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Wittgenstein´s Relation to Formalism

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1. Introduction

Diverse "isms" in the many-coloured field of "isms" that are available on the market of philosophy of mathematics have been ascribed to the writings of the later Wittgenstein concerning mathematics. He has been labelled as a sort of (strong) conventionalist, a sort of (strict) finitist, sometimes as very close to intuitionism and the like. In recent years it has become particularly popular and – in my eyes – illuminating to recognise some sort of formalism, or at least a close connection to formalism, in several aspects in the philosophy of mathematics of the later Wittgenstein. It is a notoriously hard and probably hopeless task to try to put some label on Wittgenstein's philosophy of mathematics. For this reason commentators nowadays back off from calling Wittgenstein's position a full-blooded formalism resp. some other "ism". Nevertheless passages can be found in Wittgenstein's writings whose proximity to formalist ideas are striking at first sight. "We may say that thinking is essentially the activity of operating with symbols" (BB, p. 6) for instance. Or in the *Philosophical Remarks* (PR) we read:

> An equation is a rule of syntax.
> Doesn't that explain why we cannot have questions in mathematics that are in principle unanswerable? For if the rules of syntax cannot be grasped, they're of no use at all. And equally, it explains why an infinity that transcends our powers of comprehension cannot enter into these rules. And it also makes intelligible the attempts of the formalists to see mathematics as a game with signs. (PR, p. 143)

However, in other remarks Wittgenstein distances himself explicitly from formalism as an adequate view on mathematics. "In the debate between 'formalism' and 'contentful mathematics' what does each side assert? This dispute is so like the one between realism and idealism in that it will soon have become obsolete, for example, and in that both parties make unjust assertions at variance with their day-to-day practice" (PG, p. 293). In other words, "[p]art of Formalism is right and part wrong" (WVC, p. 106). This alone is reason enough to take a look at the question of how the connection between formalism and Wittgenstein's considerations looks like.

In this present thesis I want to investigate the question to what extent a reading of Wittgenstein as him being close to formalism affects some debates that have been ignited by Wittgenstein's interpreters. One debate I will address concerns his remarks on the surveyability of mathematical proofs. In part III of his *Remarks on the Foundations of Mathematics* (RFM)[1], Wittgenstein discusses the observation, or depending on the interpretation, the demand, that a mathematical proof has to be surveyable. "'A mathematical proof must be surveyable.'" (RFM III, 1) We will see how these considerations can be interpreted in very different ways and how these diverging interpretations are connected to a formalist reading of Wittgenstein. In a reading proposed by Felix Mühlhölzer (Mühlhölzer 2005, 2010), it is argued that the concept of 'surveyability' in this passage is very special, and serves primarily the undermining of attempts to give a foundation to mathematics. The idea of a need for a foundation of mathematics is philosophically misguided in Wittgenstein's eyes. To show why this is the case, according to Mühlhölzer, Wittgenstein examines if a logical foundation such as Russell's and Whitehead's *Principia Mathematica* (PM) were trying to supply, can cope with the minimal condition of surveyability of proofs. Furthermore, Mühlhölzer argues, the best way to understand Wittgenstein's considerations on this topic is to give the notion of 'surveyability' in contexts of mathematical proofs a meaning which is very close to the meaning of 'surveyability' in a formalist outlook, such as David Hilbert's. This formalist reading is inevitably disconnecting the notion of surveyability in a proof from the notion of a surveyable presentation of our language as a goal of philosophy. This disconnection will tear apart philosophy and mathematics as activities.

Stefan Majetschak (Majetschak 2013), on the other hand argues that a formalist reading of 'surveyability' in mathematical proofs is philologically highly doubtful. Wittgenstein pays very much effort to clarify his idea of giving a surveyable presentation of our language, so it is just very plausible for Majetschak that the surveyability of language, which is at the centrepiece of Wittgenstein's later philosophy, is the same kind of surveyability that is meant by Wittgenstein when speaking about proofs. In the PI we read:

[1] This part of RFM was put together by the editors from parts of the manuscripts MS 122 and MS 117 from 1939 to 1940. A better understanding of these remarks is arguably gained by the reading of the manuscripts since some crucial parts have been sorted out for the RFM. Nevertheless, I will refer to the RFM because they are sufficient for my purposes and furthermore they are more widespread.

> A main source of our failure to understand is that we do not command a clear view of the use of our words. – Our grammar is lacking in this sort of perspicuity. – A perspicuous representation produces just that understanding which consists in 'seeing connexions'. (PI, 122)[2]

And in the same period he is also talking about surveyable proofs. It is therefore highly plausible that he had the same thing in mind, says Majetschak. Philosophy is an activity, which has to avoid historical-genetic explanations of phenomena, according to Majetschak's Wittgenstein. Historical-genetic explanations are always based on hypothetical assumptions, and such assumptions are not wanted in philosophy. Wittgenstein's proposal of giving a surveyable presentation of our language is in no need of assumptions. This avoidance of assumptions is of the same kind as we can find in mathematics. In mathematics, too, there are no historical-genetic explanations of phenomena. So, Majetschak's non-formalist reading will lead to the result that philosophy and mathematics share very similar goals and thereby share very similar methods.

A third reading, one which is implicitly non-formalist concerning the concept of surveyability in a proof, is presented by James R. Brown (Brown 1999). In Brown's overall conception, pictures can serve as mathematical proofs. In Wittgenstein Brown sees a proponent of this view. There are several diagrams, which serve as proofs in Wittgenstein's writings and some remarks, which seem to indicate that Wittgenstein had a rather lax conception of what might count as a proof. In this rather lax conception the *proof idea* is essential for the examination of proofs. This is contrary to a formalist conception of proof, since in formalist terms it is not the idea of a proof that is central but the written signs themselves. So, the question what kinds of things can serve as mathematical proofs in a Wittgensteinian framework will be affected by considerations about Wittgenstein's relation to formalism.

[2] Instead of 'perspicuous representation' – the translation of 'übersichtliche Darstellung' that can be found in G.E.M. Anscombe's translation, I will use 'surveyable presentation' since this seems to be closer to the original German version.

Another debate has been concerned with the question whether Wittgenstein wanted to change mathematical practice or not. One can find some remarks in which he emphasises the importance of not interfering with mathematics. The probably best-known quotation by Wittgenstein that is taken to be a clear indicator for his non-revisionist attitude can be found in the PI.

> Philosophy may in no way interfere with the actual use of language; it can in the end only describe it.
>
> For it cannot give it any foundation either.
>
> It leaves everything as it is.
>
> It also leaves mathematics as it is [...]. (PI, 124)

It is thus not the job of philosophy to prescribe a correct usage of an actual language. Neither it is the job of philosophy to prescribe correct mathematics. The only task of philosophy is to describe the actual usage of a language by giving a surveyable presentation of it. This seems to be quite clear and unproblematic. Let the mathematicians do their jobs and feel invited do give a description of what they are doing. But – as might be expected – things are not as clear and unproblematic as they seem to be. For example in part II of RFM, Wittgenstein is dealing with Cantor's diagonal argument and the legitimacy of transfinite set theory. At least a first reading of this part of RFM appears to be a harsh critique of this branch of mathematics. To give a foretaste:

> [O]ne pretends to compare the 'set' of real numbers in magnitude with that of cardinal numbers. [...]
>
> I believe, and hope, that a future generation will laugh about this hocus pocus. (RFM II, 22)

To make these apparent revisionist remarks on mathematical practice compatible with his explicitly non-revisionist attitude, some interpretational work is needed. Here, too, we will find some implications of a formalist reading of Wittgenstein on the question whether his remarks in RFM II are intervening with mathematics or not. One way of solving the problem is to refer to a distinction that

Wittgenstein made in 1931 (cf. WVC, p. 149), the distinction between "*calculus*" and "*prose*". This distinction suggests that the real work of mathematicians is only concerned with calculi, with formal rule-governed symbol games. And the philosopher is not in a position to evaluate any of these calculi. But mathematicians do not always stick to their calculi. Normally they speak about them in a natural language – in "prose". This is usually not a problematic issue. However, it can become one if the natural language used is misguided and suggests a specific interpretation of a calculus. And by this interpretation the whole interest of mathematicians is led to a calculus, which – on a clear view – does not deserve that much attention. The question whether a coherent formulation of Wittgenstein's non-revisionism is available given the calculus – prose distinction is also closely connected to the question to what extent Wittgenstein's views are comparable to formalist views, as we will see. As Victor Rodych argues (Rodych 1997, Rodych 2000), the intermediate Wittgenstein is much closer to formalism than the later Wittgenstein. Following Rodych, the distinction between calculus and prose is a much sharper one in the intermediate period than in the late one. If mathematics is more or less identified with formal calculi, then everything that is allowed in these calculi is proper mathematics. If philosophers are criticising the language surrounding the core – the calculus – then they are not criticising mathematics. But, says Rodych, the later Wittgenstein does not make this sharp distinction anymore. Mathematical propositions and signs get their meaning from their use *outside* of mathematics. In RFM V e.g. we read:

> 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." (RFM V, 2)

Now if it is essential to mathematics that it gets its meaning from the non-mathematical world, the question then is, whether any correct calculi is automatically a proper part of mathematics. And if mathematical meaning is embedded in a non-mathematical language, the question whether a critique of the non-mathematical language surrounding a calculus does not interfere with mathematical practice, arises. We will see how different interpreters have dealt with these questions in different ways and bear in mind Wittgenstein's relation to formalism thereby.

The writings of Wittgenstein I am mainly concerned with date from the years between 1938 and 1940. Some commentators ascribe this period to the middle or intermediate Wittgenstein and some to the later Wittgenstein. There are good reasons for both ascriptions. I chose to align myself to the latter. When referring to the intermediate Wittgenstein, we will be talking about his works from the period dating from 1929 until 1934. In this period fall *Wittgenstein and the Vienna Circle* (WVC), *Philosophical Grammar* (PG), *Philosophical Remarks* (PR) and *The Blue and Brown Books* (BB). It will show helpful to consult works from this period for reasons of interpretation of the later works. The *Philosophical Investigations* (PI) which date from a later time than the RFM will also be immensely helpful to get a better understanding of the problems surrounding the themes from RFM.

Before we begin to ask whether Wittgenstein's philosophy of mathematics is some sort of formalism or how close it is to some kind of formalism, we should take a look at what formalism is, after all. The next section will be dedicated to a brief overview of the relevant varieties of formalism and its position in the diverse philosophies of mathematics.

1.1 Formalism

'Formalism' is the name of a family of related views concerning the nature of mathematics. In this section I can just outline some general ideas of these related views. Following Stewart Shapiro (Shapiro 2000, pp. 141-171) I will differentiate between '*term formalism*', '*game formalism*' and – as a refinement of the latter – '*deductivism*'. But before doing so, I will outline a contrasting view concerning mathematics and its entities: *Platonsism*. As in the case of formalism, Platonism, too, is in its different versions much too differentiated as to be outlined satisfactorily in just a few lines. All that can be done here is to represent some of its most general ideas, so that we can contrast it with some general ideas of formalism. And then we can see how some aspects of Wittgenstein's philosophy of mathematics is related to all of this.

The general idea of Platonism concerning mathematics is that there exist not only concrete objects such as trees, houses, stones and so on, but also abstract objects like numbers, lines, circles and other mathematical objects.[3] The incentive for Platonism is that on the one hand we do have quite a lot of knowledge about geometry, arithmetics and other mathematical disciplines. But on the other hand, we are not able to see or to observe the things geometry e.g. is dealing with directly. No one has ever seen a perfect circle while everybody has seen trees before. Still, we know some truths about perfect circles, for example that the ratio of a circle's circumference to its diameter is π. And what we know about perfect circles seems to be undisputable. The knowledge we have of these even seems to be much more stable and more objective than our knowledge about trees. In a similar way we know quite a lot about numbers. For example we know that there is no largest natural number. One might wonder how we can know such a thing, since we cannot see that there is no largest natural number in the way we can see a house, for example. So, how is it possible for us to know all of that (and much more) about geometry and arithmetic? The answer that Platonists and also proponents of other philosophies of mathematics – though not all – give, is that our knowledge of mathematics is based on other ground than our knowledge of the physical world,

[3] Aside from Plato, the most well-known proponent of Platonism in the history of philosophy of mathematics is probably Kurt Gödel (cf. Gödel 1944). Nowadays, full-fledged defences of Platonism are rarely seen though they exist (e.g. Brown 2005).

such as trees and houses. It is not through the senses that we gain mathematical knowledge but through our reasoning. So what are these objects, which we cannot see but of whom we know so much only through reasoning? For the Platonist these objects differ not only in the way we come to get knowledge about them, but also in the way they exist, i.e. in their ontological status. Since mathematical objects don't exist in the physical world, otherwise we should be able to see them, they have to exist in their own, abstract world. In the Platonic Heaven. And there they always have existed and will never cease to exist. Or, put differently, they exist beyond time. And this is the reason why our knowledge about them is, once gained, undisputable and can never become outdated. Compared with our knowledge that there is no greatest natural number, our knowledge about the tallest house on earth can be outdated at some given time. If, for example, the tallest house on earth today is located in Dubai, then knowledge about this fact is true today. But if next year an even taller house were built in China, then in two years today's knowledge would be outdated. Of course, such a situation is not possible with the largest natural number. If today someone knows that there is no largest natural number, then this person can never be disproved.

That all mathematical objects occupy a special place and are real, although abstract things, may solve some of these questions of the form just raised. Still, a lot of philosophers have been doubtful if such a theory should be embraced unquestioned. For many it seems to be a bit over the target to postulate a whole distinct world, which is eternal and inhabited by abstract entities just because it is not clear how we know things about unobservable objects. A whole range of philosophies of mathematics have been developed in order to give answers to epistemological and ontological problems concerning mathematics but to circumvent the need of a Platonic Heaven too. One family of such a philosophy is the views labelled 'formalism'. As I said, there are different conceptions of what formalism as a philosophy of mathematics exactly is. What follows in the remainder of this section is a general outline of some aspects of these views with the focus on those aspects which are relevant for the discussion of Wittgenstein's ideas related to formalism.

A basic observation about mathematics is that some manipulations of signs are central to it. Everyone will agree to this. Most people will say that there is more to mathematics than just some manipulations of signs. For example in the equation '2 + 2 = 4' there are obviously signs involved and we are allowed to put these signs in this order according to the rules of arithmetic. But we are allowed to do so because these signs stand proxy for some real things, the numbers 2 and 4, in our example. Such a view is underlying Platonism, as we have seen above. On a formalist conception, the signs in our equation do *not* stand proxy for any real things. In mathematics, the formalist claims, there are signs and rules for manipulating these signs and that's it. In other words, there is no need for mathematics to be *about* anything. If you want to think that the signs are about different things, e.g. abstract objects, feel invited to think so. But it is not needed for a proper conception about the nature of mathematics that it is about anything but the signs and rules. How to spell out such a view accurately is of course a compelling undertaking. And diverse such undertakings have been put forward by various philosophers.[4]

1.1.1 Term Formalism

One rough differentiation of the views within formalism is between *term formalism* and *game formalism*. Proponents of term formalism claim that mathematics is about the symbols used in mathematics. The equation '2 + 2 = 4' is literally about the symbols '2', '+', '=' and '4'. The advantage of this kind of formalism in contrast to Platonism is spelled out by Johannes Thomae who claims that the "formal standpoint rids us of all metaphysical difficulties; this is the advantage it affords us" (Thomae 1998). Instead of dealing with abstract, eternal objects that no one can see, term formalists like Thomae (at this period) claim to advocate for a much more modest view insofar as mathematics is only dealing with concrete written (or vocalized) objects everyone can see or hear. But of course such a straightforward identification of mathematical objects like numbers with their physical representations such as numerals go hand in hand with some serious difficulties, as Gottlob Frege for instance (Frege 1893) observed. Take our equation '2 + 2 = 4'. On the left side of the equation there are obviously different symbols than on the right side. Not only are the tokens different, but so are the types, so a type-token

[4] Michael Detlefsen's article "Formalism" (Detflefsen 2007) is a good survey of the diverse conceptions of formalism in the philosophy of mathematics.

distinction won't be of any help here. In what sense is such an equation speaking of identity in term-formalist terms? Frege (a vehement opponent of any version of formalism) suggested that a formalist might solve the situation in claiming that any occurrence of '2 + 2' can in any occurrence be substituted with '4' without a change in truth-value. Concerning natural numbers and rational numbers this may be a way out for term-formalism. But when coming to real numbers, e.g. π, the situation changes. Since π has an infinite expansion, the term-formalist will have a hard time to identify the real number π with its decimal representation. Since a linguistic or symbolic entity is a finite object, how can an infinite number be identified with it? Problems as this one have not been managed to be solved satisfactorily by term formalists, so we can have a look at the other basic version of formalism – game formalism.

1.1.2 Game Formalism

As the name game formalism suggests, game formalists see a great affinity between games – chess is their favourite example – and mathematics. In chess, the essential thing to know is which moves are allowed and which moves are not allowed according to the rules. If you want to know everything about the pawn, then it is sufficient to know how the pawn is allowed to be moved on the board and what moves are not allowed. The question: "What does the wooden figure of the pawn really stand for?" seem absurd in this context. Likewise game formalism identifies everything relevant there is to mathematics with the rules that govern what manipulations of symbols are allowed and what manipulations of symbols are not. Mathematics is thus considered as solely a rule-governed game in which the symbols used are comparable with the wooden figures in a chess game. And accordingly the question: "What does the symbol '4' really stand for?" seems equally absurd. In this manner, mathematics is not about abstract objects called numbers, just as chess is not about abstract objects called pawns. Contrasted with term formalism, which explicitly states that mathematics is about its symbolisation, game formalism considers the question what mathematics is about, beside the point. A view like this seems to be attractive at least at first sight. Questions on the ontology of mathematical objects can simply be shaken off; there is as much ontology in mathematics as in chess. And since numbers are not identified with symbols in game formalism, problems of the sort term formalists face aren't of any worry. But a hard problem remains. The question of the universal applicability to (at least) the sciences needs to be dealt with by game formalists. That mathematics is an essential part to all sciences and also in more everyday situations seems to be undeniable. Without mathematics physics, biology, chemistry or paying one's bill at a restaurant would look very differently. But this is not the case with a game like chess. Probably the entire discipline of physics and all other areas not directly related to chess would look the same if the game chess had never been invented. Frege, once again, was very aware of this shortage of the analogy between mathematics and chess:

> Now it is quite true that we could have introduced our rules and other laws of the *Begriffsschrift* as arbitrary stipulations, without speaking of the meaning and the sense of the signs. We would then have been treating the signs as figures. What we took to be the external representation of an inference would then be comparable to a move in chess, merely the transition from one configuration to another. We might give someone our [axioms] and [...] definitions [...] – as we might the initial position of the pieces in chess – tell him the rules permitting transformations, and then set him the problem of deriving our theorem [...] all this without his having the slightest inkling of the sense and the meaning of these signs, or of the thought expressed by the formulas. (Frege 1903, 90)

Frege's own, non-formalist answer to the question of universal applicability of mathematics is that mathematics expresses abstract propositions which can be true or false. And the sciences, too, are concerned with finding truths, so mathematics as a tool for finding general truths can be applied to all sorts of special sciences. But these general truths are not an option for the game formalist since formalism explicitly rejects a Fregean talk of abstract propositions. The problem for the game formalist is that it is very unlikely that completely arbitrary rules will fit so well with all discoveries of the natural sciences. A natural solution is that the rules are not that arbitrary after all. A formalist account which makes use of deductive truth preserving systems is today known as *deductivism* (cf. Shapiro 2000, pp. 148-57).

1.1.3 Deductivism

For a formalist it may be convenient that Frege of all people developed a method for preserving truths in inferences. The nowadays common concept of logical consequence was established by Frege to make sure to only infer truths from truths. A deductivist conception embraces this idea but does not need the axioms of a system to be true in the sense that the axioms represent eternal truths or the like. The deductivist insists that we can treat the axioms as true but that we don't need to. The problem of applicability of mathematics to any science is solved by deductivists by the idea that it is sufficient for a scientist to find interpretations that make its axioms true. Then, because of the conception of logical consequence, all theorems come out to be true under the same interpretation. (cf. ibid., p. 150) But for the deductivist mathematician no such interpretation of the axioms is needed. Only the investigation of the consequences is her concern. Moritz Pasch encapsulates this idea:

> If geometry is to be truly deductive, the process of inference must be independent in all its parts from the meaning of the geometrical concepts, just as it must be independent of the diagrams; only the relations specified in the propositions and definitions may legitimately be taken into account. During the deduction it is useful and legitimate, but in no way necessary, to think of the meanings of the terms; in fact, if it is necessary to do so, the inadequacy of the proof is made manifest. (Pasch 1926, p. 91)

It was David Hilbert who worked out such a programme in his *Foundations of Geometry* (Hilbert 1959).[5] An important idea of Hilbert is the role that axioms play in his own conception. Since there is no room for intuition in the axioms, anything can be used as primitives in these axioms. Whether we use classical terms such as 'point', 'line' and 'planes' or less classical terms (at least in geometry) such as 'table', 'chair' and 'beer mug' does not really matter. These concepts are empty, so to speak, and only the relations holding between them are set by the axioms. Simultaneously these axioms play the roles of definitions. So, contrary to a more Platonistic conception held by Frege for example where primitives have to capture some basic

[5] In Hilbert's eyes, this formalist conception has to be applied not only to geometry, but to all branches of mathematics.

intuitions and be defined explicitly, i.e. only by terms which are known independently of these very primitives, the axioms state which relations hold between these otherwise empty primitives. And via the means of a proper deductive system, the theorems following are true under any interpretation, which makes the axioms true. Such a conception of definitions with which Hilbert came up is nowadays known as implicit definitions.[6]

This was only a very rough approximation to formalism. Of course there is much more to say about formalism generally, especially Hilbert's ideas of meta-mathematics, his finitism and the impact Gödel's incompleteness theorems had on this development to give a rudimental outline of formalism as a philosophy of mathematics. But this is not the aim of this thesis. Rather I want to explore how some formalist ideas in Wittgenstein's philosophy of mathematics can be used to approach some notoriously controversial ideas in Wittgenstein's discussions concerning mathematics. The preceding paragraphs are only a first introduction to formalism in contrast to Platonism. More aspects of formalism will become apparent as we go along.

1.2 Some Remarks on Wittgenstein's Philosophy of Mathematics

One of the difficulties of the later Wittgenstein's philosophy of mathematics is that there is no such thing as his philosophy of mathematics. At least if one does expect a coherent formulation of a philosophy which is explicitly located somewhere on the map of the traditional viewpoints. Rather the reader is confronted with scattered remarks throughout his published and unpublished material. And these remarks are intertwined with his thoughts on language, psychology, topics as understanding in general and various other fields that were catching Wittgenstein's interest. For these reasons it is notoriously difficult to single out the philosophical views Wittgenstein had on mathematics. Still, it is not a completely hopeless task to formulate some Wittgensteinian ideas on the philosophy of mathematics and to examine them by using more traditional views as object of comparison.

The perhaps most appreciated ideas of Wittgenstein's later philosophy (of mathematics) are his considerations on following a rule which are seen by many

[6] In chapter 5 of this thesis we will see how this idea of implicit definitions may be found in Wittgenstein's philosophy of mathematics.

commentators as the centerpiece of PI. It is a matter of fact that our usage of language is largely a matter of applying rules of some sort. Without the existence of rules which determine in what situations it is appropriate to use which linguistic expressions, communication is impossible. But not only communication is depending on the existence and the correct application of rules but so is our own, private thinking. Since thinking is an activity which is in large part a linguistic activity, we cannot seperate these two things and therefore, if rules are essential to language, they are essential to our thinking. Given this rather important role rules play in our lives, one may feel the urge of pointing to what a rule exactly is and what it exactly consists in to follow a rule. Surely by now at this line of reasoning, Wittgenstein's commentators disagree on what his own views concerning this topic are. Most of them will nevertheless agree that in some sense every rule seems to be underdetermined by itself. Take for example a signpost that indicates the way to the next hut. We can say that this signpost is a rule in the sense that by following it we will arrive at some point we have wanted to arrive. How do we follow that signpost? Perhaps there is an arrow on it, so we will act accordingly. And how do we know how to read an arrow? Couldn't this specific arrow mean something else than arrows usually mean? How do we know that in this situation the arrow means that the hut is in the opposite direction than the arrowhead? We would say that we have learned how to read arrows normally and what they mean in comparable situations. But how have we lerned this? Probably by means of some other rules. But why think that these rules which are used to introduce some other rules are unambiguous? If they are not, the reader might suspect why the solution is not to simply refer to some more basic rules. But perhaps they are. Aren't there some basic unambiguous rules such as '2 + 2 = 4'? And arent't rules such as this one at the bottom of our system of rules, so to speak? This might well be the case, but if there are such basal rules that constitute the others, these basal rules do not work as and cannot be explained as the non-basal rules, as we have just seen. A popular reading of Wittgenstein is that his considerations on problems such as those just outlined are showing that in his eyes there are some fundamental "grammatical" rules which can not be explained but which are constituents of our "forms of life". It is just the way we act, speak and have learned to do so which are governing such fundamental grammatical rules such as '2 + 2 = 4'. A lot of ideas and insights follow from such a view and a lot is connected to this. I hope to shed some light on a small portion of the consequences and related topics to this, though the main concern is not a direct treatment of Wittgenstein's considerations on following a rule within his considerations on mathematics.[7]

3. Surveyability

3.1 Surveyable Presentation as a methodology

Philosophy "leaves everything as it is" (PI, 124), Wittgenstein says. It is not the task of philosophy to get over our ordinary language and to invent an ideal language instead, one that would be conceived as a better tool to solve philosophical problems. Such a view of the purpose of philosophy can be found perhaps most prominently in Frege's *Begriffsschrift* (Frege 1879) and in Russell's and Whitehead's *Principia Mathematica* (Russell and Whitehead 1910, 1912, 1913) (cf. Hacker 1978, p. 150). Nor is it the aim of philosophy to find some kind of "real logical structure" underlying all languages to gain a clearer understanding of the mechanisms of language. This view can be found in the *Tractatus* (TLP) of the early Wittgenstein (cf. ibid., p.149). For the later Wittgenstein philosophy still aims at clarification but this aim is to be achieved by a surveyable presentation of our existing every day language.[8] It is a lack of surveyability, which is the main source for philosophical confusions and problems. If we get a clear survey of the rules of the usages of different concepts and their connections and disconnections to each other, then all philosophical problems will alleviate if not dissolve completely. In a way this sounds like a rather simple or even redundant task and it may be hard to acknowledge it to be an interesting philosophical method. Don't we all know how to use the concepts in our language? If we wouldn't know how to use the words of our language we couldn't communicate with each other. Wittgenstein of course grants this ability to all of us. Still, something must go wrong. Otherwise questions concerning the 'essence of knowledge' or the question of the possibility of knowing

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[7] Good overviews on Wittgeinstein's philosophy are e.g. (Wright 1980), (Frascolla 1994) and (Ramharter and Weiberg 2006)

[8] The "quasi-historical" roots of Wittgenstein's conception and his emphasis are nicely perspicuoulsy represented in (Baker and Hacker 1980 pp. 532 – 541). Especially the resemblance of Boltzmann's views on the importance of surveyable presentations are striking, although "in the case of science the insight is creative, in philosophy it is purely descriptive. (ibid., p. 539)

the pain another person suffers wouldn't arise. Questions like these only derive from a lack of overview of our grammar[9]. It is due to the fact that some concepts, such as e.g. 'knowledge', are analysed by philosophers without considering its rules of usage in our language in an appropriate way that puzzlement over the 'essence of knowledge' can even occur. So the problem of Wittgenstein's method of giving a surveyable presentation of our grammar seems not to lie in its triviality. The problem is rather that the rules of the usages of concepts are not manifest to us. The philosopher's job now is to provide such a survey of our grammar. In the remark 116 of the PI, (at least) the first step of that program is captured by Wittgenstein in the following way:

> When philosophers use a word – "knowledge", "being", "object", "I", "proposition", "name" – and try to grasp the essence of the thing, one must always ask oneself: is the word ever actually used in this way in the language-game which is its original home? –
> What we do is to bring words back from their metaphysical to their everyday use.
> (PI, 116)

So, instead of thinking of some kind of thing to which the word 'knowledge' refers, we should look at how a word like 'knowledge' is employed in everyday language. In a superficial way 'knowledge' looks like a word with a reference, such as 'house' for example. And because of that superficial resemblance we have a tendency to think that 'knowledge' is the same sort of thing with a spatial expansion for instance. And because of this tendency we are inclined to attribute all kind of properties to the concept of knowledge. As a result of those attributions we get confused and come up with all sorts of philosophical theories to get rid of these confusions. The problem is that these theories rest on a confusion from the outset, so their chances to succeed in clearing up anything are hopeless from the very beginning. If we take a closer look at the way we use 'knowledge' in contrast to the way we use the word 'house', we will see that the usages of these concepts are completely different. And by seeing this the problems which occur by misguided

[9] 'Grammar' not in the ordinary, but rather in Wittgenstein's meaning.

philosophical theories will never emerge. In this sense, "A main source of our failure to understand is that we do not command a clear view of the use of our words. – Our grammar is lacking in this sort of perspicuity. – A perspicuous representation produces just that understanding which consists in 'seeing connexions'. [...]"(PI, 122)

3.2 Surveyability of proofs

In one of Wittgenstein's discussion of the concept of proof, 'surveyability' also plays a central role. The opening remark of RFM III reads as follows:

> 'A mathematical proof must be surveyable.'[10] (RFM III, 1)

A natural question to ask now is whether surveyability of a proof is meant in the same way as the surveyable presentations of our language, which serve to avoid philosophical confusions. This question has been answered differently by various commentators. In this section I will present one negative answer and one affirmative answer given by Felix Mühlhölzer and Stefan Majetschak, respectively. Since the negative answer leads to a reading of Wittgenstein which puts him close to formalism and the affirmative answer moves Wittgenstein away from formalism, we have a paradigmatic case of how two different interpretations, which are connected to Wittgenstein's relation to formalism, lead to very different results. A third reading presented by James R. Brown argues that Wittgenstein has used pictures as rigorous mathematical proofs. Brown's stressing of Wittgenstein's use of pictures in mathematical contexts will lead to a non-formalist conception of proofs, as we shall see.

[10] I use the translation 'surveyable' for the German word 'übersichtlich'. In G.E.M. Anscombe's translation we find the term 'perspicuous'. The occurrences of 'übersichtlich', 'überblickbar', 'übersehen' and the like have been translated inconsistently and arguably in a misleading way. I will only use the translations 'surveyable', 'to survey' and 'surveyability', since they seem to be more neutral than others.

3.2.1 Formalist Surveyability – Mühlhölzer

In a paper from 2005 (Mühlhölzer 2005) and in his commentary book on part III of RFM (Mühlhölzer 2010), Felix Mühlhölzer is presenting a thorough examination of the concept of surveyability and the role it plays in mathematical proofs for Wittgenstein. Mühlhölzer notices that "the statement '[a] mathematical proof must be surveyable' sounds like an extremely strong, revisionist and, because of the 'must', breathtakingly dogmatic thesis" (Mühlhölzer 2005, p. 58). But it cannot be the case that the statement was meant in such a way, according to Mühlhölzer, since "[w]hoever has studied only a bit of mathematics knows that complicated, unsurveyable proofs are the order of the day" (ibid.). And since Wittgenstein has studied at least a bit of mathematics, Wittgenstein had to mean something different. Mühlhölzer expresses what is meant, according to him, in four so called meaning postulates. It is, I think, helpful to take a look at the statements that are immediately followed by the opening remark to understand why Mühlhölzer sums them up in his meaning postulates:

> 'A mathematical proof must be surveyable.' Only a structure whose reproduction is an easy task is called a "proof". It must be possible to decide with certainty whether we really have the same proof twice over, or not. The proof must be a configuration [Bild = picture, T.F.] whose exact reproduction can be certain. Or again: we must be sure we can exactly reproduce what is essential to the proof. It may for example be written down in two different handwritings or colours. What goes to make the reproduction of a proof is not anything like an exact reproduction of a shade of colour or a hand-writing.
> It must be easy to write down exactly this proof again. This is where a written proof has an advantage over a drawing. The essentials of the latter have often been misunderstood. The drawing of a Euclidian proof may be inexact, in the sense that the straight lines are not straight, the segments of circles not exactly circular, etc. etc. and at the same time the drawing is still an exact proof; and from this it can be seen that this drawing does not – e.g. – demonstrate that such a construction results in a polygon with five equal sides; that what it proves is a proposition of geometry, not one about the properties of paper, compass, ruler and pencil.

[Connects with: proof a picture of an experiment.] (RFM III, 1)

Before we look at the meaning postulates, we should get clear on what Wittgenstein's intention was by examining the concept of surveyability in proofs. At least in Mühlhölzer's eyes – and I think this is plausible – Wittgenstein's main intention was to undermine intended (logical) foundations of mathematics such as Russell's and Whitehead's foundational work presented in their PM. It will be helpful to keep that in mind for the following discussion.

The just quoted passage is summarized by Mühlhölzer in the four meaning postulates:

> (S1) The surveyability of a proof consists in its possibility of *reproduction*.
> (S2) This reproduction must be an *easy task*.
> (S3) We must be able to decide with *certainty* whether the reproduction produces the same proof.
> (S4) The reproduction of a proof is of the sort of a reproduction of a *picture*.
> (Mühlhölzer 2005, p. 59)

I will briefly go through these postulates one by one in order to get a better understanding of them.

(S1) "The surveyability of a proof consists in its possibility of *reproduction*" is best understood when we contrast a proof with an experiment. A proof as something non-temporal differs to an experiment in this respect. An experiment has a certain setup, which leads to a certain result. But the result yielded by an experiment is not in the same way part of the experiment itself as the result yielded by a proof is part of the proof. This can be seen clearly by considering what it means to reproduce an experiment. For the reproduction of an experiment, only the reproduction of the experimental setup is needed. If an experimental setup is reproduced to a sufficient degree, then we speak of the same experiment twice over, even if the results differ. This is not the case with a proof. If one tries to reproduce a proof and the results differ, we will never speak of the same proof twice over. So the reproduction of the whole proof including its result is necessary to reproduce a proof, whereas in case of an experiment only the reproduction of specific conditions are necessary to reproduce the experiment. As Mühlhölzer notices, this aspect of a proof in contrast to an experiment can also be found in a later section of RFM III: "To repeat a proof means, not to reproduce the conditions under which a particular result was once obtained, but to repeat every step *and the result*" (RFM III, 55). In which way this difference between an experiment and a proof is ensured by the concept of surveyability gets clearer by examining the three remaining postulates.

(S2) "This reproduction must be an *easy task*" invites the reader to very different interpretations. For example, Mark Steiner takes this to mean that "we could take all such proofs in at a glance" (Steiner 1975, p. 41). But such a reading would lead to a highly revisionist view which contradicts mathematical practice in which long and unsurveayble proofs are nothing unusual in a too obvious way. According to Mühlhölzer, "what S2 excludes are not very long and complicated proofs *per se*, but proofs that are unsurveyable in the manner in which they, e.g., represent numbers" (Mühlhölzer 2005, p. 61). And such unsurveyable representations of numbers are of the kind of foundational systems of arithmetic. A system which represents a natural number *n* "as the sum of *n* ones, such that for very big *n* it is *not* an 'easy task' [...] to reproduce the sign representing *n*" (ibid.). With other words, Mühlhölzer interprets Wittgenstein's 'surveyability' in proofs in a very specific way. A way of surveyability which is not available in notational systems, such as Russell's and Whitehead's PM without the use of abbreviations and definitions.[11]

(S3) "We must be able to decide with *certainty* whether the reproduction produces the same proof" points in the same direction. The certainty Wittgenstein has in mind here, is not of a Cartesian kind but a mere practical certainty, says Mühlhölzer. This practical certainty manifests itself in the fact that hardly ever are there any disputes amongst mathematicians on the question whether a certain proof has been reproduced or not. And that is due to the fact that the criteria of identity for proofs are fixed. The colour or the handwriting of a proof, for example, is irrelevant. So what is relevant for the proof? Mühlhölzer discusses this question by means of an example: "is it essential to an ε-δ-proof in analysis *exactly which* δ is chosen to a given ε in order to conduct the proof?" (ibid., p. 62). For the *proof idea*

[11] One of Wittgenstein's original critiques of the uses of abbreviations and definitions in foundational systems points to the fact that these abbreviations make use of non-foundational notations of numbers. But such a move is obviously cicular and therefore can be questioned by a philosopher. For a detailed discussion, see (Mühlhölzer 2010, pp. 103-197).

this is not essential. But it is not proof ideas with which Wittgenstein is concerned in RFM III, following Mühlhölzer. "Wittgenstein's criteria of identity for proofs, which determine what a correct reproduction of a proof is, are of a purely formal nature, and they do not take into account 'proof ideas'" (ibid.). Here we can find reasons why Mühlhölzer sees a close connection between Wittgenstein's ideas concerning reproduction of proofs and formalism, since for a formalist too, it is not proof ideas which are examined but written-out proofs with their specific arrangement of signs. We will look at this with a little more detail after having taken a look at (S4).

(S4) "The reproduction of a proof is of the sort of a reproduction of a *picture*" is again best understood by contrasting proofs with experiments. While the process of proving a theorem may have some similarity with performing an experiment, the non-temporal aspect of a proof has more in common with a picture of an experiment. "[T]he proof itself rather resembles the *picture* of such an experiment, in which the experiment is frozen, as it were, into something non-temporal" (ibid., p. 68), as Mühlhölzer puts it. Mühlhölzer sees in this Wittgensteinian idea to compare proofs with pictures the justification for using the term 'surveyable', since the aspect of seeing plays a central role both in a proof as well as in a picture. Furthermore, comparing proofs with pictures "is Wittgenstein's way of doing justice to the non-temporality of proofs without falling prey to platonistic myths" (ibid.). Now we can see why the reproduction of a proof is a mere copying, for Mühlhölzer's Wittgenstein. No technical aids are needed to reproduce a proof, nor do any additional assumptions, such as causality, belong to the non-temporal aspect of a proof. A remark from part IV of RFM is taken by Mühlhölzer to underline this idea:

> When I wrote "the proof must be surveyable" that meant: causality plays no part in the proof.
> Or, also: the proof must be capable of being reproduced by mere copying. (RFM IV, 41)

Many of the ideas just discussed are reminding one of ideas expressed by Hilbert and his formalism, as Mühlhölzer highlights. First of all, Hilbert uses the concept 'surveyable'[12] in some central passages when speaking about the question what mathematics actually is. For example, in *Über das Unendliche* we read: "[A] formalized proof, like a numeral, is a concrete and surveyable object. It can be communicated from beginning to end" (Hilbert 1925, p. 383). Hilbert's sense of 'surveyability' in a proof is also connected with the idea that for doing mathematics, it is essential that we see the signs occurring in proofs and that we have a "precondition of certain extra-logical discrete objects, which exist intuitively [anschaulich] as immediate experience before all thought" (Hilbert, 1922, 25). And via this ability, according to Hilbert, we identify the numeral-signs that we see on e.g. a sheet of paper with the numbers. This conception of numbers can be seen as a reaction to Frege's "contentful mathematics" which has "proved to be inadequate and uncertain" (ibid.). Mühlhölzer remarks that Wittgenstein knew all this and discussed Hilbert's ideas. Wittgenstein rejected Hilbert's formalism as a whole. A remark which makes this obvious can be found in PB, for example. "In the debate between 'formalism' and 'contentful mathematics' what does each side assert? This dispute is so like the one between realism and idealism in that it will soon have become obsolete, for example, and in that both parties make unjust assertions at variance with their day-to-day practice." (PG, p. 293) But this does not mean that he rejected every single part of it. One indication of this may be Wittgenstein's thoughts in RFM III, which we have outlined above. But since these can be interpreted in different ways, more evidence is needed. Mühlhölzer gets this evidence, among others, from the *Blue Book.* In the *Blue Book*, Wittgenstein is defending some formalist ideas against Frege's criticism. After having done this, Wittgenstein goes on and says: "We may say that thinking is essentially the activity of operating with symbols" (BB, p. 6). This view on what thinking essentially is, is of course not the final word on this topic. Especially the later Wittgenstein from the *Philosophical Investigations* has much more to say on that topic. Still, that operating with symbols is essential for mathematical thinking in the eyes of the later Wittgenstein, is a view one can definitely argue for. And, on the other hand, for Mühlhölzer it is sufficient to show that despite Wittgenstein's explicit dismissal of formalism as a theory of what mathematics consists of, he is nevertheless sympathetic with some of its ideas. For this purpose, Mühlhölzer contrasts

[12] The German word Hilbert uses is "überblickbar", which can also be found in Wittgenstein.

Wittgenstein's claim that "thinking is essentially the activity of operating with symbols" with a statement of Hilbert, namely: "this formula game is carried out according to certain definite rules, in which the *technique of our thinking* is expressed. [...] The fundamental idea of my proof theory is none other than to describe the activity of our understanding" (Hilbert 1928, p. 475; emphasis Hilbert's).

We can register that for both Wittgenstein and Hilbert, the operating with symbols is essential in mathematics. It is also clear that both dismissed the sort of contentful mathematics which Frege advocated so firmly. In the *Blue Book* we find a defence of formalism against Frege's critique of it. In the beginning of the book Wittgenstein discusses so called game-formalism (to use Shapiro's terminology again) and Frege's critique of it. The formalist idea that mathematics is nothing but a game with signs, which are allowed to be transformed by certain rules, was inacceptable for Frege, as we have seen. recall that according to him, such a view cannot explain why mathematical propositions are true, nor can it explain the universal applicability of mathematics to other domains. Only if the signs used in mathematics denote real (though abstract) entities the two desiderata – truth and universal applicability – can be accomplished. A mere game with "dead" signs is just not an interesting thing to do.

Wittgenstein's defence against Frege's attack on game formalism suggests a third alternative. It is true that a mere game with dead signs would not be very interesting, but this does not mean that these signs stand proxy for abstract objects. The application itself, the use of these symbols in our everyday language is all there is needed to make the application of mathematics to the non-mathematical world possible. His summary of Frege's critique and a glimpse of his own solution to it, reads as follows:

> Frege ridiculed the formalist conception of mathematics by saying that the formalists confused the unimportant thing, the sign, with the important, the meaning. Surely, one wishes to say, mathematics does not treat of dashes on a bit of paper. Frege's idea could be expressed thus: the propositions of mathematics, if they were just complexes of dashes, would be dead and utterly uninteresting, whereas they obviously have a kind of life. And the same, of course, could be said of any proposition: Without a sense, or without the

> thought, a proposition would be an utterly dead and trivial thing. And further it seems clear that no adding of inorganic signs can make the proposition live. And the conclusion which one draws from this is that what must be added to the dead signs in order to make a live proposition is something immaterial, with properties different from all mere signs.
> But if we had to name anything which is the life of the sign, we should have to say that it was its *use*. (BB, p. 4)

It is not the case, as Frege conceived it, that numerals denote numbers which are the extension of a concept[13] that makes mathematics universally applicable. No such thing is needed. It is the *applicatio*n of the numerals to the things in the world itself, or our use of them, which is the reason for the universal *applicability* of numbers.[14]

We can now go back to our question, whether Wittgenstein had the same kind of surveyability in mind when on the one hand speaking about surveyability as a goal of philosophical method and on the other hand speaking of surveyability in a mathematical proof. It should be clear by now that Mühlhölzer's answer to this question is a definite no. "When Wittgenstein is speaking about 'surveyability' in other contexts, say about 'surveyable presentation' in the case of philosophising [...], he has a completely different conception in mind"[15] (Mühlhölzer 2010, p. 92n; translation mine)

So, one consequence of Mühlhölzer's formalist reading of Wittgenstein's concept of surveyability in RFM III is that for Wittgenstein philosophy and mathematics as activities are quite distinct things, which have their own distinct methods. Such a consequence from a formalist reading may be surprising, since for e.g. Hilbert one aspect of formalism is the identification of mathematics and philosophy[16]. But, in Mühlhölzer's view, such an identification is precisely not the

[13] We don't need to take a detailed look on Frege's logicist conception of numbers here. It is, though highly simplifying, sufficient to regard Frege as a Platonist in this respect.
[14] More on the application of mathematics to the non-mathematical will be said in chapter 5.
[15] "Wenn W. [Wittgenstein] in anderen Kontexten von 'Übersichtlichkeit' spricht, etwa von 'übersichtlicher Darstellung' im Falle des Philosophierens [...], hat er einen völlig anderen Begriff im Sinn." (ibid.)
[16] For Hilbert, mathematical formula games were representations of human thought processes. "Thinking, it so happens, parallels speaking and writing." (Hilbert 1928, p. 475)

view Wittgenstein held. Here it is important to recall that on Mühlhölzer's reading Wittgenstein's adoption of formalist ideas only concern the specific notion of 'surveyability' just discussed, but that Wittgenstein in no way adopted the more philsophical parts of Hilbert's ideas.

Given these circumstances, it may seem even more surprising that a different, non-formalistic reading can lead to quite different results concerning the relation between philosophy and mathematics. One such reading is proposed by Stefan Majetschak and will be outlined in the next section.

3.2.2 Surveayable Proofs as a Paradigm for Surveyable Presentations – Majetschak

Stefan Majetschak (Majetschak 2013) examines Wittgenstein's philosophical method of giving a surveyable presentation of our language in a thorough way. Recall that we have characterized Wittgenstein's methodology as aiming at giving an overview of the usage of the words in our language. To possess such an overview will be of central importance when it comes to philosophical puzzlement. Majetschak too, sees in Wittgenstein's attempt at gaining a survey over our language the central method of the later Wittgenstein. Majetschak examines why this exact method of philosophising is considered as the best or even the only acceptable one for Wittgenstein. Why a surveyable presentation rather than e.g. a historically-genetic form of presentation?, Majetschak asks. (ibid, p. 107) In the *Remarks on Frazer's Golden Bough* (RFGB), Wittgenstein explicitly mentions the method of giving a surveyable presentation of some phenomena for the first time, as Majetschak notes. (Majetschak 2013, p. 108) Here, Wittgenstein contrasts the method of giving a surveyable presentation of specific phenomena with the method of giving a historically-genetic form of explanation of the same phenomena. Although Wittgenstein acknowledges that a historically-genetic form of explanation is traditionally very well accepted in the sciences, it is only one form of presentation amongst others. He writes that "[t]he historical explanation, th explanation as an hypothesis of development, is only *one* way of assembling the data–of their synopsis." (RFGB, p. 69). But, since a historical-genetic form of presentation is always and in principle a hypothetical kind of presentation, such a presentation is not an appropriate one for the philosophical method, as Majetschak reads

Wittgenstein. On the other hand, a surveyable presentation is not depending on any hypothetical theses, but lets us see connections of structural relations which would remain hidden otherwise. And only such a surveyable presentation as Wittgenstein has in mind, is capable of showing some fact as evident for limited human beings. It is – contrary to an explanation which rests on some sort of hypothesis – capable of proving something, according to Majetschak's Wittgenstein. For Majetschak it therefore is "no surprise that a reconstruction of Wittgenstein's idea of what a 'surveyable presentation' is, will probably always stem from examples which are taken from mathematics" (Majetschak 2013, p. 114; translation mine).[17] In mathematics too, there is no room for hypotheses, Majetschak goes on, and for that reason, Wittgenstein does treat the notion of surveyability in a mathematical proof in the same way as he does in context of philosophical methodology. Philosophy is therapeutical in the way, that "[w]ork on philosophy – like work on architecture in many respects – is really more work on oneself. On one's own conception. On how one sees things." (MS 112, 46)

Majetschak finds the same idea in Wittgenstein's considerations on 'surveyability' in a proof and the role it plays in human orientation (cf. Majetschak 2013, p. 115) "[Wittgenstein] has repeatedly emphasized that a mathematical proof has to be 'surveyable' and has understood a proof as an exemplary 'surveyable presentation'. Its surveyability consists in the fact that the relevant [...] connections are presented in a way that a surveyable perception of the proof leaves no room for doubting its correctness." (ibid.; translation mine)[18] The certainty that a proof gives us, lies in its surveyability which in turn makes it possible that we can remember a proof structure and reproduce it. Only via this capability of being able to be remembered and reproduced, does a proof gain its paradigmatic status, says Majetschak (cf. ibid., p. 116). For this reason, according to him, Wittgenstein uses

[17] "Es dürfte übrigens kein Zufall sein, dass eine Rekonstruktion von Wittgensteins Auffassung dessen, was die Form einer 'übersichtlichen Darstellung' ausmacht, wohl stets auf Beispiele zu sprechen kommen wird, die der Mathematik entstammen." (ibid.)

[18] "Hier hat er [Wittgenstein] wiederholt betont, dass ein mathematischer Beweis 'übersichtlich' sein müsse und ihn als eine *mustergültige* 'übersichtliche Darstellung' aufgefasst, deren Übersichtlichkeit eben darin besteht, dass sie die je relevanten, innerhalb eines mathematischen Gegenstandsbereichs aufzuzeigenden Zusammenhänge so augenfällig präsentiert, dass der sinnliche Augenschein in einem Überblick jeglichem Zweifel an der Gültigkeit der Demonstration den Boden entzieht." (ibid.)

particularly simple examples when speaking of proofs. Such an example he finds in Wittgenstein's Lectures from the Notes of Alice Ambrose and Margaret McDonald:

L	R
X	0
X	0
	0

(AWL, p. 369)

Wittgenstein calls this pattern a proof for 3 > 2 as good as any geometrical proof. We simply *see* that there are more 0's than X's in that pattern. Because we simply see the relation this pattern is not despite its simplicity but rather because of its simplicity a proof pattern. There is no room for doubting its correctness. And for these reasons we can regard such patterns as paradigms. Note, that Majetschak too, as well as Mühlhölzer, highlights the visual aspect of seeing a proof as the basal criterion for our accepting its proof status. But not the formalistic sense of seeing the particular signs in a proof are relevant for Majetschak's reading, but the idea that our accepting of a proof consists in our ability to simply see some relation the proof expresses.

A problem that arises here is that this reading amounts to a highly revisionist reading of Wittgenstein's philosophy of mathematics. If only patterns which are surveyable such as the one we have right here, are real proofs for Wittgenstein, and any pattern which is not surveyable in *such a way* is not called a proof by Wittgenstein, then this interferes quite a lot with mathematical practices, since our seeing what's going on in a proof *just like this*, is highly uncommon. A reading such as Majetschak's reading leads to a view that only proof patterns which can be remembered and if necessary reproduced identically by memory are called proofs, is changing quite a lot. Any proof which is too long to be remembered, would lose its status as a proof. Such highly revisionist implications do not seem to be a problem for Majetschak, since he is quite aware of them: "A too narrow reading [of the concept of surveyability in the context of mathematical proofs, such as Mühlhölzer's,] [...] doesn't consider the possibility that Wittgenstein could have questioned 'the established practice of mathematics'." (Majetschak 2013, p. 117n;

translation mine)[19] Given Wittgenstein's explicit non-revisionist claims, such a highly revisionist attitude seems highly doubtful.[20]

However, Majetschak concludes his reading of Wittgenstein's concept of surveyability in mathematical proofs as follows: "A mathematical proof [...] is a 'surveyable presentation' in which no hypothesis, such as in historical explanations, can be found, because the aspect which is necessary for us to call it a paradigm, shines out without doubt." (ibid. p. 120; tranlation mine)[21] But not only in mathematical proof there is no room for hypotheses, Majetschak continues. It is also Wittgenstein's aim to eliminate any hypotheses from philosophy by his method of giving a surveyable presentation. That all hypotheses must occur in Wittgenstein's philosophy, Majetschak gets from the PI: "Philosophy simply puts everything before us, and neither explains or deduces anything." (PI, 126) In philosophy, as in mathematics, the main goal is to show "[t]he aspects of things that are most important for us" which are hidden for us "because of their simplicity and familiarity" (PI, 129).

So, as Majetschak sees it, in philosophy and in mathematics the goal to achieve is to make some relevant aspects of phenomena surveyable. And in both cases this making surveyable the relevant aspects must not use any hypotheses. This is due to the view that neither in philosophy, nor in mathematics there is room for any doubt. From these considerations it follows that mathematics and philosophy are very similar activities for Wittgenstein. If this connecting of philosophy and mathematics is an consequence which Wittgenstein would have embraced, may be doubted. Not only isn't there any evidence (at least to my knowledge) in which Wittgenstein explicitly draws a parallel betwenn these two disciplines concering their aims and their methods. Furthermore the textual evidence seems to point to another direction.

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[19] Mit dieser engen Lesart vermag er zwar einigen Eigentümlichkeiten der mathematischen Praxis Rechnung zu tragen rechnet [...] zu wenig mit der Möglichkeit, dass Wittgensteins Überlegungen ja auch die 'gängige Praxis der Mathematik' in Frage stellen könnten. (ibid.)

[20] The question how far Wittgenstein's explicitly non-revisionist claims can be seen as compatible with some of his apparently revisionist remarks, will be discussed later on in chapters 4 and 5. However, a reading that suggests that Wittgenstein only regarded proofs such as the one discussed above as real proofs can – as far as I see – not be made compatible with his non-revisionist claims.

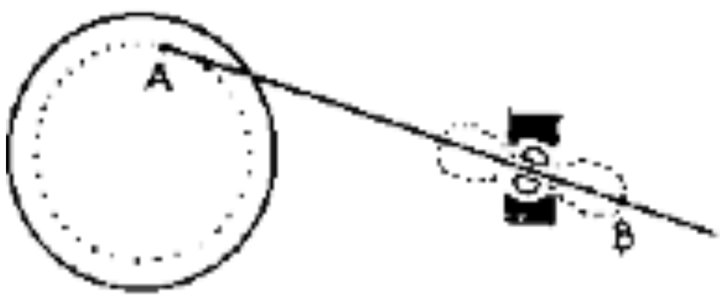
[21] "Ein mathematischer Beweis [...] ist ein 'übersichtliche Darstellung', aus der alles Hypothetische, wie es z.B. historischen Erklärungen anhaftet, getilgt ist, weil der Aspekt, der unsere Anerkennung als Paradigma verlangt, zweifelsfrei hervorleuchtet." (ibid.)

Recall that Mühlhölzer's formalistic reading of Wittgenstein's considerations on surveyability in proofs excludes the possibility of Wittgenstein's being concerned with proof ideas. Majetschak's moving together the philosophical method of giving a surveyable presentation and the concept of surveyability in a proof does not exclude proof ideas to be in the focus of Wittgenstein's interest.

Yet another position how to understand Wittgenstein's considerations on surveyability in proofs in a non-formalistic way will also allow the possibility of Wittgenstein's being concerned with proof ideas. I will outline this position in the remainder of this section.

3.2.3 Picture-Proofs are Surveyable – Brown

In his book *Philosophy of Mathematics* (Brown 2005), James R. Brown is arguing for the view that pictures can and do serve as rigorous proofs in mathematical practice. One proponent of this unusual view amongst mathematicians and philosophers of mathematics he finds in Wittgenstein. Brown is stressing the fact that pictures and diagrams are very seldomly used by the "great philosophers". Wittgenstein is an exception. There are indeed many drawings in his works and some of them are implicitly or explicitly used as proofs. One such drawing can be found in part VII of RFM and is discussed by Brown considerably. The example is the following:

> Consider a mechanism. For example this one:
>
> 
>
>
> While the point *A* describes a circle, *B* describes a figure eight. Now we write this down as a proposition of kinematics.When I work the mechanism its

> movement proves the proposition to me; as would a construction on paper. The proposition corresponds e.g. to a picture of the mechanism with the paths of the points *A* and *B* drawn in. Thus it is in a certain respect a picture of that movement. It holds fast what the *proof* shews me. Or – what it persuades me of.
>
> If the proof registers the procedure according to the rule, then by doing this it produces a new concept.
>
> In producing a new concept it convinces me of something. For it is essential to this conviction that the procedure according to these rules must always produce the same configuration. ('Same', that is, by our ordinary rules of comparison and copying.)
>
> With this is connected the fact that we can say that proof must shew the existence of an internal relation. For the internal relation is the operation producing one structure from another, seen as equivalent to the picture of the transition itself – so that now the transition according to this series of configurations is *eo ipso* a transition according to those rules for operating.
>
> In producing a concept, the proof convinces me of something: what it convinces me of is expressed in the proposition that it has proved. . . .
>
> The picture (proof-picture) is an instrument producing conviction. (RFM VII, 72)

Brown points to the facts that first, this picture does convince us of a proposition, namely: 'As the point A moves in a circle, the point B moves in a figure eight.' (cf. Brown 2005, p. 131) And secondly, that the picture is faulty. The point B should be at the middle of the figure eight, and the symmetry axis of the figure eight should be horizontal. (cf. ibid., p. 132) But despite of these flaws the picture works as a proof and convinces us of the proposition. The question Brown is concerned with is, how is it possible that a misdrawn picture can prove a proposition? Brown's answer to that question relies heavily on considerations on rule-following.

Brown's version of the rule-following argument is adopted by Kripke's version (cf. Kripke 1982), which I will outline very briefly.[22] The basic question is: How do we know how to apply any particular rule in a given situation? Consider a scenario in which a teacher wants to teach the rule of squaring numbers to a student. And let's assume further that in human history no one has ever squared a number greater than 100. The teacher presents some examples. $1^2 = 1$; $2^2 = 4$; $3^2 = 9$; $4^2 = 16$ and a few more. Now the teacher asks the students to continue. The student goes on as he was told to. When approximating the number 100 the following happens: The student writes down these equations:

.
.
.
$98^2 = 9{,}604$
$99^2 = 9{,}801$
$100^2 = 10{,}000$
$101^2 = 11$
$102^2 = 12$
.
.
.

We would all agree that something went wrong here. The student obviously has not understood how the rule of squaring a number applies. But now suppose that the student doesn't accept this criticism. He tells the teacher that it is the first time in history that someone has squared a number greater than 100. And furthermore, he claims to have been following the exact same rule as the teacher has. What are the possible replies the teacher could give in this situation? The teacher could point to the fact that she has meant something different. Or that the algorithm determining the results is a different one than the algorithm the student has used. The problem with these possible replies is that they are begging the question. Sooner or later the teacher will again have to refer to a rule that is to be applied when speaking about meanings or algorithms. And since the initial problem was how one can know how to apply a particular rule in a given situation, this cannot be a way out (our example

[22] The question what Wittgenstein really showed with his considerations on rule-following is one of the big controversies amongst Wittgenstein's interpreters. Since this is not the topic of my thesis, I am glad not to have to stir up this hornet's nest too and will simply recite Brown's (or Kripke's) version.

was only illustrating the point that it seems possible in principle to interpret a rule at one's will, since every rule seems to be flexible enough for any interpretation). Arguably, other accounts will – for different reasons – not do the job either. (for a little more detailed illustration, see Brown 2005, pp. 132-141). According to Kripke (and Brown), Wittgenstein's own solution to this problem is a sceptical solution. In summary, the sceptical solution states that there is "no fact of the matter about whether the teacher/ questioner meant one sequence rather than another." The fact that we (normally) follow a rule without hesitation is based on our (the community's) same training "and we are judged by following a rule correctly when we give the same answers as other members of the community. There is a strong tendency for all members of the community to give the same answers – a shared 'form of life'. [...] There is no explanation for this, no hidden cause [...]. Our answers are (in some loose sense) in accord with the rule; but they are *not caused* by it." (ibid., pp. 144-5)

Brown does not see a real solution in this sceptical solution, since "this conclusion [is] wholly absurd and [...] it is ridiculous to say that it is only an amazing coincidence that we all agree in developing a sequence in the same way." (ibid., p. 145) If Wittgenstein's considerations really lead to an amazing coincidence as a final answer shall not bother us for the moment. What is important here right now is that Brown reads Wittgenstein as denying the existence of rules. But, with the right platonistic attitude, the notion of a rule can be saved. And even more, with the help of the concept of surveyability, the notion of a rule can be applied in a way that even misdrawn pictures can be proofs, as we will see in a moment.

Brown distinguishes – in a Fregean fashion – the reference of a rule and its sense. For example the reference of the rule 'Take each natural number and square it' is the set $S = \{ \langle 1, 1\rangle, \langle 2, 4\rangle, \langle 3, 9\rangle, \langle 4, 16\rangle, \ldots \}$. And one of its senses is e.g. 'Take each natural number and square it'. The reference is a set containing infinitely many ordered pairs. And since it has infinitely many members, no one can grasp the whole set but only a part of it. But we can grasp its sense. In other words, we can survey the sense of the sequence n^2 but not its referent. Brown's idea is that we can speak of a mathematical rule when – under ideal circumstances – we can survey the rule.[23] Therefore there is a "rule for calculating the value of π to any decimal place" but "there is no rule for locating the twin primes" (ibid, p. 151). In a similar way that we can survey a rule, via its sense, we can also survey a picture. Brown borrows a distinction sometimes made in aesthetics, which distinguishes between two types of represenation: a 'picture' and a 'symbol'. Take the picture of Jacques-Louis David's Napoleon. The literal picture, so to speak, is a picture of the historical figure Napoleon. But it also represents qua being a symbol things like courage, leadership, glory and so on. In this way, according to Brown, a picture-proof also represents the literal picture and additionally some abstract proposition qua being a symbol. And via this dual character of pictures even a flawed picture-proof as the one under discussion can be misdrawn but still prove a proposition.

Brown's usage of the ideas surrounding rule-following considerations and the concept of surveyability are not an exegetical outline of Wittgenstein's ideas. He uses some ideas and develops them in his own account of Platonism which is of course not the intention Wittgenstein could have had. Still, I think, it is instructive to compare an account such as Brown's with an account such as Mühlhölzer's. Especially the aspect that Wittgenstein used pictures (even flawed ones) as proofs, seems quite contrary to Mühlhölzer's suggestion that Wittgenstein was not dealing with proof ideas but only with the formal aspects of proof. Recall that Mühlhölzer sees the main reason why Wittgenstein compares proofs with pictures in the capacity of this analogy to make the difference between proofs and experiments explicit. Because of this contrast Mühlhölzer sees the similarity between pictures and proofs in their being 'formalistic'. Everything there is to a proof is written out, no more things like the assumption of causal forces need to be added. This is quite contrary to what Brown has to say why proofs and pictures are alike in a Wittgensteinian outlook. It is through its not only being a picture in the literal sense but through its being a symbol that a picture, like a written out proof can serve as a proof.

We see that in the case of the concept of surveyability in a proof, very different results can be obtained by different interpretations. I hope to have shown how

[23] Brown is not very explicit about what he takes 'surveyability' to mean in Wittgenstein's philosophy of mathematics. But it is quite clear that it is not a formalistic conception of it that he has in mind, as statements as the following show: "A proof is surveyable when we can grasp it, we can take it in as a whole." (ibid., p. 151)

attributions of formalist ideas within Wittgenstein's considerations on mathematics can serve as a guideline in this debate. The next part of this thesis will deal with Wittgenstein's remarks on Cantor's diagonal argument in RFM II – another area in which commentators of Wittgenstein notoriously disagree even though the disagreement is more fine-grained, so to speak. There too, Wittgenstein's closeness to formalism is a good tool to assess the diverging interpretations one can find in the literature.

4. Wittgenstein on Cantor and Non-Revisionism

There is an apparent clash between some of Wittgenstein's remarks on what his intentions are when philosophising about mathematics and his actual philosophising about mathematics. In several passages Wittgenstein claims that philosophy may in no way interfere with mathematical practice. Such remarks can be found during the intermediate phase up until his later writings. In PI a by now very well known remark reads as follows:

> Philosophy may in no way interfere with the actual use of language; it can in the end only describe it.
>
> For it cannot give it any foundation either.
>
> It leaves everything as it is.
>
> It also leaves mathematics as it is [...]. (PI, 124)

Remarks such as the just quoted ones are taken by most commentators to tell us something about Wittgenstein's attitude towards mathematics and (his) philosophising about it. Since here the prevalent tone is that philosophy must not revise mathematical practice, Wittgenstein's attitude is mostly understood as a *non-revisionist* attitude. The situation then would seem to be quite clear. Let the mathematicians do their jobs and as a philosopher feel invited to describe what they are doing. As long as you don't mesh with what they are doing, everything is fine. But, of course, the situation is not as easy as one might expect when only being acquainted with Wittgenstein's non-revisionist claims. In his philosophy of mathematics, one can find several passages which don't seem to fit at all – at least at first sight – in a non-revisionist framework. In part II of the RFM, Wittgenstein is dealing with Georg Cantor's diagonal argument and the foundations of transfinite set theory. Transfinite set theory is nowadays an established branch of mathematics and it has also been so in the days of RFM, part II (1939).

Before treating what Wittgenstein has to say on this, let us take a look an the proof in an informal version – a version with which Wittgenstein was arguably acquainted with.

4.1 Cantor's Diagonal Argument

In 1891, Georg Cantor (Cantor 1892) proved the existence of infinite sets that have a cardinality greater than the cardinality of the infinite set of the natural numbers.

This can be shown by the following indirect method: Suppose, the set of **R** between 0 and 1 is denumerable. In that case we should be able to construct a list in which every real number between 0 and 1 occurs at some time. The list would look something like the following:

0.1398421.....
0.7222439.....
0.5000000....
0.4444444...
....

No we construct a real number r via picking out each n-th digit behind the point from the n-th line:

0.**1**398421.....
0.7**2**22439.....
0.50**0**0000....
0.444**4**444...
....

The number we obtain is therefore the number r = 0.1204.... Now we construct a new number out of r in which each digit is replaced for another one. This new number cannot be included in the list. It cannot be in the n-th line since it was construed in a way that each of its n-th digit is different from the digit in the n-th line. Therefore, the set **R** between (0 and 1) is greater than the set **N**. The set is non-denumarably infinite.

As was mentioned, this proof (in a more rigorous way) is considered to be a legitimate part of matematics by (nearly all) mathematicians themselves nowadays and at the when part II of RFM were written.

Under these circumstances, it might be surprising to read what Wittgenstein has to say about this topic. I'll just quote some of the perhaps most striking remarks to present the reader a feeling of the tone of RFM II. Wittgenstein calls Cantor's proof a "puffed-up proof":

> Our suspicion ought always to be aroused when a proof proves more than its means allow it. Something of this sort might be called 'a puffed-up proof'.
> (RFM II, 21)

Shortly afterwards, transfinite set theory is called a "hocus pocus":

> I believe, and hope, that a future generations will laugh at this hocus pocus. (RFM II, 22)

We will take a closer look at the reasons for which Wittgenstein held this apparently low view on Cantor's proof by discussing different overall approaches towards Wittgenstein's philosophy of mathematics. We will see that Wittgenstein's connection to formalism is one key to get a grip on his views concerning transfinite set theory and that it is one key to approach the debate concerning Wittgenstein's non-revisionism.

If one opposes Wittgenstein's explicit non-revisionist attitude with his (seemingly) revisionist remarks concerning mathematical practice such as e.g. in RFM II, then one can choose which of these oppositions one wants to leave untouched. Either one takes it seriously that "[p]hilosophy may in no way interfere [...] with mathematics" (PI, 124) and adjusts the reading of Wittgenstein's apparent revisionist remarks until they don't seem to be that revisionist, after all. Or, secondly, one can deny the plausibility of a non-revisionistic reading of remarks such as RFM II, and argue that the explicit non-revisionistic attitude is not stood up to. Or, thirdly, one can even argue that Wittgenstein's (apparently) non-revisionist claims weren't meant in a non-revisionist way, after all. (cf. Redecker 2006, pp. 320-341). Most commentators have chosen the first option. They take the non-revisionist claims to be the leitmotif of Wittgenstein's philosophy of mathematics and argue that by means of the correct reading, this leitmotif is sustained. In the remainder of this section, I will present some of these readings. After having done so, we can take a look what this has to do with Wittgenstein's relation to formalism.

4.2 Non-Revisionists

4.2.1 Wrigley

For Michael Wrigley (Wrigley 1977) the puzzle is solved by understanding Wittgenstein's overall philosophical approach towards mathematics. Wrigley argues for seeing in Wittgenstein a radical conventionalist[24] rather than a constructivist which was the view proposed by influential commentators such as Georg Kreisel (Kreisel 1959) and Michael Dummett (Dummett 1959). First, Wrigley notices that a constructive philosophy of mathematics is a deliberately and explicitly revisionistic philosophy of mathematics. But, given Wittgenstein's explicitly non-revisionist attitude, a constructivist reading of him is untenable. To get an understanding of Wittgenstein's intentions, Wrigley says, it is necessary to "separate mathematics itself from the things which mathematicians and philosophers have said *about* mathematics." (Wrigley 1977, p. 185; emphasise his) While mathematics itself as it is – the '*calculi*' – shall not be modified, the talking about it – the '*prose*', as Wittgenstein calls it – is in need of clarification. The distinction between 'calculus' and 'prose' was made most vividly by Wittgenstein in conversation with Moritz Schlick and Friedrich Waismann in 1931:

> It is a strange mistake of some mathematicians to believe that something inside mathematics might be dropped because of a critique of the foundations. Some mathematicians have the right instinct: once we have calculated something it cannot drop out and disappear! And in fact, what is caused to disappear by such a critique are names and allusions that occur in the calculus, hence what I wish to call *prose*. It is very important to distinguish as strictly as possible between the calculus and this kind of prose. Once people have become clear about this distinction, all these questions, such as those about consistency, independence, etc., will be removed. (WVC, p. 149)

It is not the calculus part of mathematics with which Wittgenstein is concerned but the ordinary language in which mathematicians describe the calculi. When

[24] As was noted earlier, the attribution of any kind of 'ism' is problematic concerning Wittgenstein's philosophy of mathematics. Still, there are good reasons to see constructivist elements in his views. Furthermore the formalistic elements and the conventionalistic elements in Wittgenstein aren't mutually exclusive, as far as I can see it.

doing mathematics on the calculus level, the mathematician does not run into the risk of being confused. Only when *talking about* mathematics this risk arises. In the same way in which Augustine had no difficulties using the word 'time' in every day speech, he was confused about 'the being of time', mathematicians have no difficulties doing mathematics but are confused when talking about it. (cf. Wrigley 1977, p. 185) Since this distinction is so important, one can could then ask what, according to Wittgenstein, as Wrigley reads him, is mathematics itself, or what are calculi? In his intermediate phase (e.g. in PG and WVC), Wittgenstein is quite explicit on that question. In contrast to the alternative between the view that mathematical signs stand for abstract entities, such as the sign '1' stands for the abstract entity 1, or, on the other hand, that mathematics is about the signs themselves – which is one version of formalism, term formalism, that Wittgenstein explicitly rejected. Wittgenstein proposed to compare mathematics with a game such as chess, like game formalists do, as we have seen. The figure of the pawn doesn't stand for any abstract pawn, neither is chess about the wooden figures themselves. The pawn only gets its significance from the rules according to which one is allowed to move it on the board. And in the same way do mathematical signs get their significance. It is a given set of rules, a calculus, which determines what can be done with the signs. And in the same way as chess is not about anything and can not be true or false, mathematics, the calculi, is not about anything and neither can it be true or false. This is the key for Wrigley to get a clear grip on Wittgenstein's comments concerning set theory. The mistake of all mathematicians and philosophers of mathematics was to treat mathematics as a body of propositions, i.e. to be a set of statements about something. In this regard the proponents and the opponents of Cantor's proof at the time it was established didn't differ from each other. Both sides took Cantor's results as being about the transfinite. "But both Cantor's supporters and his opponents were talking nonsense for, as Wittgenstein remarked in another context, 'the decisive move in the conjuring trick has been made, and it was the very one we thought quite innocent' (PI, 308)." (Wrigley 1977, p. 186) So, Wittgenstein cannot be understood as an opponent of Cantor's proof in the way that he criticised the proof itself. If he would have joined such critics, he would have made the same mistake he alleged both the proponents and the opponents of transfinite set theory.

But, since Wittgenstein's conventionalism apparently leaves enough room to endorse some aspects of game-formalism, especially the idea that mathematical propositions function like rules rather than being descriptive, Wittgenstein does not run into such a confusion, according to Wrigley.

4.2.2 Maddy

In *Wittgenstein's Anti-Philosophy of Mathematics* Penelope Maddy (Maddy 1993), too, recognizes that some remarks of Wittgenstein are at first sight reminding one of constructivist approaches. An example she finds in RFM VII reads as follows:

> A proof that shews that the pattern "777" occurs in the expansion of π, but does not shew where. Well, proved in this way this "existential proposition" would, for certain purposes, not be a rule. [...] is it reasonable to say of the proof concerned: it proves the existence of "777" in the expansion? This can be simply misleading.
> (RFM VII, 41)

But, like Wrigley, she argues that passages like this one can't be evidence for Wittgenstein being a constructivist if one takes his non-revisionist claims seriously. (cf. Maddy 1993, p. 55) In contrast to Wrigley, Maddy does not interpret all of Wittgenstein's remarks on mathematics as being a manifestation of his conventionalism. It is rather his way of doing philosophy, his "anti-philosophy" which sheds the right light on his views. With "anti-philosophy" she means Wittgenstein's method of not offering explanations or theses to solve philosophical problems but to "concentrate on uncovering the linguistic sleight of hand that started us philosophizing in the first place." (ibid., p. 58) So, not an improvement of our ordinary language nor of mathematical languages is what philosophy aims at, but a clear view of languages in the way they are used is what is aimed at.

Recall our example of the word 'knowledge' from chapter 3. We have seen that in Wittgenstein's eyes philosophical confusions arise when a word like 'knowledge' is regarded to function in the same way as a referring word like 'house', for example. Only a surview of the usage of a word like 'knowledge' in everyday language without any philosophical prejudice will make classical philosophical problems concerning the concept of e.g. knowledge dissappear and show these problems to be based on confusion. Or, in Wittgenstein's words, "philosophical problems arise when language *goes on holiday*." (PI, 38) And "[t]he confusions which occupy us arise when language is like an engine idling, not when it is doing work." (PI, 132) This distinction between "language idling" and "language doing work" is employed by Maddy to save Wittgenstein's critique of set theory from revisionism. We have already seen that Wittgenstein hopes and believes "that a future generation will laugh at this hocus pocus [transfinite set theory]." (RFM II, 22) In addition to this, as a reply to Hilbert's words "No one shall drive us out of the paradise Cantor has created for us" (Hilbert 1926, p. 191), Wittgenstein in his *Lectures on the Foundations of Mathematics* states:

> I would say, "I wouldn't dream of trying to drive anyone out of this paradise." I would try to do something quite different: I would try to show you that it is not a paradise – so that you'll leave of your own accord. I would say, "You're welcome to this; just look about you." (LFM, p. 103)

So, what's wrong with Cantor's paradise, and how is trying to leave mathematicians their way out of it not meddling with mathematical practice? As I suggested right above, Maddy's answers to both these questions refer to the distinction between actual use of language and "language going on holiday". There is evidence that the expressions used in set theory are an example of "language going on holiday" in Wittgenstein's opinion that Maddy finds in RFM V:

> 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. (RFM V, 2)

These signs get their meaning only by the application of mathematical signs to the non-mathematical world. Without application to the world outside mathematics, we are only dealing with meaningless signs.[25] So, this suggests that Wittgenstein only acknowledges applied mathematics to be real, meaningful mathematics, whereas pure mathematics is only a meaningless game. As Maddy reads him, Wittgenstein denies there being any application of the signs occurring in Cantor's calculus, i.e. transfinite set theory is not seen as meaningful mathematics but as a meaningless formal game. The problem with pure mathematics for Wittgenstein is, according to Maddy, that it originates in a Platonist way of thinking about mathematical objects and their properties. And this Platonist thinking is a consequence of not being clear about the words mathematicians use when they talk about mathematics. Maddy, too, refers to the distinction between calculus and prose which Wittgenstein made in conversation with Schlick and Waismann to make her point. What Wittgenstein is criticising is the prose used by mathematicians to describe their calculi. And thereby the distinction between meaningful and meaningless mathematics will become clear. "[A] piece of applied mathematics will retain its use even if the 'prose' is eliminated; its interest derives from its effectiveness in science. The interest of a piece of unapplied mathematics, on the other hand, cannot be traced to its use in science." (Maddy 1993, p. 68) Thus it is the interest in set theory that Wittgenstein wants to challenge. If the only interest in it originates in a whole misconception of mathematics and some pictures mathematicians apply due to this misconception of the calculi, this is not a direct intervention to mathematical practice, as Maddy sees it. And since there is no direct intervention, it is still legitimate to see Wittgenstein's "anti-philosophy" of mathematics as non-revisionistic, which separates his views from the views of constructivists. Still he has an "intent to curtail mathematics, albeit indirectly." (ibid., p. 70)

Unfortunately, Maddy doesn't discuss further the sort of indirect curtailment of her reading of Wittgenstein's views. She seems to be satisfied with the solution of Wittgenstein's way of criticising set theory is not directly intervening with the calculus part of it. The question whether an indirect curtailment really does live up with explicit non-revisionism is not elaborated.[26]

[25] In chapter 5 we will take a closer look at this issue and at the distinction between meaningless formal sign games and mathematics and its relations to Wittgenstein's proximity to formalism.

[26] These kind of considerations will be adressed in the next chapter.

4.2.3 Gefwert

In a similar way, Christoffer Gefwert (Gefwert 1998) uses Wittgenstein's distinction between 'calculus' and 'prose' to make sense of Wittgenstein's remarks on Cantor. Gefwert notes that for Wittgenstein the 'foundations of mathematics' are not to be found in the sort of foundational inquiry Frege, Russell, Hilbert and the likes had thought they were.[27] Rather the "basis for all mathematical understanding is the fundamental practical skill to perform proofs and computations." (ibid., p. 236) These practical skills are needed for doing elementary school arithmetics as well as for higher mathematics. The ascription of such a view to Wittgenstein – to see the 'foundations' of mathematics in the skills which are needed to execute even the most basic calculations – is given some evidence by Gefwert by a remark in RFM VII. "Once you have described the procedure of this teaching and learning, you have said everything that can be said about acting correctly according to a rule. We can go no further." (RFM VII, 27) In exercising basic mathematics and in describing what one does, there is no risk of 'prose' sneaking in and giving the calculations misleading meanings. But within higher mathematics – especially in those areas that are commonly seen as the foundations of mathematics – there is the danger of prose expressions "creeping in [...] in connection with the actual demonstrative proofs". (Gefwert 1998, p. 237) The danger of 'prose' creeping in into transfinite set theory and of giving the calculus a meaning which is not to be found within the calculus itself was highlighted by Wittgenstein in RFM II, as Gewert notes:

> The result of a calculation expressed verbally is to be regarded with suspicion. The *calculation* illumines the meaning of the expression in words. It is the *finer* instrument for determining the meaning. If you want to know what the verbal expression means, look at the calculation: not the other way about. The verbal expression casts only a dim general glow over the calculation: but the calculation a brilliant light on the verbal expression. (RFM II, 7)

[27] Of course, the foundational outlooks of these figures differ from each other quite dramatically. Still, they are similar to each other in the way that all of these tried to reduce all mathematics to a basis. And this kind of attempted reduction is criticised by Wittgenstein indiscriminately. (cf. Mühlhölzer 2010, pp. 8-9)

Gefwert interprets this passage to express the philosopher's task to "identify and exorcise" (Gefwert 1998, p. 237) the prose. In Gefwert's eyes this finding and exorcising of the prose surrounding a calculus does not interfere with mathematics itself. "Note that this activity has no impact whatsoever on the genuine content of mathematical proofs (provided that they are correct)." (ibid.) Since Gefwert also takes Wittgenstein's non-revisionistic claims seriously (cf. ibid., p. 243), it is important that the extinction of 'prose' does not affect mathematical proofs. It is rather a categorical distinction between the expressions 'infinite class' and 'finite class' that Wittgenstein wants to highlight. The starting point of the whole confusion that leads to a certain interpretation of Cantor's calculi lies in not seeing that the concepts of infinite class and the concept of finite class are not used very differently in ordinary language, according to Gefwert's Wittgenstein. "First, 'class' means *totality* when it is used in the context of a finite ('constructive') calculus, i.e. a group of objects all sharing the same property [...]. Secondly, in its 'infinite' ('classical') framework, 'class' signifies a rule-governed series, that is, the possibility of constructing a series *ad infinitum* by the reiteration of an operation." (ibid., p. 244)

In summary, Gefwert reads Wittgenstein's critique on set theory against the background of his non-revisionist claims. He too refers to the calculus – prose distinction to accomplish this task. Furthermore he highlights Wittgenstein's stressing on the different uses of 'infinite class' on the one hand and 'finite class' on the other hand.

4.2.4 Marion

Another proposal how to read Wittgenstein's critique of set theory is given by Mathieu Marion's *Wittgenstein and the Dark Cellar of Platonism* (Marion 1993). As we have seen e.g. in the discussion of Wrigley's interpretation, one idea that many commentators have taken to be central in Wittgenstein's philosophy of mathematics is his denial of theorems and proofs as being propositions which can be true or false. A piece of mathematics is not necessarily true or false on this view, it is rather like a rule which predetermines the truth or falsity of non-mathematical, i.e. empirical propositions. This is also the view attributed by Marion to Wittgenstein. Marion calls this view an "extreme anti-descriptivist" view. (ibid., p. 112) As the name suggests, in such a view the mathematician does not describe an eternal reality, which is discovered but the mathematician "is an inventor, not a discoverer" (RFM I, 68), as quoted by Marion. Wittgenstein's anti-descriptivism combined with the by now well-known distinction between calculus and prose is Marion's key to save Wittgenstein's statements from revisionism. When mathematics is regarded as being only calculus, not meaning, then, according to Marion "a bit of common sense will tell us that once an algorithm is presented, it can't be discarded later on as if it was not a good piece of mathematics." (Marion 1993, p. 113) Still, sometimes there can be found confusion amongst mathematicians about their work. The only possible root for this confusion then is the prose. "Therefore, it is a consequence of the thesis that mathematics consists in providing algorithms that philosophical mistakes must come only from the prose, and only these confusions could then be made to disappear as a result of philosophical enquiry" (ibid.). In set theory, as in any part of mathematics as a calculus, the purely formal aspects are all right. "Clearly, Wittgenstein didn't want to question the cogency of set theory either, but to question the underlying misconceptions about the calculus" (ibid., p. 114). In this case, the underlying misconception is that of the existence of an actual infinite and even worse, the attempt to describe the actual infinite. The actual infinite cannot be represented directly, since we only have finite methods for representing things directly. So, it has to be described, but a description of an infinite series is of course opposed to an extreme anti-descriptivist approach to mathematics. Marion finds this in PG: "When set theory appeals to the human impossibility of a direct symbolisation of the infinite it brings in the crudest imaginable misinterpretation of its own calculus" (PG, p. 469). Now the philosopher, even if he is not in a position to criticise the calculus part, can criticise the prose part. And this does have an effect on the calculus, as Marion admits. "And if one eliminates these confusions, there is no more need for the calculus, even *qua* calculus it is correct." (Marion 1993, p. 115) The calculus may be correct in itself but based on confused conceptions. If one clears these confused conceptions, it will die out, so to speak. But, as opposed to the the interpretations we have considered so far, Marion sees a problem here. Set theory is not and has never only been an attempt to give a foundation to mathematics and to describe the actual infinite. Set-theoretical methods are and were applied in much more "moderate" areas.[28] The example

[28] The very same point is also made by Wrigley in another paper of him (Wrigley 1980).

Marion gives is point-set topology. The question is whether Wittgenstein was aware of that. "The extent of Wittgenstein's knowledge of these questions is far from being clear. It is true that Wittgenstein did not do enough work and that he lost touch with foundational research in the mid-thirties." (ibid., pp. 115-6) Given this, Wittgenstein can be described as an attempted non-revisionist. Had he better known the applications of set-theoretical methods apart from foundational programmes, his critique would have turned out differently.

4.2.5 Frascolla

Another way to approach Wittgenstein's considerations on Cantor's proof is to focus on the meaning, the semantics of the concepts of denumerability and of non-denumerability. In his *Wittgenstein's Philosophy of Mathematics* Pasquale Frascolla (Frascolla 1994) proposes his reading by comparing a Wittgensteinian conception of meaningfulness of mathematical concepts such as 'non-denumerability' with a more commonplace conception of meaning of this concept as can be found in Kreisel (Kreisel 1959) for example. In Kreisel's view, a sentence such as "the set of all sequences of natural numbers is not enumerable" did have a meaning before its truth was established by Cantor's proof. Since the words which are involved in the sentence had a meaning all along, the composed sentence had one too. What is added to the meaning by the proof is the establishment of its truth. (cf. Frascolla 1994, p. 157) For Wittgenstein this cannot be right, according to Frascolla. It is the proof that confers the meaning to a mathematical expression. And this view is argued for by Wittgenstein by means of a discussion of the diagonal argument. This "meaning-via-proof" view, Frascolla finds at the beginning of RFM II, a statement with which we have become acquainted already:

> The result of a calculation expressed verbally is to be regarded with suspicion. The *calculation* illumines the meaning of the expression in words. It is the *finer* instrument for determining the meaning. If you want to know what the verbal expression means, look at the calculation: not the other way about. The verbal expression casts only a dim general glow over the calculation: but the calculation a brilliant light on the verbal expression. (RFM II, 7)

For Frascolla, Wittgenstein identifies the typical mistake people do in thinking about Cantor's proof by not acknowledging that certain expressions lack a meaning before the proof was established. One only thinks that one knows the meaning of these expressions, but in fact these "meanings" are only misguided analogies. And the alleged meanings of expressions such as 'non-denumerability' are transferred from a time before Cantor's proof was known to a time after it has become known. The misleading views on infinity and non-denumerability arise from this ascription of the "meaning" one had in mind before knowing Cantor's results to his calculations.

Frascolla formulates the problem of the meaning of the question "Can the set **R** of real numbers be ordered in a denumerable series?" by means of rule-following considerations. If this question did have a clear meaning, "the extension to **R** of the analogy between the structures previously acknowledged as denumerable would need to be conceivable; but that could only happen if either the analogy in question had also been acknowledged [...] or, at least, if there were a method of deciding, given an arbitrary set, whether it can be ordered in a series or not." (Frascolla 1994, p. 158) But of course, neither of the two possibilities is the case at the time before Cantor's proof was known. Only the proof gives meaning to the concept of non-denumerability of the set of real numbers. But the meaning the proof confers to the concept is *not* the meaning one has in mind when being misled by dubious analogies. To get a clear idea of its meaning, one has to look at the proof carefully and ask what it really establishes and what it doesn't. In this sense, Frascolla reads the following:

> Asked: 'Can the real numbers be ordered in a series?' the conscientious answer might be: 'For the time being I can't form any precise idea of that'. (RFM II, 16)

What the proof shows is not that there are different kinds of infinities but that the concept of real number cannot be compared with the concept of natural number in means of cardinalities of their sets. So, one precondition of the diagonal argument is questioned by Wittgenstein. The assumed ordering of the real numbers analogous to the ordering of the natural numbers cannot be taken for granted. The problem is that normally, it is taken for granted and this is the first seed of the whole misconception of transfinite set theory. What then, if we don't assume that the real numbers can be ordered in a similar way in which the natural numbers can be? And what are the implications of this with regard to the rest of the diagonal argument, Frascolla asks. First of all, (like all proofs) Cantor's proof plays the role of a paradigm. Not in the sense that it establishes an indisputable truth, but in the sense that it changes our linguistic practice. "[T]he grammar rule is adopted, that by this expression ['denumerable sequence of naturals which is different from all the denumerable sequences of naturals that belong to a given enumeration'], one must mean the diagonally generated sequence." (Frascolla 1994, p. 159) And since this grammar rule has been established, it is just natural, Frascolla says, that the following statement is excluded from our linguistic practice: "denumerable sequence of all the real numbers". (cf. ibid.) This last statement is now, after Cantor's proof has become known, senseless. This is because the meaning of this statement is determined by Cantor's method. For the classical mathematician, the meaning of this statement was the same at the time before Cantor's proof was known as at the time after it was known. The only difference for the classical mathematician is that now its falsity has been established. Frascolla stresses that it is important to notice that this way of seeing things does not make Wittgenstein a revisionist. It is only the interpretation of Cantor's proof which Wittgenstein is considering "without attacking its mathematical validity" (ibid., p. 160). But, in a different way, Wittgenstein's considerations are revisionist, or quasi-revisionary, as Frascolla puts it (adopting this term from Wrigley). Since for Wittgenstein, a new interpretation of Cantor's results which does not rest on the idea that we can compare the sizes of the set of the naturals and the set of the reals, the mathematicians' interest in transfinite set theory will be diminished drastically. In this quasi-revisionist or indirectly revisionist sense, as I have called it, Frascolla, just as e.g. Maddy and Wrigley, sees no other way around than to attribute some sort of revisionist impact to Wittgenstein's writings concerning set theory.

4.3 Intermediate Conclusion

We have seen how various interpreters of Wittgenstein's considerations on Cantor's diagonal argument in RFM II approach his writings in different manners. The interpretations considered here all agree that Wittgenstein's explicit non-revisionist statements of intent are to be taken seriously for an adequate understanding of ideas concerning Cantor's proof and set theory.

I think it would be futile to evaluate all of these interpretations and present an opinion which one of these is the best one, or the one that comes closest to Wittgenstein's actual thoughts. Rather, I would like to outline how a formalist reading of Wittgenstein can contribute to the debates just presented. Since it is quite impossible (at least for me) to give an illustration what exactly formalism as a philosophy of mathematics is, it will be equally impossible to give a kind of indication of quantity to what extent Wittgenstein can and shall be read as a formalist and then try to see how such a measuring of formalist ideas, so to speak, will clarify the debate. But I think that it can be helpful to look at some literature that has ascribed some formalist aspects to the philosophy of mathematics of the later Wittgenstein and see how these readings may contribute to the question in which way his ideas on Cantor were revisionist or non-revisionist, in a similar way in which a formalist reading of Wittgenstein can illuminate his ideas on the surveyability of a proof.

5. Wittgenstein, Formalism and Non-Revisionism

5.1 Mathematics is normative – Friederich

In the introductory part of this thesis, we have seen that some commentators see a relation between Wittgenstein's ideas on mathematics and formalism. One way to see this relation is to stress Wittgenstein's idea that mathematics is normative. "What I am saying comes to this, that mathematics is *normative.*" (RFM VII, 61) This normative aspect of mathematics was especially stressed by Wrigley (Wrigley 1977) and Marion (Marion 1993), as we have seen right above. Both argued that in this normative aspect of seeing mathematics, the solution to many seemingly puzzling statements of Wittgenstein concerning mathematics can be found. Curiously, the stressing of mathematical propositions being normative rather than descriptive can be a way to connect Wittgenstein's ideas to some of Hilbert's ideas concerning the nature of mathematics. One way of describing Hilbert's attitude towards the axioms in an axiomatic system is to regard them as normative. Simon Friederich in his *Warum die Mathematik keine ontologische Grundlegung braucht* (Friederich 2014) sees in Hilbert's method of implicit definitions the idea that parts of mathematics are only normative, not descriptive. This idea is adopted by William Tait's deflationism. In a nutshell, Tait's version of deflationism is the view that questions concerning existence and truth of mathematical entities resp. propositions can only be raised and answered within an axiomatic system, such as Hilbert's one. And this implies that questions on existence of mathematical objects or on the truth of mathematical propositions *per se* are senseless.

As we have seen in section 2, in Hilbert's system axioms are implicit definitions. Now Friederich's idea is that Wittgenstein and Hilbert share some fundamental views on the normativity of mathematics. The crucial difference between them is that for Hilbert only the axioms, the implicit definitions, have this normative status whereas for Wittgenstein all parts of mathematics are normative and not descriptive. Wittgenstein's idea that all theorems and not only axioms are normative is considered to be an expansion of Hilbert's ideas by Friederich. This Wittgensteinian expansion of the normativity of implicit definitions is taken by Friederich to be a solution to the problem of the connection between axioms which are exclusively normative in Hilbert's system and the descriptive theorems which derive from these axioms. If simply all parts of mathematics are just normative and not only the axiomatic foundation then there is no clear cut between the axioms and the theorems and therefore no problem of the connection between them. Another benefit of this view that Friederich sees is that such a Wittgensteinian normative view of mathematics may be helpful for a Tait-like deflationism to show that mathematics regarded as Hilbert's axiomatic system does not need any ontological foundation. But since going into these details would take us too far afield, it is sufficient for us to see this connection between formalism and Wittgenstein via the normative status of (some parts of) mathematics.

Although both Hilbert and Wittgenstein can be read as holding a view that mathematics is at least partially a normative activity, they differ on a variety of other questions. If Cantor's paradise is indeed a paradise or not, is one vivid manifestation of this. One reason for their radically differing views on transfinite set theory can be seen by asking what kind of normativity Hilbert means and what kind of normativity is meant by Wittgenstein. Within a Hilbertian Framework, the norms governed by the implicit definitions, are constrained to the mathematical realm. But the normativity of mathematics in a Wittgensteinian sense is not constrained to one domain. Quite the contrary is the case. For Wittgenstein it is eminently important to see mathematics as governing the way we speak in the non-mathematical world. A proposition of the kind '2 + 2 = 4' is governing the way we count apples, for instance. In the case of talking about the infinite, there is no such thing as a calculation governing the way we speak ordinarily about the infinite, according to Wittgenstein. When using the words 'infinite' or 'infinity', we use it in a context in which we want to express that there is no explicit boundary to some linguistic practice. So, to get a better view on how Wittgenstein's criticism on the diagonal argument is connected with his formalist aspects of his philosophy of mathematics, it will be helpful to understand how the application of mathematics to non-mathematical practices is required by Wittgenstein. Victor Rodych has proposed an interpretation among these lines in two papers (Rodych 1997, Rodych 2000). I will present both of them in the next sections in detail.

5.2 Strong or Weak Formalism?

Victor Rodych distinguishes four different kinds of formalism in his *Wittgenstein on Mathematical Meaningfulness, Decidability, and Application* (Rodych 1997) – strong, formalism (SF), weak formalism (WF), extreme formalism (EF) and radical formalism (RF) – and attributes weak formalism to the later Wittgenstein (as well as to the Tractarian Wittgenstein) and strong formalism to the intermediate Wittgenstein. Since only these two versions of formalism are ascribed to Wittgenstein by Rodych, it will be sufficient to only consider SF and WF. I will outline a way in which this sort of taxonomy can be helpful in the debate whether the remarks of RFM II can be read in a non-revisionist way or not alongside with Rodych's proposal in the following. First of all, let us look at how Rodych characterises strong formalism and weak formalism.

> *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, pp. 196-7)

Rodych's ascription of SF to the intermediate Wittgenstein is due to some passages in which Wittgenstein seems to explicitly reject the necessity of extrasystemic application of mathematics. For example in PR, Wittgenstein writes:

> 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.
> 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)

Talking about arithmetics taking care of its own application seems indeed to point towards what Rodych calls strong formalism. Remarks such as this one are taken by Rodych to be evidence for an understanding of the intermediate Wittgenstein that sees "meaningfulness and the truth or falsity of propositions within a calculus [which] are determined entirely by the axioms and rules of operation of that calculus, without any reference to an extrasystemic application" (Rodych 1997, p. 200). In this reading, mathematical propositions are true or false for the intermediate Wittgenstein, but the truth and falsity is exclusively determined within the formal system. Even if some extrasystemic application is added to the system, this does not affect mathematics. That this is essentially the view that the intermediate Wittgenstein had in mind, Rodych finds in a later passage of PR.

> But what does the application add to the calculation? Does it introduce a new calculus? In that case it isn't any longer the same calculation. Or does it give it substance in some sense which is essential to mathematics (logic)? If so, how can we abstract from the application at all, even only temporarily?
> (PR, p. 309-10)

As we have seen, SF as formulated by Rodych is exactly this view. The meaningfulness of mathematics is only determined by the intrasystemic rules. The calculus may or may not be applied to the non-mathematical world, but this does not affect the calculus in any way.

For the later Wittgenstein, extrasystemic application is not just something that may or may not be added to a calculus but which leaves mathematics as it is, in Rodych's eyes. In RFM, extrasystemic application is a necessary condition for mathematics. Especially in part V there are passages which indicate the need for an extrasystemic application to a formal sign game to be not only a sign game but mathematics, as Rodych points out. For instance we read that "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." (RFM V, 2) Rodych argues that the views of the later Wittgenstein are still formalistic; evidence hereof can also be found in RFM V:

> It is of course clear that the mathematician, in so far as he really is ‘playing a game’ . . . [is] acting in accordance with certain rules. (RFM V, 1)

> To say mathematics is a game is supposed to mean: in proving, we need never appeal to the meaning of the signs, that is to their extramathematical application. (RFM V, 4)

But now the application of the signs in the calculus to the non-mathematical world is an important condition for a mere sign game to be mathematics. With other (Rodych's) words, Wittgenstein abolishes his strong formalism of the intermediate writings and adopts a weak formalism. In WF, as we have seen, an application of a formal calculus to a real world domain is a necessary condition.

5.3 Formal Games and "Real Mathematics"

The remainder of this chapter is a summary of Rodych's arguments presented in his *Wittgenstein's Critique of Set Theory* (Rodych 2000). So, all quotations of Wittgenstein in the following paragraphs are used in Rodych's reading.

First Rodych highlights Wittgenstein's use of intensionalism and extensionalism in mathematics. Following (Rodych's) Wittgenstein intensions are mathematical rules and extensions are signs and their concatenations. The only thing mathematicians do and can do is use these intensions and extensions, but not describe them. The problem with set theory is that it conflates the intensions and the extensions. "By using *intensional descriptions* of 'infinite sets,' TST [transfinite set theory], *pretend*s to give us infinite *extensions*, and in this way it 'buys a pig in a poke.'" (cf. ibid., p. 284). "TST says, in effect, that we can *in principle* really represent an infinite set by an enumeration, but because of human or physical limitations, we will instead *describe* it intensionally." (cf. ibid., pp. 284-5) But this is a mistake. In mathematics we cannot describe something which is in principle possible but actually impossible. Rodych quotes a passage from PG to support this. There are only actual calculi in mathematics and these are "concerned only with the signs with which it actually operates". (PG, 469) And furthermore, the fact "that we can't describe mathematics, we can only do it [...] abolishes every 'set theory'". (PR, 159) This sharp distinction between intensions and extensions amounts to the view that the formula e.g. $m = 2n$ is not a description of a relation between two infinite classes but a *rule* of an infinite *process* which can be used to generate natural numbers without us being limited by the rule. (cf. Rodych 2000, p.285)

The next section of Rodych's paper deals with "Wittgenstein's Finitism" (ibid., pp. 285-290). In a passage of PG quoted by Rodych (ibid., p. 286), Wittgenstein says, "[w]e mistakenly treat the word 'infinite' as if it were a number word, because in everyday speech both are given as answers to the question 'how many'" (PG, 463). Before continuing with the summary of Rodych's paper, I want to say a few words on this. Above we have seen that it is the use of a word in our ordinary language that we have to survey and if we do this correctly, no philosophical confusions will arise. But the just quoted passage sounds clearly different. So what are we to do with that? It should be mentioned that the slogan "the meaning of a word is its use" is not the whole story. In PI we read "For a *large* class of cases – though not for all – in which we employ the word 'meaning', it can be defined thus: the meaning of a word is its use in the language." (PI, 43) So there seem to be cases in which the meaning of a word is not solely determined by its use. Apparently in the case of 'infinity' it does not suffice to look at its use in our every day language to get a clear understanding of its meaning. Here, its use is misleading us. A criterion for the appropriate application of 'infinite' to the concept of 'class' can be found in WVC, which Rodych also quotes (Rodych 2000, p. 286):

> We cannot imagine the same class finite at one time and infinite at another. The truth of the matter is that the word "class" means completely different things in

> the two cases. It is not one and the same concept at all that is qualified by the addition of "finite" or "infinite" (WVC, p. 228)[29]

So, according to Wittgenstein, one criterion of commensurability of two concepts is this: if it is possible for us to imagine of a concept to have a certain property and it is also possible for us to imagine of the concept to have the other property considered, then we are dealing with the same type of concept. If it is not possible to do so, we are not dealing with the same type of concept. Since it is not possible for us to imagine a class being finite and to imagine the *same* class being infinite, we are dealing with two different types of the superficially identical looking concepts of 'class'. In the case of finite classes we are speaking of extensions of classes such as {1, 3, 5, 7} for which we can meaningfully speak of its cardinality. But we are not in a position to apply the concept of cardinality to infinite classes, since the meaning of this concept is a completely different one. To apply the concept of infinity to classes is just such a confusion due to the superficial similarities of the words 'finite' and 'infinite'. Rather than speaking of actual infinite sets, we should regard the appropriateness of speaking of infinity in such cases in our infinite possibility of adding members to that set. So, 'infinity' tells us something about the rule with which we construe a class[30] or a series but not about the magnitude of that class or that series. Speaking of the finiteness of a class, on the other hand, does tell us something about its magnitude. Another passage taken from WVC is used by Rodych to emphasize Wittgenstein's categorical distinction of these two concepts:

> A correct symbolism has to reproduce an infinite class in a completely different way from a finite one. Finiteness and infinity of a class must be obvious from its syntax. In a correct language there must not even be a temptation of raising the question whether a class is finite of infinite. (WVC, p. 228)

[29] The reader may be wondering if passages from Wittgenstein's middle period such as this one are a good way of getting a good picture of the late Wittgenstein. At least Rodych argues that "Wittgenstein's finitistic rejection of *infinite mathematical extension* has not changed one iota." (Rodych 2000, p. 289)
[30] Rodych uses the terms 'set' and 'class' interchangeably.

Having this in mind, we are in a position to understand what Rodych means by ascribing some sort of finitism to Wittgenstein. The crucial question to ask whether the later Wittgenstein still maintains some finitistic ideas, says Rodych, is the following:[31] "Does Wittgenstein accept *infinite mathematical extension* in *RFM*?" (Rodych 2000, p. 288) Rodych's answer to the question is a clear no. In RFM V, for example we find indications for this. As an answer to the question whether there are three consecutive 7's in the decimal expansion of π, Wittgenstein replies: "Here you are thinking of a finite series" (RFM V, 11). It is only in the case of a finite series that we can give a definite answer to the question. In an infinite series where the pattern in question has not been determined yet, there can't be an answer since "[h]owever queer it sounds, the further expansion of an irrational number is a further expansion of mathematics". (RFM V, 9). Again, it is the superficial similarities between finite series where the question of an occurrence of a given pattern makes sense and infinite series where the same question makes no sense leads us to such confusions. By focusing on the intensional character of infinite series we come to see that irrational numbers are to be seen as rules and not as extensions. Though irrational numbers still are numbers in the way that we can calculate with them as we can calculate with rational numbers, this does not mean that irrational numbers share the extensional character of rational numbers. As a temporary upshot we can adhere Rodych's stressing that "[O]n Wittgenstein's view, neither 'infinite' nor '$\aleph_0$' is a 'quantity word'; both can only be used to refer to an *unlimited technique* or *infinite possibility*" (Rodych 2000, p. 290).

A failure to recognize this meaning of 'infinite' is one source of confusion which set-theorists fall prey to, as Rodych reads Wittgenstein. Another source of counfusion is a commonly adopted view on *continuity* and the mathematicians' attempts to describe it. An attempt of giving a conception of the set of all irrational numbers is the result of the mathematicians' idea of the number line. Since not all points on the number line are represented by rational numbers, there have to be numbers in between – the irrationals. And since these irrationals lie on the same line as the rational numbers we are in a position to compare the sizes of the sets of the two kinds of numbers. Furthermore, due to a foundationalist spirit, mathematicians

[31] There seems to be less controversy on the question whether the middle Wittgenstein maintained some finitistic ideas than on the question whether the later Wittgenstein from the RFM did so. I won't go into detail of that specific debate here.

(at least in that time) want to give "a *completed* set of real number and a 'gap-less' continuum" (ibid., p. 292), as Rodych puts it. And via Cantor's proof we see that the one infinitely great set is smaller than the other infinitely great set. But, as should be clear by now, something went wrong here, according to Wittgenstein. One thing that went wrong is an unquestioned adoption of an idea of the number line as a basis for the conception of irrational numbers. In RFM V, Rodych finds an indication for this:

> The picture of the number line is an absolutely natural one up to a certain point; that is to say so long as it is not used for a general theory of real numbers. (RFM V, 32)

Because of overstretching the idea of a number line, mathematicians come to think that irrational numbers are the same kind of things as rational numbers.
If one abandons an extensional conception of infinite sets and instead accepts an intensional conception of the infinite sets, one can make sense of Wittgenstein's remark on the impossibility of enumeration of the rational numbers.

> The rational numbers cannot be enumerated, because they cannot be counted – but one can count with them, as with the cardinal numbers. That squint-eyed way of putting things goes with the whole system of pretence, namely that by using the new apparatus we deal with infinite sets with the same certainty as hitherto we had in dealing with finite ones. (RFM V, 15)

Only because we can compare the sizes of finite sets, this does not mean that we can compare the sizes of infinite sets. Wittgenstein admits that we can assign numbers to each rational but to do so is not what is meant by the term 'enumerate'. The latter suggests, according to Wittgenstein, that it is in principle possible to count them all. And because mathematicians have a principled possibility of counting them all in mind, they are inclined to contrast such a possibility with the impossibility of counting all irrationals.

That this is done nevertheless rests on the misconceptions of infinite sets as extensions. This also means that not just Cantor's proof can't justify the conclusion that the set of real numbers is greater than the set of natural numbers. No proof could possibly ever show this, since categorically different conceptions of numbers can never be compared in such a way. As Wittgenstein sees it, the problem is that we only have a vague idea of an infinite series. And this vague idea is used to create the concept of 'all real numbers'.

As we have seen already, all of this sounds like a harsh critique on mathematical practice. A popular way of bringing in line Wittgenstein's critique of set theory with his explicit non-revisionist attitude is to refer to the calculus – prose distinction, which Wittgenstein made in WVC. On this view it is only the words surrounding the real proof that Wittgenstein is attacking. But this in no way interferes with the actual mathematical practice since philosophy "leaves everything as it is" (PI, 124). As I indicated above, it may be considered whether the distinction between calculus and prose is suitable to do the job in the way some commentators have argued for. In chapter four of his paper, Rodych is questioning the force of the calculus – prose distinction by focusing on Wittgenstein's stressing of the importance of the applicability of mathematics to the non-mathematical world. In the following parts of this chapter I will present his arguments.

There are reasons to see the intermediate Wittgenstein as a "strong formalist" as Rodych does, as we have seen in the preceding section. For a formalist "one calculus is as good as another" (PG, 334). Whatever one says or thinks about the calculus which is not to be found within the calculus itself is not relevant for it. So, if one criticizes some prose of the interpretation of the calculus, the mathematical part stays untouched. In exactly this vein we can read a remark as the following by the intermediate Wittgenstein:

> What set theory has to lose is rather the atmosphere of clouds of thought surrounding the bare calculus, the suggestion of an underlying imaginary symbolism, a symbolism which isn't employed in its calculus, the apparent description of which is really nonsense. (In mathematics anything can be imagined, except for a part of our calculus). (PG, 470)

Obviously, there is no attempt by the intermediate Wittgenstein to interfere with the calculus part of e.g. set theory. Neither was there an attempt by the later Wittgenstein to interfere with the calculus part as was indicated above. The crucial difference here between the intermediate Wittgenstein and the later Wittgenstein, according to Rodych, is Wittgenstein's rising awareness of the difficulty of only criticizing the prose part and thereby not criticizing the calculus part (cf. Rodych 2000, p. 300). Since the later Wittgenstein permanently struggles on questions concerning rule-following, one way of looking at the views of the later Wittgenstein concerning set theory is to ask what he has to say on mathematics as a rule-following activity. Mainly in the beginning of part V of the RFM, Rodych finds some interesting considerations on that topic. As was shown above, for the later Wittgenstein there is more to mathematics than simply blind rule following. Several examples are given whose purposes are to show that instruments or people, who are merely following mathematical rules without knowing what they are doing, aren't doing mathematics. Or at least we are reluctant to say so. One example is a calculating machine "which has come into existence by accident":

> Does a calculating machine calculate?
> Imagine that a calculating machine had come into existence by accident; now someone accidentally presses its knobs (or an animal walks over it) and it calculates the product 25 × 20. (RFM V, 2)

Wittgenstein denies the question that such a calculating machine calculates. His reasons for denying it lie in the way mathematical signs gain their meaning. Without a meaning outside a purely formal calculus they are dead signs and a game of arranging dead signs by following some specific rules is not enough for counting as mathematics. Therefore, directly after the just quoted passage, Wittgenstein continues:

> 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. (RFM V, 2)

Another example that points to the same direction is the "imbecile human calculating machine".

> A human calculating machine might be trained so that when the rules of inference were shewn it and perhaps exemplified, it read through the proofs of a mathematical system (say that of Russell), and nodded its head after every correctly drawn conclusion, but shook its head at a mistake and stopped calculating. One could imagine this creature as otherwise perfectly imbecile. (RFM V, 3)

Here too, Wittgenstein's point is that this human calculating machine is *not* doing mathematics. Rodych calls incidents of following a rule the way the human calculating machine does "*partially understood rule-following*" whereas a genuine mathematician is able to maintain "*fully understood rule-following*" (Rodych 2000, p. 301). The question then is: wherein lies the difference between partially understood rule-following and fully understood rule-following? The answer Wittgenstein gives is that neither the accidently occurred calculating machine whose knobs are randomly pressed, nor the otherwise imbecile human calculating machine are in a position to apply the things they are doing to the non-mathematical world. But it is not only the fact that a mathematician can apply their results to the non-mathematical world. It is also the way in which she does so. What if one treats the results gained by a calculating machine as experiments?

> Imagine that calculating machines occurred in nature, but that people could not pierce their cases. And now suppose that these people use these appliances, say as we use calculation, though of that they know nothing. Thus e.g. they make predictions with the aid of calculating machines, but for them manipulating these queer objects is experimenting. (RFM V, 4)

In this case too, there is only partially understood rule-following. Fully understood, mathematics is not treated as a special kind of experiment. Otherwise the distinct normative status of theorems and proofs gets lost. So one requirement for doing mathematics rather than just following rules in a calculus is the knowledge of how mathematical results can be used in the non-mathematical world. But still, the knowledge that mathematical results can be applied in a normative way to the non-mathematical world is not enough. Wittgenstein proposes to "imagine a situation in which calculating with $\sqrt{-1}$ [was] invented by a madman, who, attracted merely by the paradox of the idea, does the calculation as a kind of service, or temple ritual, of the absurd. He imagines that he is writing down the impossible and operating with it". (RFM V, 5) Neither in this situation is that "madman" doing mathematics (at least according to Rodych's reading). And the question whether this "madman" believes in mathematical objects and in its properties is irrelevant to the question whether he is doing mathematics. Wittgenstein also proposes to imagine another situation:

> Imagine someone who believes that mathematicians have discovered a queer thing, $\sqrt{-1}$, which when squared does yield –1, can't he nevertheless calculate quite well with complex numbers, and apply such calculations in physics? And does this make them any the less calculations?
> In one respect of course his understanding has a weak foundation; but he will draw his conclusions with certainty, and his calculus will have a solid foundation.
> Now would it not be ridiculous to say this man wasn't doing mathematics? (RFM V, 5)

Rodych takes this situation to be a clear contrast to the "madman" situation. Although Wittgenstein is not explicitly affirming the last question quoted, Rodych reads Wittgenstein as to firmly affirming it (cf. Rodych 2000, p. 302). The crucial point here is that in this situation now the person doesn't lack a conception of how to apply the sign-game to the real world. But the "madman" lacks exactly this conception. It is then the knowledge of the role a calculus plays in the non-mathematical world and the correct application to the "correct world", so to speak, that is, according to Rodych's Wittgenstein, additionally needed to the calculus to transform a mere sign-game into mathematics. In other words, a calculus which has no application to the non-mathematical world is not proper mathematics. That this view is held by Wittgenstein is strongly suggested by passages as the following, as Rodych notes: "Concepts which occur in 'necessary' propositions must also occur and have a meaning in non-necessary ones." (RFM V, 41) This is seen by Rodych as a necessary condition which he calls *extrasystemic application criterion* (APC).

Only if APC is fulfilled, is a purely formal sign-game a genuine part of mathematics. The question Rodych asks now is whether set theory fulfils the necessary condition APC. Evidently it does not. By calling Cantorian set theory "a piece of mathematical architecture that hangs in the air" (RFM II, 35), Wittgenstein denies the application of it to the non-mathematical world. However, the purely formal part of it is still legitimate. When Wittgenstein says that "these considerations may lead us to say that $2^{\aleph_0} > \aleph_1$" (ibid.), Rodych takes this remark to show exactly this. If we regard Cantor's proof only as a formal sign-game, then "Wittgenstein is (still) saying that anything is allowable in a formal calculus" (Rodych 2000, p. 304). But to apply a meaning to this calculus is the problematic part. Wittgenstein's considerations that follow in RFM II, 35 are pivotal in this sense.

> 'These considerations may lead us to say that $2^{\aleph_0} > \aleph_1$'
>
> 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. (RFM II, 35)

This is evidence for Rodych that Wittgenstein takes set theory to be a correct calculus, or sign-game. But, since it lacks application, i.e. it does not fulfil APC, it is not mathematics. And a formal sign-game which is not mathematics is because of its lacking application *meaningless*. Because of its meaninglessness it cannot give us any new information of the meaning of infinite classes. With other words, even though Wittgenstein grants that Cantor's proof is formally correct and even though "$2^{\aleph_0} > \aleph_1$" is formally correct within this calculus, the proposition that one infinite class is greater than another infinite class does not have any meaning. But it is solely propositions such as this one that are the reasons for mathematicians to be interested in set theory. The interested mathematician is invited to test his interests by considering Wittgenstein's arguments. She will thereby become aware that expressions such as 'the cardinality of an infinite class' are used too hastily. If she takes this into consideration then she will lose interest, or as Wittgenstein puts it:

> I would say, "I wouldn't dream of trying to drive anyone out of this [Cantor's] paradise." I would try to do something quite different: I would try to show that it is not a paradise – so that you'll leave of your own accord. I would say, "You're welcome to this; just look around you." (LFM, p. 103)

Can this kind of showing the mathematicians their misconceptions of certain central expressions with the aim of their losing interest in some branches of mathematics be made compatible with Wittgenstein's claims to non-revisionism? Rodych's answer to this question is the following: "The answer, I believe, is that although Wittgenstein tries very hard not to intervene and legislate, *he cannot help* doing so because he has very (very) serious misgivings about, and criticisms of, TST." (Rodych 2000, p. 306)

In summary, we can say that Rodych sees a close relation between some central ideas of what one can call game formalism and Wittgenstein's considerations on mathematics. Especially in his intermediate phase, Wittgenstein's views come close to strong formalism. This ascription can be helpful to understand why Wittgenstein seems to be more tempered during this period when speaking about the foundations of TST. But as the requirement for application of formal calculi to the non-mathematical realm gets more and more important, Wittgenstein writings on set theory get more and more criticising.

6. Conclusion

Wittgenstein's philosophy of mathematics has various facets. Not only did his views concerning philosophy, language, mathematics and other topics pass through dramatic changes during his intellectual life. His interpreters express these changes by referring to different periods of his work: The early Wittgenstein, the intermediate Wittgenstein and the later Wittgenstein are standardly taken to mark some central aspects of his philosophy. But not only are Wittgenstein's views highly varied between his different periods. What's more is that also within a given time, his thoughts on e.g.language and mathematics are extremely hard to grasp even remotely. One way to understand the later Wittgenstein's views on mathematics is to acknowledge a proximity between his ideas and some central ideas of formalism. In recent debates, different authors have done so by focusing on different aspects of Wittgenstein's philosophy of mathematics.

Felix Mühlhölzer has examined the 'surveyability' of a mathematical proof in a very thorough way. Wittgenstein's notion of surveyability of a proof takes a central place in RFM II – an assembly of Wittgenstein's manuscripts in which he discusses the possibility of a foundation of mathematics. In the discussion of RFM II, Mühlhölzer comes to the result that Wittgenstein has in mind a conception of surveyability that comes very close to the conception of surveyability, which David Hilbert – the leading figure of formalism in mathematics at the beginning of the 20th century – held. Such a formalist reading of Wittgenstein's notion of surveyability of a proof is opposed to other readings of this concept. Stefan Majetschak for example argues that when speaking of 'surveyability' of a proof, Wittgenstein means the same thing as when speaking of 'surveyable presentation' as in the context of his later philosophy of language. On this account it is the main goal of philosophy to give a surveyable presentation of our language and the concepts we use, to clarify the sources of philosophical confusion. This method is seen by Majetschak as essentially non-hypothetical, which is in turn the essential methodology of mathematics. James R. Brown provides another reading of surveyability of a proof, one that contrasts with Mühlhölzer's formalist reading. In this reading the use of pictures as proofs in mathematical contexts of Wittgenstein is highlighted. Brown argues that for Wittgenstein it is the sense (in a Fregean fashion of the term) of a picture, which we can grasp, or survey and because of this surveying of the picture's sense we can use pictures as rigorous proofs. And this is also possible when the picture is flawed, as it is the case with Wittgenstein's example that Brown discusses. An essential difference of Brown's understanding of surveyability of a proof and Mühlhölzer's understanding is that in Brown's reading, Wittgenstein is speaking of the surveyability of proof ideas. On Mühlhölzer's formalist interpretation this is exactly not the point of 'surveyability'. It is our surveying of every sign that occurs in a proof that is distinct of the role that a proof plays in our lives.

Another topic that has occupied interpreters of Wittgenstein's philosophy of mathematics is his attitude towards Cantor's diagonal argument and the foundations of transfinite set theory. In RFM III, for example, Wittgenstein seems to criticise set theory quite harshly. On the other hand, on several occasions, Wittgenstein claims that it is of greatest importance not to intervene with mathematical practice from the point of view of a philosopher. Different commentators have dealt with this problem by different means. Most of them agree that the distinction between the core of mathematics, the calculi, and the way mathematicians use to talk about the calculi, the prose, is of central importance here. The main differences among the interpretations can be found in the way this calculus – prose distinction fits in the general outlook of Wittgenstein's philosophy of mathematics. That Wittgenstein's conventionalism is the key has been suggested (Wrigley), or his "anti-philosophy" (Maddy), his theory of meaning (Frascolla) and so on. Another proposal was offered by Victor Rodych, namely that Wittgenstein's closeness to formalism can serve as a good approximation to his considerations on set theory and that this closeness to formalism can also serve as a kind of indicator up to which extent Wittgenstein's non-revisionistic agenda is held up by himself. One problem we have seen was Wittgenstein's stressing of the application of formal calculi. Without an application to the non-mathematical world, formal calculi are merely "dead" sign-games. For Wittgenstein, transfinite set theory does not have a non-mathematical application and is therefore, at least in Rodych's eyes not real mathematics.

To acknowledge some closeness to formalism in Wittgensteins's philosophy of mathematics will not solve every interpretational or other problem surrounding this area. Wittgenstein's thoughts on mathematics are much too manifold, their roots and their ramifications are far too intertwined with his thoughts on language, psychology, epistemology and many other topics to even get close to this task. But it can, as I hope to have shown, shed some light on some central ideas of Wittgenstein and it can also shed some light on some central questions surrounding the debate concerning Wittgenstein's philosophy of mathematics.

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