frege*. Blackwell.

Gibson, R. F., editor (2004). *Quintessence*. Harvard University Press.

Glock, H. J. (1996). *A Wittgenstein's Dictionary*. Blackwell, Oxford.

Glock, H. J. (2008). Necessity and Language: In Defence of Conventionalism. *Philosophical Investigation*, **31**(1), 24–47.

Gödel, K. (1983). What is Cantor's continuum problem. In P. Benacerraf and H. Putnam, editors, *Philosophy of Mathematics*, pages 470–485. Cambridge University Press, Cambridge.

Gödel, K. (1995). Is Mathematics Syntax of Language. In S. Feferman, J. Dawson, W. Goldfarb, C. Parsons, and R. Solovay, editors, *Collected Works*, volume III, pages 334–356. Oxford University Press, Oxford.

Goldman, A. (1967). A Causal Theory of Knowing. *Journal of Philosophy*, **64**, 357–72.

Hale, B. (1994). Is platonism epistemologically bankrupt? *Philosophical Review*, **103**, 229–325.

Hale, B. and Wright, C. (2001). *The Reason's Proper Study: Essays Towards a Neo-Fregean Philosophy of Mathematics*. Clarendon Press, Oxford.

Hale, B. and Wright, C. (2002). Benacerraf's dilemma revisited. *European Journal of Philosophy*, **10**, 101–129.

Hellman, G. (1989). *Mathematics Without Numbers*. Oxford University Press, Oxford.

Kahalt, J. (208). Has the Later Wittgenstein Accounted for Necessity? *Philosophical Investigation*, **31**(1), 1–23.

Kalderon, M., editor (2005). *Fictionalism in Metaphysics*. Clarendon Press, Oxford.

Kasa, I. (2010). On Field's Epistemological Argument Against Platonism. *Studia Logica*, **96**, 141–147.

Klenk, V. (1976). *Wittgenstein's Philosophy of Mathematics*. The Hague, Martinus Nijhoff.

Kreisel, G. (1958). Wittgenstein's *Remarks on the Foundations of Mathematics*. *British Journal for the Philosophy of Science*, **9**.

Kripke, S. (1980). *Naming and Necessity.* Harvard University Press, Cambridge, MA.

Kripke, S. (1982). *Wittgenstein on Rules and Private Language.* Blackwell, Oxford.

Lewis, D. (1986). *On the Plurality of Worlds.* Blackwell, Oxford.

Lewis, D. (1993). Mathematics is megethology. *Philosophia Mathematica*, **3**, 3–23.

Liggins, D. (2006). Is there a Good Epistemological Argument against Platonism? *Analysis*, **66**, 135–141.

Liggins, D. (2008). Quine, Putnam and the Quine-Putnam indispensability argument. *Erkenntnis*, **68**, 113–127.

Liggins, D. (2010). Epistemological objections to platonism. *Philosophy Compass*, **5**, 67–77.

Linnebo (2006). Epistemological Challenges to Mathematical Platonism. *Philosophical Studies*, **193**(3), 545–574.

Linnebo, Ø. (2009). Platonism in the philosophy of mathematics. *The Stanford Encyclopedia of Philosophy.* `http://plato.stanford.edu/archives/fall2009/entries/platonism-mathematics/`.

Macbride, F. (2003). Speaking with Shadows: A study of Neo-Logicism. *British Journal for the Philosophy of Science*, **54**, 103–163.

Maddy, P. (1990). *Realism in Mathematics.* Clarendon Press.

Maddy, P. (1997). *Naturalism in Mathematics.* Clarendon Press.

Maddy, P. (2005). Three Forms of Naturalism. In Shapiro [2005b], pp. 437-459.

Manely, D. (2009). Introduction. In D. J. Chalmers, D. Manley, and W. Ryan, editors, *metametaphysics new essays on the foundations of ontology*, pages 1–37. Oxford University Press, New York.

Marconi, D. (1984). Wittgenstein on Contradiction and the Philosophy of Paraconsistent Logic. *History of Philosophy Quarterly*, **1**(3), 333–352.

Marconi, D. (2010). Wittgenstein and Necessary Facts. In *Wittgenstein: Mind, Meaning and Metaphilosophy.* Palgrave Macmillan.

Pagin, P. (1987). *Ideas for a Theory of Rules.* Ph.D. thesis, University of Stockholm.

Pagin, P. (2002). Rule-following, compositionality and the normativity of meaning. In D. Prawitz, editor, *Meaning and Interpretation*, Stockholm.

Parsons, C. (1980). Mathematical Intuition. *Proceedings of the Aristotelian Society*, **80**, 145–168.

Priest, G. (1983). An Anti-Realist Account of Mathematica Truth. *Synthese*, **57**, 49–65.

Priest, G. (1987). *In contradiction: A study of the transconsistent.* M. Nijhoff, Dordrecht and Boston.

Priest, G. (2004). Wittgenstein's Remarks on Gödel's Theorem. In M. Kölbel and B. Weiss, editors, *Wittgenstein's Lasting Significance.* Routledge, London.

Putnam, H. (1971). *Philosophy of Logic.* Harper Torchbooks.

Putnam, H. (1980). Models and Reality. *Journal of Symbolic Logic*, **45**(3), 464–482.

Putnam, H. (1996). On Wittgenstein's Philosophy of Mathematics. *Proceedings of the Aristotelian Society*, **Supp. Vol. 70**, 243–264.

Quine, W. (1936). Truth by Convention. In Gibson [2004].

Quine, W. (1953). Identity, ostension, and hypostasis. In Quine [1980a] pp. 65-79.

Quine, W. (1955). Posits and Reality. In Quine [1976b] pp. 233-241.

Quine, W. (1970). *Philosophy of Logic.* Englewood Cliffs, Prentice-hall.

Quine, W. (1976a). On Carnap's views on Ontology. in Quine [1976b] pp. 203-211.

Quine, W. (1976b). *The Ways of Paradox and Other Essays.* Harvard University Press, Cambridge, MA.

Quine, W. (1980a). *From a Logical Point of View.* Harvard University Press, Cambridge, MA.

Quine, W. (1980b). On What There Is. In Quine [1980a] pp. 1-19.

Quine, W. (1981). *Theories and Things.* Harvard University Press, Cambridge, MA.

Quine, W. (1986). Reply to Charles Parson. In L. E. Hans and P. A. Schlipp, editors, *The Philosophy of W.V. Quine*, pages 396–403. Open Court, La Salle, Illinois.

Quine, W. V. (1969). Existence and quantification. In *Ontological Relativity and Other Essays*, pages 91–113. Columbia University Press, New York.

Resnik, M. D. (2005). Quine and the web of belief,. In Shapiro [2005b], pp. 412-436.

Restall, G. (2003). Just what Is full-blooded platonism? *Philosophia Mathematica*, **11**(1), 82–91.

Rodych, V. (1995). Pasquale Frascolla's Wittgenstein's Philosophy of Mathematics. *Philosophia Mathematica*, **3**(3), 271–288.

Rodych, V. (1997). Wittgenstein on Mathematical Meaningfulness, Decidability, and Application. *Notre Dame Journal of Formal Logic*, **38**(2), 195–225.

Rodych, V. (1999a). Wittgenstein on Irrationals and Algorithmic Decidability. *Synthese*, **118**(2), 279–304.

Rodych, V. (1999b). Wittgenstein's Inversion of Gödel's Theorem. *Erkenntnis*, **51**(2/3), 173–206.

Rodych, V. (2000a). Wittgenstein's anti-modal finitism. *Logique et Analyse*, **43**(171-172), 301–333.

Rodych, V. (2000b). Wittgenstein's Critique of Set Theory. *The Southern Journal of Philosophy*, **38**(2), 281–319.

Rodych, V. (2001). Gödel's 'Disproof' of the Syntactical Viewpoint. *The Southern Journal of Philosophy*, **39**(4), 527–555.

Rodych, V. (2002). Wittgenstein on Gödel: The Newly Published Remarks. *Erkenntnis*, **56**(3), 379–397.

Rodych, V. (2003). Misunderstanding Gödel: New Arguments about Wittgenstein and New Remarks by Wittgenstein. *Dialectica*, **57**(3), 279–313.

Rodych, V. (2006). Who Is Wittgenstein's Worst Enemy?: Steiner on Wittgenstein on Gödel. *Logique et Analyse*, **49**(193), 55–84.

Rodych, V. (2008a). Mathematical Sense: Wittgenstein's Syntactical Structuralism. In *Wittgenstein and the Philosophy of information:Proceedings of the 30th Wittgenstein Symposium*, pages 81–104. Ontos-Verlag.

Rodych, V. (2008b). Wittgenstein's philosophy of mathematics. In E. N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Fall 2008 edition. `http://plato.stanford.edu/archives/fall2008/entries/wittgenstein-mathematics/`.

Rosen, G. (1993). The Refutation of Nominalism (?). *Philosophical Topics*, **21**, 149–86.

Shanker, S. (1988). Wittgenstein's Remarks on the Significance of Gödel's Theorem. In S. Shanker, editor, *Gödel's Theorem in Focus*, pages 155–256. Routledge, Netherlands.

Shapiro, S. (2005a). Introduction. In S. Shapiro, editor, *The Oxford Handbook of Philosophy of Mathematics and Logic*, pages 3–27. Oxfor University Press, New York.

Shapiro, S., editor (2005b). *The Oxford Handbook of Philosophy of Mathematics and Logic*. Oxford University Press, New York.

Sider, T. (2003). Reductive Theories of Modality. In M. J. Loux and D. W. Zimmerman, editors, *The Oxford Handbook of Metaphysics*, pages 180–210. Oxford University Press, New York.

Smullyan, R. (1992). *Gödel incompleteness Theorems*. Oxford University Press, New York.

Steiner, M. (2009). Empirical Regularities in Wittgenstein's Philosophy of Mathematics. *Philosophia Mathematica*, **17**(III), 1–34.

Tennant, N. (1997). *The Taming of the True*. Oxford University Press, Oxford.

Tennant, N. (2008). Carnap, Gödel, and the Analyticity of Arithmetic. *Philosophia Mathematica*, **16**(III), 100–112.

Van Inwagen, P. (1998). Metaontology. *Erkentniss*, **48**, 233–250.

Walton, K. (1993). Metaphor and prop oriented make-believe. In Kalderon [2005], pp. 65-87.

Williamson, T. (2009). *The Philosophy of Philosophy*. Blackwell, Malden, MA, et al.

Wright, C. (1980). *Wittgenstein on the Foundations of Mathematics*. Duckworth, London.

Wright, C. (1983). *Frege's Conception of Numbers as Objects*. Aberdeen University Press, Aberdeen.

Wright, C. (2007). Rule-following without Reasons: Rule-following without Reasons: Wittgenstein's Quietism and the Constitutive Question. *Ratio*, **XX**, 481–502.

Wright, C. and Bob, H. (2005). Logicism in the Twenty-first Century. In Shapiro [2005b], pp. 166-202.

Yablo, S. (1982). Review of Alan Sidelle, Necessity, Essence, and Individuation. *Philosophical Review*, **101**(4), 878–881.

Yablo, S. (1996). How in the World. In C. Hill, editor, *Metaphysics*. University of Arkansas Press.

Yablo, S. (1998). Does ontology rest on a mistake? *Proceedings of the Aristotelian Society*, **Supplementary Volume 72**, 229–61.

Yablo, S. (2001). Go figure: a path through fictionalism. *Midwest Studies in Philosophy*, **25**, 72–102.

Yablo, S. (2002). Abstract object: a case study. *Philosophical Issues*, **12**, 220–240.

Yablo, S. (2005). The myth of the seven. In Kalderon [2005], pp. 88-115.

Yablo, S. (2008). Carnap's Paradox. Unpublished.

Yablo, S. (2009). Must Existence-Questions Have Answers? In D. J. Chalmers, D. Manley, and W. Ryan, editors, *metametaphysics new essays on the foundations of ontology*. Oxford University Press, New York.