

**QUANTIFICATION AND FINITISM**  
**A STUDY IN WITTGENSTEIN'S PHILOSOPHY OF**  
**MATHEMATICS**

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A THESIS SUBMITTED FOR THE DEGREE OF DOCTOR OF PHILOSOPHY  
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À Hélène, Nicolas, Jacques

What are the roots that clutch, what branches grow  
Out of this stony rubbish? Son of man,  
You cannot say, or guess, for you know only  
A heap of broken images ...

T. S. Eliot

And I grew wary of the sun  
Until my thoughts cleared up again  
Remembering that the best I have done  
Was done to make it plain

W. B. Yeats

# ABSTRACT

## Quantification and Finitism A Study in Wittgenstein's Philosophy of Mathematics

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My aim is to clarify Wittgenstein's foundational outlook. I shall argue that he was neither a strict finitist, nor an intuitionist, but a finitist (Skolem and Goodstein.)

In chapter I, I argue that Wittgenstein was a "revisionist" in philosophy of mathematics. In chapter II, I set up a distinction between Kronecker's divisor-theoretical approach to algebraic number theory and the set-theoretic style of Dedekind's ideal-theoretic approach, in order to show that Wittgenstein's remarks on existential proofs and the Axiom of Choice are in the constructivist tradition. In chapter III, I give an exposition of the logicist definitions of the natural numbers by Dedekind and Frege, and of the charge of impredicativity levelled against them by Poincaré, in order to show, in chapter IV, that Wittgenstein's definition of the natural number in the *Tractatus Logico-Philosophicus* was constructivist. I also discuss the notions of generality and quantification, and Wittgenstein's later criticisms of the notion of numerical equality.

In chapter V, after discussing the current strict finitist literature, I reject the contention that Wittgenstein's remarks give support to such a programme, by showing that he adhered to a potentialist view of the infinite, and, moreover, that his "grammatical" approach provides him with an argument against strict finitism. In chapter VII, I also reject the identification of his remarks about "surveyability" with the strict finitist insistence on "feasibility."

In chapter VI, I describe the *Grundlagenstreit* about the status of  $\Pi_1^0$ -statements. Wittgenstein's views on generality, induction, and the quantifiers lead to a rejection of quantification theory which sets him apart from intuitionism, and closer to finitism. I also examine Wittgenstein's argument against the Law of Excluded Middle.

In the last chapter, I discuss Wittgenstein's prescriptions for the formation of real numbers, showing that they imply a constructivization of the Cauchy sequences of the type of Bishop or of the finitist "recursive analysis", and the rejection of the intuitionistic notion of choice sequences.

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# CONTENTS

<b>Abstract</b>	<b>iii</b>
<b>Acknowledgements</b>	<b>iv</b>
<b>INTRODUCTION</b>	<b>1</b>
<b>I. PHILOSOPHY AND MATHEMATICS</b>	
1. Revisionism	6
<b>II. EXISTENCE AND THE AXIOM OF CHOICE</b>	
2. Existential and Constructive Proofs	24
3. The Axiom of Choice	34
<b>III. THE LOGICIST DEFINITIONS OF THE NATURAL NUMBERS</b>	
4. Dedekind's Chains and Frege's Hereditary Sets	43
5. Circularity and Impredicativity	54
<b>IV. THE CONSTRUCTIVISM OF THE <i>TRACTATUS LOGICO-PHILOSOPHICUS</i></b>	
6. A Constructive Definition of the Natural Numbers	62
7. Quantification	69
8. Numerical Equality, Predicativity and choice	80

<b>V. STRICT FINITISM AND THE INFINITE</b>	
9. What is Strict Finitism?	89
10. The Legacy of Aristotle and Frege	102
11. The Grammar of the “and so on”	110
12. An Argument against Strict Finitism	117
<b>VI. QUANTIFICATION AND THE EXCLUDED MIDDLE</b>	
13. Generality, Induction and the Quantifiers	122
14. On the Law of the Excluded Middle	149
<b>VII. SURVEYABILITY</b>	
15. Surveyability	166
16. Dummett’s Interpretation	171
<b>VIII. THE CONTINUUM</b>	
17. Cauchy Sequences	184
18. Choice Sequences	194
<b>CONCLUSION</b>	201
<b>Appendix 1</b>	206
<b>Appendix 2</b>	208
<b>REFERENCES</b>	214

## INTRODUCTION

The last decade saw the publication of many studies on Ludwig Wittgenstein's philosophy of mathematics, starting with C. Wright's **Wittgenstein on the Foundations of Mathematics**, in 1980.<sup>1</sup> The impetus for this renewal of interest came from the publication of S. Kripke's controversial study of the rule-following argument (PI: § 138-242), **Wittgenstein on Rules and Private Language** (Kripke 1982), and from the emergence of a new trend in Wittgensteinian scholarship, around the work of G. Baker and P. Hacker (Baker & Hacker 1980, 1985)(Shanker 1987). Almost all of these studies, however, revolve around the more philosophical parts of Wittgenstein's remarks on the foundations of mathematics, such as the rule-following argument or his peculiar form of "conventionalism" about mathematical propositions. They do not address the foundational debates which were taking place in the first half of this century, on which Wittgenstein wrote many remarks. I am talking here not just about the dispute between the logicist, formalist and intuitionist schools of philosophy of mathematics, but more specifically about the debate about the status of the so-called  $\Pi_1^0$ -statements, which was at the centre of the *Grundlagenstreit*, or about the debate about predicativity.

Therefore, I tried, in the following study, not to dwell on more purely philosophical matters, but, rather, to confront Wittgenstein's remarks on the foundations of mathematics, with the state of foundational research in his times, and, to a certain extent, today. This is the reason why one will find sections in this study devoted entirely to topics such as the logicist definitions of natural numbers,

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<sup>1</sup> The following list is not exhaustive: (Baker 1988), (Baker & Hacker 1985), (Kripke 1982), (Shanker 1987) and (Wright 1980). A volume of essays on Wittgenstein's philosophy of mathematics was also published, (Shanker 1986: Vol. 3). Three books were published in France: (Bouveresse 1987, 1989) and (Schmitz 1988). In Italian, I must mention the lengthy paper (Frasquolla 1980a).



impredicativity, or strict finitism. These sections aim to provide the backdrop against which Wittgenstein's remarks can be put in contrast and understood better.

The purpose of such an enquiry is to give a proper picture of Wittgenstein's foundational stance. Indeed, most commentators seem to agree that Wittgenstein favored some form of constructivism, but they more than often disagree as to its precise nature. For example, in his influential essay, "Wittgenstein's Philosophy of Mathematics", M. Dummett claimed that Wittgenstein was a strict finitist (Dummett 1959: 180-182).<sup>2</sup> Although this view is widely shared, it is by no means clear what is to be understood by "strict finitism" or what were Wittgenstein's intentions. According to P. Bernays, for Wittgenstein "... and the strictly constructivist view a large part of classical mathematics simply doesn't exist" (Bernays 1959: 176); while C. Kielkopf construed Wittgenstein as an "open-ended strict finitist" who

... accepts strict finitism as an adequate philosophy of only as much mathematics as can be done by strict finitist means. However, he resolves to understand the remainder of mathematics by deviating as little as possible from the strict finitist philosophy. (Kielkopf 1970: 182)

There are also scholars, such as G. Baker, who saw —again under the influence of M. Dummett— intuitionism as the model of Wittgenstein's general semantic theory, the latter being a "generalization from Intuitionism which is purged of the blatant psychologism of Brouwer's exposition" (Baker 1974: 189). Moreover, there are also scholars who simply deny that Wittgenstein was a constructivist. For example, G. Baker now claims with P. Hacker that:

... his philosophy of mathematics does not defend a form of 'strict finitism', depsychologized 'intuitionism' or 'constructivism'. His purpose was not to take sides in the debates between rival schools of mathematicians, but rather to question the presuppositions which provided the framework for their debates. (Baker & Hacker 1985: 345)

The usual argument put forward is that Wittgenstein held a form of "no-position" position (Baker 1986).

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<sup>2</sup> Not that Dummett is solely responsible for such a view: other logicians reacting to the publication of RFM, P. Bernays, G. Kreisel and H. Wang, unanimously labelled him as a strict finitist. See (Bernays, 1959:173), (Kreisel 1958: 147) and (Wang 1958: 474).

I am convinced that my rather more historical approach to Wittgenstein's remarks will help clarifying our interpretation of Wittgenstein. First of all, it seems clear to me that Wittgenstein was propounding a form of constructivism. This is why in the first section, on revisionism, I shall criticize the "no-position" position interpretation. Otherwise, the rest of this study will speak for itself.

In the second chapter I shall show that Wittgenstein's remarks on existential proofs and on the Axiom of Choice are in the constructivist tradition of L. Kronecker. In the following chapter, I shall provide the setting for a discussion of the constructivism of the TLP, by studying in detail the logicist definitions of natural numbers and the claim of impredicativity leveled against them by H. Poincaré. In chapter IV, I shall show that, in accordance with Poincaré, Wittgenstein gave a constructivist definition of the natural numbers. I shall also discuss (section 8) Wittgenstein's later criticisms of the notion of numerical equality, which plays a central role in Frege's definition of the natural numbers.

Secondly, I wish to claim that Wittgenstein's form of constructivism was neither a form of strict finitism, nor a form of intuitionism. On one hand, in chapter V, after an exposition of the current strict finitist literature, I shall indicate my reasons for believing that Wittgenstein's remarks on the infinite not only do not square with strict finitism, but that they contain an argument against it. In chapter VII, I shall oppose M. Dummett's interpretation of Wittgenstein's remarks on "surveyability" as providing a strict finitist argument. On the other hand, Wittgenstein's complete rejection of the Axiom of Choice (section 3), of quantification theory (section 13) and of choice sequences (section 18) set him apart from Brouwerian intuitionism.

The picture of Wittgenstein's foundational standpoint that will emerge is that of a "open-ended" finitist, who admits only the potential infinite, with a strong emphasis on recursive definitions and proofs, which in turn implies a rejection of quantification theory (section 13) and of non-recursive real numbers (section 17.) Strong similarities with the works of T. Skolem and R. Goodstein and the tradition of "recursive analysis" are further evidence. Other than trying to show their initial plausibility and

coherence, I shall not try to defend Wittgenstein's views against what are sometimes obvious criticisms.

Since I did not wish to enter more "philosophical" debates about Wittgenstein's philosophy of mathematics, I refrained from using M. Dummett's distinction between realism and anti-realism. Instead, I shall use a fairly uncontroversial distinction, that between Platonism and anti-Platonism, which is based on the so-called "foundational metaphors" of the mathematician as, respectively, a discoverer or an inventor. Platonism, I shall usually refer to as "descriptivism", following A. Gargani,<sup>3</sup> instead of using the more equivocal "extensionalism" used sometimes by Wittgenstein,<sup>4</sup> and extensively by other scholars such as, say, F. Schmitz.<sup>5</sup> As for Wittgenstein, it is rather obvious that he was an anti-Platonist.<sup>6</sup>

I would like to make a further distinction between moderate and extreme anti-Platonism. Against the "foundational metaphors" of the discoverer and the inventor, many philosophers, such as M. Dummett, proposed the "intermediate picture" of "objects springing into being in response to our probing" (Dummett 1959: 185). Earlier, F. Waismann also spoke for the "autonomy" of mathematics:

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<sup>3</sup> See (Gargani 1986b). There are strong similarities between what S. Gerrard called in his doctoral dissertation the "Hardyan picture" (Gerrard 1987), and what I wish to describe as the "descriptivist attitude." Wittgenstein knew G. H. Hardy's writings quite well, and he quoted them frequently (BB: 11) (AWL: 215-220, 222, 224-225) (LFM: 91, 103, 123, 139, 169-171, 239, 243). Hardy is well known for his Platonist pronouncements, in (Hardy 1929) for example, and Gerrard saw him as embodying the conceptions against which Wittgenstein was always opposed.

<sup>4</sup> For example, (PR: § 130) and (PG: 457).

<sup>5</sup> Wittgenstein probably took the expression "extensional viewpoint" from the writings of W. E. Johnson and F. P. Ramsey (he knew both of them and their writings quite well.) Ramsey spoke, with agreement, of the "extensional attitude of modern mathematics" (Ramsey 1925: 174) —see also (Ramsey 1925: 165). If an infinite sequence is treated as a function of a variable whose values are positive integers and if the function is taken in extension and not in intension, one introduces the actual infinite. This is definitely the idea that Wittgenstein rejected —in parallel with the intuitionists— while Ramsey embraced it (Ramsey 1925: 208-209). In his *Logic*, Johnson, who obviously had problems coping with the modern logic of his pupil Russell, also discussed the notions of "classes taken in extension" (Johnson 1920-4: Vol. 2; 166, 174). The use of the term "descriptivist" is prompted by consideration of passages such as: "In logic we do not have an object and the description of an object. You will say for example, 'To be sure, we cannot enumerate all the numbers of a set, but we can give a description.' That is nonsense. You cannot give a description instead of an enumeration. The one is not a substitute for the other. What we can give, we can give. We cannot reach the same target from behind." (WWK: 102).

<sup>6</sup> See (WWK: 34, 63) (PR: § 157; 159) for early statements, and (LFM: 22), (RFM: I, § 32 & 168; app. II, § 2). Or, again, this question: "Why do you always want to look at mathematics under the aspect of finding and not of doing?" (RFM: VII, § 5).

We *generate* the numbers, yet we have no choice to proceed otherwise. There is already something there that *guides* us. So we make, and do not make mathematics.

... We cannot *control* mathematics. The creation is stronger than the creator. (Waismann 1982: 33)

Those who hold this view, I wish to call moderate anti-Platonists. In contrast, Wittgenstein should be seen as an extreme anti-Platonist. This distinction and its consequences will become clearer when I shall discuss the topics of numerical equality and choice, in section 8, and the continuation of an infinite series —i.e being *guided* by the rule— at the end of section 16.

Although Wittgenstein kept writing about mathematics until the very end of his life, from the mid-thirties onwards he devoted less and less time on this topic. He became increasingly out of touch with the current literature. Accordingly, I shall concentrate on the **TLP** and on the writings, lecture and conversation notes of the transitional period (1929-1935), although I shall also discuss later writings and lectures notes, respectively **RFM** and **LFM** to complement my argument, in cases such as the notion of “surveyability”, where most remarks date from after 1939.

Finally, the problems I am addressing in this study are all linked to the question of the nature of the infinite. But there are many topics, such as Hilbert’s metamathematics, the need for a consistency proof, Gödel’s incompleteness theorems and the defects of Fregean foundations, on which Wittgenstein wrote a good deal, but which will not be discussed here because they form a field of their own.

## I. Philosophy and Mathematics

Another idea might be that I was going to lecture on a particular branch of mathematics called “the foundations of mathematics”. There is such a branch, dealt with in *Principia Mathematica*, etc. I am not going to lecture on this. I know nothing about it —I practically know only the first volume of *Principia Mathematica*.

L. Wittgenstein

### 1. Revisionism

A “revisionist” philosophy of mathematics could be roughly defined as a philosophical position calling into question methods of mathematics (or even whole mathematical theories) on more or less purely philosophical grounds. That Wittgenstein was a “revisionist” is one of the most disputed claims about his philosophy of mathematics. As I said in the introduction, Wittgenstein is commonly held to be a constructivist of some sort, either a strict finitist, or an intuitionist. Both intuitionism and strict finitism are revisionary philosophies of mathematics since they both advocate —on what could be described as philosophical grounds about the nature of mathematical objects for intuitionism or about our human limitations for strict finitism— that mathematicians should relinquish large parts of actual mathematics. It seems therefore that Wittgenstein held a revisionist philosophy of mathematics, even if we have not yet pinpointed its precise nature.

On the other hand, any reader of *PI* knows that Wittgenstein made statements such as the following which can be easily construed as a rejection of revisionism:

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, and no mathematical discovery can advance it. A “leading problem of mathematical logic” is for us a problem like any other. (*PI*: § 124)

Commentators have taken two different attitudes. On one hand, Wittgenstein is described —by commentators such as M. Dummett— as advocating anti-revisionism. But this requirement of non-interference conflicts with the numerous remarks on topics such as existence proofs, the Axiom of Choice, the Law of Excluded Middle, the diagonal method, Dedekind cuts, etc... which are revisionist in character. Therefore, Wittgenstein *is* a strict finitist and his apparent anti-revisionism is rejected as groundless: “...there is no grounds for Wittgenstein’s segregation of philosophy from mathematics” (Dummett 1959: 168).<sup>1</sup> On the other hand, commentators such as M. Wrigley used remarks such as (PI: § 124) in order to argue that Wittgenstein could not adhere to strict finitism because it would simply contradict his own requirement of non-revisionism (Wrigley 1977: 183-4).

I shall give my reasons for thinking that there is no incoherence in Wittgenstein’s thought, i.e. that he can be described as a revisionist even in the face of (PI: § 124). The reasons for Wittgenstein’s apparent anti-revisionism are twofold. They are linked on one hand with his insistence against Russell on the “descriptive” character of philosophy, and on the other hand with his view that mathematics consists solely of algorithms, or calculations.

Wittgenstein’s conception of the nature of philosophy is best seen as a reaction against B. Russell’s conception of philosophical enquiry as similar to scientific enquiry. According to Russell in **Our Knowledge of the External World** (Russell 1929), philosophy had made, in stark contrast to the sciences, no progress since its origin. But, still according to Russell, this “unsatisfactory state of things” could be corrected by emulating the method of the sciences, i.e.

... the substitution of piecemeal, detailed, and verifiable results for large untested generalities recommended only by a certain appeal to imagination. (Russell 1929: 4)

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<sup>1</sup> Dummett offers an explanation: Wittgenstein’s insistence on the non-interference of philosophy in mathematics “springs only from a general tendency of his to regard discourse as split up into a number of distinct islands with no communication between them” (Dummett 1959: 168). I would be inclined to think that there is no such “islands” in Wittgenstein’s conception of language.

More precisely, the scientific method in philosophy should be, according to Russell, the use of the “new” logic, i.e. the logic first developed by G. Frege, as opposed to Aristotelian syllogistic (Russell 1929: 63). On this view **Principia Mathematica** appears as a shining example of the use of this “new” logic to settle the problems of philosophy of mathematics.

In obvious reaction to Russell’s “logico-analytical method”, Wittgenstein presented in (PI: § 89-133) what may be called a non-cognitivist conception of philosophy.<sup>1</sup> It could be expressed briefly as follow: *philosophy is not a cognitive discipline producing knowledge expressed by true propositions*. While according to Russell philosophy is one of the sciences, differing from the others only in degree not in kind, Wittgenstein claimed that “our considerations could not be scientific ones” (PI: § 109).<sup>2</sup> Philosophical enquiry is in its essence different from scientific enquiry:

Logic ... takes its rise, not from an interest in the facts of nature, nor from a need to grasp causal connexions: But from an urge to understand the basis or essence of everything empirical. (PI: § 89)

Wittgenstein added, in a passage that prompted many commentators to see a parallel with Kant (Cavell 1962: 175):

Our investigation is directed not towards the phenomena, but, ...towards the ‘possibilities’ of phenomena. (PI: §90)

This similarity with Kant’s approach in his **Critique of Pure Reason** makes the contrast with Russell’s own empiricist philosophy even more obvious.<sup>3</sup>

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<sup>1</sup> Wittgenstein’s conception of philosophy as expounded in **PI** has its roots in **NB** and **TLP**. In fact, his conception changed only little in this respect. For the sake of brevity, I shall not discuss the early view. The reader is referred to (Baker & Hacker 1980) for a good exposition.

<sup>2</sup> See also (NB: 44).

<sup>3</sup> Wittgenstein is on the record for approving Kant’s critical method: “This is the right sort of approach. Hume, Descartes and others had tried to start with one proposition such as *Cogito ergo Sum* and work from it to others. Kant disagreed and started with what we know to be so and so, and went on to examine the validity of what we suppose we know” (LWL: 73). This description of Kant’s method is reminiscent of Kant’s own description: “And here I make a remark which the reader must bear in mind, as it extends its influence over all that follows. Not every kind of knowledge *a priori* should be called transcendental, but only that which we know that—and how—certain representations (intuitions or concepts) can be employed or are possible *a priori*. The term ‘transcendental’, that is to say, signifies such knowledge as concerns the *a priori* possibility of knowledge, or its *a priori* employment” (CPR: A 56 / B80). There are strong affinities between the two philosophers, and I suggest that Wittgenstein’s “grammatical” method

According to Wittgenstein, philosophy is concerned with our norms of representation which determine what counts as an intelligible description of reality. These norms are the rules of grammar, and Wittgenstein would therefore say: “our investigation is... a grammatical one” (PI: § 90). It is in the nature of philosophy that “philosophical questions are not solved by experience” (AWL: 3), that they are “not empirical problems” (PI: § 109). They are rather conceptual problems originating in an inadequate understanding of our way of speaking about reality, not in the ignorance of an aspect of it. The task of philosophy is therefore to *dissolve conceptual confusions by a clarification of the grammar of language*.

Clarification consists in providing grammatical reminders. Indeed, since philosophical enquiry originates in an “urge to understand the basis, or essence, of everything empirical” (PI: § 89), it cannot be an investigation directed towards the empirical, searching for new facts: “it is of the essence of our investigation that we do not seek to learn anything *new* by it” (PI: § 89).<sup>1</sup> So, if during the philosophical enquiry one is not looking for new facts, one must be looking for something already there, but —as it were— forgotten. Wittgenstein would say that in philosophy we shall remind (*besinnen*) ourselves of something we already knew (PI: § 89). This something was always in full view, and that is why we have a tendency to forget it, or to misjudge its value. Philosophy consists therefore in *grammatical reminders*.

This idea that we need to remind ourselves of something that we already know, of something plain to view, is linked with the idea that such reminders should be so evident that they would not be open to debate. I shall come back to this “triviality” thesis in a moment. It is extremely interesting to notice here the use by Wittgenstein of the German

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should be seen as a variant of Kant’s critical method (in a way, they both have the same general strategy against the same enemy, the scepticism inherent to all forms of empirism.) Of course, the variant here is “linguistic”. It was appropriately termed as “semantical Kantianism” by J. Hintikka. See (Hintikka 1981: 378) and in this connection (CV: 10).

<sup>1</sup> See also (LWL: 35).



verb *besinnen*. Indeed, it is the word *Besinnung* which H. Weyl thought necessary to add to the English text of his 1929 American lecture, “Consistency in Mathematics”, when he described Brouwer’s argument against the universal use of the Law of Excluded Middle:

I do not want to go into too much discussion to convert you to this opinion of Brouwer’s. It is entirely a matter of reflection (*Besinnung*), which has nothing to do with any epistemological, or perhaps even metaphysical theories, nor indeed with any arbitrarily declared mathematical axioms and their technical manipulation. Everybody will admit its truth provided he understands it. (Weyl 1929: 152-153)

Weyl was only repeating the words of Brouwer himself about his own insights:

The acceptance of these insights is only a question of time, since they are the result of pure reflection and hence contain no disputable element, so that anyone who has once understood them must accept them. (Brouwer 1927: 490)

The need for such reflection in Brouwer’s writings came from the fact that intuitionistically unwarranted applications of the Law of Excluded Middle do not lead to contradictions, and it is therefore impossible to refute the principle by showing it as contradictory. Brouwer must therefore ask that we reflect on these cases, and then see the validity of his argument. There is a similarity with Wittgenstein’s *Besinnung*, which is also non-deductive in character. Although it is obvious that Wittgenstein did not take his idea from Brouwer, it remains that there is a striking similarity between his conception and Brouwer’s.

Confusion often comes from the lack of a synoptic representation (*übersichtliche Darstellung*) of our usage (PI: § 122). That is why in mathematics, to give an example, we get entangled in our own rules (PI: § 125). The philosopher has to assemble grammatical reminders (PI: § 127), in order to give us a synoptic description, an *Übersicht*, with the help of which we will be able to see our way about. Philosophical problems should then disappear (PI: § 133).

As I just mentioned, Wittgenstein’s conception of philosophy as providing us with grammatical reminders or grammatical reflections instead of discovering new facts led him to draw strange consequences, one of them being the claim that if one is to advance a

thesis in philosophy, it should be a trivial one. Indeed, for Wittgenstein grammatical reminders can only be trivialities:

What we find out in philosophy is trivial; it does not teach us new facts, only science does that. But the proper synopsis of these trivialities is enormously difficult, and has immense importance. Philosophy is in fact the synopsis of trivialities. (AWL: 26)

If one tried to advance *theses* in philosophy, it would never be possible to debate them, because everyone would agree to them. (PI: § 128)

Moreover, if philosophical theses are only to be trivial, in the course of a conversation one must for reasons of strategy abandon those that are subject to discussion (LFM: 22) (AWL: 97). Of his lectures on the foundations of mathematics in 1939, Wittgenstein claimed that “the whole point is that I must not have an opinion” (LFM: 55).<sup>1</sup> These strange consequences are worth investigating, because they will lead to a proper understanding of the peculiar nature of Wittgenstein’s remarks on mathematics.

The triviality thesis leads to a dilemma. On the one hand, if Wittgenstein didn’t express any opinion, his work cannot be compared with traditional philosophy since the latter consists precisely in putting forward theses and in arguing for them. This is the widely held incommensurability thesis.<sup>2</sup> On the other hand, if Wittgenstein did indeed express opinions, then his position would be inconsistent and his own practice would seem to belie him (Dummett 1960: 434). Moreover, the thesis according to which there shouldn’t be any non-trivial thesis in philosophy is itself far from being trivial.

The idea of the incommensurability of Wittgenstein’s remarks with traditional philosophy receives support from his apparent agnosticism towards numerous traditional debates. Indeed, Wittgenstein often seems more willing to dissolve these debates than to

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<sup>1</sup> See also (LFM: 103).

<sup>2</sup> This thesis was first propounded by O. K. Bouswma (Bouswma 1961). R. Rorty associates Wittgenstein with M. Heidegger, both being what he calls “edifying philosophers” because they adroitly avoid having opinions while also avoiding having an opinion on not having an opinion (Rorty 1979: 283)! A crass misinterpretation of both authors, albeit a fashionable one... For a better defence of the interpretation of Wittgenstein as holding a “no position-position”, see (Baker 1986) and (Baker 1988: 242).

give a new solution to old questions. Philosophy of mathematics is a good case. At first sight, Wittgenstein appears critical of all the proponents in the debate on the foundations of mathematics. Moreover, he claimed that mathematics does not need a foundation (PI: § 124). From this it is easy to conclude that he rejected the very idea of a philosophical enquiry into the foundations of mathematics. This is far from being the truth, as Wittgenstein was asking for a clarification of the grammar of mathematical propositions, and this activity is quite in line with traditional philosophy:

What does mathematics need a foundation for? It no more needs one, I believe, than propositions about physical objects—or about sense impressions, need an *analysis*. What mathematical propositions do stand in need of is a clarification of their grammar, just as do those other propositions. (RFM: VII, § 16)

According to Wittgenstein, arithmetic is the grammar of numbers (PR: § 108).<sup>1</sup>

Therefore, arithmetical propositions express no knowledge:

If you know a mathematical proposition, that's not to say you yet know *anything*.

I.e., the mathematical proposition is only supposed to supply a framework for a description. (RFM : VII, § 2)

It is precisely because Wittgenstein conceived mathematical propositions as rules of grammar —this is only an analogy— conceiving these as autonomous, i.e. not rendered true or false by any reality, and not as akin to empirical propositions that he was at odds with the main schools of philosophy of mathematics of his times. According to him, we shouldn't be fooled by the superficial resemblance of mathematical propositions to empirical propositions (RFM: App. III, §2, 4): they aren't about a Platonic world of ideal forms, or about mental constructions, or about ink marks on paper. Analogous with the rules of grammar, mathematical propositions are conceived as autonomous norms of representation.

Wittgenstein held logic responsible for deforming the thinking of mathematicians and philosophers precisely because with the translation of mathematical propositions in a

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<sup>1</sup> See also (RFM: VII, § 67).

logical system such as that of **Principia Mathematica**, it is so easy to confuse the mathematical propositions with empirical propositions (RFM: V, § 48). This is why he spoke of “the curse of the invasion of mathematics by mathematical logic” (RFM: V, § 47), or of the “disastrous invasion” (*unheilvolle Einbruch*) of mathematics by logic (RFM: V § 24).

Anyone can very well see here why Wittgenstein’s remarks are compatible with traditional philosophy. If he did not take sides in the traditional debate between logicism, intuitionism and formalism, it was because he had a radically different answer to an underlying question. Otherwise, he would have had simply nothing to say, and his remarks would have been of no interest. Wittgenstein rejected Frege’s question “What is a number?” (AWL: 164). He did not asked a question such as: “What kind of reality renders true or false the propositions of mathematics?”, but rather asked questions such as: “Do the propositions of mathematics have the same status as the empirical ones?” (AWL: 27). His answer was different from the one given by proponents of the three leading schools. But it remained a non-trivial answer to be argued for, like any traditional philosophical thesis. One cannot talk of incommensurability here.

According to Wittgenstein, philosophical enquiry consists in rendering visible hidden nonsense (PI: § 119, § 464). But in doing so one does not uncover new facts, but reminds oneself of already known (grammatical) facts. Because grammatical reminders are about something already known by all, they should not be open to dispute. *Triviality therefore comes from the fact that grammar is prior to experience*. Rules of grammar cannot be true or false, because they are responsible for the prior distinction between sense and nonsense (PI: § 90).

Wittgenstein approached the problems of philosophy of mathematics in the way advocated in **PI**. He was not interested in creating a new piece of mathematics, but in *describing* things as they stand:

It is not that a new building has to be erected, or that a new bridge has to be built, but that the geography *as it is now*, has to be described. (RFM: V, § 52)

because:

... the philosophical difficulties which arise in mathematics as elsewhere arise because we find ourselves in a strange town and do not know our way. So we must learn the topography... (LFM : 44)

It is clear that the philosopher as a philosopher is not supposed to *do* mathematics instead of the mathematician. But, as the example of Russell shows, philosophers have a tendency to take the mathematician's place, an ambition criticized in typical fashion by Wittgenstein:

The philosopher easily gets into the position of a ham-fisted director who, instead of doing his own work and merely supervising his employees to see they do their work well, takes over their jobs until one day he finds himself overburdened with other people's work while his employees watch and criticize him. He is particularly inclined to saddle himself with the work of the mathematician. (PG: 369)

This criticism, however, does not work against mathematicians such as D. Hilbert or L. E. J. Brouwer. On the other hand, since they usually have a poor education in philosophy, mathematicians are prone to make all sorts of philosophical mistakes when they talk about their own activity. Wittgenstein did not consider their sayings as philosophy proper, but rather as raw material for a philosophical treatment:

... what a mathematician is inclined to say about the objectivity and reality of mathematical facts, is not a philosophy of mathematics, but something for philosophical *treatment*. (PI: § 254)<sup>1</sup>

His violent reaction to some careless statements by G. H. Hardy in his 1929 article on "Mathematical Proof" (Hardy 1929) is a good example:

The talk of mathematicians becomes absurd when they leave mathematics, for example, Hardy's description of mathematics as not being a creation of our minds. He conceives of philosophy as a decoration, an atmosphere, around hard realities of mathematics and science. .... Hardy is thinking of

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<sup>1</sup> This affirmation is frequently repeated: "The philosopher only marks what the mathematician casually throws off about his activities" (PG: 369). "Philosophy does not examine the calculi of mathematics, but only what mathematicians say about these calculi" (PG: 396). See also (RFM: V, § 5).

philosophical opinions. I conceive of philosophy as an activity of clearing up thoughts. (AWL: 225)

Again, Wittgenstein was insisting here on not propounding opinions, but rather on clearing up thoughts. But one can also see that *he thought of philosophy as having an important normative role: “description” doesn’t amount to “decoration”*. There is indeed a sense in which a “description” is “normative”. This idea was captured quite well by W. Tait:

The philosophy of mathematics ought to account for actual well established mathematical practice, to put that practice is such a light that apparent difficulties are resolved. In this sense, philosophy of mathematics is descriptive—though it is also normative, in the sense that it is normative to say: look at things this way and they make sense. (Tait 1983: 181)<sup>1</sup>

To understand Wittgenstein’s approach to philosophy of mathematics the key distinction to be made is the one between “prose” and “calculus”. *Wittgenstein saw mathematics as being essentially algorithmic in nature, as some kind of high-level abacus activity*. For example Waismann reported Wittgenstein as saying:

Mathematics is always a machine, a calculus. (WWK: 106)

A calculus is an abacus, a calculator, a calculating machine; it works by means of strokes, numerals, etc. (WWK: 106)

In **PR** Wittgenstein said of the signs in mathematics that they “*are* like the beads of an abacus” (PR: § 157).<sup>2</sup> But we do use words of our ordinary language in the process of doing mathematics, even while proving mathematical propositions. In fact, it is essential for proofs to incorporate words of ordinary everyday language. These words are what Wittgenstein called the “everyday prose that accompanies the calculus” (WWK: 129). But, Wittgenstein went even as far as to say that in mathematics there is *nothing but calculus*:

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<sup>1</sup> That philosophy of mathematics ought to account for mathematical practice does not mean that it must vindicate, say, Zermelo-Fraenkel’s set theory, because it is adopted by most. It is possible that set theory is becoming less and less useful for mathematicians. For example, A. Macintyre recently claimed that it “has become detached from the rest of mathematics” (Macintyre 1989: 367). See also (Macintyre 1980: 62).

<sup>2</sup> See also (PR: § 171).

Mathematics consists entirely of calculations.

In mathematics *everything* is algorithm and *nothing* is meaning; even when it doesn't look like that because we seem to be using *words* to talk *about* mathematical things. Even these words are used to construct an algorithm. (PG: 468)

This attitude towards mathematics is that of mathematicians before the so-called modern axiomatic method—which was put forward by D. Hilbert in geometry and number theory and by E. Noether in algebra—became widespread. A good example is the work of E. Noether's supervisor, P. Gordan, whose debate with Hilbert over the latter's proof of the existence of a complete system of invariants for every algebraic form will be discussed in the next section. H. Weyl reported that in his obituary to Gordan, M. Noether characterized him by saying: "*Er war ein Algorithmiker*". Weyl also reported that:

... there exists papers of his where twenty pages of formulas are not interrupted by a single text word; it is told that in all his papers he himself wrote the formulas only, the text being added by his friends. (Weyl 1935: 427)

One cannot but being struck by Gordan's practice: it embodies quite well the Wittgensteinian distinction between calculus and prose. One can see behind this distinction the difference in style between the old school represented by Gordan and that of the adepts of the modern axiomatic method. I shall set forth this crucial difference in the next chapter by discussing the work of L. Kronecker and the debate between Gordan and Hilbert. I hope that it will be made clear that Wittgenstein sided with the algorithmic school (Gordan, Kronecker, Brouwer, etc...) It is fitting to remark that this approach became almost extinct at the time Wittgenstein wrote, with maybe Brouwer as the sole representative.<sup>1</sup> Even Weyl, with all his constructivist sympathies, had to admit that the

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<sup>1</sup> It is currently now admitted, however, that the neglect of algorithms, since the days of Dedekind, Cantor, Hilbert and Noether is now apparently "thanks to the computer, coming to an end" (Edwards 1987: 35).

“direct methods” of Hilbert was more fruitful than the method of producing particular algorithms (Weyl 1932: 346).<sup>1</sup>

According to Wittgenstein, philosophical mistakes must come from the prose: *once a calculation is effected, it can't be undone later on*. And since mathematics is considered as nothing but a sophisticated form of abacus activity, then the source of the confusion must therefore lie in the prose:

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*. (WWK : 149)

Once you have calculated something, it is calculated for good and it would be pointless to doubt it. Only the confusions embodied in the prose could be made to disappear. And this is where Wittgenstein's interest lies, not even in the technical terms, but in the everyday terms found in the prose:

I can as a philosopher talk about mathematics because I will only deal with puzzles which arise from the words of our ordinary everyday language, such as “proof”, “number”, “series”, “order”, etc.

...  
I said “words of ordinary everyday language”. Puzzles may arise out of words not ordinary or everyday—technical mathematical terms. These misunderstandings don't concern me. They don't have the characteristic we are particularly interested in. They are not so tenacious, or difficult to get rid of. (LFM: 14-5)

Wittgenstein wanted to provide a critical analysis of our “words of ordinary everyday language”, so as to purge them of the potentially misleading pictures associated with them which might confuse the mathematicians themselves. One can see in this light Wittgenstein's critical remarks on the infinite—as, say, an enormous quantity—which will be discussed in sections 10 and 11, or his attempts at criticizing the notion of Dedekind cut as being based on geometrical intuition.

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<sup>1</sup> One must be careful: although constructivists in their practice will require more algorithms, and proponents of classical mathematics were often, as Hilbert was, propagating the axiomatic method, it would be wrong to say that these distinctions do not overlap.



Here the confusions will arise from forgetting or misrepresenting the rules for the everyday use of those words, such as “proof”, “number”, “series”, “infinite”, etc... A special case here would be being misled by an analogy and grouping different kinds of things together because we use only one word for them. The use of the word “number”, for example, masks important differences in nature between natural, rational and real numbers. They all possess different grammars, but they are all called “numbers” because of the similarities. This point was brought forth by Waismann in the famous Conference which took place in Königsberg in 1931:

The negative numbers, for example, are not an amendment to the natural numbers. Natural numbers, rational numbers--these are not different subclasses of the range of numbers, but rather, one can best describe their essence if one says: they are *different chapter-headings of grammar*. The different kinds of numbers are, so to speak, different categories of words; that is, word-categories that obey different syntax. Between these different syntactical rules there exist similarities, and for that reason, we characterize them all as numbers. (Waismann 1931: 66)<sup>1</sup>

Here grammatical reminders can, at best, show the confused conceptions as nonsensical (for example, grammatical remarks about the infinite show the talk about completed infinities as nonsensical) or simply give enough surroundings to calm worries raised by the misconceptions.<sup>2</sup>

Confusions also arise from using the prose in a way unlicensed by the algorithmic core of the theory; i.e. *there is an inconsistency between the explanations and the uses*. Such confusions could simply end up in provoking the misrepresentation of a calculus. But they can also be the cause of the invention of the calculus. These are the precise object of Wittgenstein’s enquiry into the foundations of mathematics:

The misunderstandings we are going to deal with are misunderstandings without which the calculus would never have been invented, being of no other use, where the interest is centred entirely on the words which accompany the piece of mathematics you make. (LFM : 16)

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<sup>1</sup> See (WWK: 35-36) (PG: 479) and (Kneale & Kneale 1962: 394).

<sup>2</sup> A good example here would be the case of consistency and Hilbert’s program, where Wittgenstein tried to argue, unconvincingly at times, that the narrow view of the logicians is responsible for their “superstitious fear” of contradiction (WWK: 196). As I said before, I shall not discuss this topic further.

According to Wittgenstein, this is precisely the kind of misunderstanding to be found at the origin of set theory. Indeed, the latter (be it Cantorean set theory or disguised in Russell's **Principia Mathematica**) contains an element of prose which cannot but be an ill-fated attempt at providing a *theory*:

In set theory what is calculus **must be separated off** from what attempts to be (and of course cannot be) *theory*. The rules of the game have to be separated off from inessential statements about the chessmen. (PG: 468)

Therefore, although Wittgenstein said of Russell's system in **Principia Mathematica** that *qua* calculus it is "all right" (WWK: 114), he then added to this remark that the reasons for the construction of the calculus are "wrong":

Of course, when Russell was constructing his calculus he did not intend to develop merely a game of chess, but meant to reproduce with his calculus what the word 'infinite' really meant when it is applied. But in this he was wrong. (WWK: 114)

Wittgenstein was not interested in a mathematical critique of **Principia Mathematica**:

It is my task, not to attack Russell's logic from within, but from without. That is to say: not to attack it mathematically—otherwise I should be doing mathematics—but its position, its office. (RFM: VII, § 19)

Although Wittgenstein refused to attack a theory mathematically, he could at least shed doubt on its value. In his own jargon, if calculations can't be wrong, at least their interest can be put in doubt:

What I am doing is, not to shew that calculations are wrong, but to subject the *interest* of calculations to a test. (RFM: II, § 62)

Similarly, Wittgenstein wasn't questioning the cogency of set theory, but rather pointing at the wrong reasons presiding over its conception. In a perfectly explicit passage of PG, Wittgenstein begins by claiming that set theory is an attempt at describing the actual infinite, which cannot be grasped directly by mathematical symbolism:

Set theory attempts to grasp the infinite at a more general level than the investigation of the laws of the real numbers. It says that you can't grasp the actual infinite by means of mathematical symbolism at all and therefore it can only be described and not represented. (PG: 368)

But since according to Wittgenstein “we can’t describe mathematics, we can only do it” (PR: § 159),<sup>1</sup> to describe a calculus as a description is a complete misunderstanding of its nature:

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: 469)

And, Wittgenstein goes on, such misinterpretation is, of course, at the origin of the calculus itself! But, all this does not prove the calculus to be incorrect, but “at worst uninteresting”:

It is of course this very misinterpretation that is responsible for the invention of the calculus. But that doesn’t show the calculus in itself to be something incorrect (it would be at worst uninteresting) and it is odd to believe that this part of mathematics is imperilled by any kind of philosophical (or mathematical) investigations. ... What set theory has to lose is rather the atmosphere of clouds of thought surrounding the bare calculus. (PG : p.469-70)

Wittgenstein sought misinterpretations that would bring about the birth of a calculus. If set theory was born in sin, was it not bound to be “technically” flawed, the paradoxes it faced (Burali-Forti, Russell-Zermelo) being a good indication? Wittgenstein didn’t take this approach. Set theory is a “calculus”, although surrounded by “clouds of thought”, i.e. the prose and its “conceptual confusions” (PI : p. 232). Philosophy is the clearing up of these “clouds”. What should be left is not the rejection of the calculus on mathematical grounds, but the bare calculus, without the false conceptions that make it appear so important to some. This distinction explains some of Wittgenstein’s difficult remarks such as the following:

Suppose I said, “The child of eleven has learnt  $\aleph_0$  multiplications.”  
 “Well, what’s infinite about it?” Well,  $\aleph_0$  is infinite about it. That’s all.  
 But to say “There’s something infinite about it” suggests “There’s

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<sup>1</sup> This remarks is followed by the following sentence: “And that of itself abolishes every ‘set theory’” (PR: § 159). The same view applies for logic: “In logic we do not have an object and the description of that object” (WWK: 102). This connects with what I called Wittgenstein’s extreme anti-Platonism in the introduction

something *huge* about it.” What is huge about  $\aleph_0$ ? The child who has learnt  $\aleph_0$  multiplications hasn’t learnt anything huge.

Does this show that there is no such thing as infinity? Not at all.—If I say it’s misleading to use ‘infinite’ here, this does not interfere with the mathematics of the matter. (LFM: 141-142)

Obviously, Wittgenstein is not approving the use of “ $\aleph_0$ ” by simply claiming that he “does not interfere with the mathematics of the matter”.

This explains also Wittgenstein’s response to Hilbert’s catch phrase about Cantorian set theory in “Über das Unendliche”, where in defiance against the attacks of Brouwer against set theory, he claimed:

No one shall be able to drive us from the paradise that Cantor created for us. (Hilbert 1925: 376)

To this affirmation, Wittgenstein replied:

I would try to show you that it is not a paradise—so that you’ll leave of your own accord (LFM : 103).<sup>1</sup>

Wittgenstein’s position appears to be plain common sense: it is not the job of the philosopher *qua* philosopher to intervene on the “technical” side of a theory such as set theory. He has no qualifications to do so. If this simple remark is taken as a plea for the non-interference of philosophy in mathematics, then Wittgenstein was not a revisionist. But this is far from being a complete picture of Wittgenstein’s argument. Indeed, there remains many things to be said about one non-negligible dimension of the theory, i.e. its prose. Such dimension is not unessential, because it is precisely the confusions originating in the prose that are responsible for the creation of the calculus itself. And if one eliminates these confusions, then there is no more need for the calculus, even if *qua* calculus it is correct. Therefore, the result of a proper philosophical investigation of key concepts, such as those of “finite” and “infinite” for example, found in the prose is a more careful growth of mathematical theories. That this meant revisionism<sup>2</sup> is clearly indicated

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<sup>1</sup> This rather important revisionist point is also made in (LC: 28) and (AWL: 225).

<sup>2</sup> See (Kenny 1984: 54-55) or (Moore 1990: 139), with whom I am in agreement.

in this important passage, seldom quoted by those opposing the view of Wittgenstein as a revisionist:

What will distinguish the mathematicians of the future from those of today will really be a greater sensitivity, and *that* will—as it were—prune mathematics; since people will then be more intent on absolute clarity than on the discovery of new games.

Philosophical clarity will have the same effect on the growth of mathematics as sunlight has on the growth of potato shoots. (In a cellar they grow yards long.) (PG: 381)

A careful examination of the prose might lead to the abandonment of ill-conceived theories such as Russell's system in *Principia Mathematica* against which Wittgenstein fought from the very beginning of his philosophical career, and might eventually lead to a slower growth, as mathematicians should then be more careful in creating new theories. This is nothing else than revisionism. In fact, contrary to what is often said,<sup>1</sup> Wittgenstein never said that the kind of philosophical considerations he put forward —grammatical reminders— would not affect current mathematical practice.

In accordance with my interpretation, one possible reading of Wittgenstein's remarks on set theory would suggest that Wittgenstein saw the calculations embodied in set theory as correct but the meanings usually attached to the signs as wrong. Such a reading is to be found in an article of M. Lazerowitz (Lazerowitz 1979). The basic idea is that although terms such as " $\aleph_0$ " or " $c$ " —i.e.  $2^{\aleph_0}$ — in Cantor's well-known result " $\aleph_0 < c$ ", look *as if* they refer to numbers, but they refer to the rules or formulas for constructing series:

There can be no doubt that ' $\aleph_0$ ' and ' $c$ ' do have a use in mathematics, although their use, except in semantic appearance, is not to refer to numbers. What their actual use is, as against their apparent use, can now be seen: ' $\aleph_0$ ' refers to rules or formulas for constructing series of terms, no terms of which is the last constructible by the formula, and ' $c$ ' refers to

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<sup>1</sup> For example, see (Bouveresse 1988: 49). But see also, a few pages later, Bouveresse's half-hearted attempt at keeping the door open to revisionism: "The fact that Wittgenstein and the intuitionist have almost diametrically opposed conceptions of the relations between philosophy and mathematics obviously does not mean *a priori* that the former did not find an interest and even a rationale for the modifications and restrictions that the latter proposes to introduce in mathematics" (Bouveresse 1989: 51).\*

rules for constructing from sets of terms new terms which are not in the original sets, however large those sets are made. (Lazerowitz 1979: 239)

This reading of Wittgenstein may look attractive, but nevertheless I think it is false. The gist of Wittgenstein's remarks on the infinite (to be discussed mainly in sections 10 and 11) and quantification (to be discussed in sections 7 and 13), and of the remarks directly about set theory indicates that he simply rejected the principal tenets of set theory. When Wittgenstein spoke of subjecting "the *interest* of calculations to a test" (RFM: II, § 62), he meant to criticize set theory (and Russell's logicism) quite severely, not to offer a magical reinterpretation of the bare calculus that would suddenly render it unproblematic to the eyes of a constructivist-minded philosopher. Moreover, when Wittgenstein claimed that in mathematics there is no such thing as doing and describing, he means that there is no mathematical theory which is a "description". This is precisely what he thought set theory purported to be.<sup>1</sup>

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<sup>1</sup> Again: "... we can't describe mathematics, we can only do it. (And that of itself abolishes every 'set theory'.)" (PR: § 159).

## II. Existence and the Axiom of Choice

Die ganzen Zahlen hat der liebe Gott  
gemacht, alles andere ist Menschenwerk.  
L. Kronecker

### 2. Existential and Constructive Proofs

The beginnings of constructive mathematics are usually associated with the name of L. Kronecker. His radical philosophical or theological views on mathematics are part of the mathematical folklore and the usual cursory treatment of his work and ideas by philosophers invariably ends up by condemning them for their sterility. This much was true at the beginning of this century, when his ideas in algebraic number theory were completely forgotten, superseded by the apparently more efficient method introduced by R. Dedekind in his famous *Supplement XI* to Dirichlet-Lejeune's *Vorlesungen über Zahlentheorie* (Dirichlet-Lejeune 1879: 435-627).

I found in Kronecker's finitist arithmetical approach an attitude quite similar to Wittgenstein's. The number theorist H. M. Edwards recently said of Kronecker that he "thought algorithmically" (Edwards 1987: 35). One of Kronecker's motivations for his theory was that it entails an algorithmic test for divisibility, while Dedekind "made a *virtue* of the lack of such a test in his theory" (Edwards 1990: vii). Dedekind's *ideals* already implied set-theoretical notions<sup>1</sup> and it is also comparable to Dedekind's own definition of the real numbers as cuts and to Frege's definition of the natural numbers by numerical

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<sup>1</sup> In Dedekind's theory the numbers 3 and 7 are replaced by the ideals

$$[3] = (3, 6, 9, \dots) \text{ and } [7] = (7, 14, 21, \dots),$$

i.e. by the set of all the numbers divisible by, respectively, 3 and 7. Then the number 21 is replaced by the ideal:

$$[21] = (21, 42, 63, \dots)$$

The ideal  $[21]$  contains all the products of the elements  $3m \cdot 7n$ . These are in  $[3]$  and  $[7]$ , so:

$$[21] = [3] \cdot [7]$$

(From this definition, Dedekind was able to prove that if, say,  $X$  is not a prime ideal, then it has a unique representation as the product of prime ideals.) Dedekind's ideals were infinite sets of algebraic integers.

equivalence.<sup>1</sup> As it will become more and more obvious for the reader in the next sections, one cannot but associate Kronecker's attitude with Wittgenstein and his ultimate belief that mathematics is nothing but a sophisticated abacus (i.e. that it has an algorithmic nature): For both of them the ultimate *virtue* of mathematics is the production of algorithms. In fact, although Wittgenstein never discussed issues in algebraic number theory, he was extremely critical of Dedekind's cuts and of Frege definition of natural numbers. I shall start by giving an indication of the similarity between the tradition of constructive mathematics and Wittgenstein's remarks by discussing the latter's conception of constructive and existential proofs, and by discussing also his remarks on the related topic of the Axiom of Choice. In the debate over this axiom, Wittgenstein stands clearly in the Kroneckerian tradition, alongside R. Baire and H. Lebesgue and T. Skolem.

In order to understand Kronecker's motivations one has to travel back to C. F. Gauss and the beginning of nineteenth century. Geometric images took the place of strict proof, basic notions such as the concepts of derivative and integral weren't properly defined and there was no clear idea of their range of applicability. The need for a better understanding of the basis of analysis was acutely felt. Euler, Lagrange and Gauss indicated that analysis should be built on arithmetical concepts. Then the so-called movement towards the arithmetization of analysis began. The demise of geometry (with the discovery of non-Euclidean geometries) is the reason why Gauss and, later, Kronecker put such an emphasis on arithmetic, and went on with the project of eliminating geometric notions from analysis, by "arithmetizing" it (Kronecker 1887: 252-

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<sup>1</sup> This association was made long ago by G. Kneebone: "The further step taken by Dedekind was the recognition that, in order to handle the arithmetic of a given algebraic field  $Q[\theta]$ , we do not need to be able actually to produce irreducible factors, since we can do all that is required by arguing in terms of those totalities of integers of the field that behave as if they were divisible by some common irreducible factor. These totalities are what are now known as *ideals*. The procedure here adopted by Dedekind may be compared with his own identification of the real numbers with totalities of rational numbers which behave as if separated off by boundary numbers, and also with Frege's identification of the cardinal number of a class with the totality of all classes which behave as if possessing just as many members as that class" (Kneebone 1963: 154).



253). Such a programme was carried out in the second half of the nineteenth century, with various degrees of rigour by K. Weierstrass, R. Dedekind G. Cantor and L. Kronecker. Simplifying a little, one could say that this arithmetization had to be done in two steps, from  $\mathbb{N}$  or  $\mathbb{Z}$  to  $\mathbb{Q}$  and from  $\mathbb{Q}$  to  $\mathbb{R}$ . In a sense, Kronecker effected the first step i.e. from  $\mathbb{N}$  to  $\mathbb{Q}$ , in “Über den Zahlbegriff” by showing how to eliminate from arithmetic all the foreign notions he disliked so much, namely the negative and rational numbers, the real and imaginary algebraic numbers (Kronecker 1887: 260).<sup>1</sup> If one looks at Kronecker’s achievement the other way round, his elimination of the irrational numbers could very well be interpreted as their “creation” by man. Then there is no need for finitist restrictions. This was Dedekind’s viewpoint (Dedekind 1888: 31-32).

But, although the approaches were quite different, Kronecker’s algebraic number theory was in the end similar to Dedekind’s.<sup>2</sup> I shall indicate this briefly: Dedekind considered algebraic number fields as subrings of  $\mathbb{C}$ , generated from  $\mathbb{Q}$  by the root of an irreducible polynomial  $f(x)$  with integer coefficients. Kronecker rejected the imaginary unit  $i = \sqrt{-1}$  (and therefore  $\mathbb{C}$ ), replacing it by congruences modulo  $x^2+1$  (following Cauchy.) He also could not accept a root  $\alpha$  for a polynomial with integer coefficients  $f(x)=0$ . Kronecker considered instead  $\mathbb{R}[x]/(x^2 + 1)$  and for any irreducible  $f(x) \in \mathbb{Q}[x]$  he took  $\mathbb{Q}[x]/f(x)$  which is algebraically isomorphic to Dedekind’s  $\mathbb{Q}(\alpha)$  where  $\alpha$  is a root of  $f(x)=0$ . Kronecker believed that “modular systems with infinitely many elements” should be introduced only where they could be eliminated, otherwise the inherent imprecision they bring would render them inapplicable (*nicht anwendbar*); they would be useless

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<sup>1</sup> Following Kronecker, J. Molk called the algebraic numbers: “*un simple artifice de langage*” (Molk 1885: 4).

<sup>2</sup> Even this statement is open to doubt since Kronecker’s approach has many advantages over Dedekind’s, a fact which has only recently been recognized. For example, Kronecker’s objective was to define greatest common divisors, not factorization into primes, because contrary to the latter greatest common divisors are independent of the ambient field. Therefore all the statements of the theory remain true when the ambient field is extended, while in the other case even primality depends on the chosen field and any extension requires a new theory (Edwards 1988: 143). On the advantages of Kronecker’s theory over Dedekind’s, see (Edwards 1990).

(*unnöthig*)(Kronecker 1886: 155-156). That is why he thought that irrational numbers should be eliminated from number theory, and another reason why he opposed the introduction of the “ideals” by Dedekind (Kronecker 1886: 156, note \*).<sup>1</sup>

For Kronecker these arithmetical limitations meant that he had to take a strong stance on admissible definitions and proofs. Polynomials with integral coefficients are of the form:

$$f(x) = a_0x^n + a_1x^{n-1} + \dots + a_{n-1}x + a_n$$

They form a ring. Mathematicians would usually define irreducibility as follows: if a polynomial  $f(x)$  has a rational factor, it is reducible, otherwise it isn't. In **Grundzüge einer arithmetischen Theorie der algebraischen Grössen**, Kronecker rejected such a definition:

The definition of Irreducibility given in § 1 is devoid of a sure foundation until a method is given by means of which it can be decided whether a given function is irreducible or not by the definition. (Kronecker 1882: 256-257) \*

Kronecker held that a definition in number theory or algebra is acceptable only if it could be checked in a finite number of steps whether any given number falls under it or not. This led him to reject “pure” existence proofs: *an existence proof for a number is*

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<sup>1</sup> Kronecker's condemnation of Dedekind's theory of ideals was initially fatal to his programme. His notion of modular fields was probably felt as more cumbersome by many, because of the introduction of the indeterminates. Moreover, the basics of his programme were not understood. Followers of Dedekind's approach, under the banner of the “purity of algebra”, won the battle and Kronecker ended up with no followers, and his ideas were forgotten. This explains the harsh comments on Kronecker usually found in the literature at the turn of the century, in particular in Hilbert's “sales-pitch” (to use Kreisel's apt expression) for his programme. Hilbert once called him a *Verbotsdiktator* (Hilbert 1922a: 159, 161). This is still the received opinion in philosophical circles, but it is hardly true anymore. Against the Cantor-Dedekind approach, which needed aid from analysis in proving the fundamental theorem of algebra, Steinitz went back to the ideas of Kronecker and developed a construction (the Kronecker-Steinitz construction) of the roots of algebraic equations with coefficients from an arbitrary field which has replaced since the fundamental theorem of algebra (which is a theorem of analysis). In his famous 1930 paper “The Modern Algebraic Method”, H. Hasse praised the Kronecker-Steinitz construction as “conceptually richer and less burdened with formal computations than the so-called algebraic proofs of the so-called fundamental theorem of algebra” (Hasse 1930: 1986). Anyone who needs to be convinced of the importance of the divisor-theoretic approach of Kronecker *versus* the ideal-theoretic approach of Dedekind must read the preface to H. Hasse's *Number Theory* (Hasse 1949). A. Weil has also recognized the “deep meaning of Kronecker's view” in “Number-Theory and Algebraic Geometry” (Weil 1950: 444). He pointed out the connection between his work in algebraic geometry and Kronecker's ideas in *Grundzüge einer arithmetischen Theorie der algebraischen Grössen* (Weil 1950: 448-449).

*acceptable only if it contains a method to find the number whose existence was proven in a finite number of steps.* J. Molk, one of the few students of Kronecker who took up his programme, described his position in a similar fashion:

*Definitions should be algebraic and not just logical.* It is not sufficient to say: “an object is or isn’t”. One must explain what is meant by being and not-being, in the particular domain in which we are moving. Only then are we making a step forward. If we define, for example, an irreducible function as a function which is not reducible, i.e. decomposable in other functions of a precise nature, we are not giving an algebraic definition, but a simple logical truth. In order for us to be able to give this definition *in Algebra* it must be preceded by a method by which we can obtain, in a finite number of rational operations, the factors of a reducible function. Only this method gives to the words *reducible* and *irreducible* an algebraic meaning. (Molk 1885: 8) \* <sup>1</sup>

In accordance with their views, Kronecker and Molk were able to *provide an algorithm* determining in a finite number of steps if any  $f(x)$  is reducible or not.

Kronecker’s best known student, K. Hensel, described his master’s mathematical practice in these terms:

... any definition should be so formulated as to find out in a finite number of steps if it applies to a given magnitude or not. Similarly, a proof of the existence of a magnitude can only be seen as completely rigorous if it contains a method by which the magnitude whose existence is being claimed can really be found. (Kronecker 1901: vi) \*

This is a clear and concise statement of the constructivist position on existence. I wish now to give one example of the clash between the restrictive approach of constructivism and the classical approach: Hilbert’s proof of the existence of a complete system of invariants for every algebraic form. P. Gordan originally proved that any finite system of binary forms such as  $f(x_1, x_2)$  has a finite complete system of (rational integral) invariants and covariants. He later extended this result, e.g. for ternary quadratic forms. The important fact with these proofs is that they showed how to compute the complete systems of invariants for any form.<sup>2</sup>

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<sup>1</sup> There is one notable difference with Kronecker: Molk would admit logical definitions but would ask to ultimately find the algorithm, while Kronecker would just consider them as devoid of any meaning.

<sup>2</sup> Again, the attitude of Gordan was definitively akin to Kronecker’s and Wittgenstein’s. See section 1.

Hilbert gave a first proof of his theorem in his 1890 paper: “On the Theory of algebraic Forms” (Hilbert 1890: 143). His approach was quite different since he started by proving that any collection of infinitely many forms of any degrees in the  $n$  variables  $A_1, A_2, \dots, A_m$ , has a “basis”, i.e. a finite number  $F_1, F_2, \dots, F_m$ , such that any form  $F$  of the collection can be written as

$$F = A_1F_1 + A_2F_2 + \dots + A_mF_m$$

with coefficients in the same domain as the infinite system. Then Hilbert could prove that for any system of forms every (rational integral) invariants could be expressed as the linear combination of a finite set of them.

The proof was criticized for its shortcomings by Gordan: while recognizing that the proof was correct, he felt there was a gap in its execution, because Hilbert “limited himself to the proof of the existence of that system of invariants, and renounced any discussion of their properties”,\* such as the upper bound to their number (Gordan 1893: 132). Hilbert recognized in a subsequent paper that his proof “provides no way of constructing such a system of invariants by means of a finite number of processes which can be surveyed before the start of the computation so that, for example, an upper bound for the number of the invariants of the system or for their degrees in the coefficients of the ground form can be given” (Hilbert 1893: 268). He went on to give another more satisfactory proof.

Hilbert’s two proofs were more general and simpler conceptually than Gordan’s, but on the other hand *they did not show how to compute invariants for any given system of forms*. Hilbert insisted on the importance of easier, simpler proofs, but if specific geometrical or physical invariants were needed, Hilbert’s proof remained useless to provide them. To my mind this illustrates perfectly the debate between uninformative pure existence proofs and more laborious but more informative constructive proofs. I also give the proof of a theorem of Thue on Diophantine approximations in Appendix 1. This proof is non-effective, and its application to the study of Diophantine equations led to more

non-effective results. In mathematical practice there is a need for effective proofs, independently of philosophical considerations, and I think that Wittgenstein's remarks on the weaknesses of existential proofs, to which I shall turn my attention now, reflected that need more than any deep-rooted philosophical *opinion* on the nature of mathematics.

Indeed, Wittgenstein made many remarks in **RFM** on pure existence proofs that are in the tradition of Kronecker. If someone obtains " $\exists x f(x)$ " by, say, using the Law of Excluded Middle, a constructivist would quite legitimately ask for more explanations: some mathematical transformations are needed. It is well known that direct constructive proofs give more information and lead to a better knowledge of the phenomenon involved, etc... Wittgenstein would claim that in the case of an existence proof one doesn't really understand the proposition proven, since applications are limited—because of the lack of information in the proof—and *in order to understand a proposition one must be able to apply it* (correctly).<sup>1</sup>

This is linked with the idea that you need more to be able to understand a mathematical proposition than a mere verbal understanding. This is one of the major differences between mathematics and other language-games :

One would like to say that the understanding of a mathematical proposition is not guaranteed by its verbal form, as in the case with most non-mathematical propositions. This means—so it appears—that the words don't determine the language-game in which the proposition functions.  
(RFM: V, § 25)

This is reminiscent of Brouwer's distrust of language. But, the reasons for not trusting the verbal form are quite different. Brouwer would claim that it can only render imperfectly the mathematician's thoughts, while for Wittgenstein it is because something more, i.e. some mathematical transformations giving more information, is needed. This need is connected with the idea that understanding a proof involves more than being able

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<sup>1</sup> One should remember here Wittgenstein's dictum, at the time of the Viennese conversations with Schlick and Waismann: "I understand a proposition by *applying* it" (WWK: 167).

to follow it step by step. This is expressed in this crucial remark, which will be of use when we will discuss the notion of “surveyability”:

Everything that I say really amounts to this, that one can know a proof thoroughly and follow it step by step, and yet at the same time not understand what it was that was proved.  
And in turn this is connected with the fact that one can form a mathematical proposition in a grammatically correct way without understanding its meaning. (RFM: V, § 25)

What is needed, Wittgenstein tells us, is being able to apply it correctly:

Now when does one understand it?—I believe: when one can apply it.  
It might perhaps be said: when one has a clear picture of its application. For this, however, it is not enough to connect a clear picture with it. It would rather have been better to say: when one commands a clear view of its application. And even that is bad, for the matter is simply one of not imagining that the application is where it is not; of not being deceived by the verbal form of the proposition. (RFM: V, § 25)

An existence proof  $\exists x f(x)$  without a determination of the size of the bound or of a constant, as in Thue’s theorem —see Appendix 1— gives no information as to how to construct it, or how to find it. In this case Wittgenstein would ask: How could one apply the proposition proved if one is missing information?

A proof convinces you that there is a root of an equation (without giving you any idea where)—how do you know that you understand the proposition that there is a root? How do you know that you are really convinced of anything? You may be convinced that the application of the proved proposition will turn up. But you do not understand the proposition so long as you have not found the application. (RFM: V, § 25)

Wittgenstein’s preference for constructive proofs is even more clearly indicated in the following remark where we can see that Wittgenstein compares the understanding of a pure existence proof to the understanding of an English sentence. It is so because Wittgenstein obviously did not see any mathematical transformations in such a proof. It is therefore devoid of mathematical content:

Hence the issue whether an existence-proof which is not a construction is a real proof of existence. That is, the question arises: Do I understand the proposition “There is...” when I have no possibility of finding where it exists? And here are two points of view: as an English sentence for example, I understand it, so far, that is, as I can explain it (and note how far my explanation goes). But what can I do with it? Well, not what I can do with a constructive proof. And insofar as what I can do with the

proposition is the criterion of understanding it, thus far it is not clear in advance whether and to what extent I understand it. (RFM: V, § 46)

According to Wittgenstein, in an existence proof there is a risk of thinking that one understands the proposition, while it is not really so because of the absence of algorithms. The understanding remains purely formal. Russellian symbolism has therefore a catastrophic effect, because once we understand how the logical proposition is constructed in the system of the **Principia Mathematica**, we might take it for granted, while there are still no mathematical transformations corresponding to it:

The symbols “ $(x).Fx$ ” and “ $(\exists x).Fx$ ” are certainly useful in mathematics so long as one is acquainted with the technique of the proofs of the existence or non-existence to which the Russellian signs here refer. If however this is left open, then these concepts of the old logic are extremely misleading. (RFM: V, § 13)

While for a constructivist minded philosopher such as Wittgenstein, the essential part of a proof is formed by the algorithms, there is a danger in logical proofs of overlooking them, of seeing them as inessential:

When a proof proves in a general way that *there is* a root, then everything depends on the form in which it proves this. On what it is that here leads to this verbal expression, which is a mere shadow, and keeps mum about *essentials*. Whereas to logicians it seems to keep mum only about *incidentals*. (RFM: V, § 25)

The worst is that Frege and Russell conceived their logical systems precisely in order to justify these existential procedures! No wonder Wittgenstein had some harsh words:

The curse of the invasion of mathematics by mathematical logic is that now any proposition can be represented in a mathematical symbolism, and this makes us feel obliged to understand it. Although of course this method of writing is nothing but the translation of vague ordinary prose. (RFM: V, § 46)

It is precisely in this context that Wittgenstein questioned the value of pure existential proofs:

A proof that shews that the pattern ‘777’ occurs in the expansion of  $\pi$ , but does not shew *where*. Well, proved in this way this ‘existential proposition’ would, for certain purposes, not be *a rule*. But might it not serve e.g. as a means of classifying expansion rules? It would perhaps be proved in an analogous way that ‘777’ does not occur in  $\pi^2$  but it does occur in  $\pi \times e$  etc. The question would simply be: is it reasonable to say

of the proof concerned: it proves the existence of '777' in this expansion? This can be simply misleading. It is in fact the curse of prose, and particularly of Russell's prose, in mathematics. (RFM: VII, § 41)

The danger consists in concentrating on the "prose",<sup>1</sup> or its translation in Russellian notation, instead of looking at the proof, i.e. at the algorithms:

If you want to know what the expression "continuity of a function" means, look at the proof of continuity; that will show what it proves. Don't look at the result as it is expressed in prose, or in the Russellian notation, which is simply a translation of the prose expression; but fix your attention on the calculation actually going on in the proof. The verbal expression of the allegedly proved proposition is in most cases misleading, because it conceals the real purpose of the proof, which can be seen with full clarity in the proof itself. (PG: 369-370)<sup>2</sup>

This warning against the danger of logical definitions in mathematics is recurrent in the constructivist literature: it was a favorite theme for Kronecker (compare these with our previous quotation from J. Molk), and also for Brouwer, who linked his distrust of logic with his belief in the inability of language to render thoughts properly —I already indicated how (RFM: V, § 25) was reminiscent of Brouwer.

One of the main features of all the forms of constructivism is the insistence on a deeper meaning of existence in mathematics. Clearly, Wittgenstein shared this insistence with the constructivists. But Wittgenstein's remarks are so far too vague for us to be able to distinguish his peculiar form of constructivism from, say, intuitionism.

### 3. The Axiom of Choice

In its simplest form, the Axiom of Choice says that if A is any set of non-empty and pairwise disjoint sets, then there is a set C with just one member in common with each member of A. This is written formally as follows:

$$\forall x \forall y [(x \in A \wedge y \in A) \rightarrow ((x \cap y = \emptyset \vee x = y) \wedge x \neq \emptyset)]$$

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<sup>1</sup> On the distinction between "prose" and "calculus", see section 1.

<sup>2</sup> See also (PR: § 163) and (RFM: II, § 7).



$$\rightarrow \exists C \forall x (x \in A \rightarrow \exists z (C \cap x = \{z\}))$$

I shall not discuss in detail the history of the Axiom of Choice.<sup>1</sup> The choice of an element of a set is a procedure whose extension from any finite number of steps, which poses no problem, to an infinite number of steps caused a major controversy. There are three stages here: the infinite arbitrary choice with a stated rule and with an unstated rule; and then the infinite arbitrary choice for which no rule could be stated.

Gauss himself always stopped short of a full-fledged use of the Axiom of Choice (Moore 1982: 12). In his proof that a real function “f”, continuous on a closed interval, has a root there whenever the value of “f” has opposite signs at the endpoints, Cauchy arbitrarily selected the terms of two convergent sequences in a way that each term depended on those chosen previously (Moore 1982: 82). All the same, his arbitrary choices served only as convenient shorthand for a rule that he could have supplied if he had wished to do so (Moore 1982: 82). But the first implicit uses of the principle of choice were made in particular by Dedekind and Cantor in their trials at distinguishing between finite and infinite sets.

E. Zermelo was the first to make a conscious use of this principle in his controversial first proof of the well-ordering theorem (Zermelo 1904). Zermelo’s demonstration started with an arbitrary non-empty set M. With M being the set of all non-empty subsets M' of M, Zermelo made the first statement of the Axiom of Choice:

Imagine that with every subset M' there is associated an arbitrary element  $m'_1$  that occurs in M' itself: let  $m'_1$  be called the “distinguished” element of M'. (Zermelo 1904: 139-140)

This yields, according to Zermelo, a function (here he used Cantor’s expression “covering”)  $\gamma: M \rightarrow M$  s.t.  $\gamma(M') \in M'$  for all M' in M:

The number of these coverings  $\gamma$  is equal to the product  $\prod m'$  taken over all subsets M' and is therefore certainly different from 0. In what follows we

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<sup>1</sup> See for example G. H. Moore’s excellent study: *Zermelo’s Axiom of Choice. Its Origin, Development and Influence* (Moore 1982).

take an arbitrary covering  $\gamma$  and derive from it a definite well-ordering of the element of  $M$ . (Zermelo 1904: 140)

The rest of Zermelo's proof is unimportant for us here. The French mathematicians Borel, Baire, Lebesgue and others were quick to ask how such a (choice) function could be obtained. To mathematicians such as E. Borel, the Axiom of Choice was, I should say, like an old parchment saying that a treasure exists, but with no indication how to find it. In a direct comment on Zermelo's foregoing postulate of a choice function, he wrote:

... it would be necessary to give at least a theoretical means of determining the distinguished element  $m'$  in any given subset  $M'$ , and this problem appears extremely difficult if we suppose that, ..., the set  $M$  is the continuum. (Borel 1905: 194) \*

Borel rejected Zermelo's use of uncountably many arbitrary choices. But he left open the possibility of countably, or denumerably many such choices, accepting therefore the axiom of countable choice. As late as 1949, Borel still voiced the same objections to the Axiom of Choice (Borel 1949: 200-201; 227-228). In the famous exchange of letters on this topic, Baire and Lebesgue went farther than Borel. The latter saw the same difficulties with countably many choices as with uncountably many:

I see sometimes difficulties as grave in reasoning with a denumerable infinite number of choices as in reasoning with a transfinite number of them. (Borel 1914: 156) \*

In fact, Lebesgue stood, in his own admission, closer to Kronecker on the topic of mathematical existence:

It all boils down to this old question: *Can we prove the existence of a mathematical entity without defining it?*

It is obviously a matter of convention; but I believe that we can only build solidly if *we admit that we can prove the existence of an entity only by defining it*. From this viewpoint, which is close to Kronecker's... (Borel 1914: 154) \*

Baire also went farther than Borel in rejecting the possibility of countably many choices and in admitting only finitely many choices (Borel 1914: 153-154).

It is in this context that Wittgenstein's remarks on the Axiom of Choice are best seen. Wittgenstein knew the Axiom of Choice under its Russellian name of Multiplicative Axiom.<sup>1</sup> It was defined by Whitehead and Russell in the following terms:

If  $k$  is a class of mutually exclusive classes, no one of which is null, there is at least one class  $m$  which takes one and only one member from each member of  $k$ . (PM: \*88)

F. P. Ramsey criticised their interpretation of the axiom in "The Foundations of Mathematics", because the class whose existence is asserted has to be definable by a propositional function, and this, he claimed, renders the axiom doubtful. Instead, Ramsey proposed that we understand by "class":

... any set of things homogeneous in type not necessarily definable by a function which is not merely a function in extension... (Ramsey 1925: 208-209)<sup>2</sup>

Ramsey did not ask that the choice function be definable in formal terms. This, according to him, has the advantage of rendering the axiom a "most evident tautology" (Ramsey 1925: 209). In fact, Whitehead & Russell originally had a problem because of their ontological convictions —linked with the vicious circle principle. Therefore, they had to introduce all functions in a non-extensional manner.<sup>3</sup>

Moving in the opposite direction to Ramsey, Wittgenstein had reservations about the plausibility of such a selection, if it is said to be carried over a class with an infinite number of subclasses:

What gives the multiplicative axiom its plausibility? Surely that in the case of a finite class of classes we can in fact make a selection [choice]. But what about the case of infinitely many subclasses? It's obvious that in such a case I can only know the law for making a selection. Now I can make something like a *random* selection from a finite class of classes. But is that *conceivable* in the case of an infinite class of classes? It seems to me to be nonsense. (PR: § 146)

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<sup>1</sup> And Russell probably gave it the name "Multiplicative Axiom" because he first came across it in connexion with cardinal multiplication, i.e. in order to construct a class for the product of a denumerable infinity of cardinals (Grattan-Guinness 1975: 491).

<sup>2</sup> Ramsey clearly adopted an extensionalist attitude about mathematics, which is very well expressed by this quotation.

<sup>3</sup> I shall discuss Ramsey's extensionalism in section 5.

Obviously, *Wittgenstein was rejecting the very idea of an arbitrary infinite choice of elements for which no rule could be stated*. This reaction is in the tradition of Kronecker, and similar to the reaction of the French semi-intuitionists. He stood closer to Baire and Lebesgue, since there is no reason to believe that he would have accepted, with Borel, the Axiom of Choice in denumerable contexts.

Wittgenstein thought that in order to understand a mathematical proposition one has to be able to apply it, i.e. to command a clear view of its application. At the time Wittgenstein wrote, the Axiom of Choice was the perfect example of a proposition which was not properly understood. Mathematicians kept discovering unsuspected uses, for example, in previous work of Baire and Borel. In **RFM** Wittgenstein made two remarks on the Axiom of Choice in this connection :

There are here, I believe, cases in which someone can indeed apply the proposition (or proof), but is unable to give a clear account of the kind of application. And the case in which he is even unable to apply the proposition. (Multiplicative Axiom) (RFM: V, § 25)

And:

We might say: if you did not understand *any* mathematical proposition better than you understand the Multiplicative Axiom, then you would *not* understand mathematics. (RFM: VII, § 33)

Reactions to Zermelo's formulation of the Axiom of Choice were the expression of a *malaise* in the mathematical community at that time: Zermelo's first proof was clearly not satisfying (with its use of non-mathematical notions such as space and time) and moreover, the Axiom of Choice had a limited role. It was not well understood, in particular by the critics themselves.<sup>1</sup> Peano, for example, remained "neutral":

Are we to believe now that the proposition is true or that it is false? Our opinion is neutral. (Peano 1906: 148)\*

Even Whitehead & Russell, although they made the effort of rendering obvious all their uses of the Multiplicative Axiom in **Principia Mathematica**, remained agnostic.

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<sup>1</sup> Those who defended it were quick to point out that detractors such as Baire or Borel did use it in their work in an essential way. This would indeed undermine their criticisms.

*There is nothing wrong with Wittgenstein's remarks, if he implied that mathematicians did not understand the Axiom of Choice and its applications. But, needless to say, this is hardly the case today, after extensive investigations of proofs which are based on it, and also of theorems which could be proved without it, such as the Schröder-Bernstein Theorem.<sup>1</sup>*

Wittgenstein's standpoint on the Axiom of Choice was not really different from T. Skolem's. T. Skolem was one of the first logicians to take up a Kroneckerian finitist stance. He could not accept the reduction of arithmetical notions to set-theoretical ones, neither could he accept the Axiom of Choice:

So long as we are on purely axiomatic ground there is, of course, nothing special to be remarked concerning the principle of choice...but if many mathematicians—indeed, I believe, most of them—do not want to accept the principle of choice, it is because they do not have an axiomatic conception of set theory at all. They think of sets as given by specification of arbitrary collections; but then they also demand that every set be definable. We can, after all, ask: What does it mean for a set to exist if it can perhaps never be defined? It seems clear that this existence can be only a manner of speaking, which can lead only to purely formal propositions—perhaps made up of very beautiful *words*—about objects *called* sets. But most mathematicians want mathematics to deal, ultimately, with performable computing operations and not to consist of formal propositions about objects called this or that. (Skolem 1922: 300)

Here, we find a few of the favorite themes of Wittgenstein's later philosophy. First there is the deep mistrust of the axiomatic method, echoed by Wittgenstein's warnings against the intrusion of logic in mathematics. We can see Wittgenstein's position in the words Skolem's uses to describe the attitude of the mathematicians towards the Axiom of Choice: whoever is interested in "performable computing operations" —and Wittgenstein never ceased to put the emphasis on this aspect as primordial in mathematics— would

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<sup>1</sup> The Schröder-Bernstein, or Cantor-Bernstein theorem states that for two cardinal numbers  $\kappa$  and  $\lambda$ : If  $\kappa \leq \lambda$  and  $\lambda \leq \kappa$  then  $\kappa = \lambda$ . The theorem was conjectured, but not proved, by Cantor (Cantor 1895-7: 91). The original proof by Dedekind made use a principle equivalent to the Axiom of Choice. But today's usual proof, originating in the writings of Schröder and Bernstein, with "mirrors" does not need anything equivalent to the Axiom of Choice. See (Enderton 1977: 147), and (Levy 1979: 85) for a slightly different proof using the same basic idea.

react negatively at the appearance of the Axiom of Choice. This was quite a normal reaction from the mathematicians at the turn of the century, and Wittgenstein was not overreacting.

I must insist on the strictly constructivist character of Wittgenstein's remarks on the Axiom of Choice. It is true that Wittgenstein's remarks express views similar to the visceral reaction of constructivist minded classical mathematicians to the appearance of the axiom. He certainly remains in good company, with Baire, Lebesgue and Skolem, as we saw. But on this point, two things must be said.

First, weak forms Axiom of Choice are now admitted and play a role in constructive mathematics. A good example is the "Axiom of Dependent Choices" which is used by constructive mathematicians of E. Bishop's school and with the help of which one can obtain most of the arithmetic of the real line.<sup>1</sup> As we shall see in a moment, the Axiom of Choice does not constitute a serious problem for someone who adopts the intuitionist language and its interpretation of the quantifiers and, therefore, various weak forms of the Axiom of Choice are used in the intuitionist school. It is therefore clear that although the full Axiom of Choice is unacceptable for constructivist mathematicians, weak forms are used, in particular contexts.

More interesting for us is the second point. As I just said, the Axiom of Choice does not cause much trouble to the intuitionist. This is explained by the intuitionistic reading of the quantifiers. For example, the following axiom of countable choice, or AC00:

$$\forall n \exists m [A(n, m) \rightarrow \exists a \forall n A(n, a_n)]$$

with the variable  $a$  ranging over functions from  $\mathbb{N}$  to  $\mathbb{N}$ , or the more general AC0, for any arbitrary domain  $D$ :

$$\forall n \exists d \in D [A(n, d)] \rightarrow [\exists \varphi \in (\mathbb{N} \rightarrow D) \forall n A(n, \varphi_n)]$$

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<sup>1</sup> For a discussion of the Axiom of Dependent Choices within ZF, see (Levy 1979: 168-169) and for a constructivist reading of it see (Bridges & Richman 1987: 12).

are used by the intuitionists (Troelstra & van Dalen 1988: 189-190) (Troelstra 1977b: 928, 1028). Dummett's brief discussion in **Elements of Intuitionism** is helpful in understanding why such axioms are acceptable for the intuitionist. He points out that on a Platonistic interpretation of the quantifiers,  $AC_{\infty}$  is obviously true because we can define  $a$  by using the least number principle as:

$$a(n) = \min\{m \mid A(n,m)\}$$

but such a justification is not available for the intuitionist, because under his interpretation the least number principle does not hold (Dummett 1977: 52-53). Nevertheless, under an intuitionist interpretation of the quantifiers, for each  $n$  we can find an  $m$  for which we can effectively prove  $A(n,m)$ . The function  $a$  is constructive and when applied to  $n$  gives a suitable  $m$  (Dummett 1977: 53).<sup>1</sup>

We are therefore facing one of the major points of disagreement between Wittgenstein and the intuitionists. Indeed, we saw that Wittgenstein rejected the possibility of an arbitrary infinite choice of elements for which no rule could be stated as nonsensical, while the intuitionists agree with some weak forms. One must therefore distinguish between classical mathematicians such as Borel who adopt the language of classical logic but nevertheless entertain constructivist scruples and the intuitionists. And Wittgenstein definitely appears closer to the classical mathematician with constructivist (finitist) scruples than to the intuitionist. The reason underlying Wittgenstein's stance remain obscured to us so far. I would contend that *the differences between the attitude of the intuitionist and that of Wittgenstein on the Axiom of Choice hinge on their different readings of the quantifiers*. But I haven't discussed Wittgenstein's reading of the quantifiers yet, so I shall leave the matter unsettled for the moment. Hopefully, the next

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<sup>1</sup> See also (Dummett 1977: 314) for the axioms of choice found in Kreisel and Troelstra's theory CS of choice sequences. There are limitations to the intuitionist's use of the Axiom of Choice. For example, Myhill objected that the axioms of the creative subject would break the principle of continuity for lawlike and choice sequences, i.e. one would still make choices while losing continuity (Myhill 1967). Goodman and Myhill also proved that there are cases in intuitionistic versions of ZF where choice implies  $p \vee \text{not-}p$  for every  $p$  (Myhill & Goodman 1978).

sections will provide us with the opportunity to discuss Wittgenstein's reading, as I shall approach quantification in the TLP by contrasting Wittgenstein's constructivism about the natural numbers series with the impredicative second-order definitions found in the logicist literature (Dedekind and Frege.)

It is seldom noticed —especially in philosophical circles— that most uses of the Axiom of choice in, say, number-theoretic proofs or algebraic theories such as Galois theory are eliminable. An important consequence of K. Gödel's work on the consistency of set theory and the Axiom of Choice and the Continuum Hypothesis was pointed out by G. Kreisel (Gödel overlooked it):

A consequence of Gödel's work on the consistency of the axiom of choice and the continuum hypothesis is this: if an experimental theorem can be proved in standard set theory from these axioms it can also be proved without them: one 'relativises' the proof to so-called constructible sets and classes, and observes that an arithmetical theorem is its own relativised form since the integers are absolute. (Kreisel 1956: 165) <sup>1</sup>

The result says that if one possesses a proof in ZF of a number-theoretical statement  $\alpha$  which uses the Axiom of Choice, then one can find a proof in ZF of  $\alpha$  which makes no use of the Axiom of Choice.<sup>2</sup> This very important result is quite instructive about mathematical practice. The use of the Axiom of Choice in number theory therefore makes for simplicity in proofs and in exposition, and it is permitted because it is known to be ultimately eliminable.

I take it that the usual practice in algebra today is to avoid as much as possible use the Axiom of Choice or its many equivalents, and to make any use of them clear. A good example is found in Garling's introduction to Galois Theory (Garling 1986). He warns:

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<sup>1</sup> In connection with Wittgenstein's remarks, see (Kreisel 1978a: 107, note 4) and (Kreisel 1978b: 80).

<sup>2</sup> This result does not hold, however, if one makes strong assumptions about large cardinals, and therefore without ZF.



“You should avoid using it unless it is really necessary” (Garling 1986: 14).<sup>1</sup> One could do most of algebra without it.<sup>2</sup>

For all these reasons, it is clear that *a complete rejection even from the standpoint of constructive mathematics is out of order*. These considerations weaken considerably the strength of Wittgenstein’s criticisms, who appears as a “doctrinaire”. Notwithstanding the fact that he made only a few short remarks, that they are banal and they express no new opinion or argument, such a reaction is justifiable only in its historical context. On the other hand, I do not see that these remarks fare worse than boundless faith in the Axiom of Choice, to be found for example in Hilbert’s enthusiastic comments such as:

... the essence of the Axiom of Choice is based on a general logical principle which is necessary and indispensable for the foundation of mathematical induction. (Hilbert 1922b: 178) \*

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<sup>1</sup> He himself made only three uses of Zorn’s Lemma in his whole book (in the proofs of theorems 8.2, 8.3 and 18.5.)

<sup>2</sup> As far as algebra is concerned, most uses of the Axiom of Choice concern algebraic closures of fields. But because of the essentially finite combinatorial character of algebra, one could also avoid entirely the use of Zorn’s Lemma: “Algebra is principally concerned with finite discrete operations, and it would have been possible, at the cost of not establishing the existence of algebraic closures, to have avoided all use of the axiom of choice” (Garling 1986: vii). This comes as no surprise in Galois Theory, whose core is number-theoretic. On the other hand, the so-called “modern algebra”, initiated in the school of E. Noether, is replete with algebraic closures which need Zorn’s Lemma. Indeed, to come back to our example of the previous section, Kronecker’s number-theoretical approach precisely avoids the transfinite construction of algebraically closed fields by the use of his “indeterminates”, i.e. “by adjoining new algebraic numbers to  $\mathbb{Q}$  as needed” (Edwards 1990: 97).

### III. The Logician Definitions of the Natural Numbers

«Alle Zahlen». Hier wissen wir, daß der Satz falsch aufgefaßt wurde, und daß die vollständige Induktion gar nichts mit der Allheit der Zahlen zu tun hat.

L. Wittgenstein

#### 4. Dedekind's Chains and Frege's Hereditary Sets

In his *Arithmetices principia nova methodo exposita*, G. Peano introduced his celebrated axiomatisation of arithmetic, with the following five axioms (Peano 1889: 113):<sup>1</sup>

- 1)  $0 \in \mathbb{N}$
- 2)  $\forall x \exists y [(x = 0) \vee (x = Sy)]$
- 3)  $\forall x \forall y [(Sx = Sy) \rightarrow (x = y)]$
- 4)  $\forall x [(x \in \mathbb{N}) \rightarrow (Sx \neq 0)]$
- 5)  $\forall x_1 \dots \forall x_n \{ A(0) \wedge [\forall x (A(x) \rightarrow A(Sx))] \} \rightarrow (\forall x A(x))$   
(with  $A(x)$  any formula with free variables  $x, x_1, \dots, x_n$ )

In these axioms, the three mathematical primitives are the individual constant “0”, the predicate “is a natural number” and the unary operation symbol “S” for the “successor” function.

The original foundational ambition of set theory was of giving a set theoretical definition of the concept of natural number.<sup>2</sup> This was to be done by identifying the numbers with the help of the other two primitives, which would be previously defined in

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<sup>1</sup> In this section, while describing the works of Peano, Dedekind, Frege and Whitehead & Russell, I deleted some unimportant details and I rendered all formulas in an uniform notation. This was done for the sake of brevity. I do hope that in doing so I didn't introduce any crucial distortion. Here, for example, it should be said that Peano originally gave nine axioms, but four of them were about equality and are in fact pertaining to the underlying logic. In this very paper Peano, probably under the influence of (Dedekind 1888) which he had read, used 1 instead of 0; but he changed to the latter in later papers.

<sup>2</sup> This reduction of numbers to sets lost much of its meaning with T. Skolem's results on categoricity, which was in fact prefigured already in informal remarks of Dedekind and Peano themselves. See for example Skolem's lucid analysis: (Skolem 1922: 299). Of course, second-order formalizations are categorical, but this is another debate in which I shall not enter here.

purely set theoretical terms. I shall not discuss the approach of G. Cantor in this section. I shall rather concentrate on a particular variant of this approach, which we could describe, borrowing an expression from W. V. O. Quine, as “set theory in disguise”, namely the attempts at a *reduction* of mathematics to logic. This attempt at a reduction was perpetrated initially by R. Dedekind and G. Frege and later in the monumental work of A. N. Whitehead and B. Russell: **Principia Mathematica**.

This approach had an extraneous philosophical motivation for Frege, Russell and his French propagandist L. Couturat. In the third part of the **Begriffsschrift**, Frege attacked Kant’s thesis that all mathematical judgments are synthetic *a priori*. It is the first formulation of his so-called “logician” thesis. If all fundamental mathematical concepts could be defined in logical terms only, then it would appear that mathematical propositions, derived from these definitions, would be analytical in nature. While Frege was still hesitant at the time of the publication of the **Grundlagen der Arithmetik**, saying that he only hoped to “have made it probable that the laws of arithmetic are analytic judgements and consequently a priori” (FA: § 87); Russell and Couturat were quick to proclaim the Kantian doctrine dead.<sup>1</sup>

The logicist strategy was, similarly to the set theoretical approach, to define natural numbers, and the principle of mathematical induction in purely logical terms. In the third chapter of **Introduction to Mathematical Philosophy**, Russell gave in informal terms a definition of the natural number series using only “0” and “successor”. The crucial step was the definition of mathematical induction with the help the Fregean notion of an “hereditary property”, which was called the “ancestral relation” by Whitehead and Russell (PM: \* 90). One peculiar feature of Russell’s presentation, which will attract our attention because it provides a nice link with Wittgenstein’s ideas, is that the definition of the natural numbers series was meant to get rid of the expression “and so on”:

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<sup>1</sup> See (Couturat 1904: 303) for some harsh words against the Kantian doctrine by Russell’s French epigone.

What are the numbers that can be reached, given the terms “0” and “successor”? Is there any way by which we can define the whole class of such numbers? We reach 1, as the successor of 0; 2, as the successor of 1; 3, as the successor of 2; and so on. It is this “and so on” that we wish to replace by something less vague and indefinite. (Russell 1919: 20-21)

This innocent remark is in fact rather important because the expression “and so on” Russell wishes to eliminate is precisely the reference to the process of iteration which characterizes in the eye of all constructivists the natural number series. It is precisely in trying to eliminate this “and so on”, and therefore any reference to the potentiality of the processes involved, that both Dedekind and Frege (and with Frege, Whitehead & Russell) produced their definitions of the natural number series  $\mathbb{N}$ . I shall give brief sketches of both Dedekind’s and Frege’s programmes.

This is precisely where the set-theoretical notions are imported, any constructivist would say illegitimately, in the foundations of arithmetic. I shall point out in the next section that these definitions by Dedekind and Frege are circular, in the sense of Poincaré. Both Dedekind’s definition, with the notion of an simply infinite system and Frege’s, with his notion of “hereditary set” are definitions of a totality, that of the natural numbers, in which a reference to the totality of what Russell would call the “inductive classes” is made. These rather remarkable constructions of the logicians were rejected by constructivists, such as Poincaré, who in a sense refused to eliminate the “and so on” from their definition. After discussing Poincaré’s objection of circularity, I shall indicate that in contrast to Dedekind and Frege-Russell, Wittgenstein provided a definition of the natural number series in the TLP which is definitively in the constructivist tradition. That Wittgenstein, in contrast to the logicians, wouldn’t eliminate the “and so on” is already seen from 6.03:

The general form of the cardinal number is:  $[0, \xi, \xi+1]$ . (TLP: 6.03)

Dedekind published in 1888 a little book, *Was sind und was sollen die Zahlen?*, written in reaction to the publication of Kronecker’s “Über den Zahlbegriff” the

previous year (Dedekind 1888: 31). In it, he started by defining a “thing” as any “object of our thought” (Dedekind 1888: § 1), and he called “system” any arbitrary set of such “objects of our thought”:

... a system  $S$  (an aggregate, a manifold, a totality) as an object of our thought is likewise a thing; it is completely determined when with respect to every thing it is determined whether it is an element of  $S$  or not. (Dedekind 1888: § 2)

In a footnote to this very passage he replied to Kronecker, saying that the restrictions imposed on the formation of concepts by Kronecker were simply unjustified:

In what manner this determination is brought about, and whether we know a way of deciding upon it, is a matter of indifference for all that follows; general laws to be developed in no way depend upon it; they hold under all circumstances. I mention this expressly because Kronecker not long ago... has endeavored to impose certain limitations upon the free formation of concepts in mathematics which I do not believe to be justified... (Dedekind 1888: § 2, note a)

The difference of approach between the two number theorists is clearly stated here. It would be interesting here to know where Wittgenstein stood on this issue. Fortunately, there is a passage from the conversations with Schlick and Waismann where Wittgenstein commented on Dedekind’s strategy. We find him completely at odds with Dedekind:

Nothing is more suspect than too great generality. In giving his definition of the infinite Dedekind pretends he has no idea that afterwards he will be dealing with *numbers*. Perhaps the definition will fit lions! All this is nonsense. (WWK: 103)

I shall come back to Wittgenstein’s criticisms and to his own definition of numbers towards the end of the section. I should like only to point out that Wittgenstein disapproved Dedekind’s “general definitions”, that he stood in the tradition of Kronecker. Dedekind spoke of a “transformation” (*Abbildung*)  $\phi$  of a system  $S$  as a law:

... according to which to every determinate element  $s$  of  $S$  there *belongs* a determinate thing which is called the *transform* of  $s$  and denoted by  $\phi(s)$ ; we say also that  $\phi(s)$  *corresponds* to the element  $s$ , that  $\phi(s)$  *results* or is *produced* from  $s$  by the transformation  $\phi$ , that  $s$  is *transformed* into  $\phi(s)$  by the transformation  $\phi$ . (Dedekind 1888: § 21)

and he then gave the following definition of finite and infinite systems:

A system  $S$  is said to be *infinite* when it is similar to a proper part of itself; in the contrary case  $S$  is said to be a *finite* system. (Dedekind 1888: § 64) <sup>1</sup>

When came the time to prove the existence of an infinite systems, Dedekind's proof took a "transcendental" form: <sup>2</sup>

Theorem: There exist infinite systems.

Proof: My own realm of thoughts, i.e. the totality  $S$  of all things, which can be objects of my thought, is infinite, For if  $s$  signifies an element of  $S$ , then the thought  $s'$ , that  $s$  can be an object of my thought, itself an element of  $S$ . If we regard this as a transform  $\phi(s)$  of the element  $s$  then as the transformation  $\phi$  of  $S$ , thus determined, the property that the transform  $S'$  is part of  $S$ ; and  $S'$  is certainly proper part of  $S$ , because there are elements in  $S$  (e.g. my own ego) which are different from such thought  $s'$  therefore are not contained in  $S'$ . (Dedekind 1888: § 66)

Hilbert thought that the proof was deficient because of its reference to the "totality of all things".<sup>3</sup> But one should notice that, at any rate, the idea of the proof was at the origin of E. Zermelo's Axiom of Infinity (not to be confused with Russell's) (Zermelo 1908: 204).

While Frege would define one set of objects as the natural numbers, Dedekind actually defined a class of structures, with any one of such structure being able to serve as the set of natural numbers (this was possible because of Dedekind's use of the famous notion of "abstraction".) Dedekind called these structures "simply infinite systems" and he gave the following four conditions for a simply infinite system  $S$  (Dedekind 1888: § 71):

- (1)  $(\forall x)[(x \in S) \rightarrow (\phi(x) \in S)]$
- (2)  $S = \cap \{X \mid (1 \in X) \wedge ((\forall x)(x \in X \rightarrow \phi(x) \in X))\}$
- (3)  $(\forall x) [(x \in S) \rightarrow (1 \neq \phi(x))]$
- (4)  $\phi$  is one-to-one

<sup>1</sup> This distinction between the finite and the infinite is to be found already in Cantor (Cantor 1932: 119) and in B. Bolzano's *Paradoxien des Unendlichen* (Bolzano 1851: § 20).

<sup>2</sup> The expression is from (Hilbert 1904: 131).

<sup>3</sup> See (Hilbert 1904: 131). This point is debatable. In conversation D. Isaacson pointed out to me that since Dedekind's axiomatisation is second-order, he simply had a domain with second-order quantification over it, and it is not clear if any paradox can be derived from the comprehension axioms at his disposal. It is also fitting to remark here that Russell's critique of Dedekind's proof (and of Bolzano's related one) made no use of a possible derivation of paradoxes in Dedekind's system (Russell 1919: 137-139).

The strong similarity with the axioms given the following year by Peano is striking.<sup>1</sup> Arithmetic was to be the laws derivable from these essential conditions of any simply infinite systems:

If in the consideration of a simply infinite system  $N$  set in order by a transformation  $\phi$  we entirely neglect the special character of the element, simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the order-setting transformation  $\phi$ , then are these elements called *natural numbers* or *ordinal numbers* or simply *numbers*,...With reference to this freeing the elements from every other content (abstraction) we are justified in calling the numbers a free creation of the human mind. (Dedekind 1888: § 73)

Dedekind's idea was that the natural number series should contain a "base-element" from which it can proceed without end. The third condition defines 1 as such a base-element, which is not the successor of any number. The first condition states that every number has a successor. Moreover, every number has only one successor: the relation  $\phi$  is therefore a one-to-one function. This is the fourth condition. The crucial step is the second condition, where *the system  $S$  is defined as the intersection of all classes  $X$  which contain the base-element 1 and which contain  $\phi(x)$  whenever  $x \in X$  and  $\phi$  has a value for  $x$* . Dedekind arrived at this definition after "lengthy reflection" (Dedekind 1890: 100) in order to define the natural numbers series  $N$  in such a way as to include all the elements  $n$  that normally belong to it and at the same time avoid the inclusion of undesirable elements  $t$ ,<sup>2</sup> without "the most pernicious and obvious kind of vicious circle" (Dedekind 1890: 100-101), which would consist in presupposing a knowledge of the natural numbers beforehand. Dedekind gave a description of such a mistaken approach in those terms:

If one presupposes knowledge of the sequence  $N$  of natural numbers and, accordingly, allows himself the use of the language of arithmetic, then of course, he has an easy time of it. He need only say: an element  $n$  belongs to the sequence  $N$  if and only if, starting with the element 1 and counting on and on steadfastly, that is, through a finite number of iterations of the mapping  $\phi$ , I actually reach the element  $n$  at some time; by this procedure,

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<sup>1</sup> See (Kneale & Kneale 1962: 473) for example. In fact, one could claim that Peano simply took Dedekind's conditions as his axioms. He himself admitted using Dedekind's book (Peano 1889: 103).

<sup>2</sup> This is the same problem Frege had to solve, with the possibility that a definition of numbers includes Julius Caesar as a number (FA: § 56).

however, I shall never reach an element  $t$  outside of the sequence  $N$ .  
(Dedekind 1890: 100)

Dedekind was facing the problem of excluding undesirable elements because he was defining a class of structures of which any could serve as the natural numbers, but he could not use previous knowledge of the language of arithmetic to define this class of structures. He could not say that  $n$  belongs to  $N$  if  $n$  is 1 or a value of  $\phi$  after a finite number of iterations of  $\phi$  starting from 1. In other words, he was trying to get rid of the “and so on” mentioned at the beginning of this section. This is why he introduced the notion of chain (Dedekind 1888: § 37, 44), the use of which he explained in the following terms to Keferstein (Dedekind 1890: 101). An element  $n$  of  $S$  belongs to  $N$  if and only if  $n$  is an element of every part  $K$  of  $S$  that possesses the following properties (with  $\phi(K)$  the image):

- (1)  $1 \in K$
- (2)  $K \supseteq \phi(K)$

Dedekind went on to provide a principle of mathematical induction for the (finite) numbers (Dedekind 1888: § 59, 60, 80):

$$[(1 \in M) \wedge ((\forall x)(x \in N \cap M \rightarrow \phi(x) \in M))] \rightarrow (M \supseteq N)$$

and he then gave a general theorem on definitions by recursion which states that for any function  $g$  from (an arbitrary)  $S$  to  $S$ , and with  $\omega$  a determinate element; there is one and only one function  $f$  of  $N$  such that:

$$\begin{aligned} S &\supset f(N) \\ f(1) &= \omega \\ f(\phi(y)) &= g(f(x)) \end{aligned}$$

From this he was able to give recursive definitions for addition (Dedekind 1888: § 135), multiplication (Dedekind 1888: § 147) and exponentiation (Dedekind 1888: § 155). Dedekind ended his book by some results on cardinal numbers: he proved that for any finite set  $S$  there is a unique natural number  $n$  such that there is a one-to-one



correspondence between  $S$  and the initial segment of the natural numbers (Dedekind 1888: § 160):

$$I_n = \{x \mid 1 \leq x \leq n\}$$

He would then define the cardinal of the finite set  $S$  as the natural number  $n$  such that  $S$  is in one-to-one correspondence with  $I_n$ . We can see that Dedekind's definition of cardinals was limited to finite sets. On the other hand, Frege's approach, which we shall discuss in a moment, would enable him to prove that there exists an infinite cardinal.

This concludes my brief survey of Dedekind's achievements in **Was sind und was sollen die Zahlen?**. I hope to have shown the crucial character of the role played by the notion of chain in the definition of a simply infinite system. I shall indicate in the next section how it contains a circular reference *the intersection of all* classes  $X$  which contain 1 and  $\phi(x)$  whenever  $x \in X$  and  $\phi$  has a value for  $x$ .

It is a noticeable fact that with respect to their reduction of mathematical induction Dedekind's system and Frege's system in the **Begriffsschrift** or in the **Grundlagen der Arithmetik** are the same. This fact was already recognized by Dedekind himself in both the preface to the second edition of his book (Dedekind 1888: 42-43) and in his letter to H. Keferstein (Dedekind 1890: 101). But Dedekind's notion of system, his notion of a transformation of a system, and his proof of the existence of infinite systems, which was reproduced above, contain psychologistic elements which would be excluded from Frege's investigations. In the introduction to his **Grundgesetze der Arithmetik** Frege complained that these notions were "not reduced to acknowledged logical notions" (GA1: 4). His approach was quite different. Its most striking feature was that he associated numbers with extensions of concepts and then started with a definition of cardinal numbers instead of a definition of ordinal numbers.

Frege defined the cardinal number of a set  $S$  as the set of all sets in one-to-one correspondence to  $S$ .<sup>1</sup> A relation  $R$  is one-to-one if for every  $x, y$  and  $z$ :

$$\begin{aligned} ((zRy) \wedge (zRx)) &\rightarrow (x=y) \\ ((xRz) \wedge (yRz)) &\rightarrow (x=y) \end{aligned}$$

Sets are said to be numerically equal if and only if there is such a relation  $R$  between their elements. This way Frege is able to define equality in number between sets without using numerical terms (I shall indicate in section 8 Wittgenstein's objections to this approach.) He could also prove that each set has an unique cardinal number and that the cardinals of sets in one-to-one correspondence are identical.

According to Frege 0 is the number of the empty set (FA: § 74):

$$0 = \{\emptyset\}$$

With the help of the relation of immediate succession, which he defines, for  $C(X)$  the cardinal number of a set  $X$ , as (FA: § 76):

$$n \text{ S } m \equiv (\exists x, X) \{((x \in X) \wedge (C(X) = n)) \wedge (C(X - \{x\}) = m)\}$$

Frege could then define the number 1 as the set of all sets numerically equal to 0, i.e. to  $\{\emptyset\}$ . Here Frege needed to restrict the numerically equal sets to those definable in logical terms. So his definition of 1 was (FA: § 78):

$$1 = \{ S \mid (\exists y) [(y \in S) \wedge (\forall x)((x \in S) \rightarrow (x=y))]\}$$

In a manner perfectly similar to Dedekind, Frege would then define  $\mathbf{N}$  as the intersection of all sets  $X$  such that 0 is an element and when  $x$  is an element and  $y$  is its successor, then  $y$  is also an element.

The most difficult stumbling block was to be the definition of mathematical induction in purely logical terms. This was to be done with the help of the notion of “hereditary property” —Frege gave it such a name because he had in mind the fact that

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<sup>1</sup> This definition is to be found in (FA: § 72), while the notion of one-to-one correlation is introduced in (FA: § 63).

“every child of a human being is in turn a human being” (BG: §24)— which plays the same role chains played within Dedekind’s system. For a given relation  $f$ , Frege defined another relation  $f'$ , expressed by the famous formula “ $h$  follows  $g$  in the  $f$ -series”. If  $f$  is the relation of parent to child,  $f'$  is the relation of ancestor to descendant —this is why this relation  $f'$  is now known as the “proper ancestral” of  $f$  (BG: § 26). “ $h$  follows  $g$  in the  $f$ -series” means that  $h$  possesses every property which belongs:

- 1) to any object to which any possessor of the property has the relation  $f$
- 2) to any object to which  $g$  has the relation  $f$

Any property which satisfies both conditions is hereditary in the  $f$ -series.

The original notion of “hereditary property” is formulated in FA, in the following manner:

If every object to which  $x$  stands in the relation  $\phi$  falls under the concept  $F$ , and if from the proposition that  $d$  falls under the concept  $F$  it follows universally, whatever  $d$  may be, that every object to which  $d$  stands in the relation  $\phi$  falls under the concept  $F$ , then  $y$  falls under the concept  $F$ , whatever concept  $F$  may be. (FA: § 79)

If one assumes as the only order relation that of “being the immediate successor of” and 0 as the basis of the succession, then one can obtain the natural number series from the relation “successor of”. The formula would be:

$$N = \cap \{X \mid (0 \in X) \wedge [(\forall x) ((x \in X) \wedge (y S x)) \rightarrow (y \in X)]\}$$

Mathematical induction is a consequence of the definition of  $N$ . The definition of an “hereditary property” was therefore crucial. In Frege’s own words:

Only by means of this definition of following in a series is it possible to reduce the argument from  $n$  to  $(n+1)$ , which on the face of it is peculiar to mathematics, to the general laws of logic. (FA: § 80)

But let us go back for a moment to our original quotation from Russell’s **Introduction to Mathematical Philosophy**. Again, it could be said that the Fregean notion of “following in the  $f$ -series” is just another way of getting rid of the “and so on”. Again, in Frege’s own words:

We have no need always to run through all the members of a series intervening between the first member and some given object, in order to ascertain that the latter does follow after the former. Given for example, that in the f-series b follows after a and c after b, then we can deduce from our definition that c follows after a, without even knowing the intervening members of the series. (FA: § 80)

Frege has therefore shown that the natural numbers are constituted by 0 and all its successors, its “posterity” in the language of Russell (Russell 1919: 22)), without using the “and so on”: the natural numbers series is the set including 0 and all the terms possessing the hereditary properties with respect to the relation “immediate successor of” which 0 possesses.

Frege could then introduce the proof that every natural number  $m$  has a successor  $n$  (FA: § 82). He also gave an important proof that there exists an infinite cardinal,  $\aleph_1$ : the number of  $\mathbb{N}$  is infinite because  $\mathbb{N}$  is numerically equal to  $\mathbb{N} \cup \{\mathbb{N}\}$ ; therefore it succeeds itself and cannot be finite (FA: § 83, 84). It is not surprising therefore that Frege —siding with Cantor since his  $\aleph_1$  is more or less the equivalent of Cantor’s  $\aleph_0$ — had no qualms about the introduction of actually infinite numbers (FA: § 85). He sees his logical definition of  $\aleph_1$  as “perfectly clear and unambiguous” and this is enough according to him to give it a meaning (FA: § 84). But such a definition is only possible because of the particular notion of “following in a f-series” which is set-theoretical in nature.

In *Principia Mathematica* Whitehead and Russell took over Frege’s relation of heredity, to which they gave the name of “ancestral relation” (PM: Vol. I, Section E) and a class was called “inductive” whenever it is “hereditary” (Russell 1919: 21-22) —this is why I spoke earlier of “the totality of all inductive classes” in this context— and in this respect their logical system was similar to Frege’s, only the terminology was different.

## 5. Circularity and Impredicativity

In his excellent paper, “Il concetto di infinito nella filosofia della matematica di Wittgenstein”, P. Frasnquolla pointed out that the constructions of Dedekind and Frege, which I briefly described, had a serious defect from the point of view of constructivism:

... in Frege’s definition, as in Dedekind’s the reference to the *totality* of the inductive classes (Russell), i.e. to the totality of the hereditary classes of which 0 is a member, appears as essential (with the difference that Dedekind used 1 instead of 0).

For whoever doubts the reliability or more directly the legitimacy of the classical concept of set, this reference to the totality of the inductive classes could not go unnoticed; and in fact Poincaré did not miss the chance of attracting attention to the impredicative nature of the logical definition of the natural numbers. (Frasquolla 1980a: 645) \*

The claim is that both Dedekind’s and Frege’s definitions of the natural numbers are circular, because the totality to be defined is presupposed in the definition.<sup>1</sup> It is now customary wisdom to consider these second-order definitions as impredicative (Parsons 1983: 132-133). Moreover, any weakening of the definitions will not succeed in avoiding impredicativity.<sup>2</sup>

<sup>1</sup> One must not forget that Whitehead and Russell’s attempt at predicatively defining the set of natural numbers in *Principia Mathematica* was also a failure, according to a result by J. Myhill, who states that: “...the property of being a natural number (in Russell’s sense) is not predicatively definable from  $\in$  and  $=$  taken as primitive” (Myhill 1974: 27).

<sup>2</sup> H. Wang reported in “Eighty Years of Foundational Studies” a definition suggested by M. Dummett which reads as follows (Wang 1958: 491):

$$Ny =_{df} (\forall a) [(0 \in a \wedge (\forall x) (x \neq y \rightarrow Sx \in a) \rightarrow y \in a] \wedge (\exists a) (0 \in a \wedge (\forall x) (x \neq y \rightarrow Sx \in a))$$

And in a more recent paper, A. George gave a revised version of a definition originally given by W. V. O. Quine (George 1987: 515):

$$Nx =_{df} (\forall a) (x \in a \wedge (\forall y) (Sy \in a \rightarrow y \in a) \rightarrow 0 \in a) \wedge (\exists a) (x \in a \wedge (\forall y) (Sy \in a \rightarrow y \in a))$$

Both definitions are quite similar (Parsons 1987: 211, note 23). They are both predicative if the set-theoretic variables range over finite collections only, otherwise they would simply fail to pick up just the natural numbers and leave aside intruders such as Julius Caesar. It is true that both definitions are not impredicative in the narrow sense of the word, i.e. the set defined is not required to lie within the range of the quantifier “ $\forall a$ ” of the definition. But in a less restricted sense of impredicativity, these definitions remain impredicative. Indeed, in order to understand these definitions one needs to see that “ $\forall a$ ” contains an isomorphic copy of the natural number series, i.e. sets of finite ordinals:

$$\{0\}, \{0,1\}, \{0,1,2\}, \{0,1,2,3\}, \dots$$

I therefore agree with Isaacson when he says that: “I am inclined ... to consider that the weak second-order definition does not fare significantly better on the score of avoiding impredicativity than the one based on full second-order logic” (Isaacson 1987: 156). George sees in the fact that he came out with his example of a predicative definition that there is an inherent imprecision in the idea of impredicativity (George 1987: 517). To this criticism, I would reply that the interest of Poincaré’s remarks about impredicativity and circularity lies precisely in their informal presentation. That one cannot subject it to a precise definition is no defect of the notion of impredicativity. On the contrary, it is when it is embodied in a

Particularly interesting for us is the reaction of H. Poincaré to early trials at a proof of the principle of mathematical induction, which he held, it is well known, to be a synthetic a priori judgement. Poincaré considered suspicious the set theoretical approach to natural numbers, since here one usually starts by accepting the actual infinite and then one develops the theory of transfinite numbers and only then one introduces the natural numbers as part of the former. In his first paper on these topics, he described this *détour* via the transfinite in the following terms:

... we should start by establishing the general properties of transfinite cardinal numbers, then distinguish within these a small class, that of the ordinary whole numbers. Thanks to this detour we could demonstrate all the propositions relative to the small class (i.e. all our arithmetic and algebra) without using any non-logical principles. (Poincaré 1905-6: 154)\*

Poincaré gave a whole host of arguments against such an approach, some of them rather unsatisfactory (e.g. in this very passage he claimed that this approach was contrary to any “sane psychology” (Poincaré 1905-6: 154).) But the interesting argument for us is the claim that any proof of mathematical induction involves a vicious circle.

Poincaré introduced the idea of a “vicious circle” while discussing Hilbert’s first formulation of his proof theory and of his need for a consistency proof (Hilbert 1904). This is a version of the argument of the *pétition de principe* (Poincaré 1905-6: 821-822), i.e. that some form of induction was to be used inside metamathematics itself, in order to prove the validity of mathematical induction in ordinary, naïve, arithmetic; and that this new form of induction had to be at least as strong as the ordinary form, therefore invalidating the whole process.<sup>1</sup> Poincaré claimed that there was a vicious circle:

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formal principle such as Russell’s “vicious circle principle”, that the notion loose its interest for foundational research. See (Kreisel 1962: 311, note).

<sup>1</sup> Poincaré’s argument went right to the heart of the matter. It has been argued that this was the lesson from Gödel’s second incompleteness theorem: the second form of induction could not be weaker than the first, but must be stronger (see for example J. van Heijenoort’s introduction to (Weyl 1927b)). But, as D. Isaacson pointed out to me, there is a trade off here. In Gentzen’s proof of consistency of Peano Arithmetic a single application of a stronger form of induction (up to  $\epsilon_0$ ) applied to only one primitive recursive predicate validates all the instances of  $\omega$ -induction applied to predicates of any arithmetical complexity. This point is alluded to in (Goldfarb 1988: 64-65).

... the fundamental proof needed to be “completed”, and to be completed “the definition of a finite number” was needed. But this definition itself relied on that of the smallest infinite, and this definition in turn relied on the litigious proof. This is called a “vicious circle”. (Poincaré 1905-6: 26)\*

This argument should not be confused with the charge of impredicativity. Later Poincaré and then Russell himself would define the notion of a “vicious circle” in conjunction with the set theoretical paradoxes of C. Burali-Forti and J. Richard, and this led them to the famous definition of an impredicative function. It is important to keep in mind the fact that Poincaré’s argument of circularity was always expressed in informal terms —contrary to Russell’s.<sup>1</sup> Poincaré defined in these words the notion of a definition implying a “vicious circle”:

These are still definitions by postulates, but the postulate is here a relation between the object to be defined and *all* the individuals of the same type, of which the object to be defined is itself supposed to be a part (or rather of which entities are supposed to be a part which can only be defined by the object to be defined)... According to the pragmatists such a definition implies a vicious circle. (Poincaré 1912: 7)\*

Modifying Russell’s original use of the word “non-predicative”,<sup>2</sup> Poincaré asked that we call “non-predicative” all definitions implying such a vicious circle (Poincaré 1905-6: 307). But the circularity involved in the logicist definitions is not “vicious” in the sense that it does lead to paradoxes. The argument of circularity is not so much pointing at a logical defect of the definitions, but rather putting in doubt the fact that they are genuine definitions.<sup>3</sup> Such improper definitions are frequent in classical mathematics. Informally a set  $S$  is defined impredicatively if given by:

$$S = \{ x \mid \forall y \in A P(x,y) \}$$

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<sup>1</sup> It is often Poincaré’s informal notion which is preferred to Russell’s “vicious circle principle”, because the latter is too “weak”. Again, see (Kreisel 1962: 311, note).

<sup>2</sup> “Norms (containing one variable) which do not define classes I propose to call *non-predicative*; those which do define classes I shall call *predicative*” (Russell 1906: 34).

<sup>3</sup> This analysis of impredicativity differs from Russell’s. It is also H. Weyl’s viewpoint in his *The Continuum* (Weyl 1918).

and if  $S$  is already in  $A$ . Notions as elementary as, for example, the greatest lower bound (g.l.b.) are defined impredicatively.<sup>1</sup> The g.l.b. is given by the intersection, and the real number which is defined as a Dedekind cut is an element of the set over which one takes the intersection that defines it; i.e. if we have:

$$\text{g.l.b.}(S) = \cap \{ r \mid r \in S \}$$

and  $\text{g.l.b.}(S)$  may be in  $S$ .

One should not forget here that Poincaré's charge of circularity was also directed at the multiple attempts at a proof of mathematical induction, not just the attempts by Hilbert but also the many logicist attempts made by Whitehead & Russell or Peano. In the same paper, he criticized Whitehead and Russell's proof of mathematical induction to be found in an early paper of Whitehead (Whitehead 1902: sec. III). Poincaré's argument was that their definition of an "inductive number" was impredicative because it was defined with the help of the notion of inductive class in which already appears the notion of "inductive number" (Poincaré 1905-6: 309-310). He rightly pointed out that such definitions could only be sustained if one adheres to the actual infinite:<sup>2</sup>

It is the belief in the existence of an actual infinite which gave birth to these non-predicative definitions. I mean: in these definitions appears the word *all*....The word *all* has a precise meaning when we talk of a finite number of objects; but in order still to possess such a meaning when the objects are in an infinite number, an actual infinite is needed. Otherwise *all* these objects could not be conceived as being given previously to their definition and then if the definition of a notion  $N$  depends on *all* the objects  $A$ , it could be marred by a vicious circle, if in the objects  $A$  there are some which we couldn't define without using the notion  $N$  itself. (Poincaré 1905-6: 316) \*

Poincaré does not seem to have been aware of the original attempts by Frege and Dedekind, which were the basis of those he criticized. Nevertheless, his charge of

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<sup>1</sup> There are many examples in the literature. See (Weyl 1919: 111-112), for H. Weyl's analysis of the impredicativity of the usual definition of the least upper bound, or (Kreisel 1960: 372) for a more elegant and simple formulation.

<sup>2</sup> Indeed, if no satisfactory reinterpretation of the formula defining inductive numbers is possible, the only alternative to a rejection is an extreme Platonism about the existence of Frege's hereditary sets (i.e. properties), in order to salvage the definition. We shall see that this was the position adopted by Ramsey and later by Gödel.



circularity could easily be extended. Indeed, there is a circularity in a definition of *all* natural numbers in terms of *the intersection of all* the hereditary sets (or chains, or inductive classes) to which 0 belongs and to which the successor of  $x$  belongs if  $x$  belongs to it. Here the totality to be defined is presupposed: in the definition the totality of the natural numbers is needed among the totality of all inductive classes. Because they wanted to get rid of any reference to the “and so on”, Dedekind and Frege both produced a set-theoretic definition of natural numbers which is circular.<sup>1</sup>

The Fregean conception can only be sustained if one adheres to the descriptivist viewpoint according to which numbers are already given to us in their totality, i.e. if we accept the actual infinite. Since this was already presupposed at any rate, such an obvious circularity was never thought by classical mathematicians and logicians to be causing any problem. The circularity was indeed obvious enough. Wittgenstein pointed to it in the TLP. Apart from the constructivists, F. Ramsey, F. Kaufmann, R. Carnap and K. Gödel also discussed the matter during the late twenties and early thirties. Ramsey mentioned the circularity in his “Foundations of Mathematics” (Ramsey 1925). His immediate reaction was to ask for an extensionalist attitude, according to which all properties and their extensions are already given to us. This would lead to a reintroduction of the actual infinite.<sup>2</sup> Ramsey pointed out that there is nothing wrong with referring to a man as the “tallest in a group”, even though he would then be identified by means of a totality of which he is himself a member (Ramsey 1925: 192). But, of course, here the domain is finite. Ramsey would claim that there is no viciousness either with  $\forall x f(x)$  ranging over

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<sup>1</sup> The accusation of circularity was levelled by most constructivists of the day. For example H. Weyl said as early as 1918, in *The Continuum*: “... I became firmly convinced (in agreement with Poincaré, whose philosophical position I share in so few other respects) that *the idea of iteration, i.e., of the sequence of the natural numbers, is an ultimate foundation of mathematical thought*—in spite of Dedekind’s ‘theory of chains’ which seeks to give a logical foundation for definition and inference by complete induction without employing our intuition of the natural numbers. ... Moreover, I must find the theory of chains guilty of a *circulus vitiosus*’ (Weyl 1918: 48). See also his paper “Über die neue Grundlagenkrise der Mathematik” (Weyl 1921: 149), and his *Philosophy of Mathematics and Natural Sciences* (Weyl 1927a: 48–49).

<sup>2</sup> See also (Watson 1938: 444).

an infinite domain because “our inability to write propositions of infinite length” is “logically a mere accident” (Ramsey 1925: 192). Indeed, Ramsey asked that we regard all properties as already given to us, in analogy with the case of the tallest man in the group, before any attempt at a description. Therefore, the fact that we describe some properties impredicatively is a mere empirical fact. This is a crucial aspect of Ramsey’s viewpoint; since by allowing impredicative definitions, he is satisfied with using the simple theory of types. Since there is no need of ramification, contrary to Russell’s system in the **Principia Mathematica**, there is also no subsequent need of the Axiom of Reductibility, which was, with the Axiom of Infinity, a crucial defect for logicism because they weren’t “logical” axioms.

But Kaufmann and Carnap rejected this “theological” viewpoint (Carnap 1931: 50). Instead, they tried to find a solution by reinterpreting the same Fregean definition of the natural numbers using a distinction due to Kaufmann between “individual” (Carnap used the term “numerical” instead) and “specific” generality. According to both of them, the belief that we must survey all the elements (i.e. the individuals) of the domain in order to verify a generalized statement is a confusion between these two kinds of generality (Kaufmann 1930: 71, 76) & (Carnap 1931: 51).<sup>1</sup> “Specific” generality is not to be understood as established by verifying it for all the individuals, it is somewhat “analytic”:

We do not establish specific generality by running through individual cases but by logically deriving certain properties from certain others.  
(Carnap 1931: 51)

Gödel reviewed Carnap’s paper in 1932, reporting his way out (Gödel 1932: 245). Twelve years later, Gödel came back to the topic in “Russell’s Mathematical Logic”: for

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<sup>1</sup> It is rather surprising that Carnap enlisted Kaufmann in the logicist enterprise, since Kaufmann clearly held constructivist views, in a similar fashion to all those, such as H. Weyl and O. Becker, who were influenced by E. Husserl. Carnap was in fact quite aware of this fact, and in **The Logical Syntax of Language** he listed Kaufmann, with...Wittgenstein, as allies of the intuitionist (Carnap 1934: 46-49). Carnap saw very well that Wittgenstein was a constructivist of some sort, but not an intuitionist.

him Carnap's solution, even if it is not without difficulties,<sup>1</sup> has the advantage of getting rid of the circularity (Gödel 1944: 455-456). The problem with Carnap's programme is that in order to establish that all truths of mathematics are analytic, he had to face Gödel's incompleteness theorems, and could not obtain his result from deduction from any usual axiom system. This is why he used a version of the so-called  $\omega$ -rule.<sup>2</sup> Adding this rule to a strong enough axiom system to represent Peano Arithmetic renders this system complete with respect to all truths expressible in the language of arithmetic.<sup>3</sup> The  $\omega$ -rule is therefore a useful tool to extend provability beyond Peano Arithmetic. But it then requires for its justification acceptance of modes of argument which lies beyond those usually needed for Peano arithmetic. And any weakened version of the  $\omega$ -rule which could be justified by these doesn't extend provability beyond Peano Arithmetic.<sup>4</sup> The aim of Carnap's definition of analyticity is defeated. I do not see how the charge of impredicativity could be circumvented this way.

I agree with G. Kreisel that *there are no convincing arguments from the classical standpoint which bestows a predicative character to these inductive definitions of numbers* (Kreisel 1960: 388). At any rate, Gödel did not need Carnap's solution, since he opted for Ramsey's extreme Platonism. Gödel clearly saw that there was a problem with the classical account of universal quantification as an infinite logical product and with the inductive definitions of numbers, but he claimed that the problem is for those adopting the "constructivist standpoint" according to which mathematical entities are constructed by

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<sup>1</sup> Gödel pointed out that the translation of the sentence containing a symbol for an impredicatively defined notion will again contain a symbol for that notion. Moreover, if one reduces "All" to an infinite logical product while reading the quantifier as meaning "analyticity" or "necessity" or "demonstrability", then one has to reestablish a hierarchy of orders and further problems beset this approach if one keeps admitting impredicative definitions (Gödel 1944: 457-458).

<sup>2</sup> The rule DC2 in (Carnap 1934: 38).

<sup>3</sup> See the Theorem 14.3 (Carnap 1934: 40) for Carnap's version of this result.

<sup>4</sup> On these matters see D. Isaacson's forthcoming (Isaacson 1990). Doubts about the  $\omega$ -rule have been raised recently. A proof theorist such as J.-Y. Girard is now claiming that there is "no room" for the  $\omega$ -rule in his linear logic (Girard 1988: 27).

ourselves. But for those not holding such a viewpoint, i.e. for those adopting Platonism, or a descriptivist attitude, there are no problems:

... even if “all” means an infinite conjunction, it seems that the vicious circle principle ... applies only if the entities involved are constructed by ourselves... If ... it is a question of objects that exists independently of our constructions, there is nothing in the least absurd in the existence of totalities containing members, which can be described (i.e., uniquely characterized) only by reference to this totality. (Gödel 1944: 456)

As a solution, Gödel adopted Ramsey’s extreme form of Platonism, assuming that mathematical objects exist independently of us, and that they have a reality as legitimate as that of physical bodies (Gödel 1944: 456). To this ultimate form of Platonism, the circularity of the definitions is not denied: it is only claimed that it causes no problem.

## IV. The Constructivism of the Tractatus Logico-Philosophicus

### 6. A Constructivist Definition of the Natural Numbers

Poincaré's criticisms of the logicist definition of the natural numbers as circular or impredicative probably influenced Wittgenstein (Frasquolla 1980a: 645-646).<sup>1</sup> It is possible Wittgenstein had Poincaré's remarks in mind when, in the **TLP**, he criticized the Frege-Russell definition of the ancestral relation from his own standpoint:<sup>2</sup>

If we want to express in logical symbolism the general proposition "b is successor of a" we need for this an expression for the general term of the formal series:  $aRb$ ,  $(\exists x) : aRx . xRb$ ,  $(\exists x,y) : aRx . xRy . yRb, \dots$  The general term of the formal series can only be expressed by a variable, for the concept symbolized by "term of this formal series" is a *formal* concept. (This Frege and Russell overlooked; the way in which they express general propositions like the above is, therefore, false; it contains a vicious circle.)

We can determine the general term of the formal series by giving its first term and the general form of the operation, which generates the following term out of the preceding position. (TLP: 4.1273)

Consequently, Wittgenstein's own definition of formal series (i.e. series which are ordered by *internal* relations)<sup>3</sup> is on the following model (TLP: 4.1252):

$aRb$   
 $(\exists x) : aRx . xRb$   
 $(\exists x,y) : aRx . xRy . yRb$   
and so on

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<sup>1</sup> There is no evidence that Wittgenstein ever read Poincaré, although it is possible that, at least, he heard of him during his discussions with Russell in Cambridge before the Great War.

<sup>2</sup> According to G. E. M. Anscombe this accusation was a "peculiarly vicious blow" to Russell (Anscombe 1959: 128). The circularity was to be found in \*90.163, \*90.164 and the proof of \*90.31. The second edition of *Principia Mathematica* included an appendix in which this objection was dealt with (Anscombe 1959: 128n). But it is now generally agreed that Russell's attempt in the Appendix B of the second edition at providing a predicative definition of natural numbers was unsuccessful. On this point see (Myhill 1974). I. ul-Haque also noted Wittgenstein's objection (ul-Haque 1978: 50). But he wrongly claimed there is no circularity involved (ul-Haque 1978: 52). According to ul-Haque, the problem with the logicist reduction would be the reduction of Peano's third axiom, which needed the Axiom of Infinity. I do not deny this well-known fact (already noticed by Wittgenstein), it is just not relevant for the point I am trying to make here.

<sup>3</sup> The notion of "internal relation" plays the crucial role here: "The internal relation which orders a series is equivalent to the operation by which one term arises from another." (TLP: 5.232)

Here,  $b$  is said to be the successor of  $a$ . We can see that contrary to the logicist tradition, the “and so on” is not eliminated.<sup>1</sup> The same can be said about his definition of the natural numbers series to which I shall turn presently.

The general form of the proposition is  $[\bar{P}, \bar{\xi}, N(\bar{\xi})]$  (TLP: 6). This general form *shows* that any proposition is the result of successive applications of  $N(\bar{\xi})$  to the elementary propositions “ $\bar{P}$ ”. With “ $N$ ” being the famous operator of joint negation, analogous to H. Sheffer’s stroke “ $|$ ” —there are difficulties with this conception of Wittgenstein, which I shall not discuss here. Then Wittgenstein claimed that we can see how by an operation we can generate one proposition out of another:

If we are given the general form of the way in which a proposition is constructed, then thereby we are also given the general form of the way in which by an operation out of one proposition another can be created.  
(TLP: 6.002)<sup>2</sup>

Consequently, Wittgenstein introduced the general form of an operation  $\Omega'(\bar{\eta})$ , which reads as follows:

$$[\bar{\xi}, N(\bar{\xi})]'(\bar{\eta}) \quad (\text{i.e. } [\bar{\eta}, \bar{\xi}, N(\bar{\xi})])$$

In 6.02, Wittgenstein constructed the formal series generated by applying the general form of an operation to itself from an arbitrary base. The series would be:

$$x, \Omega'x, \Omega'\Omega'x, \Omega'\Omega'\Omega'x, \dots$$

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<sup>1</sup> The same point is made in (Anscombe 1959: 127) and (Frasquolla 1980a: 642).

<sup>2</sup> This is also the reason why Wittgenstein spoke of mathematics as a logical method (TLP: 6.2). As such, mathematics is an attempt at describing the internal relations which hold between propositions in virtue of the fact that they are generated from each other by an operation (we can also see here why mathematical propositions are not *sinnlos* but pseudo-propositions (TLP: 6.2).) Therefore the general form of an operation is essential to mathematics, and it is derived from the general form of a proposition (this is easily seen by comparing the formulas of (TLP: 6) and (TLP: 6.01).) It is because logic and mathematics have the general form of the proposition as their common ancestor that mathematics is a “logical method”. Waismann’s notes on the philosophy of mathematics include a nice statement of the similarity between logic and arithmetic: “The logical particles are truth-operations. Thus the meaning of the word “or” is the operation that turns the sense of the propositions “ $p$ ”, “ $q$ ” into the sense of the proposition “ $p$  or  $q$ ”. This operation is expressed by the structure of a truth-function. Truth-functions can be constructed systematically. The numbers come into existence through repeated applications of the operation  $+1$ ” (WWK: 216).

But with the following definitions:

$$x = \Omega^0 x \quad \text{Def.}$$

$$\Omega' \Omega^n x = \Omega^{n+1} x \quad \text{Def.}$$

the series can be re-written as:

$$\Omega^0 x, \Omega^{0+1} x, \Omega^{0+1+1} x, \Omega^{0+1+1+1} x, \dots$$

Then, with the further definitions:

$$0 + 1 = 1 \quad \text{Def.}$$

$$0 + 1 + 1 = 2 \quad \text{Def.}$$

$$0 + 1 + 1 + 1 = 3 \quad \text{Def.}$$

And so on

the formal series can finally be re-written as (this last reformulation does not appear in 6.02):

$$\Omega^0 x, \Omega^1 x, \Omega^2 x, \Omega^3 x, \dots$$

This explains why Wittgenstein called numbers “the exponent of an operation” (TLP: 6.021). I shall leave aside the problems linked with this peculiar definition.<sup>1</sup> I wish only to point out that the dots terminating the formal series indicate that there is no limit to the application of the operation to its own result. It is the “and so on” which is therefore not eliminated from the definition of the numbers.

Wittgenstein’s additions in 1923, in the margins of Ramsey’s copy of the TLP leave no doubt about his intentions.<sup>2</sup> In the bottom margin of the page 155, i.e. below section 6.02 where he defined the natural numbers, Wittgenstein wrote:

The fundamental idea of math. is the idea of *calculus* represented here by the idea of *operation* [there is no full stop]. (Lewy 1967: 421)

and on the right hand side margin next to 6.02, he wrote:

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<sup>1</sup> For example, I suppose that a Fregean could claim that Wittgenstein has only introduced “numerals” here.

<sup>2</sup> These were originally reported by C. Lewy (Lewy 1967).

Number is *the* fundamental idea of calculus and must be introduced as such. (Lewy 1967: 422)<sup>1</sup>

The idea that the “and so on” cannot be eliminated from the definition of the natural numbers, but must on the contrary play a fundamental role in this definition is clearly expressed by Wittgenstein when he says that the essential characteristic of the concept of “operation” is that “there is no full stop”, and that the essential characteristic of mathematics is precisely the concept of “operation”.

Since Wittgenstein seemed to be aware of and agreed with Poincaré’s critique of Russell’s definition of natural numbers, I think that it would be wrong to use R. Fogelin’s expression and speak of a “naïve constructivism” of the TLP (Fogelin 1987: chap. VI). *The disagreement between Wittgenstein and the logicians (Frege and Russell) is as deep as it could be:*<sup>2</sup> Wittgenstein consciously rejects here one of the fundamental conception of the logicist programme, siding irrevocably with the constructivists. Russell’s reaction in the introduction to the TLP indicates that he obviously didn’t understand Wittgenstein’s intention. He wrote:

There are some respects, in which, as it seems to me, Mr Wittgenstein’s theory stands in need of greater technical development. This applies in particular to his theory of number (6.02 ff.) which, as it stands, is only capable of dealing with finite numbers. No logic can be considered adequate until it has been shown to be capable of dealing with transfinite numbers. I do not think there is anything in Mr Wittgenstein’s system to make it impossible to fill this lacuna. (TLP: Introduction, p. 21)

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<sup>1</sup> I should at least point out that Wittgenstein came to repudiate this view later: “Numbers are not fundamental to mathematics” (Z: § 706); and “Number is not at all a “fundamental mathematical concept”” (PG: 296). The explanation that follows in PG shows that the necessity of a constructive definition is not rejected as such: “So far as concerns arithmetic, what we are willing to call numbers is more or less arbitrary. For the rest what we have to do is to describe the calculus—say of cardinal numbers—that is, we must give its rules and by doing so we lay the foundations of arithmetic.” The accent is put instead on the set of rules as founding arithmetic.

<sup>2</sup> At this point I should mention F. Waismann’s *Introduction to Mathematical Thinking*. The Chap. 8 contains a critique of the logicist reduction of the principle of mathematical induction by Dedekind, Frege and Russell, along the same lines as the one I just described. Waismann saw his critique as the vindication of Poincaré’s remark that “the principle of induction is not demonstrable in a logical way” (Waismann 1951: 98). In the “Epilogue” Waismann mentioned that, among other ideas, the views on induction expressed in the chapter 8 were taken from an “unpublished manuscript of Ludwig Wittgenstein which he has been allowed to peruse” (Waismann 1951: 245). As far as I can see, textual evidence indicate that PR was the manuscript mentioned by Waismann. All this confirms my reading of Wittgenstein on mathematical induction.



But from the foregoing it should be clear that Wittgenstein never intended to provide for transfinite numbers in his definitions, and that he would not accept that his theory was inadequate because of this lacuna. On the contrary, he thought that Frege and Russell's definitions implied a vicious circle. And he considered it as essential that the definition of natural numbers shows that "there is no full stop", while Frege and Russell clearly intended to eliminate this reference to the "and so on". Moreover, contrary to Russell's opinion, it is not clear, *if ever it is possible*, how Wittgenstein's definitions could be extended to include transfinite numbers.

The definition of numbers in 6.02 gave no limit to the possibility of applying the operation which generates the numbers to the results of its own application. One has to remember that the peculiarity of an operation, contrary to a function, is that the result of applying the operation may be used in turn as the basis for another application of the operation (TLP: 5.251). This is the only way to progress in a formal series (TLP: 5.252). And the repeated application of an operation to its own result, Wittgenstein calls its "successive application" (TLP: 5.2521). And the concept "successive application" is precisely the concept of the "and so on":

The concept of the successive application of an operation is equivalent to the concept "and so on". (TLP: 5.2523)

Infinity is a property of the symbolism which is *shown* and any attempt at expressing any limit would lead us in the domain of nonsense. This line of thought runs contrary to Frege's approach as described in the previous section, with his claim that we can refer properly to an actually infinite set,  $\omega_1$ . Wittgenstein's rejection of logicism, and his conscious constructivism are linked with two of the TLP's most central distinctions, i.e. the distinction between saying and showing and the corollary distinction between sense and nonsense. To use P. Frasuolla's words:

In this way the analysis of the arithmetical infinite leads to the heart of Wittgenstein's first theory of language. (Frasuolla 1980a: 640) \*

Wittgenstein's well-known intention in the TLP was to:

... draw a limit to thinking, or rather—not to thinking, but to the expression of thoughts. (TLP: Preface)

On the inside of this rather rigid limit would be the domain of “sense”, and on the other side lies “nonsense”. To this distinction corresponds the distinction between what can be said, and what can only be shown. Indeed, attempts at saying what can only be shown are, as such, transgressions of the bounds of sense, i.e. they are nonsense. The limit was to be drawn by a careful examination of logical symbolism. Wittgenstein’s purpose in the TLP was not, contrary to Frege’s and Russell’s intentions, to construct a perfect symbolism, but to uncover the necessary symbolism underlying every language, i.e. the logical form that every language must possess in order for it to make sense. The logical form is therefore what propositions have in common with reality and in virtue of which they are able to represent it. But then propositions could not represent the logical form:

Propositions can represent the whole reality, but they cannot represent what they must have in common with reality in order to be able to represent it—the logical form.  
To be able to represent the logical form, we should have to be able to put ourselves with the propositions outside logic, that is outside the world.  
(TLP: 4.12)

If it cannot be said, it can be shown (TLP: 4.1212):

The propositions *show* the logical form of reality. They exhibit it. (TLP: 4.121)

There are therefore things that show themselves in the symbolism itself, without which no proper understanding of how the symbolism works is possible. And because we can see them we can understand how the symbolism works. And the infinite is precisely one such property of the symbolism which shows itself. *We see that the constructivist model plays a crucial role within the TLP, since it is determining the border between sense and nonsense.*

I must say a word about the notion of “internal relation” (TLP: 4.122), whose role was only recently properly evaluated through the work of G. Baker & P. Hacker and A.

Gargani.<sup>1</sup> Following Gargani, I shall contend that as far as mathematics is concerned, Wittgenstein's "internal relations" are "constructive" procedures.

The most important feature of the notion of internal relations is that *two entities are said to be internally related if it is inconceivable that they do not stand in such a relation*:

A property is internal if it is unthinkable that its objects does not possess it.

(This blue colour and that stand in the internal relation of brighter and darker eo ipso. It is unthinkable that *these* two objects should not stand in this relation.) (TLP: 4.123)<sup>2</sup>

The use of samples is a good example: if I use a patch as a sample for a colour word, say "red", it is unthinkable that the patch itself fails to be red because it is precisely that which defines what is "red". External relations might obtain: the cover of a particular book might be red, but it is thinkable that at one point in time it ceases to be red —i.e. the colour faded under the sun.<sup>3</sup> This crucial distinction has therefore to do with the notion of "necessity" and it is linked with the distinction between *norms of representation* (or rules of grammar), where internal relations are set up, and *empirical propositions*.<sup>4</sup>

The notion of internal relation plays a crucial role within the TLP, since Wittgenstein defines formal series as series "which are ordered by *internal* relations" (TLP: 4.1252), and, as I indicated in this section, the notion of formal series plays in turn an essential role in the definition of numbers. All arithmetic progressions or formal series, as Wittgenstein called them are given by an "operation" "by which one term arises from the other", i.e. they are given by an internal relation (TLP: 5.232).

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<sup>1</sup> See (Baker & Hacker 1984) and (Gargani 1985, 1986a, 1986b).

<sup>2</sup> See also (LWL: 9) and (M: 294).

<sup>3</sup> See (PI: § 429).

<sup>4</sup> This further distinction is not based on the form of type-sentences, but on *different uses of the proposition*. It therefore differs from the traditional analytic/synthetic distinction criticized by Quine. This point was put forward in (Glock 1986: 517). Quine's critique of the analytic/synthetic distinction concentrates on the notion of "cognitive synonymy" (Quine 1951: 28), and therefore on the epistemological side of the distinction. Wittgenstein's distinction between norms of representation and empirical propositions is not epistemological in nature, since it is based on a distinction between uses of the proposition. This leaves open the possibility that Wittgenstein's distinction avoids Quine's critique or even that it undermines it. These considerations open up a field of exceedingly interesting investigations, but they fall out of the scope of this study.

During the transitional period, the notion of “internal relation” played essentially the same role. For example, Wittgenstein claimed in **PR** and **PG** that one cannot talk about the totality all natural numbers unless one refers to the law of their generation itself —i.e. an infinite process— this law being an internal relation (**PR**: § 130) (**PG**: 457). Wittgenstein’s notion of internal relation is to be understood in arithmetic as the infinite process which generates the natural numbers, as opposed to the extension of the process, which would be “external”. *In mathematics, internal relations are conceived as constructive procedures:*

An internal relation, one might say, lies in the essence of things. An internal relation is never a relation between two objects, but you might call it a relation between two concepts. And a sentence asserting an internal relation between two objects, such as mathematical sentence, is not describing objects but constructing concepts. (**LFM**: 73)

This implicit constructivism in the notion of “internal relations” is another proof of the deep-rooted constructivism of Wittgenstein’s philosophy of mathematics.<sup>1</sup>

## 7. Quantification

It is now time to pose and scrutinize more closely Wittgenstein’s remarks on mathematics in the **TLP**, and especially to turn our attention to one of its most obscure corners, its theory of quantification. We just saw that Wittgenstein proposed a constructive definition of natural numbers, with the emphasis on the “and so on” in reaction against what he saw, following Poincaré, as a circularity in the Frege-Russell reduction *via* the ancestral relation. But the root of Wittgenstein’s positions is to be found in his account of generality and quantification.

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<sup>1</sup> Another important property of internal relations is that *they cannot be analysed in a pair of relations with some third entity*. This property was put forward in **PR** in reaction against the views of Russell in **The Analysis of Mind** (Russell 1921). On this topic see A. Gargani’s excellent paper, “Wittgenstein on Intentional Acts” (Gargani 1986a).

The theory of quantification is of central importance to the discussion of the infinite. As early as the **Principles of Mathematics**, Russell claimed that:

... the concept *all numbers*, though not itself infinitely complex, yet denotes an infinitely complex object. This is the inmost secret of our power to deal with infinity. (Russell 1903: 73)

W. Goldfarb has shown that the quantifiers were not fully understood until the thirties and the work of K. Gödel (Goldfarb 1979). But it is a remarkable fact that, although it is agreed that the twenties witnessed an important discussion on the nature of the quantifiers, almost nobody had a look at the **TLP**. Indeed, Goldfarb himself mentioned this book only once, *en passant* (Goldfarb 1979: 353), and most commentators of Wittgenstein avoided this very topic.<sup>1</sup> On the other hand, F. P. Ramsey found Wittgenstein's treatment of quantification so sound that he repeatedly made it his own, a sufficient indication in itself of the importance of Wittgenstein's ideas.<sup>2</sup>

Systematic use of variable binding operators in mathematics goes back at least to the early nineteenth century and Cauchy's introduction of the idea of a limit. The universal quantifier in mathematical logic had two distinct sources: one is Frege, of course, but it also appeared in the algebraic tradition in logic of G. Boole, C. Peirce and E. Schröder.

The universal quantifier as we now know it is usually attributed to Frege, who introduced it in his **Begriffsschrift**, in 1879.<sup>3</sup> Let me sketch M. Dummett's account of Frege's conception of quantification (Dummett 1973: Chap. 2 & 15). According to Frege the universal quantifier was a second-level concept with one argument place to be filled up by any first-level predicate.<sup>4</sup> The result of filling the place with any such predicate would be a sentence which would be determinately true or false. The truth-value would then be a product of truth-values, which is the result of applying the predicate to each object of the

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<sup>1</sup> (Anscombe 1959: Chap. 11), (Gargani 1966: 137-143) and (Baxley 1980) being some of the few exceptions.

<sup>2</sup> See (Ramsey 1925a: 159-160) (Ramsey 1925b: 28-29) (Ramsey 1927: 54-57) for example.

<sup>3</sup> For Frege's own account, see (BG: § 11-12) and the article "Funktion und Begriff", (CP: 150-151).

<sup>4</sup> The expressions "first" and "second-level" were used in "Funktion und Begriff". In the **Begriffsschrift**, Frege spoke of "first" and "second-order".

domain of the variable bound by the quantifier. In Frege's case, the domain was invariably the totality of all objects; and the quantifier would always be an *infinite* product of truth-values. If the result of applying the predicate to all the objects is always true, then the quantification is true; but if the result is false only once, then the quantification is false. Dummett is eager to point out that when the domain of quantification is finite—and surveyable—then the truth-value of the quantified statement could in principle be determined as a finite product; but when the domain of quantification is infinite—or finite but unsurveyable—this assumption would be open to question, because it would be claimed that we cannot determine its truth conclusively. This is annoying for Frege since for him the domain of quantification was always the totality of objects. But Frege would avoid the problem by replying that we only need to have *a general grasp of the totality*, in Dummett's words, that “we, as it were, survey it in thought as a whole” (Dummett 1973: 517).<sup>1</sup> This is the reason why Frege would say of a generalized statement such as “All men are mortal” that the fact that it implies that Chief Akpanya, who is unknown to him, is mortal is not part of the thought asserted by this proposition.<sup>2</sup> Frege insisted that it is sufficient that we have a grasp of the meaning of the generalized proposition, of the “thought” expressed by it. If it was possible to speak of a semantic theory here, it would be correct to say that at the semantic level it is implied by Frege's account that the universal quantifier amounts to a logical product, and the existential quantifier to a logical sum, but he did not make this connection since for him it is not the case at the level of meaning (sense).<sup>3</sup>

Following G. Peano (Peano 1889), Russell adopted the Fregean quantifiers but introduced a new symbolism for them. In the system of the *Principia Mathematica* for

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<sup>1</sup> This is the reason why Frege's account could not be said to be similar to the substitutional interpretation.

<sup>2</sup> This remark was made in the review of E. Husserl's *Philosophie der Arithmetik* (CP: 205). See also (FA: § 47).

<sup>3</sup> For the distinction between the semantic and meaning levels, see (Dummett 1982: 57).

any propositional function  $\hat{f}x$  there is a range of values, which consists in all the propositions which can be obtained by giving every possible determination to  $x$  in  $fx$ . A value of  $x$  “satisfies”  $\hat{f}x$  if it renders  $fx$  true. The fact that every possible determination to  $x$  render  $fx$  true is symbolized by  $(x).fx$  which may be read as “ $fx$  always” or “ $fx$  is always true”. The fact that some determinations of  $x$  render  $fx$  true is symbolized by  $(\exists x).fx$  which may be read as “there exists an  $x$  for which  $fx$  is true” or “there exists an  $x$  satisfying  $\hat{f}x$ ”. These new symbolisms are not defined, they embody new primitive ideas (PM: 15).

Although Russell didn’t share Frege’s distinction between sense and reference, he had a rather similar strategy to explain quantification. Indeed, he considered propositions such as “All men are mortal” as a particular kind of judgements, which he called “general judgements”. Of these he said:

Our judgement that all men are mortal collects together a number of elementary judgements. It is not, however, composed of these, since (e.g.) the fact that Socrates is mortal is no part of what we assert, as may be seen by considering the fact that our assertion can be understood by a person who has never heard of Socrates. In order to understand the judgement “all men are mortal,” it is not necessary to know what men there are. (PM: 45)

The idea expressed here is similar to that of Frege: there is no need to have a complete knowledge of the domain (i.e. the range of value) in order to understand the generalized proposition (i.e. the general judgement.)

Within the algebraic tradition initiated by G. Boole, C. S. Peirce introduced the universal and the existential quantifier as, respectively, a product and a sum. In 1885 (therefore after Frege) he wrote:

Here, in order to render the notation as iconical as possible we may use  $\Sigma$  for *some*, suggesting a sum, and  $\Pi$  for *all*, suggesting a product. Thus  $\Sigma_i x_i$  means that  $x$  is true of some one of the individuals denoted by  $i$  or

$$\Sigma_i x_i = x_i + x_j + x_k + \text{etc.}$$

In the same way,  $\Pi_i x_i$  means that  $x$  is true of all these individuals, or

$$\Pi_i x_i = x_i x_j x_k, \text{ etc. (Peirce 1885: 228)}$$

It is an interesting fact that Peirce used  $\Sigma$  and  $\Pi$  which already had a precise use in mathematics. For example,  $\Sigma$  is used in the definition of an integral as an infinite sum of all values of a function. This definition of the quantifiers was taken up later by E. Schröder and L. Löwenheim.

I shall now turn to the TLP. I must point out at the outset that Wittgenstein had both a theory of *quantification* and a theory of *generality*.<sup>1</sup> Wittgenstein initially approved the use of the Russellian symbolism “ $(x).fx$ ” (or “ $\forall x f(x)$ ”) for expressing generality, because other symbolisms would run into difficulties (TLP: 4.0411).<sup>2</sup> But his own treatment is rather different than that of Russell, who pointed out this very difference in his introduction to the TLP:

Wittgenstein’s method of dealing with general propositions [i.e. “ $(x).fx$ ” and “ $(\exists x).fx$ ”] differs from previous methods by the fact that the generality comes only in specifying the set of propositions concerned, and when this has been done the building up of truth-functions proceeds exactly as it would in the case of a finite number of enumerated arguments  $p, q, r, \dots$  (TLP: Introduction, p.14)

I already mentioned that the general form of the proposition is  $[\bar{P}, \bar{\xi}, N(\bar{\xi})]$  (TLP: 6), and that this means that any proposition is the result of successive applications of  $N(\bar{\xi})$  to the elementary propositions “ $\bar{P}$ ”. For “ $\bar{\xi}$ ” the values of the variable “ $\xi$ ” must be determined (TLP: 3.316, 3.317, 5.501). The determination of the variables is the description of the proposition which the variable stands for, and according to Wittgenstein there are three possible kinds of description of these propositions (TLP: 5.501):

- 1) Direct enumeration
- 2) As values of a propositional function  $fx$ ; i.e. “Giving a function  $fx$  whose *values* for all values of  $x$  are the propositions to be described” (TLP: 5.501).
- 3) Through a formal law which constructs the propositions.

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<sup>1</sup> T. Baxley has insisted on this distinction in (Baxley 1980).

<sup>2</sup> Anscombe discusses these difficulties in (Anscombe 1959: 140-141).



What seems to be the most important fact about these kinds of description is that their differences are not essential:

How the description of the terms of the expression in brackets takes place is unessential. (TLP: 5.501)

Now Wittgenstein can proceed. In 5.51 he wrote :

If  $\xi$  has only one value, then  $N(\bar{\xi}) = \sim p$  (not  $p$ ); if it has two values, then  $N(\bar{\xi}) = \sim p \cdot \sim q$  (neither  $p$  nor  $q$ ). (TLP: 5.51)

For general propositions the values of the variable “ $\xi$ ” are given as the values of a function:

If the values of  $\xi$  are all the values of a given function  $fx$  for all values of  $x$ , then  $N(\bar{\xi})$  will be the same as  $\sim(Ex)fx$ . (TLP: 5.52)

So for any “ $fx$ ” whose values are “ $fa$ ”, “ $fb$ ” and “ $fc$ ”,  $N(fx)$  would be a proposition of the form: “ $\sim fa \wedge \sim fb \wedge \sim fc$ ”. The door was then open for Wittgenstein to understand “ $\forall x f(x)$ ” and “ $\exists x f(x)$ ” as, respectively, a logical product and a logical sum, integrating the algebraic approach of Peirce. He made this point quite clearly in his 1932-33 lectures, according to G. E. Moore’s notes:

He said that there was a temptation, to which he had yielded in the *Tractatus*, to say that  $(x).fx$  is identical with the logical product ‘ $fa \cdot fb \cdot fc \dots$ ’, and  $(\exists x).fx$  identical with the logical sum ‘ $fa \vee fb \vee fc \dots$ ’, but that this was in both cases a mistake. (M: 297)

We need not worry for the moment about the reasons why Wittgenstein was ultimately to think his conceptions in the TLP were mistaken.

In this treatment of quantification, generality is distinguished from the truth-functions. Indeed, *generality comes with the specification of the arguments as the values of a given propositional function; and this differs from the truth-functions per se which*

are the logical product for the universal quantifier and the logical sum for the existential quantifier. This is what Wittgenstein expressed by saying:<sup>1</sup>

I separate the concept *all* from the truth-function.  
 Frege and Russell have introduced generality in connexion with the logical product or the logical sum. Then it would be difficult to understand the propositions “ $(\exists x).fx$ ” and “ $(x).fx$ ” in which both ideas lie concealed.  
 (TLP: 5.521)

The distinction between generality and the truth-functions, i.e. the quantifiers is quite crucial. Wittgenstein made some further remarks on quantifiers which I shall try to explain, in order to shed light on this distinction.:

That which is peculiar to the “symbolism of generality” is firstly, that it refers to a logical prototype, and secondly, that it makes constants prominent. (TLP: 5.522)

The “symbolism of generality” makes constants prominent in the sense that it is not “ $aRb$ ” but “ $\forall x \ xRb$ ” which renders “ $Rb$ ” prominent. Because in “ $\forall x \ xRb$ ” the expression (ie. the propositional function) “ $Rb$ ” collects a class of propositions “ $aRb$ ”, “ $bRb$ ”, “ $cRb$ ”... (TLP: 3.315) of which “ $\forall x \ xRb$ ” asserts that they are all true (Ramsey 1925b: 28). If “ $\forall x \ xRb$ ” puts the emphasis on “ $Rb$ ” as collecting together a class of propositions; it is also pointing to a “logical prototype” “ $xRy$ ” (with “ $R$ ” turned into a variable.) A “logical prototype” is the result of turning all constants into variables (TLP: 3.315). Wittgenstein also considered that “The formal concept is already given with an object that falls under it” (TLP: 4.12721). This meant that given “ $fa$ ”, the range of “ $fx$ ” with “ $x$ ” being a variable is already given. But here “ $f$ ” is still a constant, and we do not have the “logical prototype” proper. But we can still see what Wittgenstein meant by “referring to the logical prototype”. Wittgenstein made the further claim that: “The generality symbol occurs as an argument” (TLP: 5.523). Although Wittgenstein used the German *Allgemeinheitsbezeichnung*, as in 5.522 and 3.24, he indicated in a letter to Ogden that “symbol” would be preferable to “symbolism”, since he was referring here “to

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<sup>1</sup> This point was explained in clear terms by Ramsey in “Facts and Propositions” (Ramsey 1927: 55).

the variable  $x$  or  $y$  etc. in  $(\exists x, y)$  and not to the whole complex of symbols as before" (LO: 49). Generality is given with the variable.

Therefore I can sum up by saying that the expression " $xRb$ " collects all the propositions of the same form as " $aRb$ ". *So given " $aRb$ " we can pass to " $xRb$ ". Now once we have " $xRb$ " as the set of all propositions similar to " $aRb$ ", the expression " $\forall x xRb$ " is just the proposition which is a truth-function of these propositions. And the generality is expressed by the use of the variable " $x$ ".*

The foregoing explanations should give us a rough indication of Wittgenstein's treatment of the quantifiers. From this account I gather that *Wittgenstein made the same assumption as Frege, i.e. the assumption that there is no problem with quantification over an infinite domain*. Wittgenstein avoided Frege's "thoughts" which helped the latter in explaining universal quantification over infinite domains. Wittgenstein's theory of symbolism provided him with another way of making the same assumption, with the idea that the generality expressed by the variable " $x$ " in " $fx$ " is already understood when the particular case " $fa$ " is understood, without having to postulate Frege's "thoughts".

In Wittgenstein's jargon this treatment of generality also means that *although no enumeration is possible it is still possible to consider the universal quantifier as a logical product*. This assumption amounts to construing the quantifiers as "structural" concepts.<sup>1</sup> This was the mistake Wittgenstein made in the **TLP** which he recognized as such in the 1932-3 lectures. I quote another passage from G. E. Moore's notes: <sup>2</sup>

... in the *Tractatus* he had made the mistake of supposing that an infinite series was a logical product—that it *could* be enumerated, though we were unable to enumerate it. (M: 298)

It is now time to throw in Wittgenstein's considerations on generality, in order to possess all the elements to understand why Wittgenstein ultimately thought he was

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<sup>1</sup> I use the expression "structural" here as opposed to "constructive". The perfect example of a "structuralist" in this sense of the word would be Dedekind.

<sup>2</sup> See also (PG: 268-269).

mistaken in his account. According to him the fact that logical propositions are generally valid does not distinguish them from non-logical propositions, since to be general in logic means no more than to be *accidentally valid for all things*:

The mark of logical propositions is not their general validity.  
To be general is to be accidentally valid for all things. An ungeneralized proposition can be tautologous just as well as a generalized one. (TLP: 6.1231)

Clearly, a proposition such as “All men are mortal” possesses such “accidental” general validity. The gist of this passage is therefore that logical propositions do not possess general validity in an essentially different way than a proposition such as “All men are mortal”. Wittgenstein would then distinguish and oppose an “essential” form of general validity to the “accidental” form (one should not be confused by the use of the term “logical” as synonymous to “essential”):

Logical general validity, we could call essential as opposed to accidental general validity, e.g. of the proposition “all men are mortal”. (TLP: 6.1232)

What makes “essential” validity essential is that it holds for all members of a formal series *because it is given by the operation, or internal relation which generates all of its members*. To use Wittgenstein’s own distinctions, in the case of a logical proposition the values of the variable would be given either by direct enumeration, or as values of a given function. Then any appearance of generality is only “accidental”. And “essential” generality is expressed by the fact that the values are given by a formal law. In Wittgenstein’s jargon, a formal law is an *operation*, or an internal relation. Such “essential” general validity or universality was considered by Wittgenstein as an essential feature of mathematical propositions, and this is the reason why he rejected set-theoretical axiomatisations. They are “logical”, therefore not dealing properly with the notion of “essential” generality:

The theory of classes is altogether superfluous in mathematics.  
This is connected with the fact that the generality which we need in mathematics is not the *accidental* one. (TLP: 6.031)

The gist of this passage of the **TLP** is clear enough: generality in mathematics is “essential”, i.e. because it is given by an internal relation, and in logic it isn’t really generality: if a logical proposition is generally valid it is as good as being generally valid by accident. If we conflate logicist systems such as that of the **Principia Mathematica** with other set-theoretical ones, we can understand easily Wittgenstein’s stance: the peculiarity of mathematics is that it is related to generality through the notion of “operation”. The difference between a function and an operation is that the former cannot be its own argument while the result of an operation can be its own basis (TLP: 5.251) (WWK: 217). We saw that the very possibility of a successive application of an operation was for Wittgenstein the “and so on” which took an important part in his definition of the natural number series. Here Russell is guilty of promoting confusion between “accidental” generality (in the empirical case) and “essential” generality (in the case of a series given by a formal law) by representing both by the same symbolism, i.e. that of the propositional function.<sup>1</sup> And for Wittgenstein to “found” mathematics on Russellian logic is therefore making an enormous mistake. Moreover, there is the danger of taking seriously the talk about the actual infinite which comes with logic but which is alien, Wittgenstein would claim, to mathematics.<sup>2</sup>

In his lecture at Königsberg in 1931, F. Waismann introduced a distinction between “totality” and “system”, which bears some affinity with the conception of generality in the **TLP**. In the lecture Waismann was expounding Wittgenstein’s viewpoint on the foundations of mathematics, after lengthy preparatory discussions with Wittgenstein himself in Vienna.<sup>3</sup> Following (TLP: 5.25 & 5.251), he pointed out that there is an

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<sup>1</sup> Wittgenstein was later to express that point in saying that “ $(x)fx$ ” was originally meant to symbolize statements of ordinary language such as “All men are mortal”, and was then extended to mathematical statements, “where very different grammars apply” (AWL: 68). There is then a danger of overlooking important grammatical distinctions.

<sup>2</sup> And finally, on the top of that, there is the ultimate mistake of deriving a philosophical theory in the form of Platonism, or descriptivism in order to account for these wrong moves.

<sup>3</sup> See (WWK: 102-107) and especially (WWK: 213-217) which contains exactly the same material as in the manuscript of the Königsberg lecture.

essential difference between, say, the chairs in the conference room and natural numbers, which is expressed by distinguishing between a totality, which is empirical and given by a propositional function, and a system, given by an operation:

An empirical totality goes back to a *property* (a propositional function); a system to an *operation*. (Waismann 1931: 64)

As I said, according to Wittgenstein generality in mathematics is not “accidental”, and this is why he flatly rejected set theory, or logicism (TLP: 6.031). Waismann expressed the same attitude towards the “essence”, if I may use such a word, of mathematics:

In mathematics we are always confronted with systems, and not with totalities. (Waismann 1931: 65)

We can see from the similarities between Wittgenstein’s **TLP** and Waismann’s remarks at Königsberg that the distinction between “essential” generality and “accidental” generality survives in the transitional period. In fact, I claim that this distinction is one of the major features of Wittgenstein’s approach of the “foundations” of mathematics, never to be relinquished but rather to be refined as early as 1929 in a constructivist critique of logicism, set theory and classical mathematics. The logicist viewpoint is overlooking the distinction between, on the one hand, empirical totalities and “accidental” generality and, on the other hand, systems and “essential” generality. This confusion is to be found in Fregean and Russellian theories of quantification, *which imports the accidental generality of logic in the foundations of mathematics*. It is for this reason that the logicians are then in a position to speak about numbers from the point of view of completed totalities, without referring to the “and so on”. I shall indicate in a later section on the Russellian quantifiers how this stance evolved in a critique of the use of the universal quantifier in mathematics.

There is one point in my account of the **TLP** which was left obscured. The differences between the three modes of descriptions were said to be unessential (TLP: 5.501). On the other hand, I just claimed that the differences were important, since in mathematics the description was done by formal laws or internal relations. I think this

could be explained by the fact that Wittgenstein didn't consider seriously the case of an infinite domain of quantification, were the difference in the manner of presentation would be essential. One possible explanation is that since in the TLP all talk about existence of objects is banished. This is the reason why Wittgenstein rejected Russell's Axiom of Infinity, and why he did not talk about cardinality. Therefore, he did not discuss questions related to the infinity or the finiteness of the domains of quantification. So there was no significant difference in his eyes between an enumeration and a series given by a formal law.

I claim, however, that all the elements of the later criticisms of the classical standpoint held in the TLP were already present in it. A sparkle was needed to ignite Wittgenstein's criticisms, and it obviously was the intuitionistic criticism of classical mathematics, which was at the centre of the *Grundlagenstreit* of the late twenties. I shall come back to this extremely important topic in section 13.

## 8. Numerical Equality, Predicativity and Choice

The notion of numerical equality or equinumerosity as based on one-to-one correspondence is widely used today in set theory (even in intuitionistic set theory) without being questioned. But it was not adopted without criticisms. As Frege mentioned in *Grundlagen der Arithmetik*, it became popular during the later half of the 19th century (FA: § 63), with G. Cantor and E. Schröder using it. The notion was essential to the logicist definitions of cardinal numbers of Frege and Russell (see section 4.) In *The Principles of Mathematics* Russell described the notion of numerical equality in the following terms:

Under what circumstances do two classes have the same number? The answer is, that they have the same number when their terms can be correlated one to one, so that any one term of either corresponds to one and only one term of the other.

...  
 We must say: Two classes have the same number when, and only when, there is a one-one relation whose domain includes the one class, and which is such that the class of correlates of the terms of the one class is identical with the other class. (Russell 1903: 113)

Reacting to these very passages in Russell's book, the French philosopher of mathematics P. Broutroux made this critical remark:

But, again, what do the elements of the classes under consideration have in common, if it is not precisely the numbers by which they are ordered, the abstraction of which we are trying to obtain. Either the postulated correspondence already implies the notion of number, or it is impossible to affirm that it does not change when we move from one pair of elements to another. (Boutroux 1904: 916) \*

Poincaré apparently agreed with this critique, since he said while commenting on Boutroux's paper, that the notion didn't have the clarity that the logicians believed it had (Poincaré 1905-6: 830). But such criticisms are unfounded since the notion of equinumerosity "does not demand an order" (Russell 1919: 17).

L. E. J. Brouwer himself did not make much use of the notion of one-to-one correspondence in his early writings. In his doctoral dissertation, the notion of numerical equivalence was not primitive. It was a relation between completed ordered sets and presupposed the notion of number (Brouwer 1907: 15). But the notion of one-to-one correspondence eventually became more respectable to Brouwer when he introduced sets that are not necessarily well-ordered, i.e. when he introduced spreads and species after the First World War.<sup>1</sup>

In the conversations with M. Schlick and F. Waismann, Wittgenstein made some critical remarks directed towards Frege's use of numerical equality in his definition of cardinal numbers.<sup>2</sup> Wittgenstein's rejection of numerical equality as a primitive notion is

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<sup>1</sup> See (van Stigt 1990: 312-314).

<sup>2</sup> One will also find remarks about the logicist definition of natural numbers and their use of numerical equality in PR, PG and RFM, which will be discussed here. One of the 1939 lectures on the foundations of mathematics, i.e. LFM, Lecture XVI was almost entirely dedicated to this question. I must honestly admit, however, that I didn't find any clear-cut argument in it worth discussing here. For the sake of completeness I should mention that one will find another argument against Frege in the notes of



close in spirit to these remarks of Boutroux and the early Brouwer, although the argument is completely different. It is worth discussing them now that we have seen Frege's definition of the natural number series, the impredicativity of logicist definitions and Wittgenstein's constructive definition. Wittgenstein's criticisms will shed light on his standpoint about predicativity, and also about the Axiom of Choice, a question that I left partly unsettled at the end of section 3.

Wittgenstein saw Frege's attempt at defining cardinal numbers *via* numerical equality as circular because the very notion of numerical equality already presupposes the concept of number. Therefore one cannot base numbers on numerical equality. Wittgenstein expounded his argument in his familiar informal manner:

Imagine I have a dozen cups. Now I wish to tell you that I have got just as many spoons. How can I do it?

If I had wanted to say that I allotted one spoon to each cup, I would not have expressed what I meant by saying that I have just as many spoons as cups. Thus it will be better for me to say, I can allot the spoons to the cups. What does the word "Can" mean here? If I meant it in the physical sense, that is to say, if I mean that I have the physical strength to distribute the spoons among the cups—then you would tell me, We already knew that you were able to do that. What I mean is obviously this: I can allot the spoons to the cups because there is the right number of spoons. But to explain this I must presuppose the concept of number. It is not the case that a correlation defines number; rather, number makes a correlation possible. This is why you cannot explain number by means of correlation (equinumerosity). You must not explain number by means of correlation; you can explain it by means of possible correlation, and this precisely presupposes number.

You cannot rest the concept of number upon correlation. (WWK: 164-165)<sup>1</sup>

Wittgenstein's argument was that *the numerical equality is not determined by the correlation, but rather it is the numerical equality which makes the correlation possible* (Waismann 1951: 109). Wittgenstein would later describe this idea in a more colourful

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Waismann (WWK: 221-225) which could be summed up by the following quotation: "what is wrong with the Frege-Russell definition is that it does not specify a method of verification" (WWK: 222). There is a distinctive verificationist flavor to it which renders it less interesting.

<sup>1</sup> In (PR: § 118) Wittgenstein used this very example about cups and spoons, and Waismann was later to use it also, to make again the same point in (Waismann 1951: 107-113).

way by saying of Russell's definition of sameness of number: "He puts the cart before the horse" (PG: 355).

That Frege didn't see it this way is to be attributed to what I termed in the introduction the *descriptivist* attitude which is ever so typical of set theory—it was notoriously that of Cantor—<sup>1</sup> which consists in acting as if the objects are *already given before any attempt at what can only be a description* (see the introduction.) This is the attitude detected by Poincaré, which is concomitant with the adoption of the actual infinite (see section 5.) Again, I couldn't insist more on the fact that *this opposition between the descriptivist attitude (of the logicians) and the constructivist attitude of Wittgenstein is one of the most fundamental one of his whole philosophy*. It is already present in the TLP, but more and more consciously put forward in his later philosophy. In philosophy of mathematics, it is pervading all his criticisms of the logicist programme and of set theory.

One will find the perfect example of this descriptivist attitude in a passage of the **Grundgesetze der Arithmetik**, Vol.1 where Frege asked: What happens when we correlate two concepts? His answer was that the situation is similar to drawing an auxiliary line in Geometry; because the act of drawing doesn't not "create" the line:

In both cases we are just making ourselves aware, only grasping, what has already been there. (GA1: 88) \*

It is while quoting by heart those very words of Frege that Wittgenstein continued the conversation, adding:

This dictum sounds very paradoxical. It is connected with Frege's distinction between "objective" and "real".

What Frege means is evidently that it is possible to draw a line. But possibility is not yet reality. A straight line is drawn only when it has been drawn. And this is how it is with numbers too. (WWK: 165)<sup>2</sup>

and he went on sharpening his argument:

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<sup>1</sup> A very good example is to be found in (Cantor 1932: 187).

<sup>2</sup> Wittgenstein also referred to this passage from **Grundgesetze der Arithmetik** in (PG: 281, 355-356) and (RFM: I, § 21).

When Frege and Russell attempt to define number through correlation, the following has to be said:

A correlation only obtains if it has been *produced*. Frege thought that if two sets have equally many members, then there is already a correlation too.... Nothing of the sort! A correlation is there only when I actually correlate the sets, i.e. as soon as I specify a definitive relation. But if in this whole chain of reasoning the *possibility* of correlation is meant, then it presupposes precisely the concept of number. Thus there is nothing at all to be gained by the attempt to base number on correlation. (WWK: 165)

One will find here favorite themes of the later Wittgenstein already at work. For example, in these last quotations he insists on making the distinction between possibility and reality. This is a crucial distinction for his notion of infinite, since he would claim that the latter can only be understood as a possibility, contrary to our tendency to believe that in mathematics everything is before us. Moreover, when he said that he *can* correlate the spoons and the cups, he interpreted the word “can” as an empirical possibility (i.e. it is physically possible to correlate them) and as a grammatical possibility (i.e. the correlation is possible because there are as many spoons as there are cups). This distinction will also appear as crucial in his conception of the infinite in mathematics, which can be understood only as a grammatical possibility. I shall discuss these topics at length in Chapter V. It is important here to construe Wittgenstein as trying to resist the descriptivist approach by propounding an interpretation of numerical equivalence which does not presuppose that the correlation was already there before it is effected. The stress is therefore on the “can”.

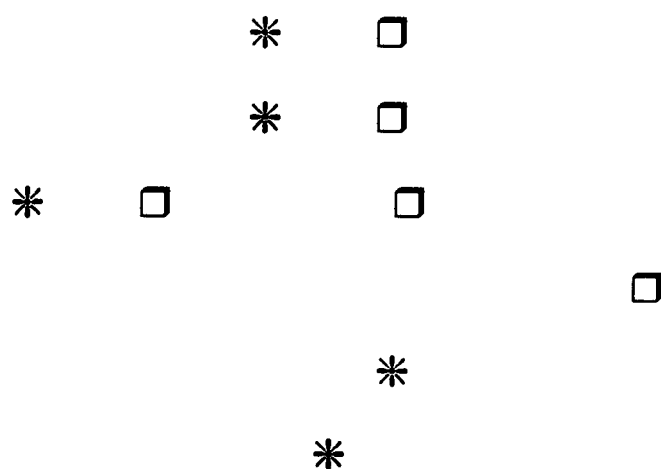
I must mention that Wittgenstein’s “modal” argument against numerical equality, with the insistence on the “possibility” of a correspondence, was taken over not only by F. Waismann (Waismann 1951: 107-113), but also by R. L. Goodstein. In **Constructive Formalism**, Goodstein repeated Wittgenstein almost word by word:

How do we know whether two elements correspond or not? A cup standing on a saucer have an obvious correspondence, so too a bird carrying a twig in his beak, or a pencil mark on a sheet of paper; but it is patently false to say there *is* a correspondence between the members of any two classes (of the same number of terms); and if one says that, even when there is no actual correspondance, such a correspondence always *can* be established, the possibility to which we refer must be a logical possibility, a consequence of, not a condition for, the two classes having an equal number of terms.

*The concepts of number and function are defined by the transformation rules for number and function signs. It is not a one-to-one related pair of classes that determines a function, but a function which determines a pair of one-to-one related classes. (Goodstein 1951: 19)*

Goodstein's equational calculus made no use of equinumerosity, since it is not set-theoretical. I may therefore say that although this very argument remains controversial, as we shall see, it is important to see that it is a consequence of a non-set-theoretical approach.

There are many difficulties with Wittgenstein's argument.<sup>1</sup> One obvious problem with it is that it might not work in the finite case. If someone is shown **Fig. A**:



and that person is asked “Are there as many stars as squares?”, he or she does not need to count the stars and the squares, i.e. to use numbers. That person may simply use his or her pen and join the stars to the squares:

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<sup>1</sup> For example, see M. Dummett's criticisms of this argument, which he attributed to Waismann (Dummett 1963: 150-151).

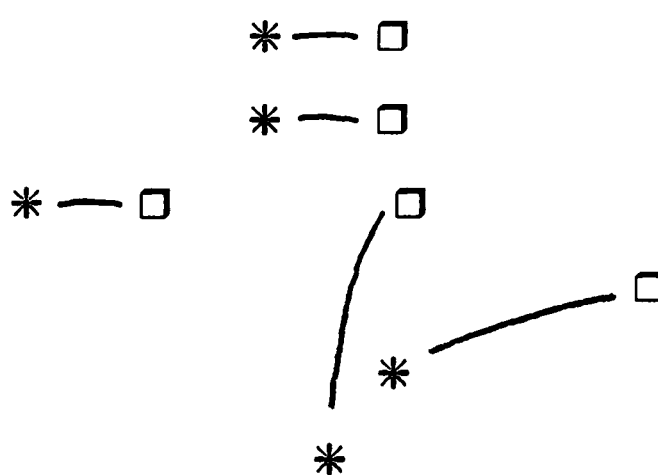
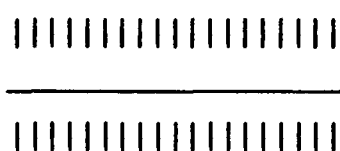


Fig. B

This is the reasoning behind the very idea of numerical equivalence (Enderton 1977: 128-129) and there is no defect in it. But I do not see Wittgenstein's remarks as an attempt at criticizing this reasoning. It is rather an attempt at interpreting differently what we achieve when we actually do the correlation. The usual descriptivist interpretation would point out in a Fregean manner that the correlation was already there before we did the pairing. As I said, Wittgenstein would oppose to this reading a more modal one, with the stress on the "possibility", i.e. the "can", against the "actual" of the set-theorists:

Can I know there are as many apples as pears on this plate, without knowing how many? And what is meant by not knowing how many? And how can I find out how many? Surely by counting. It is obvious that you can discover that there are the same number by correlation, without counting the classes.



In Russell's theory only an *actual* correlation can show the 'similarity' between the classes. Not the *possibility* of correlation, for this consists precisely in numerical equality. Indeed, the possibility must be an *internal* relation between the extensions of the concepts, but this internal relation is only given through the equality of the 2 numbers. (PR: § 118)

There are also discussions of elementary examples equivalent to **Fig. A** and **Fig. B** in (AWL: 92), (LFM: 160) and (RFM: I, § 25-40).<sup>1</sup> As I take it, the gist of these passages is also that Wittgenstein *does not deny* that, say, **Fig. B** is a proof that stars and squares are “the same in number” (RFM: I, § 32). But, again, his interpretation of what we do when we decide to count this as a proof of the numerical equivalence of the stars and squares is not descriptivist:

The proof doesn't *explore* the essence of the two figures, but it does express what I am going to count as belonging to the essence of the figures from now on.—I deposit what belongs to the essence among the paradigms of language.

The mathematician creates *essences*. (RFM: I, § 32)

It seems that although Wittgenstein's argument remains dubious, he nevertheless pointed at an assumption made by the descriptivist which, in Dummett's terms, “stands in need of philosophical justification” (Dummett 1963: 151), i.e. that the descriptivist assumes that the correlation already obtains before it has been carried out, or before it has been shown to be possible in principle. To paraphrase Dummett, the correlation appears to give us information about a state of affairs whose existence is independent of our carrying it out.<sup>2</sup>

One important consequence of admitting the descriptivist interpretation in the finite case is that some impredicativity is acceptable, such as in the case used by Ramsey — already mentioned in section 5— of the definition of the median height of a population (Ramsey 1925: 192). Clearly this is not obtainable prior to our measuring the population, but one can see how it could be said to be there prior to the measurements. But on Wittgenstein's interpretation no such reasoning is possible, because one can only speak of the median height *after* effecting the measurements and then calculating it. There is

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<sup>1</sup> The 1939 lectures contain yet another argument against “correlation”. There are many possible correlations, and none is the preferred one. Providing any new correlation is inventing “a new way of looking at things”, or giving an “extension of this idea of one-one correlation” (LFM 161). But then Wittgenstein claimed: “But you haven't even yet correlated any two things” (LFM: 160).

<sup>2</sup> Dummett's argument is presented in a different manner.

nothing preexisting, waiting to be discovered. Even calculating the median height is “creating” the “essence”. Wittgenstein is here adopting an *extreme form of anti-Platonism* (see the introduction.) His argument *leaves no room for impredicativity*. Wittgenstein could be described as a *strict predicativist*.

Finally, a last critique of Wittgenstein’s argument against numerical equality, which should help us to see the differences between Wittgenstein and Brouwerian intuitionism. If I read him properly, Wittgenstein claimed that in order to specify what is meant by saying that two sets are numerically equal, one should not say that *there is*, but rather that *there could be* a one-to-one mapping between the two sets. And Wittgenstein’s argument is that unless one adheres to the actual infinite, one cannot explain this “*could*” without reference to numbers, i.e. without any circularity. But one could counter Wittgenstein by pointing out that *there would be such a one-to-one mapping if there was a choice function*. This would simply solve his problem about numerical equality.<sup>1</sup>

An immediate reaction would be to point to Wittgenstein’s rejection of the possibility of an arbitrary infinite choice (section 3). The intuitionist will have no qualms about a choice function here. *This is one of the major differences between Wittgenstein’s own brand of constructivism and Brouwerian intuitionism.*<sup>2</sup>

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<sup>1</sup> This idea was pointed to me by Prof. M. Dummett in conversation. In connection with it, see Russell’s famous example of the millionaire who bought  $\aleph_0$  pairs of boots and  $\aleph_0$  pairs of socks, in *Introduction to Mathematical Philosophy* (Russell 1919: 126-127).

<sup>2</sup> There is, however, an argument very similar to Wittgenstein’s in H. Weyl’s *Philosophy of Mathematics and Natural Sciences*, which is aimed rather at Cantor’s view of the primacy of cardinal numbers: “But the criterion of numerical equivalence makes use of the possibility of pairing, which can be only ascertained if the acts of correlation are carried out one after another in temporal succession and the elements of the sets themselves are thereby arranged in order” (Weyl 1927a: 34). If we forget about the extraneous element of time, the argument is indeed similar to Wittgenstein’s: numerical equivalence is based on the correlation which must be effected first. It is not preexisting.

## V. Strict Finitism and the Infinite

Das Unendliche ist nur eine façon de parler...  
der endliche Mensch sich nicht vermisst,  
etwas Unendliches als etwas Gegebenes und  
vor ihm mit seiner gewohnten Anschauung zu  
Umspannendes betrachten zu wollen.

C. F. Gauss

... en fin de compte, en dépit des apparences,  
tout doit se ramener au fini.

R. Baire

### 9. What is Strict Finitism ?

The idea of strict finitism has its origin in an article by P. Bernays, “Sur le platonisme dans les mathématiques” (Bernays 1935). It is to be found in a passage where he discussed intuitionism. In order to show that intuitionistic foundations contain an essential element of idealization, Bernays radicalized the intuitionistic argument against existence in classical mathematics: What if we have stronger restrictions on acceptable existential assertions? Bernays took exponentiation as an example:

Intuitionism makes no allowances for the possibility that, for very large numbers, the operations required by the recursive method of constructing numbers can cease to have a concrete meaning. From two integers  $k, l$  one passes to  $k^l$ ; this process leads in a few steps to numbers which are far larger than any occurring in experience, e.g.  $67^{257^{729}}$ . Intuitionism, like ordinary mathematics, claims that this number can be represented by an Arabic numeral. Could not one press further the criticism which intuitionism makes of existential assertions and raise the question: What does it mean to claim the existence of an Arabic numeral for the foregoing number, since in practice we are not in a position to obtain it? (Bernays 1935: 265)

Following Bernays, one can distinguish practical possibility, or feasibility from theoretical possibility. Strict finitism would then be the requirement that the domain of the theory be restricted to that of the feasible. Although Bernays rejected such an approach as being too restrictive, it isn't without meaning: the great French mathematician E. Borel, once said, in 1947, that “when the finite becomes very large, it raises the same



difficulties as the infinite” (Borel 1947: 979).<sup>\*</sup> In fact, Borel expressed strict finitist doubts even earlier on, in 1927:

That we should consider a number as virtually known when its computation is theoretically possible but needs an amount of time and effort out of proportion with human possibilities seems to me a more serious question. (Borel 1927: 272) <sup>\*</sup>

Borel gave the following example: Let us take the first four decimals of  $\pi$  which are 1415. We then calculate the first 1415 decimals of  $\pi$ , a calculation which is still possible. But then we calculate the number of decimals equivalent to 1415 numerals, etc... Borel asked:

If we go on the same way a thousand times, we would define a numbers whose practical calculation would not only need a multitude of human lives, but, even supposing they were known, whose writing would necessitate an amount of paper whose weight would be largely superior to that of the globe.

Should we consider that the last numeral of the thousandth number so defined is calculable by us? (Borel 1927: 272) <sup>\*</sup>

In the polemic around the Law of Excluded Middle in which he was then taking part, Borel reluctantly sided with the intuitionists because of these considerations (Borel 1927: 274).<sup>1</sup> At the end of his life, Borel came back to this topic and wrote *Les nombres inaccessibles* (Borel 1952).

In “Eighty Years of Foundational Studies”, H. Wang promoted strict finitism—he called it “anthropologism”—to the rank of a foundational thesis, alongside finitism, intuitionism, predicativism and Platonism (Wang 1958: 473-476). Wang also elaborated on some strict finitist themes in one of his lesser known philosophical essays, “Process and Existence in Mathematics” (Wang 1961). In both papers Wang referred to Wittgenstein’s RFM, in fact the second paper could be seen as a discussion on some of the ideas contained in it—which he quoted without giving the references.

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<sup>1</sup> But only to add in the same breath that such cases being at the fringe and not the center of the mathematicians preoccupations, his “liberalism” would dictate to him that the use of the Law of the Excluded-Middle was admissible!

But for a long time A. Esenin Volpin was alone trying to develop a strict finitist programme under the title of ultra-intuitionism (Esenin-Volpin 1959, 1970, 1981). This is of course misleading since intuitionism makes an essential use of ideal processes, while strict finitism does not. Esenin-Volpin reasoned on lines similar to Bernays and Borel: while a Platonist would give rules that preserve truth as known by God or a Demiurge, and the intuitionist would restrict his rules to the knowledge of an “ideal” mathematician, Esenin-Volpin considered that while humans are able to understand the idea of a potential infinite, they can only grasp the small finite. He therefore asked that *the large finite should be understood as potential*, with the “feasible”, the “possible” and the “actual” becoming different modalities.

Considering assertions such as

$$[(A0) \wedge \forall a (A(a) \rightarrow A(a+1))] \rightarrow A(10^{12})$$

Esenin-Volpin asked: How could we use  $10^{12}$  times the Modus Ponens, since in seconds  $10^{12}$  constitute more than 20 000 years? Nobody has counted up to  $10^{12}$ .<sup>1</sup> By definition, one can reach the  $10^{12}$ -th member of the sequence of the natural numbers in  $10^{12}$  steps. But the expression “n steps” presupposes that n is a natural number. So this explanation of how to construct the number  $10^{12}$  in a natural number series involves a vicious circle considered by Esenin-Volpin as “no better than that involved in the impredicative definitions in set theory” (Esenin-Volpin 1970: 5).<sup>2</sup>

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<sup>1</sup> Similar considerations are to be found in R. O. Gandy’s “Limitations to Mathematical Knowledge” where he set the upper limit to any inscription to be possibly considered by mathematicians between  $10^{14}$  and  $10^{20}$  bits (binary digits); and for the limitation “in principle”, he guessed somewhere between  $10^{50}$  and  $10^{500}$  (Gandy 1982: 131). J. Mycielski also questioned the preeminence of Peano Arithmetic, and its axiom of complete induction: “Such integers as  $10^{100}$  are too large to represent material strings of bars or numbers of steps of physical processes. For those integers  $n$  the axiom of induction  $\varphi(0) \wedge (\forall x < n) [\varphi(x) \rightarrow \varphi(x+1)] \rightarrow (\forall x < n) \varphi(x)$  cannot physically justified” (Mycielski 1989: 316). Mycielski, who worked for many years in set theory, is a recent convert to strict finitism. He defined his “intentionalism” in these words: “Intentionalism says that pure mathematics is a description of finite structures consisting of finitely many individually imagined objects (the term intentionalism is chosen for its contrast with extentionalism which accepts actually infinite sets and leads naturally to Platonism...)” (Mycielski 1989: 315). Mycielski’s arguments are based on Skolem’s. Although he draws on his previous work (Mycielski 1981, 1986), his programme is quite recent and remains to be developed further.

<sup>2</sup> For a similar argument see (Nelson 1986: 174).

A strict finitist such as Esenin-volpin would therefore require that every mathematical domain be straightforwardly finite, with an upper limit. This is an extremely reductive approach. For example, number theorists working on diophantine equations often try to extract upper bounds for the size of all the solutions of a given equation. Although these quantitative bounds are obtained in a perfectly effective way they might be enormous. For example, A. Baker extended his own result stated in section 2 to curves  $f(x,y) = 0$  of genus 1, degree  $n$  and height  $H$ , where all integral zeros  $x_0, y_0$  of the curve satisfy:

$$\max(|x_0|, |y_0|) < \exp \exp \exp 2(H) 10^n 10$$

(Baker 1975: 45). Would this still be intelligible to a strict finitist? Although this bound, and the bound given for Thue's theorem in Appendix 1, might appear huge, when  $f$  and  $k$  are given the use of computers and techniques of numerical analysis can always, in conjunction with Baker's methods, solve the relevant types of diophantine equations. Rather, strict finitist doubts might come about recursive functions such as the Ackermann function, which under a particular description says that  $A_5(3,2)$  has  $2^{16}$  2's stacked in an exponential tower:

$$2^{2^{2^{\cdot^{\cdot^2}}}}$$

Such doubts were recently substantiated by Parikh, Nelson and others, as I shall indicate in a moment.

Esenin-Volpin introduced the notion of "feasible numbers", i.e. numbers up to which it is possible to count: the number 0 is feasible, and if  $n$  is feasible, then  $n < 10^{12}$ ; and so  $n'$  is also feasible. Each feasible number can be obtained from 0 by adding '. That sequence, called  $F$ , forms the natural numbers series. Esenin-Volpin didn't believe in the existence of a series containing  $10^{12}$ . So  $10^{12}$  is not in  $F$ , but it is in the traditional number series —of which only the *possibility* could be proven. There are therefore

distinct natural number series of different length. Accordingly, strict finitism has the following negative effect: there is no uniqueness up to isomorphism of the natural number series. 10 and 12 are feasible numbers but  $10^{12}$  isn't; so strict finitism has this other negative effect: the natural number series are not closed with respect to the values of a primitive recursive function such as exponentiation. Obviously, the principle of mathematical induction breaks down, and this is the reason for the appearance of paradoxes akin to the Sorites paradox in strict finitism (Dummett 1959: 182).<sup>1</sup>

Esenin-Volpin was primarily concerned with proving the consistency of ZF, but so far he has only claimed a proof of consistency of ZF with any finite number of inaccessible cardinals. While he had characterized correctly the main features of a strict finitist critique of classical conceptions, Esenin-Volpin's peculiar programme, with his own methods of investigation which takes "extreme directions" (with such strange names as: Extra-ultra-intuitionism, trans-ultra-intuitionism, pragma-ultra-intuitionism, etc... (Esenin-Volpin 1970: 44-45)) has attracted little attention.<sup>2</sup> However, it should be noted that it is an interesting fact that Esenin-Volpin's proof theory (his "reasoning theory") lends itself to a study by means of non-standard analysis, if we forget the modal setting. This is because here *arbitrarily large but finite sets play the role of infinite sets as in non-standard analysis* (Geiser 1974: 82).

I shall now turn to the recent work on weak systems initiated by an article from R. Parikh (Parikh 1971), which gave new impetus to the fundamental ideas dealt with by Esenin-Volpin.<sup>3</sup> Parikh renewed the discussion about feasible numbers by relating

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<sup>1</sup> One can also mention here that other assumptions of classical mathematics about the clarity of the notions of identity and distinctness and about the fact that only a language without formalized modalities and tenses is needed, are rejected. J. Geiser studied Esenin-Volpin's proof theory with the help of non-standard models; showing further results: the Law of Excluded-Middle, the Deduction Theorem and the Rule of Contradiction fail in it (Geiser 1974: 86-87).

<sup>2</sup> Esenin-Volpin introduced some changes to his programme in (Esenin-Volpin 1981) in order to reduce the dependence on these extreme directions.

<sup>3</sup> For an excellent overview of the literature, see (Macintyre: 1987). It seems fair to say that most of the current research on weak systems doesn't have a strict finitist motivation in the sense of Esenin-Volpin. But this is not the case with, in particular, the work of R. Parikh who proposed a "concrete or

feasibility to the weak system now known as  $I\Delta_0$ .<sup>1</sup> Parikh's system (it was called PB) is a subsystem of Peano Arithmetic where induction is applied only to formulas with bounded quantifiers (Parikh 1971: 504).<sup>2</sup> While Esenin-Volpin only provided a vague argument to the effect that  $N$  isn't closed under exponentiation, Parikh proved that provably computable functions of  $I\Delta_0$  have a polynomial growth. Exponentiation grows faster and is not provably computable and there is no  $\Sigma_1$ -definition of the graph of a total function which restricts on  $N$  to  $2^x$  (Macintyre 1987: 50). In other words *exponentiation is not total*. Extensions of  $I\Delta_0$  were studied mainly by Parikh and A. Wilkie & J. Paris:  $I\Delta_0 + \text{exp}$ , where "exp" says that  $2^x$  is total or  $I\Delta_0 + \Omega_1$ , where  $\Omega_1$  says that  $x^{\log x}$  —with  $\log$ =length to base 2, and  $x^{\log x}$  is polynomial time computable— is total (Wilkie & Paris 1987).

S. Cook introduced an equational theory (called PV) in the style of Skolem and Goodstein (Goodstein 1957) of polynomial time computable functions (Cook 1975). In PV proofs are said to be "feasibly constructive" in the following sense:

If an identity  $f(x)=g(x)$  has a proof  $Pr$  in PV, then there is a polynomial  $p_{Pr}(n)$  such that  $Pr$  provides an uniform method within  $p_{Pr}(|x_0|)$  steps that a given natural number  $x_0$  satisfies  $f(x_0)=g(x_0)$  (Cook 1975: 83)

If such an uniform method exists the equation is said to be polynomially verifiable (Cook 1975: 83). Results by S. Buss put forward the system  $S_2^1$ , which is a fragment of Bounded Arithmetic  $S_2$ : it was shown by him to be conservative over PV (Buss 1986: 109). I shall give some details of Buss's **Bounded Arithmetic**: a formula of Bounded

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anthropomorphic point of view in mathematics" (Parikh 1971: 495) and of S. Cook who claimed constructive mathematics as one of his main motivations, while referring precisely to Parikh (Cook 1975: 83). E. Nelson's **Predicative Arithmetic** also has an important philosophical dimension.

<sup>1</sup> For a description of  $I\Delta_0$  see (Wilkie & Paris 1987: 261).

<sup>2</sup> I should mention that Parikh's system was not strictly speaking "feasibly constructive" and his main theorem is "impredicative" in the sense of Nelson: Parikh has shown that for  $B$  a bounded formula such that  $\vdash_{PB} \exists x B$  there is some bounded term  $b$  not containing  $x$  and  $\vdash_{PB} \exists x ((x \leq b) \wedge B)$  (this is Parikh's Theorem 4.4 (Parikh 1971: 505).) But the bound on  $b$  requires time exponential in the length of the decimal notation of  $b$  to check all the possible values of the quantified variable (Cook 1975: 83) (Nelson 1986: 174).

Arithmetic is a formula of first-order logic which may contain the usual logical symbols, and the following non-logical ones:  $0, S, \cdot, +$ , with their usual meanings and

$x \# y$	$= 2^{ x  \cdot  y }$ , the so-called “smash” function
$ x $	$= \lceil \log_2(x+1) \rceil$ , the length of the binary representation of $x$
$\lfloor \frac{1}{2}x \rfloor$	divide by 2 and rundown, the “shift right” function <sup>1</sup>
$\leq$	less than or equal to

Bounded quantifiers are of the form:  $(\forall x \leq t)$  and  $(\exists x \leq t)$ , with  $t$  any term. There are also sharply bounded quantifiers:  $(\forall x \leq |t|)$  and  $(\exists x \leq |t|)$ . They correspond respectively to polynomially and logarithmically bounded quantifiers (Buss 1986: 2, 19-20). Buss established a hierarchy of bounded formulas analogous to the arithmetic hierarchy. Then he set up two systems,  $T_2^i$  and  $S_2^i$ , with a common base, a finite open fragment with the set of formulas with sharply bounded quantifiers:  $\Sigma_0^b = \Pi_0^b = \Delta_0^b$ ; and if  $A \in \Sigma_1^b$ , then:

$$(\forall x \leq r) \text{ is in } \Pi_{i+1}^b$$

$$(\forall x \leq |r|) \text{ and } (\exists x \leq |r|) \text{ are in } \Sigma_{i+1}^b$$

and dually (Buss 1986: 20) (Macintyre 1987: 53).

The system  $T_2^i$  is based on the open fragment with a conventional induction scheme for  $\Sigma_i^b$  formulas, and  $S_2^i$  with the scheme:

$$A(0) \wedge (\forall x [A(\lfloor \frac{1}{2}x \rfloor) \rightarrow A(x)] \rightarrow (\forall x A(x))$$

for  $A$  any  $\Sigma_i^b$  formula (Buss 1986: 3).

Both  $U_i T_2^i$  and  $U_i S_2^i$  are equivalent to  $I\Delta_0 + \Omega_1$  (Macintyre 1987: 53). Buss was mostly interested in connections with computational complexity (Stockmeyer 1977), his central theorem stating that any function which is  $\Sigma_i^b$ -definable in  $S_2^i$  is  $\text{PTC}(\Sigma_i^P)$  (PTC: polynomial time closure of Stockmeyer’s  $\Sigma_i^P$ ), and conversely. An interesting corollary states that  $S_2^1$ , with the famous “smash” function  $\#$  and its peculiar growth rate, “has as provably computable functions *exactly* the polynomial-time computable functions”

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<sup>1</sup>  $\lfloor x \rfloor$  and  $\lceil x \rceil$  denote respectively the greatest integer  $\leq x$ , and the least integer  $\geq x$ .

(Macintyre 1987: 53). The most obvious criticism of constructive logics such as Buss's is their logical weakness:

At present it seems we can prove nothing of interest in  $S_2^1$ . The axioms are perfect for computability, but give us no flexible constructions. (Macintyre 1986: 54)

The system  $IA_0 + \Omega_1$  was also studied by E. Nelson in his book **Predicative Arithmetic**. Nelson's work is close in spirit to the original idea of Bernays and the work of Parikh (Nelson 1986: 50, 178). He starts with a critique of the principle of induction of Peano Arithmetic, i.e.:

$$I(\phi): [\phi(0) \wedge \forall x (\phi(x) \rightarrow \phi(Sx))] \rightarrow \forall x \phi(x)$$

The truth of these induction axioms is usually carried by our recognition that a natural number is something that we can reach by constructing the series:

$$0, S0, SS0, SSS0, SSSS0, \dots$$

In a similar fashion to Esenin-Volpin, Nelson takes this justification at face value:

... numbers are symbolic constructions; a construction does not exist until it is made; when something new is made, it is something new and not a selection from a preexisting collection. There is no map of the world because the world is coming into being. (Nelson 1986: 2)

He therefore claims that this “predicative” (or “genetic”) construction of natural numbers entails that we can justify only those axioms  $I(\phi)$  where the property  $\phi(x)$  can be verified by reference only to the numbers constructed at the time “ $x$ ” appears. For example, if we take the formula  $\pi(x)$  as expressing the property:

“ $x$  is either even or odd”,

then the proof of “ $\forall x \pi(x)$ ” is predicatively correct because by the time “ $x$ ” is constructed we have a number “ $y$ ” such that “ $2 \cdot y = x$ ” or “ $2 \cdot y + 1 = x$ ”. But when a formula contains an unrestricted existential quantification, i.e. if we take the formula “ $\mu(x)$ ” as expressing:

“there exists a non-zero number divisible by every number between 1 and  $x$ ”,

then no predicative justification is possible of “ $\forall x \mu(x)$ ” (Nelson 1986: 1) (Wilkie 1990: 327-328).<sup>1</sup>

Nelson chose as his basic system R. Robinson’s theory  $Q$  which is the weakest system of any arithmetical interest (Robinson 1952). It consists in the usual axioms of Peano Arithmetic for 0,  $S$ ,  $+$ ,  $\cdot$ , without the usual induction postulate, but with the following formula: <sup>2</sup>

$$(x \neq 0) \rightarrow \exists y (Sy = x)$$

Nelson proved the startling fact that  $IA_0 + \Omega_1$  is interpretable in  $Q$ ,<sup>3</sup> and then developed increasingly stronger theories interpretable in  $Q$  (such as  $Q_4$ .) He then sharpened his critique of classical mathematics by showing that  $IA_0 + \exp$  is *not interpretable in  $Q$*  (Nelson 1986: chap. 31), an indication, according to him, that such a system (and stronger ones like Peano Arithmetic) could be inconsistent. On the other hand he has shown that his own system of Predicative Arithmetic proves that there is no short proofs of contradiction in  $Q$  (by Gödel’s second incompleteness theorem it cannot prove the consistency of  $Q$ , since it is interpretable in it). *From his point of view, adding exponentiation, i.e.  $\forall n \mathcal{E}(n)$ , to the symbols of arithmetic leads to impredicative finitary reasoning, and renders the consistency of the theory doubtful:*

The principal objection to adjoining  $\forall n \mathcal{E}(n)$  is that the consistency of the theory is doubtful. One can give a proof of its inductive consistency assuming that superexponentiation is total, or of its full formal consistency assuming that supersuperexponentiation is total. But to prove the consistency of the theories with these additional assumptions, one needs further assumptions yet. It is as if an attorney were to attempt to establish the reliability of a client by bringing in a character witness, and then a

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<sup>1</sup> A similar argument is to be found in (Parikh 1971: 502-3).

<sup>2</sup> That the existential quantifier is not really unbounded is easily shown if we introduce “ $<$ ”:

$$(x \neq 0) \rightarrow \exists y < x (Sy = x)$$

Moreover, if there is any worry about the use of the quantifier, it can be eliminated by introducing a new symbol for subtraction, “ $\dot{-}$ ”. The formula would then be rewritten as:

$$(x \neq 0) \rightarrow S(x \dot{-} 1) = x$$

<sup>3</sup>  $IA_0 + \Omega_1$  is certainly more powerful than  $Q$ , because more theorems can be proved in it than are provable in  $Q$ . But on the other hand the consistency of  $Q$  is not provable in  $IA_0 + \Omega_1$ .



character witness to the character witness, and so forth, each one more mafioso than the predecessor. Impredicative finitary reasoning is a residue of Platonism that has been uncritically accepted by the finitists. (Nelson 1986: 177)

Nelson claims to be a nominalist in matters of philosophy, and the “genetic” point of view he adopts is akin to strict finitism in the sense of Esenin-Volpin. His distinction between the genetic and the formal is the same as the distinction between predicative and impredicative (Nelson 1986: 79). In typical strict finitist fashion, Nelson argues that “Perhaps there is no such number as  $80^{5000}$ ” (Nelson 1986:50) or about  $2^{65536}$ , i.e.  $2 \uparrow 5$  in superexponentiation, that “there is not a scintilla of evidence that it stands for a genetic number” (Nelson 1986:75)). Indeed, if we think of numbers as being of the form:

0, S0, SS0, SSS0, SSSS0, ...

Nelson then points out that:

... if one produces occurrences of S at the rate of one every  $10^{-24}$  seconds, which is about the time it takes light to traverse the diameter of a proton, and if the age of the universe is taken to be twenty billion years, then it will take more than  $10^{19684}$  ages of the universe before  $2 \uparrow 5$  occurrences of S have been produced (and by the same token, what genetic meaning can  $10^{19684}$  have?) (Nelson 1986: 75)

From these passages, we can see that Nelson’s rhetoric is strict finitist in essence.

Nelson’s results are quite impressive: it is an unsuspected mathematical fact that Bounded Arithmetic is suitable for developing interesting parts of number theory; and that it is interpretable in Q (or rather in  $Q_4$ , which contains Buss’s smash function #.) Both facts combined show that large parts of mathematics can be done in the weakest possible system. This fact certainly contributes to deflate the idea that strong systems such as Peano Arithmetic or any stronger set theory are needed. However, there is a problem with Nelson’s programme: his “predicativism” allows him induction only for bounded formulas, but he allows unbounded quantifiers notably in proofs of interpretation in  $Q$ .<sup>1</sup>

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<sup>1</sup> This criticism was made in (Macintyre 1987: 54), (Pudlák 1988: 989) and (Wilkie 1990: 330). This situation manifests itself in the fact that, to take Wilkie’s example, the statement “ $\forall x \exists y (2^x = y)$ ” is not predicatively derivable, while the statement “ $\forall x \exists p (p \text{ is a predicative proof of } “\exists y (2^x = y)”)$ ” is predicatively derivable (Wilkie 1987: 330).

There is no explication for this discrepancy between the arithmetic and the logic, which undermines the philosophical aspect of the project.

On the philosophical side, Nelson's nominalism is clearly nothing more than a form of strict finitism. But, notwithstanding the fact that it leaves him with a serious unanswered problem with his programme (i.e. unexplainable unbounded quantification in the interpretation), it can be easily dissociated from the actual value of his results for understanding of exponentiation, and weak systems in general. One must not forget that systems such as  $I\Delta_0 + \Omega_1$  came to be studied, by Buss for example, because of their connexion with polynomial-time computability, not for strict finitist reasons.

Nelson's nominalism has a more interesting aspect. It broadly amounts to the rejection of the semantic view of mathematics, in a formalist fashion: "...the subject matter of mathematics is the expressions themselves together with the rules for manipulating them—nothing more" (Nelson 1986: 173). Someone adopting a semantical viewpoint would consider a mathematical expression in decimal or stroke notation as denoting the same abstract objects—the former notation being only more practical. But, for Nelson "the invention of positional notation was the creation of a new kind of number" (Nelson 1986: 173). This is strangely similar to Wittgenstein's idea that the introduction of a new notation amounts to the introduction of new numbers (RFM: III, § 12, 51) and in particular, introducing exponentiation too means introducing new numbers, via the introduction of a new technique (RFM : III, § 47).<sup>1</sup>

Certainly with a relativization scheme one can still continue to consider numbers as being

0, S0, SS0, SSS0, SSSS0, ...

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<sup>1</sup> I shall discuss this topic again at the end of section 16.

but only to a certain extent, as Nelson would claim. It is possible to introduce new number systems such as the binary or the decimal system, i.e. to put it in a simple fashion, we can go this way:

$$I \ II \ III \ II \ III \ \dots \rightarrow 0 \ 1 \ 01 \ 11 \ \dots \rightarrow 0 \ 1 \ 2 \ 3 \ 4 \ \dots$$

But there is no realistic way to reinterpret results back in the stroke system:

$$I \ II \ III \ II \ III \ \dots \leftarrow 0 \ 1 \ 01 \ 11 \ \dots \leftarrow 0 \ 1 \ 2 \ 3 \ 4 \ \dots$$

This argument implies the rejection of the isomorphism of the natural number series. It is similar to Esenin-Volpin's. There is therefore one way in which Wittgenstein might be said to be a strict finitist. But, as I shall indicate in a moment, such a claim is perfectly acceptable to non-strict finitists.

This brief survey was intended to give the reader a good idea of the principal features of most strict finitist programmes and of their philosophical motivation. *The central philosophical theme is obviously "feasibility". In the traditional approach, originating with Bernays, the epistemological argument about the practical limitations of humans to carry on operations plays a central role. Humans are conceived as finite beings who cannot apprehend the infinite*, in contrast for example with the tradition of Bolzano, Cantor, Dedekind and Zermelo. Moreover, for a strict finitist not only the actual infinite does not exist but there is no difference between the large finite and the potential infinite, since it is practically impossible for us to reach some large finite numbers.

Although more recent logicians such as Parikh, Cook and Nelson often revert to the traditional strict finitist rhetoric found in the writings of Bernays, Borel and Esenin-Volpin, the development of a Bounded Arithmetic with bounds on quantifiers and of connections with structural complexity and polynomial-time computability has brought a more precise definition of "feasibility". *It has given a new life and respectability to the topic*. It would be wrong to go farther and speculate on what Wittgenstein would have thought of it had he lived long enough. The very least I can claim for the moment is that

there is no similarity between Wittgenstein's remarks and the strict finitist rhetoric about our human limitations. I shall substantiate this claim in the next sections.

The most obvious point of contact between Wittgenstein's remarks on mathematics and strict finitist programmes is the rejection of the isomorphism of the natural number series, i.e. the idea that the introduction of a new notation amounts to the introduction of new numbers. But it is important to dissociate this claim from the strict finitist rhetoric. Indeed, *this claim is just the expression of the rejection of the semantical viewpoint*, by both Nelson and Wittgenstein, and this rejection does not imply any strict finitist restriction on the size of any extension. It is perfectly compatible with a more "open-ended" kind of finitism, to which Wittgenstein might be said to adhere.<sup>1</sup>

Strict finitism must not be confused with finitism as such. In "Constructive Reasoning" (Tait 1968) and "Finitism" (Tait 1981), W. W. Tait has argued that "finitist reasoning is essentially primitive recursive reasoning in the sense of Skolem" (Tait 1981: 524). This form of reasoning is quite different from strict finitism reasoning with its supplementary restriction to the domain of the "feasible". Although the position Tait adopted has been criticized,<sup>2</sup> it is commonly accepted, and I shall take it for granted. In

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<sup>1</sup> Nelson should not be classified as a strict finitist *à la* Esenin-Volpin. For example, the notion of polynomial-time computability, which associates to the size of the input a polynomial, say " $x^{\log x}$ ", to calculate the time —i.e. the number of step taken by a Turing machine— to obtain the result, does not fix an absolute upper limit such as  $10^{12}$ : it is only a possibility "in principle" of computation, since the input might be so large that the computation is at any rate impossible. Secondly, Nelson uses the tools of A. Robinson's non-standard analysis, thus preserving the constructive core of the theory while he is able to obtain further non-trivial classical results (often in a more elegant way) and even to uncover new mathematical phenomena. The interest of E. Nelson's **Predicative Arithmetic** lies in extending it to  $Q^*$ , which contains  $Q_4$  and  $\#$  and a unary predicate  $\phi$  with the axioms saying that  $\phi$  respects each bounded function symbol and bounded nonlogical axiom. It also contains the constant  $N$  with the axioms  $\epsilon(2 \uparrow N)$ ,  $\epsilon(2 \uparrow 2 \uparrow N)$ , etc... and the axiom  $\sim\phi(N)$  (Nelson 1986: 179).  $Q^*$  remains interpretable in  $Q$ , and it could be shown to be consistent. Now,  $\phi(n)$  is similar to "n is standard" in Nelson's own version of nonstandard analysis, i.e. his internal set theory (Nelson 1977). The latter's axioms can be found in  $Q^*$ . For example, the axiom  $\sim\phi(N)$  is similar to the idealization principle. Nelson is grafting the methods of A. Robinson's non-standard analysis onto R. Robinson's  $Q$ . With such an approach, Nelson was able to reconstruct major results in probability theory (Nelson 1987). I should also mention that in "Locally Finite Theories", J. Mycielski also developed a locally finite (i.e. every finite part of which has a finite model) theory to which he added new numbers which play "the role of infinite integers in a new, finitistic, nonstandard analysis" (Mycielski 1986: 62).

<sup>2</sup> In particular by G. Kreisel. See (Kreisel 1958b, 1970). (Tait 1981) is a reply to Kreisel's criticisms.

section 13, I shall give my reasons to think that Wittgenstein was closer to the finitism of Skolem (Skolem 1923) than any other foundational viewpoint.

It is now time to turn our attention towards Wittgenstein's later philosophy of mathematics in order to see if there is any links between its concepts and the strict finitist rhetoric. In the next sections (10 to 12) I shall turn to Wittgenstein's conception of the infinite. It should come out from this forthcoming discussion that Wittgenstein's approach is consciously non-epistemological, and that in fact it is lacking the very basis out of which traditional strict finitist doubts (about our ability in principle to carry on procedures) grow. But, I shall indicate in section 13 that Wittgenstein's reading of the quantifiers is nevertheless different from that of the intuitionist. In sections 15 and 16, I shall discuss the notion of surveyability in RFM. Again, I shall dissociate Wittgenstein's requirement that proof be surveyable from the strict finitist's emphasis on "feasibility". I shall conclude by a discussion of M. Dummett's interpretation of Wittgenstein as a strict finitist. I hope to show conclusively that this is not the proper interpretation of Wittgenstein's conception of mathematics.

## 10. The Legacy of Aristotle and Frege

In the second volume of the *Grundgesetze der Arithmetik*, one of the main arguments of G. Frege against the late nineteenth century formalists such as J. Thomae, E. Heine or even H. Hankel was about their conception of the infinite. For such formalists, mathematics was about meaningless signs, i.e. marks of ink on paper, and accordingly an infinite series was seen by them as what we can concretely write down. But according to the rules adopted, there would be no last term to that series. This leads to a strange situation: the series is nothing more than what we can write down and we cannot

humanly write down an infinite series. It is then a possibility for God to realize. Frege saw the problem exactly in those terms:

Does the possibility exist? For an almighty God, yes; for a human being, no. (GA2: §125)

Indeed, there is no more sense in talking about an infinite row of houses than in talking about an infinite series of numbers conceived in the manner of the formalists: we cannot admit of a row of houses that it is endless, neither can we admit of such a series of numbers that it is infinite. The formalist conception run into absurdities (GA2: §127).

These difficulties disappear when one adopts the view that in mathematics signs stand for a reality: one can then refer to the infinite with finite signs, provided the proper definitions are adopted. Frege doesn't need to talk about "possibility". The number of the set of all finite cardinals has an identical status to that of any finite cardinal, since it is defined according to the same model (FA: § 84).<sup>1</sup>

Frege's strategy could be summed up as follows: either we have dead signs (marks of ink on paper), or we recognize that the signs denote an independent and preexisting reality. Frege argued that the formalist view leads to absurdities; thus proving by *reductio* the rightness of the idea of an "arithmetic with content" (*inhaltlich*) (GA2: §128). In obvious reference to these criticisms by Frege, Wittgenstein pointed out in his Viennese conversations with M. Schlick and F. Waismann that there is a third possibility: <sup>2</sup>

For Frege the alternative was this: either we deal with strokes of ink on the paper or these strokes of ink are signs *of something* and their meaning is what they go proxy for. The game of chess itself shows that these alternatives are wrongly conceived—although it is not the wooden chessmen we are dealing with, these figures don't go proxy for anything, they have no meaning in Frege's sense. There is still a third possibility, the signs can be used the way they are in a game. (WWK: 105)

Wittgenstein's strategy is clear: While *agreeing with Frege's criticisms of crude formalism*, he wants to show that we don't need to adopt Frege's "arithmetic with

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<sup>1</sup> See section 4 for more details about Frege's line of thought.

<sup>2</sup> It isn't really fair to say here that Frege didn't see the possibility! See (GA2: § 96).

*content*". Obviously, for Wittgenstein, as for Frege, a potential infinite doesn't make sense if it is conceived as a possibility for God, not for humans. But, according to Wittgenstein, Frege has no right to conclude from this that the infinite has nothing to do with the "possible", that is not "potential", but in a certain sense "actual". There are two senses of the "possible", i.e. empirical and grammatical, and Frege confused them, as we shall see.

Wittgenstein pointed out in **PR** the danger of thinking of the "possible" as becoming "actual", especially in connexion with a Platonist conception of mathematics as timeless—a mistaken view adopted by Frege: <sup>1</sup>

The word "possibility" is of course misleading, since someone will say, let what is possible now become actual. And in thinking this, we always think of a temporal process and infer from the fact that mathematics has nothing to do with time, that in its case possibility is (already) actuality. (PR: §141; also PG: 471)

Both Frege and the formalists he criticizes make the same mistake. They both admit that with our finite means, we cannot grasp the infinite. The only difference is that the formalist needs an almighty God to write down what he as a man cannot write down; while Frege claims that there exists an infinite, and we can refer to it with finite means. Coupled with the view that mathematics are timeless, his approach is tantamount to seeing the infinite in mathematics as "actual". The almighty God of the formalists and the actual infinite have the same role: to help us, finite beings, reach the infinite.

For Wittgenstein, to give any status of "reality" to what we call "possible" is nonsense: "It is one of the most deep rooted mistakes of philosophy to see possibility as a shadow of reality" (PG: 283). But on the other hand "there is a danger of falling into a positivism" (PG: 283) consisting in admitting, as the formalists do, only what is actually given.

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<sup>1</sup> Frege would say: "The laws of number, however, are timeless and eternal" (PW: 237). Cantor also held the view that the laws of numbers are eternal. In a letter to C. Hermite he wrote: "... natural numbers, considered separately or in their actual infinite totality, exist to the highest degree of reality as eternal thoughts of the divine intellect." (Meschkowski 1967: 262) \*

In order to understand more clearly Wittgenstein's peculiar notion of "possible", it is useful to look at his discussion of a machine (PI: §193-4; RFM: I, §120-5; IV, § 49; VII, § 72). His main question was: What does it mean that by inspecting a machine, or its plans, we know the possibility of its movements? If we consider the "machine-as-symbol" in contrast with the "actual machine", the possibility of movement appears as a shadow of the movement itself:

... it may look as if the way it moves must be contained in the machine-as-symbol far more determinately than in the actual machine. As if it were not enough for the movements in question to be empirically determined in advance, but they had to be really—in a mysterious sense—already present. (PI: §193)

What this attitude shows is that we are getting confused by the way we talk of machines. To dissolve the mystery one has to understand that this possibility precedes experience and defines what we consider as a movement of the machine. It is not that the possibility of movement is something almost realized: it is in comparison with what we call the movement of the machine that we can observe and discuss the movement of the "actual machine". The "machine-as-symbol" is not an abstraction from the "actual machine"; it is a norm for judging the movements of the "actual machine".

If we confuse this norm with an empirical possibility, then the tendency is to attribute to it a special kind of reality; i.e. it looks as if it is the shadow of a reality. We can therefore talk of a "grammatical possibility",<sup>1</sup> for which we do not suppose a corresponding reality because it belongs not to what we are talking about but to our way of talking. Therefore, Wittgenstein distinguished between "empirical" (or physical) and "grammatical" (or logical) possibility,<sup>2</sup> the mathematical infinite being of the latter kind. Here one must try, according to Wittgenstein, to avoid describing the possibility of continuing an infinite series as a "kind of shadowy reality":

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<sup>1</sup> Wittgenstein seems to have first used the expression "grammatical possibility" as early as 1930 (WWK: 104).

<sup>2</sup> See (PG: 452) for example.



Of course the natural numbers have only been written down up to a certain highest point, let's say  $10^{10}$ . Now what constitutes the *possibility* of writing down numbers that have not yet been written down? How odd is this feeling that they are all somewhere already in existence! (Frege said that before it was drawn a construction line was in a certain sense already there.)

The difficulty here is to fight off the thought that possibility is a kind of shadowy reality. (PG: 281) <sup>1</sup>

It is worth noticing that a quite similar distinction is to be found in Aristotle's discussion of the infinite in the **Physics**:

We must not take 'potentially' here in the same way as that in which, if it is possible for this to be a statue, it actually will be a statue, and suppose that there is an infinite which will be in actual operation. (**Physics**, III, vi, 206a 18-21)

In this passage Aristotle is warning us not to confuse the kind of potentiality associated with the infinite with an empirical potentiality (the bronze and the statue).<sup>2</sup> This is, according to Wittgenstein, how one should understand the (grammatical) infinite: an expression ending with the words "and so on" doesn't point towards a possibility waiting to be realized (an empirical possibility) but shows *a possibility of the symbolism*, i.e. the formalism doesn't forbid us to continue:

To say that a technique is unlimited does *not* mean that it goes on without ever stopping—that it increases immeasurably; but that it lacks the institution of the end, that it is not finished off. (RFM: II, § 45) <sup>3</sup>

or:

"We won't bother about an end"

It might also be said: "for us the series is infinite". (RFM: V, § 14)

and:

But now it seems as if this involved *denying the existence* of something in logic: perhaps generality itself, or what the dots indicate; whatever is incomplete (loose, capable of further extension) in number series. And of course we may not and cannot deny the existence of anything. So how

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<sup>1</sup> In this passage Wittgenstein was referring to a remark by Frege in **Grundgesetze der Arithmetik** (GA1: 88). See also (PG: 355-356). For a discussion of Wittgenstein's remarks in connection with the notion of numerical equality, see section 8.

<sup>2</sup> F. Schmitz is mistaken in thinking that Wittgenstein is opposed to Aristotle on that point (Schmitz 1988 :106).

<sup>3</sup> See also (RFM: II, § 26-7; V, §15 , 19).

does this indeterminacy find expression? Roughly thus: if we introduce numbers substitutable for a variable  $a$ , we don't say of any of them that it is the last, or the highest.

...  
Of an end to the possibility, I cannot speak at all. (PG: 281-282) <sup>1</sup>

More precisely, the "possibility of the symbolism" is the indication *in the symbolism* that one must continue. Wittgenstein expressed this in his typical fashion by saying that the symbol  $|0, \xi, \xi+1|$  is an "arrow",

with the "0" as its tail and the " $\xi+1$ " as its tip. It is possible to speak of things which lie in direction of the arrow, but misleading or absurd to speak of all possible positions for things lying in the direction of the arrow as an equivalent for the arrow itself. (PG: 467)

This conception is close to Aristotle's notion of *Apeiron*. Aristotle was also denouncing conceptual confusions in a similar manner:

It turns out that the infinite is the opposite of what people say it is: it is not that of which no part is outside, but that of which some part is always outside ... So, that is infinite, of which it is always possible to take some part outside, when we take according to quantity. But that of which no part is outside, is complete and whole: that is how we define 'whole', as meaning that of which no part is absent—e.g. a whole man or a whole box. And as what is whole in a particular case, so is that which is whole in the primary sense: [it is that] of which no part is outside. (That outside which absence is, is not all, whatever may be absent.) ('Whole' and 'complete' are either exactly the same or very close in nature. Nothing is complete unless it has an end, and an end is a limit.) (*Physics*, III, vi, 206b 33-207a 15)

Aristotle is claiming that there is no whole out of which there is nothing. What is complete has an end (*telos*), and the end is a limit (*peras*); and the infinite is incomplete, has no end, therefore no limit; as its name says: *Apeiron*, or "absence of limit". According to Aristotle, as for Wittgenstein, the infinite is what is lacking the "institution of an end." The "absence of limit" (*Apeiron*) may be regarded as the open possibility of more:

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<sup>1</sup> For earlier statements, see also (M: 299); or (PR: § 123): "... the endless path doesn't have an end 'infinitely far away', it has no end." The conflict of this potentialist viewpoint with Cantor's  $\omega$  should be obvious to anyone. Wittgenstein would disagree very much with Russell's explanation in *Introduction to Mathematical Philosophy*: "The cardinal number  $\aleph_0$  is the limit (in order of magnitude of the cardinal numbers 1, 2, 3, ...  $n$ , ..., ... What makes  $\aleph_0$  the limit of the finite numbers is the fact that, in the series, it comes immediately after them, ..." (Russell 1919: 97).

In general, the infinite is in virtue of one thing's constantly being taken from another—each thing taken is finite, but it is always one followed by another; ... (Physics, III, vi, 206a 27-8).

The point of this comparison with Aristotle was only to clarify the similarities: *Wittgenstein's conception of the infinite is clearly in the Aristotelian tradition*, as opposed to the consciously anti-Aristotelian tradition of the actual infinite, of Bolzano, Cantor and Russell.<sup>1</sup>

The infinite was conceived by Wittgenstein as a “qualification of the concept “possible”” (WWK: 229). An infinite possibility is expressed by a law of construction, not by a proposition asserting it: <sup>2</sup>

Infinity is the property of a law, not of its extension. (LWL: 13)

Wittgenstein also said, in the same spirit:

The infinite number series is only the infinite possibility of finite series of numbers. It is senseless to speak of the *whole* infinite number series, as if it, too, were an extension.

Infinite possibility is represented by infinite possibility. The signs themselves only contain the possibility and not the reality of their repetition. (PR: § 144)

Again, the same point is made by Wittgenstein in the following passage taken from a short manuscript written in 1931:

If we wish to say infinity is an attribute of possibility, not of reality, or: the word “infinite” always goes with the word “possible”, and the like—then this amounts to saying: the word “infinite” is always part of a *rule*. (PR: App. I, 313)

Let us take Wittgenstein's example—which was also Aristotle's, as A. Moore pointed out (Moore 1990: 138)— of the infinite divisibility of a given distance: to say that “this distance can be divided in two parts” means that the statement “the distance is

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<sup>1</sup> The potentialist conception of the infinite was adopted by constructivists of all tendencies, including more finitist mathematicians such as Wittgenstein's student R. L. Goodstein. What he had to say about the infinite is very similar to what Aristotle and Wittgenstein said: “To say that the system of number signs is unlimited, or infinite, says only that no limit to the application of the number sign generating the process is specified. The *possibility* of constructing number signs is unlimited, but what is constructed will always terminate” (Goodstein 1951: 20).

<sup>2</sup> One easily sees here that this conception is reminiscent of the saying-showing distinction in the TLP.

divided in two” has a sense. But to say “the distance is infinitely divisible” does not mean that the statement “the distance is divided in an infinite number of parts” has a sense. Such a proposition has no sense (there is no infinite verification (WWK: 247).) It means that here “the number word “two” can be replaced by any arbitrary word” (WWK: 229-30), because nothing forbids us to continue (see also (RFM: II, §45)).

From the idea that infinity is to be construed as a possibility in the rules follows the idea that the infinite is not a number (see (WWK: 188, 226, 228), (PR: § 138, 142) and (LFM: 32, 255)). The illusion that it is a number comes from our everyday speech where both numbers and the infinite are given as answers to the question: How many? (PR: §172). As a corollary, there is the further illusion that the infinite is to be compared with a finite quantity, and then considered as a quantity, as an enormous one; while “it isn’t itself a quantity” ((PR: § 138), see also (WWK: 187), (RFM: V, §19) (LFM: 141-2)):

Where the nonsense starts is with our habit of thinking of a large number as closer to infinity than a small one. As I’ve said, the infinite doesn’t rival the finite. The infinite is that whose essence is to exclude nothing finite. (PR: § 138)

It is fitting to remark here that this is also a typically intuitionistic argument. Wittgenstein certainly knew the finitist implications:

If one were to justify a finitist position in mathematics, one should say just that in mathematics “infinite” does not mean anything huge. To say “There’s nothing infinite” is in a sense nonsensical and ridiculous. But it *does* make sense to say we are not talking of anything huge here. (LFM: 255)

It should therefore be clear to anyone that there is a “logical” or “grammatical” difference between an infinite series and a finite one (WWK: 203, 226, 228), not to be transgressed. Infinite series can only be obtained by their law of construction, and it is the law itself which should act as the “description” of the infinite series. Then the difference with the finite collections would be obvious:

An infinite class is from the start something completely different from a finite class. (WWK: 70)

Confusion is created by the Russellian symbolism, which does not make clear from the start, by the syntax, if a class is infinite or not (this will be discussed at the end of section 13.) Here the “and so on” plays a crucial role, as it did in the TLP.

## 11. The Grammar of the “and so on”

I explained earlier (section 6) that in the TLP, the expression “and so on” was considered by Wittgenstein as an essential feature of the description of an infinite process such as the process by which one generates the natural numbers. In the words of P. Frasuolla, “the logic of the infinite is, for Wittgenstein, the logic of this “and so on”” (Frasuolla 1980a: 641).<sup>\*</sup> In a very coherent fashion, the expression “and so on” turns out to be as important in the late Wittgenstein’s grammatical conception of the infinite, which I shall now explore in more details.

As I just pointed out, Wittgenstein insisted upon seeing infinity as the property of a law, or rule, but not as the property of its extension —i.e. infinity is a property of the rule for generating the natural numbers, not of the (completed) set of all natural numbers. Infinity is only the possibility inscribed in the rule of continuing indefinitely to apply it (i.e. the rule doesn’t forbid us to stop). This is precisely what is expressed by the use of the words “and so on”.<sup>1</sup> But if this is the role played by the expression “and so on”, it becomes of the utmost importance that *it should not be understood as an abbreviation*. Because otherwise, this would mean that it stands for an already given or completed infinite extension. This is, I think, the most important point in Wittgenstein’s grammar of the “and so on.”

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<sup>1</sup> F. Kaufmann expressed similar views, in rather clear terms: “When we operate with the expression ‘and so on’, as we do in describing infinite constructions, the stress must therefore lie on the word ‘so’; and that is determined by a law” (Kaufmann 1930: 136).

The reader will find frequent allusions to this point in Wittgenstein's later writings. I shall discuss here two examples. The first one is in **RFM**, and concerns the natural numbers:

"How does one count in the decimal system?"—"we write 2 after 1, 3 after 2, ....14 after 13 ....124 after 123 *and so on*"—That is an explanation for someone who, while there is indeed something he doesn't know, does understand "and so on". And understanding it means not understanding it as an abbreviation: it does *not* mean that he now sees a much longer series in his mind than that of my examples. That he understands it comes out of his now making certain applications, in his saying *this* and acting *so* in particular cases. (RFM: VII, § 27)

This quotation clearly indicates that Wittgenstein took the "and so on" to mean, when properly understood, that one can continue doing the same, i.e. apply the rule, indefinitely. It is not an abbreviation for an infinite series, consisting of an infinite number of applications of the rule already made, which it would then only describe.

One will find the same idea in a passage from the **PG**, where Wittgenstein discussed recurring decimals such as in  $\frac{1}{3}$ :

We regard the periodicity of a fraction, e.g. of  $\frac{1}{3}$  as consisting in the fact that something called the extension of the infinite decimal contains only threes; we regard the fact that in this division the remainder is the same as the dividend as a mere *symptom* of this property of the infinite extension. Or else we correct this view by saying that it isn't an infinite extension that has this property, but an infinite series of finite extensions; and this is *of this* that the property of the division is a symptom. We may then say: the extension taken to one term is 0.3, to two terms 0.33, to three terms 0.333 and so on. That is a *rule* and the "and so on" refers to the regularity; the rule might also be written "l 0.3, 0.ξ, 0.ξ3 l" (PG: 428)

Then Wittgenstein went on pointing out that the law showing that  $\frac{1}{3}$  is 0.3 recurring shows that the regularity is 3, not that there is regularity as opposed to irregularity. The rule "l 0.3, 0.ξ, 0.ξ3 l", or as later in this passage the sign " $\dot{0}.3$ ", is not describing the infinite extension as containing (regularly) threes, but the fact that each new step of the division will produce a 3. One should not confuse this with the infinite extension. The confusion appears when one understands the "and so on", or here the sign " $\dot{0}.3$ ", as an abbreviation. That is why Wittgenstein added:

When I said ‘the “and so on” refers to the regularity’ I was distinguishing it from the “and so on” in “he read all the letters of the alphabet: a, b, c and so on”. When I say “the extensions of  $1/3$  are 0.3, 0.33, 0.333 and so on” I give three *three* extensions and a rule. That is the only thing that is infinite....

One can say of the sign “ $0.\dot{3}$ ” that it is not an abbreviation. And the sign “| 0.3, 0.ξ, 0.ξ3 |” isn’t a substitute for an extension, but the undervalued sign itself; and “ $0.\dot{3}$ ” does just as well. It should give us food for thought, that a sign like “ $0.\dot{3}$ ” is *enough* to do what we need. It isn’t a mere substitute in the calculus there are no substitutes. (PG: 428-429)

Wittgenstein expressed the same idea even earlier, in his Cambridge lectures (Academic Year 1931-32):

If one is divided by three there is no such thing as an infinite series of threes. There is a law that one divided by three is 0.3 recurring. We confuse the infinite possibility of writing threes with threes written down. (LWL: 108)

Of course, the “and so on” could be understood as an abbreviation, but only in the case of finite series, as in the case of reading the letters of the alphabet. In the case of finite sequences, Wittgenstein said that employment of “and so on” or the dots “...” would amount to “laziness”. He called these the “dots of laziness” (M: 298) (AWL: 6). But, again, Wittgenstein would then insist that in the case of infinite series the “and so on” has a completely different grammar and should not be understood as an abbreviation—for an infinite extension. In his early Cambridge lectures Wittgenstein made the claim that the “and so on” “*can* have a strict and exact grammar” (LWL: 89), distinguishing between the use for finite and infinite series, using the same examples:

a,b,c,---,and so on. Here --- or “and so on” stands for the rest of the alphabet, a definite number of letters.

This is quite different from  $\frac{1}{3} = 0.33\text{---}$ and so on. Here there is no definite number of digits, nor could there be for some superior being. The two examples have different grammars and rules. 0.33---is not a makeshift: it has an exact grammar. (LWL: 90)

In the 1939 lectures on the foundations of mathematics, Wittgenstein was still making the same point:

There are two ways of using the expression “and so on” If I say, “The alphabet is A, B, C, D, and so on”, then “and so on” is an abbreviation. But if I say, the cardinals are 1, 2, 3, 4, and so on”, then it is not. — Hardy speaks as though it were always an abbreviation. As if a superman would write a huge series on a huge board—which is alright, but as nothing to do with the series of cardinals. (LFM: 255)<sup>1</sup>

The distinction between the different grammars of the “and so on” in the infinite and the finite cases is linked with Wittgenstein’s remarks on quantification which will be discussed in section 13.

In discussing the possibility of indefinitely applying a rule, there is a tendency to speak in terms of our human abilities to do so. This is typical of the strict finitist rhetoric (see section 9.) It is usually said that humans are beings of finite powers. Therefore they cannot complete an infinite process, but merely apply a rule for a finite number of steps. Platonists, such as Russell, would retort that these human limitations are only “medical” impossibilities,<sup>2</sup> and that we can, in turn, “logically” conceive completed infinite processes.<sup>3</sup> The dispute between the strict finitist and the Platonist therefore centres around what I shall call the *epistemological problem* of our “medical”, or “practical” limitations.

In Wittgenstein’s jargon, this epistemological problem is of the domain of “empirical” possibility, as opposed to what he called “grammatical” possibility. Therefore, Wittgenstein’s characterization of an infinite process as a possibility of the symbolism, as “grammatical”, is designed precisely to avoid arguing at the level of epistemology. Wittgenstein did not deny that there are practical limitations to our ability to

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<sup>1</sup> In (RFM: V, § 19), Wittgenstein also spoke of the “and so on” as not being an abbreviation for “some gigantic extension.”

<sup>2</sup> As far as I know, the expression “medically impossible” comes from Russell, in an article to which Wittgenstein sometimes refer in RFM: “The Limits of Empiricism” (Russell 1936: 143).

<sup>3</sup> Russell, for example, would argue in the following way: “But is it *logically* impossible that there should be an omniscient Deity? And if there is such a Deity, may he not reveal the answer to a mathematical Moses? And would not this be a demonstration? It seems to follow that, if a form of words *p* is syntactically correct, we always “know what is meant by the statement that *p* is demonstrated”. If revelation is rejected as demonstration, it will be found that we do not know of the existence of Cape Horn unless we have seen it” (Russell 1936: 143–4). Russell’s argument shows underlying confusions...



carry out a procedure. It would be foolish to do so. But he deemed these “empirical” limitations inessential:

The rules for a number-system—say, the decimal system—contain everything that is infinite about the numbers. That, e.g. *these rules* set no limits on the left or right hand to the numerals; *this* is what contains the expression of infinity. Someone might perhaps say: True, but the numerals are still limited by their use and by writing materials and other factors. That is so, but that isn’t expressed in the *rules* for their use, and it is only in these that their real essence is expressed. (PR: § 141)

This passage contains an important argument against those who interpret Wittgenstein as a strict finitist: *Wittgenstein certainly admits that there are limitations in our ability to write down numbers, but to regard these limitations as essential, as the strict finitist would, is, according to him, to miss the point that these are not expressed by the rules and that only what is expressed by the rules is the essence.* It is precisely because the “and so on” is not to be understood as an abbreviation that we should not speak of an “empirical” possibility or impossibility—with, then, a possibility for a Demiurge, or God—in connection with infinite processes, such as calculating the decimal expansion of  $1/3$ , as in previous quotations from PG, or that of  $\pi$ , as in this passage taken from PI:

We should distinguish between the “and so on” which is, and the “and so on” which is not, an abbreviated notation. “And so on ad inf.” is *not* such an abbreviation. The fact that we cannot write down all the digits of  $\pi$  is not an human shortcoming, as mathematicians sometimes think. (PI: § 208)<sup>1</sup>

If impossibility there is, it is of a grammatical, or *conventional* nature:

... when one says “You can’t count through the whole series of cardinal numbers” one doesn’t state a fact about human frailty but about a convention which we have made. Our statement is not comparable, though always falsely compared, with such a one as “it is impossible for a human being to swim across the Atlantic”; but it *is* analogous to a statement like “there is no goal in an endurance race.” (BB: 54)<sup>2</sup>

<sup>1</sup> One should compare (LWL: 107). Here, the connection with Wittgenstein’s arguments against the Law of Excluded Middle is evident. I shall come back to this question later.

<sup>2</sup> Wittgenstein could be said to be a “conventionalist”. But his own brand of conventionalism must be distinguished from the “moderate conventionalism” of the Vienna Circle, according to which analytic truths are “true by convention”, and tautologies of logic are true *consequences* of the truth-table definitions (conventions) of the propositional connectives. In “Truth by Convention” (Quine 1935) and “Carnap and Logical Truth” (Quine 1954), W. V. O. Quine has criticized Carnap’s explanation of necessary truths with

Here Wittgenstein makes again the same point: that mathematical infinity has nothing to do with an “empirical” impossibility, i.e. that it is of grammatical, or conventional nature.

The epistemological problem of our human limitations is therefore deemed irrelevant to the understanding and definition of the concept of the infinite. *It is not merely irrelevant but dangerous*, because it gives rise to theories which may not be false, but may be misleading (it is important to keep in mind here the important passage from (PG: 469) quoted in section 1.) Wittgenstein described the possibility of such a confusion with the following example of an “infinite helix.” He called the following a “grammatical remark”:

Our normal mode of expression carries the seeds of confusion right into its foundations, because it uses the word “series” both in the sense of “extension”, and in the sense of “law”. The relationship of the two can be illustrated by a machine for making coiled springs, in which a wire is pushed through a *helically* shaped passage to make as many coils as desired. What is called an infinite helix need not be anything like a finite piece of wire, or something that that approaches the longer it becomes; it is the law of the helix, as it is embodied in the short passage. Hence the expression “infinite helix” or “infinite series” is misleading. (PG: 430)

Once the distinction between finite series and infinite process is clearly made, as with this example, it is easy to see that the strict finitist doubts can only occur if one is misled by the expression “infinite series”. *Wittgenstein is therefore trying to point at and avoid the confusions embodied in this expression, confusions from which arise strict finitist epistemological doubts.*<sup>1</sup>

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the help of the metalogical concept of “L-truth”, his conclusion being —broadly summarized— that the fact that conventions have consequences is itself a necessary truth which cannot, in turn, be explained as a consequence of linguistic conventions, on pain of an infinite regress. With Quine departing from the moderate conventionalism of Carnap in direction of a more pronounced empirism —see also (Quine 1951)— a possible alternative would be a “full-blooded” form of conventionalism, attributed by M. Dummett to Wittgenstein, and according to which the rules of grammar have no logical consequences, i.e. they are “always the *direct* expression of a linguistic convention” (Dummett 1959: 170). This is not the place for a critical discussion of M. Dummett’s exegesis. For such a critical discussion, see the second part of G. Baker’s *Wittgenstein, Frege and the Vienna Circle*, especially (Baker 1988: 259-266).

<sup>1</sup> For a similar reading of Wittgenstein, see (Shanker 1986: 126). P. Frasuolla defended the same point of view even earlier, showing that according to Wittgenstein this grammatical confusion is at the origin of the notion of an actual infinite, in the writings of Russell for example (Frasuolla 1980: 653).

If any more textual evidence is needed, I should point out that the rejection of the epistemological problem is also the gist of a passage from the notes of one of the 1939 lectures, previously discussed by G. P. Baker (Baker 1981: 68), when Wittgenstein asked his class: How many numerals have you learned to write down? To this question, A. M. Turing answered:  $\aleph_0$ , and Wittgenstein agreed (LFM: 31). Not that he was a great fan of higher cardinals (they are, as Kreisel would say, one of his “pet aversions” (Kreisel 1969: 102) and obviously from the context he seems to agree with Turing’s use of  $\aleph_0$  for rhetorical purposes only), but the point Wittgenstein was trying to make was precisely against strict finitist answers of the kind: “the number of numerals which I will ever write down.” For Wittgenstein, no matter how great the number of numerals one will ever write down, it is “irrelevant” (LFM: 32). The point, once again, is that the “essence” is in the rule itself:

The point is that the technique of learning  $\aleph_0$  numerals is different from the technique of learning 100, 000 numerals.  
Take the biggest numeral which has ever been mentioned. What is the difference between learning a technique of counting numerals up to that numeral and learning a technique which did not end at that numeral?  
Well, it might have been that one’s teachers said, “This series has no end.”  
(LFM: 31-2)

One has to look at the rule and what it says and not at its extension. The epistemological question of the practical limits to carry on a procedure is precisely not about what is said in the rule but about its extension.

In fact Wittgenstein’s rejection of the epistemological problem is so strong that he went as far to deem it as a problem of psychology:

The objection that ‘the finite cannot grasp the infinite’ is *really* directed against the idea of a psychological act of grasping or understanding.  
(RFM: V, § 6)

Wittgenstein even agreed with a remark from G. H. Hardy —although, of course, Wittgenstein disagreed strongly with Hardy’s Platonism— calling the epistemological problem of our human limitations a question of theology:

That ‘the finite cannot understand the infinite’ should surely be a theological and not a mathematical war-cry. (Hardy 1929: 5)

In Zettel Wittgenstein quoted this very passage from Hardy, and then added:

True, the expression is inept. But what people are using it to try and say is: “We mustn’t have any juggling! How comes this leap from the finite to the infinite?” Nor is the expression all that nonsensical—only the ‘finite’ that can’t conceive the infinite is not ‘man’ or ‘our understanding’ but the calculus. And *how* this conceives the infinite is well worth an investigation. (Z: § 273)

These last two comments should be seen more as the expression of his lack of patience with this misconception, than as providing cogent arguments against it. At any rate, they are again indications of Wittgenstein’s rejection of the epistemological problem. I have now adduced more than enough textual evidence showing that Wittgenstein conceived the finite and infinity as properties of rules, not of their extensions precisely in order to avoid a characterisation of the problem of the infinite in mathematics in epistemological terms, as the strict finitists do (and, with them, the Platonists.) It is therefore clear from our discussion that *Wittgenstein’s conception of the infinite bears no affinity with that of the strict finitist.*

Finally, I wish to point out the continuity of Wittgenstein’s thoughts on the infinite. Indeed, it is one of Wittgenstein’s oldest and most enduring ideas that propositions must represent a situation off their own bat:

In this way the proposition represent the situation—as it were off its own bat. (NB: 26)

This is one of the keys to a proper understanding of the TLP, but also of his later views on infinity. It is not that a proposition should describe an infinite fact, since this is impossible, but the infinite possibility is of another kind, still in the symbols themselves:

Whereas infinite—or better *unlimited*—divisibility doesn’t mean there’s a proposition describing a line divided into infinitely many parts, since there isn’t such a proposition. Therefore this possibility is not brought out by any reality of the signs, but by a possibility of a *different* kind in the signs themselves. (PR: § 139)

It is precisely this different kind of possibility that I tried to describe in the preceding two sections.

## 12. An Argument against Strict Finitism

In “Wang’s Paradox”, M. Dummett claimed that there is a danger for intuitionism in the fact that strict finitism renders its position unstable, in a strict finitist way (Dummett 1970: 249). Before him D. van Dantzig contended that if the intuitionist was consistent, he wouldn’t call  $10^{10^{10}}$  a finite number (van Dantzig 1955: 277). C. Wright described the argument in the form of a *Modus Tollens*:

... arguments essentially analogous to those which the mathematical intuitionists, at least when their case is presented in the way which Dummett has recommended, use to support their revisions of classical logic and mathematics lead to a yet more radical *strict finitist* outlook; this outlook, however, is incapable of issuing a coherent philosophy of mathematics; therefore there must be something amiss with the arguments which lead to it, and, by analogy, with the original intuitionistic arguments also. (Wright 1980b: 107)

Dummett’s strategy is to point to the vagueness of strict finitist predicates such as “intelligible”, when the strict finitist asks that a numeral be intelligible, or “surveyable” when he asks that a proof be surveyable. This vagueness renders them susceptible to the Sorite Paradox mentioned in section 9. For Dummett these expressions are for that reason semantically incoherent and strict finitism which admits them is vitiated. By way of contrast, Wright argued in his article “Strict Finitism” *for* strict finitism by showing that it is impossible for the intuitionist to draw the line “where he does draw it, and for refusing to travel on down the constructivist road in company of the strict finitist” and by trying to “beat off” Dummett’s objection of incoherence (Wright 1980b: 109). In any event, I would like to propose another strategy to replace Dummett’s. I am not interested in disputing the coherence of strict finitist notions. I wish to point out that Wittgenstein’s remarks on the finite and the infinite with the crucial distinction between “grammatical”

and “empirical” possibility contain a reply to the strict finitist challenge. Dummett’s worries *via* the *Modus Tollens* would just disappear.

The basic idea is that one ought to leave the strict finitist’s territory, namely epistemological arguments based on human limitations. In fact, the Platonist will also defend his position with similar arguments, rejecting our limitations as merely “medical”, not “logical” and by claiming that we can conceive of a more powerful creature such as God which would transcend our limitations. And the intuitionist arguing for the possibility in principle seems to be caught in the middle, fighting against the Platonist on his right and trying to avoid the strict finitist consequences on his left. This is the typical way of characterizing the diverse positions in philosophy of mathematics, and precisely the terms in which Dummett would describe them:

The intuitionist holds that the expressions of our mathematical language must be given meaning by reference to operations which we can in principle carry out. The strict finitist holds that they must be given meaning by reference only to operations which we can in practice carry out. The platonist, on the other hand, believes that they can be given meaning by reference to operations which we cannot even in principle carry out, so long as we can conceive of them as being carried out by beings with powers which transcend our own. (Dummett 1977: 60)

Wittgenstein, whose view of the infinite appears so far as similar to the intuitionistic one, would argue that it is wrong to characterize the debate in terms of possibilities for a finite human being or for a God. In fact he would claim that this kind of “epistemological” characterization appears when one is not making the proper “grammatical” distinction between finite sequences and infinite processes. It is very important also to notice that the criticisms made by Wittgenstein turn out to be precisely arguments an intuitionist would agree with! I would therefore claim that a more valuable construal of the constructivist position is along lines similar to Wittgenstein’s “grammatical” remarks.

Once we defend the intuitionistic view of the infinite with Wittgensteinian arguments, we are not standing on an epistemological ground and *there is therefore no more risk of a radicalization of the arguments in the strict finitist direction*. Moreover, we

*have the possibility of criticizing both the strict finitist and the Platonist at the same time, for their nonsensical construal of the distinction between finite sequences and infinite processes.*

It is important to notice that Wittgenstein's argument against Platonism is in all respects acceptable to the intuitionist. There is much similarity between Wittgenstein's conception of the infinite and the intuitionistic one, as expressed by Dummett:

... the thesis that there is no completed infinity means, simply, that to grasp an infinite structure is to grasp the process which generates it, that to refer to such a structure is to refer to that process, and that to recognize the structure as being infinite is to recognize that the process will not terminate. (Dummett 1977: 56)

The intuitionist says that to grasp an infinite structure is to grasp the process that generates it, Wittgenstein claimed in a similar manner that the infinite is the property of a law (process), not of its extension. The intuitionist recognizes that an infinite structure is a process that will not terminate and Wittgenstein spoke of a possibility of the symbolism, i.e. of the fact that it sets no limits to its application.

We saw that Wittgenstein insisted on not conceiving the infinite as a (huge) quantity. This is also an intuitionistic argument: since Brouwer the intuitionists have accused the Platonist of illegitimately transferring a picture appropriate to the finite case to the infinite one (Brouwer 1923: 336). Dummett does not say anything different:

The Platonistic conception of an infinite structure as something which may be regarded both extensionally, that is, as the outcome of a process, and as a whole, that is, as if the process were completed, thus rests on a straightforward contradiction: an infinite process is spoken of as if it were merely a particularly long finite one. (Dummett 1977: 57)

The distinction between the finite and the infinite is a conceptual one, the infinite being a possibility of the symbolism. It is only when it is recognized as such that it becomes irrelevant to restrict to practical possibility, and it is no proper line of argument to invoke, as the Platonist does, an "hypothetical being whose powers transcend our own" (Dummett 1977: 59).

By now it should be clear to any reader that Wittgenstein's conception of the infinite bears no affinity with the strict finitist conception. Wittgenstein's "grammatical" approach is certainly akin to the intuitionistic epistemology, but Wittgenstein's argumentation has an added advantage over the intuitionistic approach: one is then able to criticize the Platonist conception without having to face the challenge of the strict finitist, and one can also reject strict finitism by the same token independently of the coherence or incoherence of its notions.

These brief remarks indicate strong similarities between Wittgenstein and the intuitionists on the topic of infinity. These similarities are rather deceptive and they mask a different approach of quantification. I shall now turn to this topic in order to show how Wittgenstein's viewpoint is to be distinguished from that of the intuitionist.



## VI. Quantification and the Excluded Middle

### 13. Generality, Induction and the Quantifiers

In 1921, H. Weyl published a paper, “Über die neue Grundlagenkrise der Mathematik”, in which he declared that he would abandon his own foundational programme, set forth in *The Continuum*, and join L. E. J. Brouwer. D. Hilbert was probably irritated by Weyl’s change of mind and his enthusiastic claims such as: “*Brouwer—das ist die Revolution!*” (Weyl 1921: 158). In a long series of papers, Hilbert began a decade long assault on intuitionism, i.e. the famous *Grundlagenstreit*. The whole debate centred (predictably) upon Brouwer’s criticism of the Law of Excluded Middle and of the use of the universal quantifiers in mathematics —i.e. on the so-called  $\Pi_1^0$ -statements. *Wittgenstein was not ignorant of this debate*. He probably first heard of Brouwer by reading F. P. Ramsey’s 1925 paper “The Foundations of Mathematics”, a copy of which he got from M. Schlick, with whom he discussed it. Of course, he also knew about intuitionism directly from hearing one of the two lectures given by Brouwer in Vienna in April 1928 (Brouwer 1928). Moreover, Wittgenstein owned a copy of Hilbert’s 1925 paper “Über das Unendliche” (Hilbert 1925)<sup>1</sup>, and also read and discussed with Waismann an earlier paper of Hilbert, “Neubegründung der Mathematik (Erste Mitteilung)” (Hilbert 1922a).<sup>2</sup>

There is also strong evidence that he discussed these matter carefully with Ramsey. Indeed, according to Moore, Wittgenstein returned to Cambridge in 1929 “for the sake of having the opportunity of frequent discussion with F. P. Ramsey” (M: 252). Wittgenstein

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<sup>1</sup> See (Kreisel 1983: 295).

<sup>2</sup> See (WWK: 119-121).

acknowledged his debt to Ramsey in the Preface to **PI**, and the latter wrote of Wittgenstein:

During the last two terms I have been in close touch with his work and he seems to me to have made remarkable progress. He began with certain questions in the analysis of propositions which have now led him to the problems about infinity which lie at the root of current controversies on the foundations of Mathematics. At first I was afraid that lack of mathematical knowledge and facility would prove a serious handicap to his working in this field. But the progress he has made has already convinced me that this is not so, and that here too he will probably do work of the first importance. (M: 254)

This letter of Ramsey is very instructive, since it indicates that the topic of their conversations was precisely the problem of infinity and the quantifiers in mathematics which was at the heart of the *Grundlagenstreit*.

Alas, there are no records of these conversations between Ramsey and Wittgenstein. But manuscripts (such as **PR**) and lecture notes (such as **M**) of that period remain, and they tell us a fascinating story. In the twenties Ramsey published papers defending Wittgenstein's account of quantification in the **TLP** and a strong form of Platonism. But U. Majer has recently argued—it seems to me conclusively—that Ramsey underwent a radical conversion 1929, the year of his conversations with Wittgenstein, when he adopted a finitist position akin to that of Weyl (Majer 1989).<sup>1</sup> Ramsey changed his mind precisely about quantification, adopting Weyl's intuitionist reading of the quantifiers. A careful reading of Wittgenstein's writings *circa* 1929 show that he also came to realize that there was a problem with quantification over infinite domains. And his reflections on quantification from 1929 onward bear the mark of the intuitionist critique. One can see here the powerful impact of Brouwer's critique of classical mathematics upon his contemporaries.<sup>2</sup>

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<sup>1</sup> Ramsey's change of mind about finitism was also recorded by A. Ambrose in her article of 1935 in *Mind*: "Finitism in Mathematics" (Ambrose 1935: 188 & 340). It is not clear to me, however, what kind of evidence she had for such a claim.

<sup>2</sup> Majer claimed that Wittgenstein was influenced by Weyl through his conversations with Ramsey (Majer 1988: 551), (Majer 1989: 243, note 28). In the absence of any record of these conversations, it is impossible to verify such a conjecture. I shall avoid talking of "influences".

Brouwer was very keen on the distinction between the finite and the infinite, pointing out the properties of calculation over finite domains that do not carry over in the infinite case. One of these properties is the logical Law of Excluded Middle:

$$\varphi \vee \neg\varphi$$

As early as 1908, Brouwer published a critique of the use of the Law of Excluded Middle in infinite contexts: according to him in order to assert that  $\varphi$  is either true or false, one must either have a construction of  $\varphi$  or a construction that proves that a construction of  $\varphi$  cannot obtain. For Brouwer there was no proof that this could always be the case:

...the question of the validity of the *principium tertii exclusi* is equivalent to the question *whether unsolvable mathematical problems can exist*. There is not a shred of a proof for the conviction, which has sometimes been put forward, that there exists no unsolvable mathematical problems. (Brouwer 1908: 109)

In the case of infinite series the principle of mathematical induction cannot always help. Without proof, a  $\Pi_1^0$ -statement such as Fermat's Last Theorem, which says that:

$$\forall xyz \forall n > 2 (x^n + y^n \neq z^n)$$

cannot be said to be either true or false, because it is impossible to perform infinitely many steps for a verification which would consist, say, in a procedure for sequentially inserting all natural numbers in both sides of the equation.

Brouwer was well aware of the non-contradictoriness of the Law of Excluded Middle:

We conclude that in infinite systems the *principium tertii exclusi* is as yet not reliable. Still we shall never, by an unjustified application of the principle, come up against a contradiction and *thereby* discover that our reasonings were badly founded. For then it be contradictory that an embedding were performed, and at the same time it would be contradictory that it were contradictory, and this is prohibited by the *principium contradictionis*. (Brouwer 1908: 110)<sup>1</sup>

The fact that applications of the Law of Excluded Middle would never lead to contradictions renders its critique more difficult, and Brouwer had, in his own words “to

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<sup>1</sup> See also (Brouwer 1923: 336, note 2).

rely exclusively on exhortation to reflect and reason” (Brouwer 1933: 431) (van Stigt 1990: 250).<sup>1</sup> Accordingly, Brouwer didn’t claim that the Law of Excluded Middle is straightforwardly *false*, i.e. that we have:  $\neg(\varphi \vee \neg\varphi)$ , but that he accepted instead:  $\neg\neg(\varphi \vee \neg\varphi)$  (Heyting 1956: 100), (Smorynski 1988: 18). An intuitionist would therefore avoid asserting the Law of Excluded Middle in certain contexts, but would not reject it straightforwardly.

But Brouwer was not always careful in his writings. For example, in a paper published in the *Mathematische Annalen* in 1919 called “Intuitionistische Mengenlehre” he wrote:

My conviction is that the principle of the solvability of all mathematical problems and the Law of Excluded Middle are both *false*... (Brouwer 1919: 231, note 4) \*

This is probably the reason why H. Weyl saw an important difference between his treatment of quantification and the Excluded Middle and Brouwer’s:

With regard to the (...) usage of the terms “all” and “any”, I think one does not hit the right spot by referring to the validity or invalidity of the principle of the excluded middle. (Weyl 1929: 151)

When Weyl published *The Continuum*, he did not reject the Law of Excluded Middle,<sup>2</sup> nor did he reject the classical reading of the logical connectives and quantifiers. But when he came closer to Brouwer’s views, i.e. when he published “Über die neue Grundlagenkrise der Mathematik” in 1921 —two years after the publication of Brouwer’s “Intuitionistische Mengenlehre”— he was keen on marking the differences with Brouwer’s account:

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<sup>1</sup> This is also why Brouwer said in 1923: “... an incorrect theory, even if it cannot be inhibited by any contradiction that would refute it, is none the less incorrect, just as a criminal policy is none the less criminal even if it cannot be inhibited by any court that would curb it” (Brouwer 1923: 336). The need for reflection (*Besinnung*) was discussed in section 1.

<sup>2</sup> A good example is the following quotation: ““Fermat’s last theorem”, for example, is intrinsically meaningful and either true or false. But I cannot rule on its truth or falsity by employing a systematic procedure for sequentially inserting all numbers in both sides of Fermat’s equation. Even though, viewed in this light, this task is infinite, it will be reduced to a finite one by mathematical proof (which, of course, in this notorious case, still eludes us)” (Weyl 1918: 49).

Brouwer justifies his view by claiming that there is no ground for the belief that all such questions about existence [Existentialfrage] *can be solved*, that the proof of the validity of the Law of Excluded Middle must reside, according to him, in the declaration that a method exists that would lead to a decision in one way or another of the questions about existence for any property F. It is well-known that this standpoint was first defended by Kronecker. By way of contrast, in my attempt at providing a foundation for Analysis, I have supported the view that it is not a question of whether we are able to solve a problem, with help from, say, the deductions of formal logic, but it is rather a question of *knowing the facts of the case...* (Weyl 1921: 155-156) \*

Although Brouwer would cast the issue in terms of the distinction between finite and infinite domains, and so would Wittgenstein with his grammatical distinction, Weyl would cast the problem in epistemological terms:

... it is incorrect to describe the intuitionistic point of view by saying that the *tertium non datur* applies or does not apply in the case referred to, according as M is a finite or an infinite set. The issue does not lie in the distinction between the finite and the infinite but it depends on whether M is given as an aggregate of objects which are individually exhibited, one by one (and therefore indeed finite), or not. (Weyl 1929: 151)

According to Weyl's conceptions, the Law of Excluded Middle applies in the case of singular propositions, while it doesn't in the case of universally or existentially quantified sentences. The difference with Brouwer's approach appears to reside in the fact that in accordance with Brouwer's pronouncements one would ultimately modify the rules of propositional logic (this was done by A. Heyting), while Weyl was happy with classical propositional logic but rejected only parts of the quantification theory, in particular part of the equivalence:

$$\forall x f(x) \leftrightarrow \neg \exists x \neg f(x)$$

(Majer 1989: 244). Weyl's interpretation of existential and general sentences differ from Brouwer in that the arguments are not framed in terms of the distinction between the finite and the infinite, but in terms of the exhibition of the objects. To use Weyl's own jargon, he did not consider that existential sentences are judgements proper but that they are *Urteilabstrakte* or "judgement-abstracts":

*An existential sentence—e.g. "there is an even number"—is not at all a judgement which assert a fact in the proper sense.* Existential-facts are an

empty invention of logicians. “2 is an even number”: this is a real judgement expressing a fact; “there is an even number” is only a *judgement-abstract* obtained from this judgement. (Weyl 1921: 156) \*

Similarly, general sentences are not judgements but *Anweisungen auf Urteile* or “rules for judging”:

The general “Every number has the property F”—e.g. “for each number  $m$ ,  $m+1 = 1+m$ ”—is equally little a real judgement, it is rather a general *rule for judging*. (Weyl 1921: 157) \*

Weyl took the example of the general sentence “All the pieces of chalk are white”. If three pieces of chalk are exhibited to me and I say “All the pieces of chalk are white”, then this sentence is a judgement which is either true or false, it is an abbreviation for a finite conjunction:

This piece is white  $\wedge$  this piece is white  $\wedge$  this piece is white.

This sentence expresses a proper judgement because the pieces of chalk are exhibited and an enumeration is possible. But if the exhibition is impossible as in the case of the natural number series, which is infinitely long, the sentence “All numbers are even” cannot be interpreted as an abbreviation for an infinite conjunction:

1 is even  $\wedge$  2 is even  $\wedge$  3 is even  $\wedge$  ...

because this has “obviously no meaning” (Weyl 1929: 152). Even *if the conjunction was finite*, but the pieces could not be exhibited one by one, i.e. if the collection was finite but unsurveyable, the general sentence would still not express any judgement (Weyl 1929: 151).

What Weyl meant when he said that general sentences are “rules for judging” was that they justify the deduction of an infinity of singular judgements, but that they are not to be judged as true themselves. This means that

$$\forall x f(x) \rightarrow f(a)$$

is to be taken as an axiom in the quantification theory (Majer 1989: 245-246). The other axiom would concern the existential sentences. With a similar analysis to the one I just

gave for general sentences Weyl pointed out that existential sentences can only be inferred from real particular judgements, without which they are nothing in themselves. They too cannot be judged true. So the axiom should be:

$$f(a) \rightarrow \exists x f(x)$$

These two axioms are similar to those introduced by Bernays in his formalization of the classical predicate calculus (Hilbert & Bernays 1934: 104) (Hilbert & Ackermann 1937: 68). They are also admitted by the intuitionists (Heyting 1946: 119). Moreover, the most important consequence of Weyl's analysis of quantified statements is that infinite conjunctions cannot be negated, and therefore that the Law of Excluded Middle does not apply:

... it is completely senseless to negate such sentences, therefore the possibility of formulating the Law of Excluded Middle in regard to these sentences disappears. (Weyl 1921: 158)

There is therefore no substantial difference between Weyl's account of quantification and the intuitionist account. The only difference is in the manner of presentation.<sup>1</sup>

Hilbert also equated the quantifiers with logical product and sum. In his famous 1925 paper, "Über das Unendliche", he introduced a trichotomy between *real propositions*, *finitary general propositions*, and *ideal propositions*.<sup>2</sup> Elementary or real propositions are simply equations involving primitive recursive functions and numerals, such as "2 + 3 = 3 + 2." These propositions have intuitive content and are verifiable by direct computation. They can be negated, and the Law of Contradiction and the Law of Excluded Middle hold for them. But besides these unproblematic propositions one will

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<sup>1</sup> In (Majer 1988) & (Majer 1989), Majer claimed that there exist an essential difference between Brouwer's and Weyl's intuitionism, which hinges precisely on their reading of the quantifiers. But as I said, it seems clear that Weyl's approach to the quantifiers, while different in its arguments, leads to the same results as Brouwer's critique. There is no such difference.

<sup>2</sup> The usual descriptions of Hilbert's programme only distinguish between finitary (real) propositions and transfinite (ideal) formulas. But this trichotomy is set forth quite clearly by Hilbert (Hilbert 1925: 380-381). This crucial distinction was pointed out to me by D. Isaacson in conversation. See also (Smorynski 1988: 58-59).

encounter other finitary propositions of problematic character. Hilbert claimed that an expression such as:

There exists a prime number  $x$  such that  $p < x \leq p!+1$

is an abbreviation for the logical sum, or disjunction:

$(p+1) \vee (p+2) \vee (p+3) \vee \dots \vee (p!+1)$  is prime

(Hilbert 1925: 377-378). Hilbert wanted to specify that such disjunctions were admissible, because with  $p!+1$  one never leaves the domain of finite totalities. Within these “ $\exists x f(x)$ ” is an admissible statement. But Hilbert immediately made the further claim that the passage to the infinite raises difficulties:

So, for example, the proposition that, if  $a$  is a numeral, we must always have

$$a + 1 = 1 + a$$

is from the finitist point of view *incapable of being negated*. This will become clear for us if we reflect upon the fact that the proposition cannot be interpreted as a combination, formed by means of “and”, of infinitely many numerical equations, but only has a hypothetical judgement that comes to assert something when a numeral is given. (Hilbert 1925: 378)

Hilbert’s claim is that when the domain of quantification is infinite propositions of the form “ $\forall x f(x)$ ”, i.e. finitary general propositions or  $\Pi_1^0$ -statements, cannot be understood as infinite logical products because they cannot be negated.<sup>1</sup> This is the very basis of Hilbert’s limited rejection of the principle of the Excluded Middle, which was later recognized by Brouwer as a major success of the intuitionistic critique of formalism (Brouwer 1927: 491).

Hilbert would recognize the existence of transfinite formulae as *ideal* propositions to be added to the finitary propositions, singular and general, in order to reestablish the validity of the laws of classical logic, i.e. of the Law of Excluded Middle. His metamathematical programme consisted in trying to obtain a *real* proposition which would

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<sup>1</sup> The idea expressed here is, in F. Waismann’s words, that “the incorrectness of a general statement about numbers by no means implies the existence of a counter-example” (Waismann 1951: 98). We could read Wittgenstein as making the same point in PR: “I cannot deny the *generality* of a general arithmetical proposition” (PR: § 169).



be the proof of the consistency of those parts of mathematics using *ideal* propositions. I am not interested here in the fate of Hilbert's programme. I merely wish to point out that Hilbert's (limited) rejection of the Law of Excluded Middle is analogous to Weyl's. They both amount to denying that the negation of a universally quantified statement is an existential statement. In fact, Hilbert's notion of "hypothetical judgement" in our previous quotation (Hilbert 1925: 378) bears a strong similarity with Weyl's notions of "rule for judging" and "judgement-abstract". The problematic domain is now well circumscribed: the finitary general propositions that are incapable of being negated.

It is rather fitting to remark here that Hilbert admitted (albeit only once!) in a late paper that his finitist point of view was exactly that of Kronecker:

At the same time, therefore at an already old age, Kronecker held a clearly defined conception, which he explained through numerous examples, and which coincides in essentials, today, with our finitary standpoint. (Hilbert 1931: 487) \*

The connection between Kronecker's finitist approach and the viewpoint of Brouwer, Weyl and Hilbert on the very matter of universal quantification is quite crucial for my thesis, considering my earlier claim of deep similarities between Wittgenstein's philosophy of mathematics and Kronecker's (section 2.)

Ramsey's manuscripts indicate clearly that he became a proponent of Weyl's views in the last year of his life. For example, in "Principles of Finitist Mathematics" Ramsey claimed at one point: "The proper method seems to be Weyl's" (Ramsey 1929a: 256), and then expounded Weyl's theory. Ramsey also gave brief indications of arguments against the other rival "finitist" theories of Skolem (Skolem 1923), Hilbert and Brouwer's intuitionism. In "General Propositions and Causality", Ramsey generalized Weyl's analysis. Ramsey thought that there are two kinds of general propositions: conjunctions and "variable hypotheticals" (Ramsey 1929b: 237). General propositions are conjunctions when the objects are all concretely given, as in "Everybody in Cambridge voted". Otherwise general sentences such as "All men are mortal" are what he called "variable

hypotheticals”. These are not conjunctions because, first, they “cannot be written out as one”, secondly they are never used as conjunction, and thirdly because they express “an inference we are at any time prepared to make, not a belief of the primary sort” (Ramsey 1929b: 237-238). Variable hypotheticals are therefore not truth-functions, and Ramsey finds them akin to causal laws. Their inferential status is described by Ramsey in terms strikingly similar to Weyl’s:

Variable hypotheticals are not judgements but rules for judging “If I meet a  $\phi$ , I shall regard it as a  $\psi$ ”. This cannot be *negated* but it can be *disagreed* with by one who does not adopt it. (Ramsey 1929b: 241)

I shall not discuss further Ramsey’s notion of “variable hypotheticals” here. The point of these remarks has been to indicate the influence of Weyl upon Ramsey.

Moore’s lectures notes of 1932-33 clearly indicate Wittgenstein’s change of mind about the conception of quantification in TLP. I already pointed out in section 7 that at the time of these lectures Wittgenstein was already considering as mistaken the idea that the quantifiers be understood as conjunctions or disjunctions (M: 297).<sup>1</sup> He expressed the same views in PG:

My view about general propositions was that  $(\exists x). \phi x$  is a logical sum and that though its terms aren’t enumerated *here*, they are capable of being enumerated (from the dictionary and the grammar of language).  
For if they can’t be enumerated we don’t have a logical sum. (PG: 268)

Wittgenstein was by then speaking of the class “primary colour” as “defined by grammar” (M: 297). Here an enumeration is possible, and the proposition “in this square there is one of the primary colours” is identical with the logical sum “In this square there is either red or green or blue or yellow”. *Wittgenstein claimed that his mistake in the TLP was to suppose that in all classes “defined by grammar” general propositions were identical with a logical product or a logical sum as in the case of “primary colours”*. In parallel with Weyl and Ramsey, Wittgenstein could only see the universal quantifier as a

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<sup>1</sup> See also (AWL: 5-6).

logical product when the dots were “dots of laziness”, i.e. an abbreviation for an enumeration. Moore reported:

He said he had been misled by the fact that  $(x).fx$  can be replaced by  $fa . fb . fc . \dots$ , having failed to see that the latter expression is not always a logical product: that it is only a logical product if the dots are what he called “the dots of laziness”, as where we represent the alphabet by “A, B, C ...”, and therefore the whole expression can be replaced by an enumeration; but that it is not a logical product where, *e.g.* we represent the cardinal numbers by 1, 2, 3, ..., where the dots are not the “dots of laziness” and the whole expression can not be replaced by an enumeration. (M: 298)<sup>1</sup>

No infinite enumeration is possible, therefore infinite series such as the natural number series cannot be represented as a logical product. This argument bears resemblance with those of Weyl and Ramsey.

According to Wittgenstein to regard all general propositions as logical product or sums and therefore as *truth-functions* is a mistake similar to seeing  $\frac{dy}{dx}$  as a quotient while it is *only the limiting value of a sequence of quotients* (M: 298). There are other variable binding operators in mathematics, such as  $\lim_{\Delta x \rightarrow 0}$  in:

$$\lim_{\Delta x \rightarrow 0} \frac{\Delta y}{\Delta x} = \frac{dy}{dx}$$

But they are of a different nature than the universal quantifier. Moore wrote:

He pointed out that  $\Sigma 1 + \frac{1}{2} + \frac{1}{4} \dots$  approaches a limit, whereas a logical product does not approach any limit. (M: 299)

There are even more similarities with Weyl. I indicated earlier that the meaning of the universal quantifier according to Weyl could be summed up by:

$$\forall x f(x) \rightarrow f(a)$$

Judging from Moore’s notes, at one point Wittgenstein agreed:

He said that when he wrote the *Tractatus*, he would have defended the mistaken view which he then took by asking the question: How can  $(x).fx$

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<sup>1</sup> Wittgenstein’s insistence that the universal quantifier be seen as a logical product only in the finite case squares very well with the “grammatical” remarks on the “and so on” discussed in sections 10 and 11.

possibly entail  $fa$ , if  $(x).fx$  is not a logical product? And he said that the answer to this question is that where  $(x).fx$  is not a logical product, the proposition “ $(x).fx$  entails  $fa$ ” is “taken as a primary proposition”, whereas where it is a logical product this proposition is deduced from other primary propositions. (M: 298)

There are, however, major differences between Weyl and Wittgenstein. First of all, according to Weyl the determination of the truth of singular or quantified statements was that of exhibiting the objects, while according to Wittgenstein it was rather a question of naming them:

We might come to a queer conclusion, that since only a finite number of circles is distinguishable we have here a finite disjunction. Now is this so? No. We do not have a disjunction here, for there are no distinguishing marks in the language for the various circles. Similarly for the order “Paint me a shade between white and blue”. There is not a finite disjunction here; there is not a disjunction. One feels like saying “One must mean *one of the possible ones* between white and blue”, and one also feels that there are but a limited number of possible ones. But there is no means of naming them, and so a disjunction cannot be constructed. (AWL: 123-124)<sup>1</sup>

Secondly, Wittgenstein thought that “Weyl lumps several different things together” (WWK: 82). Indeed, Weyl’s argument is that general sentences such as “ $\forall x P(x)$ ” cannot have a negation because this would be equivalent to a purely existential sentence which tells us that there is an “ $x$ ” such that it does not possess the property “ $P$ ” without telling us where to find it or to construct it. According to Weyl existential statements can only be judgement-abstracts, and this is why it cannot be the equivalent of the negation of an universal statement. But Wittgenstein came up with a different explanation: *generality is expressed correctly not by a “ $\forall xP(x)$ ” but by an induction:*<sup>2</sup>

A statement about *all* numbers is not represented by means of a proposition, but by means of an induction. Induction however, cannot be denied, nor can you affirm it, for it does not assert anything. (WWK: 82)

Inductions are not as such proper propositions and because of this *they cannot be negated*.

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<sup>1</sup> This is reminiscent of the TLP, where talk of existence of objects is forbidden. Instead one had a discussion about the means at one’s disposal within language.

<sup>2</sup> See (LWL: 14) and (PG: 432). These passages will be discussed below.

In order to go to the heart of the matter, I must give more details about Wittgenstein's remarks on proofs by induction. According to the Viennese philosopher there are mainly two kinds of proofs: <sup>1</sup>

In mathematics there are *two kinds of proof*:

1. A proof proving a particular formula. This formula occurs in the proof itself, as its last step.
2. Proofs by induction. Here it is first of all striking that the proposition to be proved does not occur in the proof itself at all. Thus the proof does not actually prove the proposition. That is to say, induction is not a procedure leading to a proposition. Rather, induction allows us to see an infinite possibility, and in this alone does the nature of proof by induction consist. (WWK: 135)

The distinguishing feature of proofs by induction is that the proposition proved does not appear as the last step of the proof. Therefore the proof is not a proof of the proposition *per se*, instead it “allows us to see an infinite possibility”, i.e. it has the form of a template—this is similar to what Hilbert had to say about induction—for particular proofs, as in the case, for example, of an algebraic proposition such as the associative law “ $a + (b + c) = (a + b) + c$ ”:

A recursive proof is only a general guide to an arbitrary special proof. A signpost that shows every proposition of a particular form a particular way home. It says to the proposition  $2 + (3 + 4) = (2 + 3) + 4$ : ‘Go in *this* direction ..., and you will arrive home.’ (PR: § 164)

Therefore, the problem takes the following form:

To what extent, now, can we call such a guide to proofs the proof of a general proposition? (PR: § 164)

Wittgenstein's answer is that the induction does not assert its generality, it *shows* it, it “allows us to *see* an infinite possibility” (my italics): <sup>2</sup>

An algebraic proposition always gains only arithmetical significance if you replace the letters in it by numerals, and then always only *particular* arithmetical significance.

Its generality doesn't lie in itself, but in the possibility of its correct application. And for that it has to keep on having recourse to the induction.

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<sup>1</sup> Wittgenstein would say later, in RFM: “mathematics is a MOTLEY of techniques of proof” (RFM: III, § 46). But this does not affect his remarks on proofs by induction.

<sup>2</sup> The relation with the saying-showing distinction is rather obvious here.

That is, it does not assert its generality, it does not express it; the generality is, rather, shown in the formal relation to the substitution, which proves to be a term of the inductive series. (PR: § 168)

When we try to describe what has been *shown* by a proof of induction the tendency is to use the word “all”. But the consequence of Wittgenstein’s interpretation of proofs by induction is that the proposition containing the universal quantifier “adds something to the proof”, i.e. “it does not follow from it” but goes proxy for it (WWK: 135). Therefore, the proof by induction may be a proof of generality, but not a proof for “all” numbers:

A proof by induction, if it were a proof, would be a proof of generality, not a proof of a certain property of all numbers, (PR: § 168)

Moreover, the proof does not assert its generality, it *does not assert anything* (WWK: 82). I shall give another quotation from Waismann’s notes:

Now axioms (e.g. the alphabetical rules of algebra,  $a + b = b + a$ , etc.) can be laid down, which, though they are, as such, arbitrary, are of course constructed in accordance with complete induction. I can operate by means of these basic rules by reducing every equation to them. But one thing these rules cannot express is the result of complete induction. This result is subsequently manifested in the applicability of the rules to concrete numbers, but the nature of complete induction is not expressed in the form of a proposition or in the form of an axiomatic system; it is mathematically inexpressible. Complete induction shows itself in the structure of equations. (WWK: 34)

Wittgenstein is therefore inclined to think that the result of a proof by induction, since it does not assert anything, is not a genuine proposition. This is why he said that the Fundamental Theorem of Algebra is not a “genuine mathematical *proposition*” (PR: § 168). This is also the case of Fermat’s Last Theorem, supposing we had a proof of it:

I say: the so-called ‘Fermat’s Last Theorem’ isn’t a proposition. (Not even in the sense of a proposition of arithmetic.) Rather, it corresponds to a proof by induction. (PR: § 189)

This denial of the full status of “mathematical proposition” to such theorems will play an important role in the next section, which is about “propositions” to which the Law of Excluded Middle does not apply.

In 1923 T. Skolem published a paper, “Begründung der elementaren Arithmetik durch rekurrende Denkweise ohne Anwendung scheinbarer Veränderlichen mit

unendlichen Ausdehnungsbereich" (Skolem 1923), of which Wittgenstein owned a copy.<sup>1</sup> Skolem had just read *Principia Mathematica* and as a (radically finitist) solution to the problem of the paradoxes of set-theory, he proposed to develop a fair part of elementary arithmetic without the use of unbounded quantifiers as a way of avoiding the paradoxes and the need for the theory of types:

*If we consider the general theorems of arithmetic to be functional assertions and take the recursive mode of thought as a basis, then that science can be founded in a rigorous way without the use of Russell and Whitehead's notions "always" and "sometimes". This can also be expressed as follows: A logical foundation can be provided for arithmetic without the use of apparent logical variables. To be sure, it will often be advantageous to introduce apparent variables; but we shall require that these variables range over only finite domains, and by means of recursive definitions we shall then always be able to avoid the use of such variables. (Skolem 1923: 304)*

What Skolem called the "recursive mode of thought" consisted in using primitive recursive definitions for the introduction of new functions and in the use of mathematical induction for proofs.<sup>2</sup> It appears to me that *the resulting system of Primitive Recursive Arithmetic (hereafter: PRA) embodies quite well Wittgenstein's ideas about generality*. Indeed, Skolem allowed the use of bounded quantifiers only as shorthand notation. He used free-variables only when they range over finite domains. In the tradition of Kronecker, Skolem rejected pure existence proofs:

*... that work, too, is a consistently finitist one; it is built upon Kronecker's principle that a mathematical definition is a genuine definition if and only if it leads to the goal by means of a *finite* number of trials. (Skolem 1923: 333)*

He could not accept existence as the negation of a general proposition with an universal quantifier. This stance on existence places him alongside Brouwer and Weyl. But, *although it refuses to apply the Law of Excluded Middle to negations of such sentences Brouwerian intuitionism still contains such negations, while they simply cannot be*

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<sup>1</sup> See the editor's footnote to (PR: § 163).

<sup>2</sup> Wittgenstein criticized Skolem's use of mathematical induction: (PR: § 163) and (PG: 397-399). I shall discuss these remarks in a moment.

*expressed in Skolem's PRA (it is syntactically impossible, since universality is expressed by the use of a free variable.) Intuitionism is therefore wider than Skolem's PRA, and farther in spirit from Wittgenstein's constructivism.*

Clearly, in Wittgenstein's opinion the question of the nature of existential statements has nothing to do with the problem of the universal statements and their negation, as in Weyl's analysis of  $\Pi_1^0$ -statements:

Weyl pretends there may indeed be universal statements but they do not have negations, on the ground that an existential statement is a 'judgement-abstraction' and that only construction (finding a number) tells us anything. But in reality these are two completely different things—a universal statement is correctly expressed by means of induction and as such it naturally cannot be negated. (WWK: 81)

In the eyes of Wittgenstein, the problem with classical logic is therefore not with the impossibility of negating universal statements, but in "regarding an extension as a totality" (WWK: 81), i.e. with the quantification theory itself! Indeed, according to Waismann's notes, at this point Wittgenstein discussed an example derived from Brouwer's famous example about the decimal expansion of  $\pi$ , pointing out the difference between statements involving finite segments of an expansion and those involving the totality of an infinite expansion (such as  $\pi$ 's):

The claim that a number occurs at a certain place is of course an assertion and can as such be in turn negated. Such a negation simply says that at the place in question that number does not occur. The error arises from regarding an extension as a totality. For it makes good sense to say: If 7 occurs at the 25th place, then 7 occurs between the 20th and the 800th place of  $\pi$ —then by saying it I have told you exactly that and nothing else. (WWK: 81-82)

The proposition '7 does not occur between the 20th and the 30th place' is verified in a different way from the proposition '7 does not occur at all'. But if it is verified in a different way, then *it is a different proposition*. If you answer the question whether the figure 7 occurs in the expansion of  $\pi$  by saying: Yes, it occurs at the 25th place, you have answered only the question whether 7 occurs at the 25th place but not the question whether 7 occurs at all. (WWK: 82)<sup>1</sup>

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<sup>1</sup> This discussion is linked with Wittgenstein's critique of the universality of the Law of Excluded Middle. See (WWK: 71) and (M: 303) for example. These passages will be discussed in relation to this topic in the next section.



Wittgenstein claims here that a proof that “7 occurs at the 25th place of the decimal expansion of  $\pi$ ” is only a proof that 7 occurs at the 25th place —or the proper answer to the question “does the 7 occur at the 25th place in the decimal expansion of  $\pi$ ?— not a proof that it occurs at all in the decimal expansion of  $\pi$  —or an answer to the general question “Does 7 occur at all in the decimal expansion of  $\pi$ ?”. This is quite strange, since we would be ready to consider a proof that 7 occurs at the 25th place is a proof that it occurs in the decimal expansion of  $\pi$ . But Wittgenstein was trying rather to point out that any general question does not make sense, unless one considers the decimal expansion of  $\pi$  *in extension, as a totality*. If a question is nonsensical, any putative answer to it is also, and conversely:

If the question has a sense, then the answer has a sense too, no matter if it turns out positive or negative. (WWK: 82)

From these notes of Waismann, one can already see that Wittgenstein’s objections to classical quantification theory run deeper than Weyl’s. Indeed, the claim that generality can only be shown by an induction appears to imply a rejection of the use of the universal quantifier. Therefore, although there are affinities between the intuitionist standpoint of Weyl and Wittgenstein’s own positions, I claim that Wittgenstein went farther in completely rejecting quantification theory.

There are other definite indications that Wittgenstein was closer to Skolem than to intuitionism (either Brouwer’s or Weyl’s) and rejected altogether the use of the universal quantifier in any logical system representing arithmetic. This is apparent in passages from **PR** such as:

But you can’t talk about all numbers, because there is no such thing as all numbers. (PR: §124)

or:

... I mean: you can’t say ‘(n)  $P_n$ ’ precisely because ‘all natural numbers’ isn’t a bounded concept. But then neither should one say a general proposition follows from a proposition about the nature of number. But in that case it seems to me that we can’t use generality—all, etc.—in mathematics at all. There’s no such thing as ‘all numbers’, simply because

there are infinitely many. And because it isn't a question here of the amorphous 'all', such as occurs in 'All the apples are ripe', where the set is given by an external description: it's a question of a collection of structures, which must be given precisely as such (PR: §126)

In these remarks Wittgenstein clearly rejected the use of the universal quantifier " $\forall x f(x)$ " for infinite series in arithmetic. In these passages Wittgenstein was aiming precisely at the descriptivist viewpoint underlying classical quantifier theory. One would be tempted to stress the similarity between the arguments contained in these remarks and the criticisms levelled at the classical interpretation of the universal quantifier from the verificationist viewpoint of the (Dummettian) intuitionist. I briefly indicated Frege's conception of universal and existential quantification in section 7. For Frege the domain of quantification was invariably the totality of all objects and the quantifier would always be an infinite product of truth-values. The descriptivist viewpoint criticized by Wittgenstein is precisely the point of view of a man, rather a Demiurge, who could survey the whole infinite domain of the natural number series (or the whole decimal expansion of  $\pi$ ), and then talk about "all" the numbers. In Frege's words, Wittgenstein was against the idea of the survey of an infinite domain, to see if the application of the predicate always yields the truth or not, so as to determine the truth-value of the quantified statement.

That "all the numbers" share one property is not something established "externally" as in the case of all the apples being said to be ripe because we can survey them (this is what Wittgenstein described as the "amorphous 'all'"). There is here an obvious parallel between Wittgenstein's "external" and Dummett's "accidental"<sup>1</sup> in the following description of the intuitionist interpretation of the universal quantifier:

... when the domain of quantification is infinite...a universally quantified [statement] cannot be thought as being true accidentally, that is independently of there being a proof of it, a proof which must depend intrinsically upon our grasp of the process whereby the domain is generated. (Dummett 1977: 57)

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<sup>1</sup> The term "accidental" was also used by Wittgenstein in the TLP (TLP: 6.031, 6.1232).

This suggests initially a similarity between the verificationist account of the intuitionists, and Wittgenstein's account. However, as I already pointed out this similarity is only on a superficial level.

In this very passage of **PR** (and of **PG**), Wittgenstein used against the Fregean interpretation of the quantifiers an analogy similar to Frege's own example of the infinite row of houses, in order to show that there is "no path to infinity":

The situation would be something like this: we have an infinitely long row of trees, and so as to inspect them, I make a path besides them. All right, the path must be endless. But if it is endless, then that means precisely that you can't walk to the end of it. That is, it does not put me in a position to survey the row. (PR : § 123) & (PG: 455)

A survey one by one of all the natural numbers is not what will convince us that they share a given property. But this doesn't amount to saying that such a survey is impossible. To say this would be to accept the terms of the debate between the strict finitists and their opponents: a person cannot survey all the natural numbers, God can. A passage from **PG** shows clearly that this was not Wittgenstein's intention:

What is its verification? —if there is no method provided for deciding whether the proposition is true or false, then it is pointless, and that means senseless. But then we delude ourselves that there is indeed a method of verification, a method which cannot be employed, but only because of human weakness. This verification consist in checking all the (infinitely many) terms of the product  $\varepsilon(0) \cdot \varepsilon(1) \cdot \varepsilon(2) \dots$ . Here there is confusion between physical impossibility and what is called 'logical impossibility'. For we think we have given sense to the expression "checking of the infinite product" because we take the expression "infinitely many" for the designation of an enormously large number. And when we hear of the "impossibility of checking the infinite number of propositions" there comes before our mind the impossibility of checking a very large number of propositions, say when we don't have sufficient time. (PG: 452)

The gist of this passage is that Wittgenstein rejected the interpretation of the universal quantifier as an infinite logical product. He saw in the strict finitist (or the Platonist, since they amount to the same) description, which conflates physical or empirical impossibility with logical or grammatical impossibility, the error that makes people believe the universal quantifier could be an infinite logical product. Wittgenstein struggled to point out that this way of putting things is itself nonsense:

It isn't just impossible "for us men" to run through the natural numbers one by one, it's *impossible*, it means nothing. Nor can you say, "A proposition cannot deal with all the numbers one by one, so it has to deal with them by the concept of number", as if this were a *pis aller*: "because we can't do it like this, we have to do it another way." But it is not like that: of course it's possible to deal with the numbers one by one, but that doesn't lead to the totality. For the totality is only given as a concept. (PR : § 124)<sup>1</sup>

Wittgenstein insists here that the very idea of a survey of all the natural numbers "means nothing". But it does not mean nothing because one cannot deal with the numbers one by one. It might be the case that we can't,<sup>2</sup> but this is not what he was pointing at. Rather, he stressed that in order to talk about the natural numbers one has to refer to the "concept", i.e. the infinite process, not to its infinite extension. Further on in the same passage he said:

It is difficult to extricate yourself completely from the extensional viewpoint: You keep thinking 'Yes, but there must still be an internal relation between  $x^3 + y^3$  and  $z^3$  since the extension, if only I knew it, would have to show the result of such a relation.' Or perhaps: 'It must surely be either *essential* to *all*  $n$  to have the property or not, even if I can't know it.' (PR : § 130) & (PG: 457)

According to Wittgenstein, one cannot talk about all the natural numbers unless one refers to the "concept" which is nothing else than the law of their generation itself, i.e. an infinite process. But this law is the internal relation set up between the two members of the equality in " $x^3 + y^3 = z^3$ ." Here, internal relations are understood as the infinite arithmetical processes themselves, as opposed to their extension, which would be "external".

As I mentioned earlier, there is a similarity between Wittgenstein's remarks and the verificationist account.<sup>3</sup> Both accounts are trying to debunk the Fregean interpretation. But the strategies are rather different. The difference between the Fregean and the

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<sup>1</sup> Reproduced almost word by word in (PG: 456-457).

<sup>2</sup> In his "verificationist period", Wittgenstein would have said that there is no infinite verification (WWK: 247).

<sup>3</sup> Here "verificationism" does not refer to the verificationism of the Vienna Circle (and for that matter of Wittgenstein in his "verificationist phase".) It refers to Dummett's discussion. It goes without saying that this form of verificationism does not reflect Dummett's own positions either.

verificationist account is to be found in the domain of undecidable statements, i.e. statement for which we lack, even in principle, the means for a determination of their truth-value. Dummett's verificationist would not argue against Frege's interpretation in the case of decidable predicates because in this case the truth-value of a corresponding quantified statement could in principle be determined. But in the case of undecidable predicates the verificationist would say that we cannot determine the truth of the corresponding quantified statement conclusively. This argument is of semantical nature and quite different from Wittgenstein's. *For him it is not a question of verification, but rather that infinite processes are of a different nature and classical quantification is not the proper way to express generality in the case of infinite processes, precisely because quantification requires the descriptivist viewpoint which is also rejected by the verificationist.*

This difference in the arguments leads directly to an important distinction between Wittgenstein's logic and that of the verificationist about meaning. While an intuitionist would still use the universal quantifier for quantification over infinite domains but would interpret such totalities as potential only, Wittgenstein would just shed the quantifiers altogether. In his Cambridge lectures (Lent term, 1930) he made the following comment:

The rule for infinity can be expressed symbolically as follows:  $[f(1), f(\xi), f(\xi+1)]$ . Note that we have to go on step by step, starting from  $f(1)$ . This is not the kind of generality represented by  $(x)\phi x$ . (LWL: 14)

And in PG:

The point of our formulation is of course that the concept "all numbers" is given by a structure like " $!1, \xi, \xi+1!$ ". The generality is *set out* in the symbolism by this structure and cannot be *described* by an  $(x).fx$ . (PG: 432)

It is clear here that Wittgenstein wanted to draw a distinction between the universal quantifier " $\forall x f(x)$ " and expressions such as " $[f(1), f(\xi), f(\xi+1)]$ " or " $!1, \xi, \xi+1!$ ". The quantifiers " $\forall x f(x)$ " and " $\exists x f(x)$ " would be used only for finite sequences, because they

express “external” or “accidental” generality, which is acceptable only for finite extensions.<sup>1</sup>

An expression such as “[f(1), f(ξ), f(ξ+1)]” is inadequate.<sup>2</sup> Wittgenstein probably chose it hastily in order to underline the contrast with the universal quantifier. In line with his earlier views in TLP,<sup>3</sup> Wittgenstein had in mind recursive specifications. *This is a strong indication of his preference for a quantifier-free system of the kind proposed in 1923 by Skolem and later by his own student R. L. Goodstein.* Another solution would be the introduction of a third and new quantifier. But it is rather unlikely that Wittgenstein had in mind a restricted use of the universal quantifier only for finite sequences, while he was thinking of another quantifier for infinite sequences.<sup>4</sup> Indeed there seems to be no need for a new quantifier here, since expressions such as “[f(1), f(ξ), f(ξ+1)]” or rather recursive specifications, represent adequately generality in mathematics. Wittgenstein was indeed asking that *the infinite possibility be easily read from the symbolism itself.* In its description we must see everything:

To explain the infinite possibility, it must be sufficient to point out the features of the sign which lead us to assume this infinite possibility, or better: what is actually present in the sign must be sufficient, and the possibilities of the sign, which once more could only emerge from a description of the signs, do not come into the discussion. And so everything must be already contained in the sign ‘|1, x, x+1|’—the expression for the rule of formation. (PR: App. 1, p.314)

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<sup>1</sup> Such finite quantifiers are rather similar to Carnap’s “limited universal operator” and “limited existential operator” in *The Logical Syntax of Language*. Those were defined as, respectively, a finite sum and a finite product. (Carnap 1934: 20-21).

<sup>2</sup> Of course such expressions cannot really be called the general form of a recursive definition with  $n + 1$  variables. According to Skolem it would have the form:

$$\begin{aligned} f(x_1, x_2, \dots, x_n, 0) &= a(x_1, x_2, \dots, x_n); \\ f(x_1, x_2, \dots, x_n, Sy) &= b(x_1, x_2, \dots, x_n, y, f(x_1, x_2, \dots, x_n, y)) \end{aligned}$$

A more simpler general form would be:

$$f(x, 0) = a(x); \quad f(x, Sy) = b(x, y, f(x, y))$$

In both cases “a” and “b” are either constant or identity functions or functions previously defined by recursive definitions.

<sup>3</sup> See the connections with (NB: 49), the predicativist critique of (TLP: 4.1273) and the general form of a formal series in (TLP: 5.2522).

<sup>4</sup> The later Wittgenstein was not interested in constructing a proper symbolism, contrary to his intentions in the TLP. Nevertheless, this remains an interesting insight for logicians. So far as I know, only Y. Gauthier has taken up, without previous knowledge of these remarks, the idea of a third quantifier. See (Gauthier 1985).

When the process of dividing 1 by 3, which leads to the quotient 0.3 and the remainder 1, is described, the “infinite possibility of going on with the same result must be contained in this description” (PR: App. 1, p.314).

This account fits nicely Wittgenstein’s criticisms of the Russellian symbolism, made during conversations with Schlick and Waismann, which we are now in a position to understand fully:

We cannot imagine the same class at one time finite 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’. Russell promoted this mistaken idea by creating a symbolism which represents both kinds of classes in exactly the same way. He was thus entirely prevented from recognizing the true significance of the difference in question. 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. (WWK : 228)

It is true that often it is difficult to tell if a given class is finite or not.<sup>1</sup> But this came as no surprise to Wittgenstein because he saw Russell as promoting a symbolism where the distinction between the finite and the infinite is irrelevant precisely because he had no qualms about the idea of an actual infinite. Couturat would say of the values of a variable: “*en nombre fini ou infini, peu importe*” (Couturat 1904b: 1050). Limiting the use of “ $\forall x f(x)$ ” to finite series and using expressions such as “[ $f(1), f(\xi), f(\xi+1)$ ]” for adequate description of infinite series would fulfill the role a syntactical distinction. Therefore, Wittgenstein’s rejection of the use of the expression “ $\forall x f(x)$ ” in number theory is explained by the fact that he would restrict its use for finite sequences.

Wittgenstein’s student, R. L. Goodstein also rejected quantification theory.<sup>2</sup> But he went farther than Skolem by dispensing altogether with the propositional calculus and

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<sup>1</sup> One could even define a class in such a way that it is impossible to say if it is finite or infinite. For example one could give the instruction: “Add one until you reach the first odd-perfect number, and then stop”. Since there is no proof of the odd-perfect conjecture, i.e. no odd number is perfect—a perfect number is equal to the sum of all its positive divisors other than itself—nobody can say if this series will turn out to be finite or infinite.

<sup>2</sup> In his preface to *Constructive Formalism*, Goodstein acknowledged his debt to Wittgenstein: “Of the many friends who have helped, encouraged and inspired this work, first and foremost I must mention

mathematical induction postulated by Skolem and developing a pure (i.e. logic-free) equation calculus (Goodstein 1957), in which all propositions are equations of the form  $A=B$  where  $A$  and  $B$  are primitive recursive functions or terms, and where the rules are substitution and uniqueness (instead of induction rules.)

A full discussion of Goodstein's equational calculus falls outside the scope of this study. But there is one point worth mentioning, namely Goodstein's attribution of the replacement of induction rules by uniqueness rules to Wittgenstein, in his critique of Skolem's 1923 paper (Goodstein 1972: 280-281). In his writings, Wittgenstein made many remarks on Skolem's proof by induction of the associative law (Skolem 1923: 305-306):

$$(A) \quad (a + b) + c = a + (b + c)$$

Skolem previously defined addition by recursion:

$$a + 0 = a, \quad a + (b + 1) = (a + b) + 1$$

The proof by induction of (A) was in two steps:

- 1) If  $c = 0$ , we have  $(a + b) = (a + b)$ , since  $a + 0 = a$
- 2) If we suppose that (A) holds for a value  $c$ , then we have:

$$\begin{aligned} (a + b) + (c + 1) &= ((a + b) + c) + 1 \\ &= (a + (b + c)) + 1 \\ &= a + ((b + c) + 1) \\ &= a + (b + (c + 1)) \end{aligned}$$

which proves, according to Skolem that (A) holds for  $c + 1$  for unspecified  $a$  and  $b$ .

One of Wittgenstein's many comments on this proof consisted in rewriting it replacing induction by the rule of inference:

$$\varphi(0) = \psi(0), \varphi(Sx) = F(\varphi(x)), \psi(Sx) = F(\psi(x)) \vdash \varphi(x) = \psi(x)$$

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Ludwig Wittgenstein, to whose lectures in Cambridge between 1931-34 and the many conversations I was privileged to have with him, I am immensely indebted; only in recent years have I grown to understand how much he taught me" (Goodstein 1951: 10).



(PG: 397). Goodstein was quick to point out this was a rule affirming the *uniqueness* of the function defined by recursion:

$$\varphi(0) = a, \varphi(Sx) = F(\varphi(x))$$

a notion he deemed “far more intuitively acceptable” (Goodstein 1972: 281). *Goodstein’s equational calculus is based precisely on the notion of the uniqueness of a function defined by recursion instead of the notion of mathematical induction.* This is one interesting example of a positive development emerging from Wittgenstein’s remarks on mathematics. I must point out that it is an improvement of Skolem’s PRA, as such an indication of the depth of Wittgenstein’s thought on the subject of finitism.<sup>1</sup> It should be clear, however, that Wittgenstein’s remarks on induction are not a critique of the method itself but rather of the language in which it is presented (Goodstein 1972: 281). This is ever so typical of Wittgenstein’s philosophy of mathematics: no “interference” with the mathematicians, i.e. no critique of the methods and results but a critique of the language, because it is the breeding ground of philosophical mistakes (see section 1.)

Wittgenstein was extremely annoyed with the possible misunderstanding of this proof of the associative law. Wittgenstein was constantly arguing against the set-theoretical idea that a proof by induction is a proof for *all* numbers, since he was so keen on distinguishing generality from the “all” of the universal quantifier:

We are not saying that when  $f(1)$  holds and when  $f(c + 1)$  follows from  $f(c)$ , the proposition  $f(x)$  is *therefore* true of all cardinal numbers; but: “the proposition  $f(x)$  holds for all cardinal numbers” *means* “it holds for  $x = 1$ , and  $f(c + 1)$  follows from  $f(c)$ ”.

Here the connection with generality in finite domains is quite clear, for in a finite domain that would certainly be a proof that  $f(x)$  holds for all values of  $x$ , and *that* is the reason why we say in the arithmetical case that  $f(x)$  holds for all numbers. (PG: 406)<sup>2</sup>

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<sup>1</sup> I cannot but disagree entirely with S. Shanker’s polemics when he says that Wittgenstein’s remarks on Skolem’s proof must be “discomfiting” for those who wish to portray him as a finitist of some sort (Shanker 1987: 199).

<sup>2</sup> Also: “A proof by induction, if it were a proof, would be a proof of generality, not a proof of a certain property of all numbers” (PR: § 168).

Skolem had also carefully chosen his words, speaking not of (A) holding for “all” values, but holding “generally”:

Thus the proposition holds generally. This is a typical example of a recursive proof (proof by mathematical induction). (Skolem 1923: 306)<sup>1</sup>

As I said earlier at the beginning of this section, H. Weyl initiated a predicativist programme for the foundations of mathematics in *The Continuum* (Weyl 1918) but was soon to abandon it in favor of Brouwer’s intuitionism. As a consequence, the ideas contained in his book were never “publicized” properly, and his predicativist programme played no important role at the time of the *Grundlagenstreit*. Since, his ideas were developed to a large extent, in particular by P. Lorenzen and S. Feferman.<sup>2</sup> I claimed in section 8 that Wittgenstein was a strict predicativist. It is worth saying a word on the relations between his views and Weyl’s original programme. Weyl built his critique of classical mathematics around the notion of a *circulus vitiosus*, a notion taken over from Poincaré’s notion of impredicativity. As a consequence, he rejected the notion of the actual infinite, and quantification on infinite sets. But Weyl assumed one such closed infinite totality, the set  $\mathbb{N}$  of the natural numbers, with quantification over it (Feferman 1988: 63). This is why Weyl accepted, at that time, classical logic for number theoretic statements, i.e. that:

$$\forall x f(x) \leftrightarrow \neg \exists x \neg f(x)$$

According to what I just claimed, Wittgenstein would not agree with even quantification over the natural numbers (see our quotations from (PR: § 124 & 126).) This is major difference between Weyl’s predicativist programme and Wittgenstein’s

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<sup>1</sup> So when Wittgenstein discussed the expression “This proposition is proved for all numbers by the recursive procedure” and calls it “so very misleading” (PG: 406), he could not be attacking Skolem.

<sup>2</sup> In fact, Feferman now conjectures about Weyl’s theory (he calls it *W*) that “all scientifically applicable mathematics can be formalized in (a subtheory of) *W*, and hence does not require the assumption of impredicative set theory or of uncountable cardinal numbers for its eventual justification” (Feferman 1988: 89-90); i.e. that “higher set theory is dispensable in scientifically applicable mathematics” (Feferman 1987: 153) (see also (Feferman 1987: 202-207).) This corresponds to Weyl’s original intention in *The Continuum* (Weyl 1918: 108).

viewpoint. The proof-theoretic reduction of classical logic to intuitionistic logic effected by K. Gödel and G. Gentzen during the first half of the thirties had the consequence that one could reinterpret even the natural numbers as a “potential” infinite. This consequence reduces the differences between Weyl’s predicativism and Brouwerian intuitionism, but not with Wittgenstein’s finitism.

Notwithstanding the differences, the similarities that I unveiled between Wittgenstein on one hand and Skolem —and Goodstein— on the other hand suggest that the Austrian philosopher’s approach to quantification should be seen in contrast to Brouwer’s: while the intuitionists, i.e. Brouwer or Weyl, would keep the quantifiers and reinterpret them, Wittgenstein had a tendency to opt for a quantifier-free system. The difference between Wittgenstein and intuitionism hinges on this very matter. Skolem’s Primitive Recursive Arithmetic goes further than Brouwer and Weyl —who retained in the end a minimum of quantification theory— in its banishment of quantifiers, and is therefore even more akin to Wittgenstein’s own constructivism. The finitist viewpoint of Skolem’s PRA, to which I associate Wittgenstein, is in itself perfectly respectable. I wish to point out that the arguments which lead Wittgenstein to it do not appear to constitute a standpoint from which he could criticize or undermine the intuitionist’s position. My intention in this section was only to establish the link between Wittgenstein and finitism *à la* Skolem.

On the other hand, there are no points of contact between Wittgenstein and strict finitism. In fact, a strict finitist criticism of universal quantification would take the following form: “There are limitations in our capacity to survey the domain of quantification, therefore the only quantified statements which can be determinately true or false are those whose quantifier is ranging over a domain which is not only finite, but also whose surveyability is humanly feasible.” The previous sections (especially 10 to 12) gave clear indications that there is nothing in Wittgenstein’s remarks that would corroborate the idea that he made the strict finitist distinction between the feasible and the

large finite. Moreover, Wittgenstein was not denying the existence of infinite domains. He was just claiming that it is a crucial syntactical matter that one distinguishes between quantification over finite domains and infinite series where only recursive definitions take place and no quantifiers are allowed.

*It should be clear by now that Wittgenstein's approach of infinity is to be distinguished on the one hand from strict finitism and on the other hand from Brouwerian intuitionism.* Although more finitist in content than intuitionism, Wittgenstein's own brand of constructivism is very critical of strict finitism.

The strict finitist account that I just described raised the question of surveyability, to which I shall turn in the next section but one. The discussion about the quantifiers in the twenties was closely linked with the criticism of the Law of Excluded Middle by the intuitionists. I have claimed so far that while Wittgenstein thought there was no room for classical quantification theory in arithmetic, he was repudiating the Law of Excluded Middle. I would like now to say a little bit more about Wittgenstein's stance on the Excluded Middle, in order to clarify his positions with respect to Brouwer's.

#### **14. On the Law of Excluded Middle**

Wittgenstein's remarks on the Law of Excluded Middle are also the subject of controversy. Many commentators of the later Wittgenstein saw him as limiting the use of the Law of Excluded Middle. For example, R. Fogelin listed "his attacks upon the unrestricted use of the Law of Excluded Middle" as one of the intuitionistic themes found in Wittgenstein's writings (Fogelin 1968: 267).<sup>1</sup> The so-called "anti-realist" commentators were more careful: while Dummett spoke of the "ambivalence" of

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<sup>1</sup> More recently, A. W. Moore said of Wittgenstein that he "shared Brouwer's mistrust of uncritical application of the law of the excluded middle" (Moore 1990b: 17).

Wittgenstein's attitude towards the Law of Excluded Middle (Dummett 1959: 178), C. Wright pointed out that no explicit rejection of it is to be found in Wittgenstein's writings. Rather one might find some shared common background for its rejection:

A hasty reader might form the impression that Wittgenstein wanted to reject the principle for reasons very similar to those of the intuitionists. In fact though, no such explicit rejection is to be found. What there is, in common with the intuitionists, is some of the background from which their rejection stems. (Wright 1980: 142)

The very idea that Wittgenstein had any qualms about the universal validity of the Law of Excluded Middle was, however, rejected strongly by P. M. S. Hacker, who spoke of Wittgenstein's remarks on this very topic as "distorted out of all recognition by the anti-realists' Procrustean efforts" (Hacker 1986: 331). Hacker's argument relies on writings of the transitional period where Wittgenstein claims that when the Law of Excluded Middle does not apply, one cannot talk of genuine propositions. This is, according to him, a clear indication that Wittgenstein didn't intend to reject the Law of Excluded Middle. I shall argue that Hacker has simply misunderstood the sense of Wittgenstein's "critical" remarks on Brouwer's critique of the Excluded Middle, that he has told us only one half of the story.

Wittgenstein's reaction to Brouwer and Weyl's criticisms of the Law of Excluded Middle appears, at first sight, essentially negative:

I need hardly say that where the law of the excluded middle doesn't apply, no other law of logic applies either, because in that case we aren't dealing with propositions of mathematics. (Against Weyl and Brouwer.) (PR: § 151)<sup>1</sup>

In the first of his Vienna lectures, in March 1928 —to which Wittgenstein attended— Brouwer introduced his famous *duale Pendelzahl* or pendulum number: let  $d_v$  be the  $v$ th digit of the decimal expansion of  $\pi$  and  $m = k_n$  if at  $dm$  for the  $n$ th time the segment  $d_m d_{m+1} \dots d_{m+9}$  forms the sequence 0123456789. Now, if  $v \geq k_1$  then  $cv = (-1/2)^{k_1}$ , otherwise  $cv = (-1/2)^v$ . The sequence  $c_1, c_2, c_3, \dots$  forms the real number  $r$  —

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<sup>1</sup> See also (M: 302).

the so-called pendulum number—for which it is impossible to tell if  $r = 0$ ,  $r > 0$  or  $r < 0$ .

<sup>1</sup> This number has some peculiar properties. According to Brouwer, it:

... is not rational, although its irrationality is absurd, and not comparable with zero, although its incomparability with zero is absurd. (Brouwer 1928: 163)\*

It is on that account that Wittgenstein rejected Brouwer's *duale Pendelzahl* as not a true real number (WWK: 73). Indeed, Wittgenstein made it an essential requirement that every real number be effectively comparable with any rational number. I shall discuss this point in section 17.

Brouwer's intention in building this number was to provide a counterexample to the Law of Excluded Middle. He claimed that:

... this pendulum number is neither equal nor unequal to zero, in contradiction with the Law of the Excluded Middle. (Brouwer 1928: 161)\*

To this affirmation, Wittgenstein replied:

Brouwer is right when he says that the properties of his pendulum number are incompatible with the law of the excluded middle. But, saying this doesn't reveal a peculiarity of propositions about infinite aggregates. Rather, it is based on the fact that logic supposes that it cannot be *a priori*—i.e. logically—impossible to tell whether a proposition is true or false. For, if the question of the truth or falsity of a proposition is *a priori* undecidable, the consequence is that the proposition loses its sense and the consequence of this is precisely that the propositions of logic lose their validity for it. (PR: § 173)

P. Hacker reached the conclusion, based on the previous quotations, that Wittgenstein never rejected the Law of Excluded Middle. According to Hacker's Wittgenstein:

The very idea of undecidable propositions is nonsense, ... We cannot understand an 'undecidable proposition of mathematics' because *there is nothing to understand*. For if it is 'undecidable' it is not a proposition. (Hacker 1986: 127)

Wittgenstein's claim that where the Law of Excluded Middle does not hold no other law of logic holds appears to stem from the fundamental belief in the TLP that it is of the

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<sup>1</sup> This description is taken from (Brouwer 1923: 337).

nature of any proposition to be either true or false: if a sign has a sense, it therefore determines a possibility which the facts either satisfy or not. From this Hacker concludes that Wittgenstein was saying that the propositions for which the Law of Excluded Middle does not hold are lacking sense, and that constitutes in itself an argument against the rejection of the Law of excluded Middle. This is clearly a misinterpretation of Wittgenstein's remarks.

Indeed, Wittgenstein's insistence on the universal validity of the Law of Excluded Middle appears, at first sight, to be linked with his early belief—as early as NB—in the “bipolarity of the proposition”, i.e. that the propositions are capable of being true *and* of being false (Hacker 1986: 32). But, *Wittgenstein always considered it a peculiarity of mathematical “pseudo-propositions” that they are not bipolar*.<sup>1</sup> Indeed, there is bipolarity if one is able to imagine a situation where the contrary of a given proposition obtains. Wittgenstein claimed at the time of PR that there is no such thing in arithmetic, because one cannot imagine that the contrary of an arithmetical equation obtains. Negation is therefore different in mathematics:

Negation in arithmetic cannot be the same as the negation of a proposition, since otherwise, in  $2 \times 2 \neq 5$ , I should have to make myself a picture of how it would be for  $2 \times 2$  to be 5. (PR: § 203)<sup>2</sup>

*This is precisely linked with Wittgenstein's claim, originating in TLP but still holding at the time of PR, that even simple arithmetical equations, not just universally quantified propositions, “aren't a kind of proposition”:*

It seems clear that negation means something different in arithmetic from what it means in the rest of language. If I say 7 is not divisible by 3, then I can't even make a picture of this, I can't imagine how it would be if 7 were divisible by 3. All this follows naturally from the fact that mathematical equations aren't a kind of proposition. (PR: § 200)<sup>3</sup>

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<sup>1</sup> Hacker also admits that Wittgenstein recognized that mathematical propositions are not bipolar (Hacker 1986: 133).

<sup>2</sup> See also (PR: § 202): “It is quite clear that negation in arithmetic is completely different from the genuine negation of a proposition.”

<sup>3</sup> Wittgenstein's choice of vocabulary (i.e. “make a picture” and “imagine”) is rather unfortunate with its psychologistic flavor.

As I take it to be, Wittgenstein's point, at the time he wrote *PR*, is that arithmetical propositions such as those involving unrestricted universal quantification over the natural numbers—to keep to this case—are not statements assessable as true or false. *Since Wittgenstein agreed that the propositional connectives are to be defined by (two-valued) truth-tables, he did not consider it possible that one could subject these propositions to such connectives, if they are to be meaningful statements.* In other words, *such arithmetical propositions are not genuine statements.* This is why Wittgenstein said in (*PR*: § 151) that in the cases where the Law of Excluded Middle fails to apply to a proposition, “no other law of logic applies either”.<sup>1</sup>

Far from being a rejection of Brouwer's criticisms, this line of thought runs parallel to it. For Brouwer mathematical constructions are mental constructions imperfectly expressed in language, and this was for him good reason to be suspicious of the validity of the laws of logic such as the Law of Excluded Middle. According to Wittgenstein, some arithmetical propositions are not genuine statements<sup>2</sup> and this was the reason for claiming that the laws of logic do not hold for these.

Although some passages in *PR* might lead us —if, like P. Hacker, we interpret them erroneously— to believe that Wittgenstein rejected straightforwardly any proposition for which the Law of Excluded Middle does not hold as not being genuine mathematical

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<sup>1</sup> One can find other examples of such “pseudo-propositions” outside mathematics. Indicative conditionals are a good example. Dummett recently pointed out that they should be construed as expressing claims (Dummett 1990), and that they are as such justified or not, but not true or false. This explains why philosophers who tried in the past to give a truth-functional account of indicative conditionals ran into all sorts of trouble.

<sup>2</sup> Wittgenstein would not claim that they are expressions of mental constructions! But this largely agreed upon affirmation is somewhat controversial. Consider this passage about mathematical problems: “But it isn't like that: The difficult mathematical problems are those for whose solution we don't yet possess a *written* system. The mathematician who is looking for a solution then has a system in some sort of psychic symbolism, in images, ‘in his head’, and endeavours to get it down on paper. Once that's done, the rest is easy. But if he has *no kind* of system, either in written or unwritten symbols, then he can't *search* for a solution either, but at best can grope around.” (*PR*: § 151) Here Wittgenstein definitely speaks of images in the head of the mathematician, of a “psychic symbolism.” This is dangerously close to the intuitionistic idea of mathematical constructions as mental images.



propositions, I wish to point out that he was also much more careful in later formulations.

He was recorded by Moore as implying that these propositions must have a sense:

... he expressly said that though the words (1) 'There are five consecutive 7's in the first thousand digits of  $\pi$ ' have sense, yet the words (2) 'There are five consecutive 7's *somewhere* in the development' have none, adding that 'we can't say that (2) makes sense because (2) follows from (1)'. But in the very next lecture he seemed to have changed his view on this point, since he there said 'We ought not to say "there are five 7's in the development" have no sense', having previously said 'It has whatever sense its grammar allows', and having emphasized that 'it has a very curious grammar' since 'it is compatible with there not being five consecutive 7's in any development you can give'. If it has a sense, although a 'very curious' one, it does presumably express a proposition to which the Law of Excluded Middle and the other rules of Formal Logic do apply; but Wittgenstein said nothing on this point. (M: 303)

Wittgenstein also wrote in PG that one could perfectly well conceive a system where the Law of Excluded Middle does not hold:

The word "proposition", if it is to have any meaning at all here, is equivalent to a calculus: to a calculus in which  $p \vee \sim p$  is a tautology (in which the "law of the excluded middle" holds). When it is supposed not to hold, we have altered the concept of proposition. But that does not mean we have made a discovery (found something that is a proposition and yet doesn't obey such and such a law); it means we have made a new stipulation, or set up a new game. (PG: 368)

Wittgenstein agrees here that one can "set up a new game", i.e. a logical system in which the Law of Excluded Middle does not hold, *provided one does not interpret this move as a discovery of some kind but as a modification of the concept of proposition.*<sup>1</sup> I do not see, therefore, any theoretical objection to A. Heyting's formalization of intuitionistic logic.

There was an argument directed against Brouwer, however. Wittgenstein sensed in the intuitionist's discourse a remnant of the descriptivist picture which he disliked so much. It is one of Wittgenstein's major arguments: Brouwer spoke as if he just

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<sup>1</sup> This is also the gist of the following passage, in which, incidentally, *he does not deny that Brouwer has made an important discovery*: "If a logic is made up in which the law of excluded middle does not hold, there is no reason for calling the substituted expressions propositions. Brouwer has actually discovered something which it is misleading to call a proposition. He has not discovered a proposition, but something having the appearance of a proposition" (AWL: 140).

discovered some special fact about a certain class of propositions, in the same way as a physicist would speak of nature. As early as **PR**, Wittgenstein wrote:

If someone says (as Brouwer does) that for  $(x).f_1x = f_2x$ , there is, as well as yes and no, also the case of undecidability, this implies that ' $(x)...$ ' is meant extensionally and we may talk of the case in which all  $x$  happen to have a property. In truth, however, it's impossible to talk of such a case at all and the ' $(x)...$ ' in arithmetic cannot be taken extensionally. (PR: § 174)

Or, further on:

Of course, if mathematics were the natural science of infinite extensions of which we can never have exhaustive knowledge, then a question that was in principle undecidable would certainly be conceivable. (PR: § 174)

In the Cambridge lectures of 1934-35, he said:

To say the law of excluded middle does not hold for propositions about infinite classes is like saying: "In this stratum of atmosphere Boyle's law does not hold". (AWL: 140)

Finally, I shall cite a long but instructive passage of **PG** where Wittgenstein clearly agreed with the spirit of Brouwer's critique, but not its specifics:

When Brouwer attacks the application of the law of excluded middle in mathematics, he is right in so far as he is directing his attack against a process analogous to the proof of empirical propositions. In mathematics you can never prove something like *this*: I saw two apples lying on the table, and now there is only *one* there, so A has eaten an apple. That is, you can't by excluding certain possibilities prove a new one which isn't already contained in the exclusion because of the rules we have laid down. To that extent there are no genuine alternatives in mathematics. If mathematics was the investigation of empirically given aggregates, one could use the exclusion of a part to describe what was not excluded and in that case the non-excluded part would not be equivalent to the exclusion of the others.

The whole approach that if a proposition is valid for one region of mathematics it need not necessarily be valid for a second region as well, is quite out of place in mathematics, is completely contrary to its essence. Although many authors hold just this approach to be particularly subtle and to combat prejudice. (PG: 458)

In all these quotations, Wittgenstein appears to make the claim, especially clearly presented in the last paragraph of the last quotation, that one cannot make a distinction between finite and infinite classes and then reject the Law of Excluded Middle only for the latter classes. If it is so, and this is rather sad to admit, it strikes me as a completely absurd claim to make. Indeed, most of Wittgenstein's later philosophy of mathematics is

built on the “grammatical” distinction between finite sequences and infinite series. But this is precisely a distinction that runs parallel to Brouwer’s. Wittgenstein’s claim here would boil down to saying that one cannot recognize any substantial differences in the “grammar” of finite sequences and infinite series. If it is so, why bother with the distinction in the first place? I do not wish to find Wittgenstein guilty of such a gross inconsistency. In a more charitable way, one can read these quotations as containing an argument against what Wittgenstein saw as the reintroduction, as it were by the back door, of the descriptivist viewpoint in Brouwer’s arguments against the Law of Excluded Middle. Wittgenstein is arguing against the form of Brouwer’s arguments, against his “sales-pitch”.

Even so, it appears to me that Wittgenstein clearly misconstrued Brouwer’s critique by describing it as implying the descriptivist viewpoint since the point of Brouwer’s counterexamples, such as the *duale Pendelzahl*, is precisely to avoid it. Brouwer’s ultimate intention was to show that incompletely defined objects, such as random real numbers in classical analysis, or intuitionistic lawless or choice sequences, require another underlying logic. But the *duale Pendelzahl* is a perfect counterexample since it is given by a constructive function —the trick is that it is given in such way that nobody can determine from the rule only if the number  $r$  is equal or not to 0. There is no underlying descriptivism here.<sup>1</sup>

This is an important point. In the early thirties, Wittgenstein was thinking in terms of a *Satzsystem*, within which every problem would be decidable by an algorithm (Frasquolla 1984: 299). The conceptions of the TLP gave place to a verificationism according to which the meaning of a proposition is its method of verification:

How a proposition is verified is what it says. ...

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<sup>1</sup> It is rather sad that able commentators such as P. Hacker and J. Bouveresse not only did not see that Wittgenstein’s critique of Brouwer is ineffective, but on the contrary take it for granted (Hacker 1986: 124-125, 332) (Bouveresse 1988: 103-105).

The verification is not *one* token of the truth, it is *the* sense of the proposition. (PR: § 166) (PG: 458-459)

The distinction between genuine propositions and pseudo-propositions slowly disappears at this stage. Mathematical propositions became genuine propositions in PG, but only if a method to decide their truth, i.e. an algorithm, exists:

The method of checking the truth corresponds to the sense of a mathematical proposition. If it's impossible to speak of such a check, then the analogy between "mathematical proposition" and the other things we call propositions collapses. (PG: 366)

One must understand the requirement of a "method of checking" as the requirement that one possesses an effective method to arrive at a decision. Wittgenstein is asking that all questions expressible within a given system be solvable by such methods. But, still, these remarks of Wittgenstein definitely do not imply that he defended the universal validity of the Law of Excluded Middle against the attacks of the intuitionists. In his argument, Hacker is ignoring the crucial connection between this set of remarks and the remarks about mathematical problems or conjectures such as Fermat's Last Theorem or Goldbach's Conjecture. Such a connection definitively indicates that there is nothing "negative" associated to the lack of a method to arrive at a decision. Indeed, at the same time Wittgenstein held that the propositions for which the Law of Excluded Middle does not hold were not genuine statements, he also held that mathematical conjectures were also lacking sense:

I said: Where you can't look for an answer, you can't ask either, and that means: Where there's no logical method for finding a solution, the question doesn't make sense either.

Only where there's a method of solution is there a problem (of course that doesn't mean 'Only when the solution has been found is there a problem'). (PR: § 149)

Thus Fermat's proposition makes no *sense* until I can *search* for a solution to the equation in cardinal numbers. (PR: § 150)

Mathematical conjectures are "signposts" (PR: § 150) for new mathematical enquiries, and therefore not lacking sense, just as much as the propositions for which the Law of Excluded Middle does not hold were not devoid of sense:

The proposition with its proof doesn't belong to the same category as the proposition without the proof. (Unproved mathematical propositions—signposts for mathematical investigation, stimuli to mathematical constructions.) (PG: 371)<sup>1</sup>

They have as much sense as their expression in prose has, and they receive their sense from their proof:

Why do I say that we don't discover a proposition like the fundamental theorem of algebra, and that we merely construct it?—Because in proving it we give it a new sense that it didn't have before. Before the so-called proof there was only a rough pattern of that sense in the world-language. (PG: 374)

You can see the misleading way in which the mode of expression of world-language represents the sense of mathematical propositions if you call to mind the multiplicity of a mathematical proof and consider that the proof belongs to the *sense* of the proved proposition, i.e. determines that sense. (PG: 375)

The connection between mathematical conjectures and Brouwerian examples about the appearance of the sequence “five consecutive 7's” in the decimal expansion of  $\pi$  was explicitly made by Wittgenstein in his lectures, as recorded by Moore:

What he did say was that ‘All big mathematical problems are of the nature of “Are there five consecutive 7's in the development of  $\pi$ ?”’ and that they are therefore quite different from multiplication sums, and ‘not comparable in respect of difficulty’. (M: 303-304)

Wittgenstein was aware that he simply risked eliminating most mathematical problems of any interest: if one cannot find a solution to a particular conjecture from within existing systems then the conjecture wouldn't be recognized as a genuine mathematical statement any more. Mathematical propositions must have a sense before their proof is obtained:

“My explanation mustn't wipe out the existence of mathematical problems. That is to say, it isn't as if it were only certain that a mathematical proposition made sense when it (or its opposite) had been proved” (PR: § 148).

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<sup>1</sup> See also (PR: § 150). There are obvious parallels with the empiricism of I. Lakatos here (Lakatos 1974). But for Lakatos a proof renders a proposition only more convincing, while for Wittgenstein it gives it its sense. Moreover, for Lakatos mathematical discovery is on a par with scientific discovery. Such a view is totally inadmissible for Wittgenstein, for whom mathematics is a normative activity. See (Shanker 1986: 114-116).

His answer to this problem, although rather weak, helps us see the connection between mathematical conjectures and questions of the type “Do five consecutive 7’s appear in the decimal expansion of  $\pi$ ?”. In (PR: § 151), just after the passage quoted at the beginning of this section, Wittgenstein said:

Wouldn’t all this lead to the paradox that there are no difficult problems in mathematics, since, if anything is difficult, it isn’t a problem?  
But it isn’t like that: The difficult mathematical problems are those for whose solution we don’t yet possess a *written* system. (PR: § 151)

Much later, in **RFM**, the same idea reappears, but this time in connection with questions about the decimal expansion of  $\pi$ :

But does this mean that there is no such problem as: “Does the pattern  $\phi$  occur in this expansion?”?—To ask this is to ask for a rule regarding the occurrence of  $\phi$ . And the alternative of the existence or non-existence of such a rule is at any rate not a mathematical one.  
Only within a mathematical structure which has yet to be erected does the question allow of a *mathematical* decision, and at the same time become a demand for such a decision. (RFM: V, § 20)

The distinction between the sense of an unproved and a proved mathematical proposition is one of the greatest problems of Wittgenstein’s philosophy of mathematics. It indeed seems to imply that we have here two “theories” of meaning, i.e. that mathematical problems have a “minimal” meaning, to use A. Ambrose’s expression (Ambrose 1935: 322-323). Not only did Wittgenstein have to defend the validity of such a claim, he also had to develop the “theory” of the “minimal” meaning of unproved mathematical propositions. He didn’t. I shall not make such an attempt here. But it was important for our purpose that one sees the connection between Wittgenstein’s remarks on the Excluded Middle and those on mathematical problems or conjectures.

I pointed out in the last section the intuitionist critique of existence as the negation of universal quantification by Brouwer and Weyl (which was also taken over by Hilbert). I claimed that both Ramsey and Wittgenstein agreed with this critique. This was implied in Wittgenstein’s rejection of his early theory of the quantifiers as logical sums and products. Such a critique implies, as it is clear in the case of Hilbert’s metamathematics

for example, a limited rejection of the Law of Excluded Middle. Luckily, there exists a crucial passage where Wittgenstein, while commenting upon H. Weyl's paper "Die heutige Erkenntnislage in der mathematik" (Weyl 1925) to Schlick and Waismann, clarified the link between his rejection of quantification theory and his rejection of the Law of Excluded Middle in the most straightforward and clear manner :

A statement about *all* numbers is not represented by means of a proposition, but by means of induction. Induction, however, cannot be denied, nor can you affirm it, for it does not assert anything. Therefore, where there is a statement it can be negated; and where a certain structure cannot be negated, there is no statement either. The law of excluded middle however does not apply—simply because we are not dealing with propositions here. (WWK: 82)

In this crucial passage Wittgenstein condensed his thoughts on the foundations of mathematics: a universal statement, i.e. a statement about *all* numbers, is correctly expressed by an induction, not by of the form —this thought was well documented in the previous section. More precisely, the universal statement is not a *genuine statement* since it is expressed by an induction and not by " $\forall xP(x)$ ". Because we are therefore not dealing with genuine statements, i.e. it is not assessable as true or false, no negation is possible, and the Law of Excluded Middle does not hold —another thought documented in this section and the previous one.

I shall now indicate why Wittgenstein's analysis in RFM of Brouwer's typical example of the appearance of the pattern 0123456789 in the decimal expansion of  $\pi$  provided him also with enough ground for rejecting the Law of Excluded Middle. Brouwer's example is a formula of the type:

$$(1) \quad \exists x f(x) \vee \neg \exists x f(x)$$

with "x" ranging over an effectively enumerable infinite set (i.e. in Wittgenstein's terms an infinite series) and "f(x)" an effectively decidable predicate. An intuitionist would say here that one needs here to show either that we can effectively find an "x" such that "f(x)" or that we cannot find one. But there are things we can't say about certain decimal

expansions, such as that of  $\pi$ , unless the calculation up to the relevant point is already done, because there is no information given with the rule with which we are able to calculate the values of the decimal expansion of a number such as  $\pi$ . This was Wittgenstein's latest view in RFM:

To say of an unending series that it does *not* contain a particular pattern makes sense only under special conditions.

That is to say: this proposition has been given a sense for certain cases.

Roughly, for those where it is in the *rule* for the series, not to contain the pattern...

Further: when I calculate the expansion further, I am deriving new rules which the series obey. (RFM: V, § 11)

And a little bit further:

"But surely all members of the series from the 1st up to the 1,000th, up to the  $10^{10}$ -th and so on, are determined; so surely *all* the members are determined." That is correct if it is supposed to mean that it is not the case that e.g. the so-and-so-many'th is *not* determined. But you can see that *that* gives you no information about whether a particular pattern is going to appear in the series (if it has not appeared so far). *And so we can see that we are using a misleading picture.* (RFM: V, § 11)

It is precisely against this "misleading *picture*" that Wittgenstein gave the following warning in PI:

Here it happens that our thinking plays us a trick. We want, that is, to quote the law of excluded middle and to say: "Either such an image is in his mind, or it is not; there is no third possibility!"—We encounter this queer argument also in other regions of philosophy. "In the decimal expansion of  $\pi$  either the group "7777" occurs, or it does not—there is no third possibility." That is to say: "God sees—but we don't know." But what does that mean?—We use a picture; the picture of a visible series which one person sees the whole and another not. The law of excluded middle says here: It must either look like this, or like that. So it really—and this is a truism—says nothing at all, but gives us a picture. And the problem ought now to be: does reality accord with the picture or not? And this picture *seems* to determine what we have to do, what to look for, and how—but it does not do so, just because we do not know how it is to be applied. Here saying "There is no third possibility!"—expresses our inability to turn our eyes away from this picture: a picture which looks as if it must already contain both the problem and its solution, while all the time we feel that it is not so. (PI: § 352)

Wittgenstein's gave a new twist to his argument here: the Law of Excluded Middle says nothing but "gives us a picture", a misleading one. This picture is, of course, the



descriptivist picture of the expansion as already given to us —rather to God— against which the whole of his later philosophy of mathematics is devised.

One should easily understand why Wittgenstein would refuse to apply the Law of Excluded Middle to statements about the decimal expansion of  $\pi$ , as the intuitionists would do. First of all, *precisely because there is no such “picture” of an already given decimal expansion from which, had we had the powers of a God to survey it completely, we could tell if a segment appears or not.* Secondly, because nothing in the information given to us by the rule to produce the decimal expansion of  $\pi$  can help us answering a question about the appearance of a certain pattern in it. So, unless we have calculated as far as its appearance in the sequence, we can never tell if the pattern appears or not, therefore the Law of Excluded Middle does not apply.

These later remarks are tainted with Wittgenstein’s later preoccupation with rules. Their importance will be assessed at the end of section 16. I shall point out here that Wittgenstein already held a similar analysis of (1) during the transitional period. Indeed, Waismann recorded in 1929 Wittgenstein as saying:

There can be no such question as, Do the figures 0, 1, 2 ... 9 occur in  $\pi$ ? I can only ask if they occur at *one* particular point, or if they occur among the first 10, 000 figures. No expansion, however far it may go, can refute the statement ‘They do occur’—therefore this statement cannot be verified either. What is verified is an entirely different assertion, namely that this sequence occurs at *this or that point*. Hence you cannot affirm or deny such a statement, and therefore you cannot apply the law of the excluded middle to it. (WWK: 71)

During the academic year 1931-32, Wittgenstein was quoted as saying:

Will three consecutive sevens ever occur in an evaluation of  $\pi$ ? People have an idea that this is a problem because they think that if we knew the whole evaluation we should know, and the fact that we don’t know is merely a human weakness. This is a subterfuge. The mistake lies in the misuse of the word infinite, which is not the name of a numeral.  
“If we find that three consecutive sevens occur, then we have proved that they do; but if we don’t find them we still have not proved that they do

not.” This gives us no criterion for falsehood, but only for truth. (AWL: 107)<sup>1</sup>

And Moore also reported that Wittgenstein

... said that if anyone actually found three consecutive 7's this would prove that there are, but that if no one found them that wouldn't prove that there are not; that, therefore, it is something for the truth of which we have provided a test, but for the falsehood of which we have provided none; and that therefore it must be a quite different sort of thing from cases in which a test for both truth and falsehood is provided. (M: 303)

Here again we have Wittgenstein claiming that “ $\neg\neg\exists x f(x)$ ”, even during a period dominated with verificationism, and the demand that a test be provided for the truth or falsehood of every proposition.

One of the distinguishing features of Russian constructivism is Markov's Principle, which reads:

$$\forall x (A(x) \vee \neg A(x)) \wedge \neg \forall x \neg A(x) \rightarrow \exists x A(x)$$

or,

$$\forall x (A(x) \vee \neg A(x)) \wedge \neg\neg\exists x A(x) \rightarrow \exists x A(x)$$

for “ $A(x)$ ” any effectively decidable predicate. Markov's Principle is often interpreted by other constructivists as a reintroduction of the Excluded Middle. Since Wittgenstein never discussed any similar case, there is no ground to affirm would have agreed—or, for that matter, that he would disagree—with Markov's Principle. Such an agreement would have constituted another important distinction between him and Brouwer.

I think I have adduced sufficient evidence to conclude that Wittgenstein's remarks of the transitional and later periods imply a rejection of the unrestricted use of the Law of Excluded Middle. Therefore, one can now understand why already in PR Wittgenstein saw “something recalcitrant” to the application of the Law of Excluded Middle:

Now there is something recalcitrant to the application of the law of excluded middle in mathematics.

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<sup>1</sup> It is fitting to notice the connection made here by Wittgenstein with the debate over the notion of infinity, which was discussed in sections 10 to 12. Again, we find Wittgenstein rejecting a characterization of the debate in epistemological terms, i.e. in terms of human abilities.

(Of course even the name of this law is misleading: it always sounds as though this were the same sort of case as ‘A frog is either green or brown, there isn’t a third type’.) (PR: § 201)

One can also understand why he later said that there was “something wrong” with the insistence on the validity of the Law of Excluded Middle:

When someone hammers away at us with the law of excluded middle as something that cannot be gainsaid, it is clear that there is something wrong with his question.

When someone sets up the law of excluded middle, he is as it were putting two pictures before us to choose from, and saying that one must correspond to the fact. But what if it is questionable whether the pictures can be applied here? (RFM: V, § 10)

And why he said that it has a “shaky” sense:

In the law of excluded middle we think that we have already got something solid, something that at any rate cannot be called in doubt. Whereas in truth this tautology has just as shaky a sense (if I may put it like that), as the question whether  $p$  or  $\sim p$  is the case. (RFM: V, § 12)

Finally, a quotation from an unlikely source, the first volume of the **Remarks on the Philosophy of Psychology**:

The law of excluded middle does not say, as its form suggest: there are only these two possibilities, Yes and No, and no third one. But rather: “Yes” and “No” divide the field of possibilities into two parts. And that of course need not be so. (“Have you stopped beating your wife?”) (RPP I: § 274)

With such textual evidence, I would go farther than Dummett or Wright in their cautious judgements, and say that *Wittgenstein clearly rejected the universal validity of the Law of Excluded Middle*. The only distinction with Brouwerian intuitionism is in the arguments provided on both sides. Wittgenstein did not recognize the validity of Brouwer’s counterexamples such as his *duale Pendelzahl*. The reasons for this denial are linked with Wittgenstein’s prescriptions for the formation of real numbers, one of which is that real numbers must be effectively comparable to any rational number. I shall discuss this prescription in section 17. On the other hand, Wittgenstein’s analysis of proofs by induction and  $\Pi_1^0$ -statements, and his remarks about the decimal expansion of  $\pi$  provide him with enough ground for a rejection of the unrestricted use of the Law of Excluded Middle.

It must be said that Wittgenstein's approach forced him to recognize that propositions for which the Law of Excluded Middle does not hold have a different kind of —i.e. a “minimal”— meaning, and this lead him onto slippery ground. Moreover, his criticisms of Brouwer's critique, i.e. the claim that the descriptivist viewpoint creeps in by the back door in intuitionism, indicate that he hasn't understood it fully.

## VII. Surveyability

La qualité essentielle d'une démonstration est de forcer à croire, de sorte que ceux qui ne sentent pas cette force, ne sentent pas la démonstration même, c'est à dire qu'ils ne l'entendent pas.

P. Fermat

### 15. Surveyability

In order to characterize Wittgenstein as a strict finitist, one might try to describe him as preoccupied with “feasibility”. In fact, early commentators of **RFM** based their interpretations on an amalgamation of the strict finitist insistence on feasibility with Wittgenstein’s requirement that proofs must be surveyable. For example, H. Wang said:

Wittgenstein makes many cryptic observations ... which become understandable if we keep in mind his preoccupation with the conception of mathematics as a feasible activity. (Wang 1958: 474)<sup>1</sup>

Strict finitists such as Esenin-Volpin would claim that any extension is determinate if it is strictly finite, i.e. if it contains only feasible numbers. This lead to the idea that the length of natural number series changes with a change of notation, and therefore that the models of the natural numbers are not isomorphic. And this lead in turn to the abandonment of the principle of mathematical induction, and so on. This much was discussed in section 9. Nobody would claim that Wittgenstein actually held such views,<sup>2</sup> but the *related* view that, in Dummett’s words:

... the sense of an arithmetical predicate, e.g. ‘is prime’, is given, not by a method that may ‘in principle’ be used to decide its application, but by the criterion we accept in practice. (Dummett 1973: 506)

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<sup>1</sup> The passages given as textual evidence by Wang, such as the first paragraph of (RFM: III, §2) and the last of (RFM: III, §45), are on the topic of surveyability. In their reviews of **RFM**, M. Dummett and G. Kreisel also understood Wittgenstein as a strict finitist because of his requirement that proofs must be capable of being “taken in”; or because of his idea that a change of notation that renders an unsurveyable proof into a surveyable one amounts to give a proof while before there was none.

<sup>2</sup> Dummett was careful enough to explain how for Wittgenstein “this is not the correct way in which to draw the contrast” (Dummett 1959: 182).

But for any predicate there will always be a number too large for the practical application of any given criterion, because the computation needed would then be unsurveyable, no predicate has therefore a determinate sense for “all” natural numbers. This interpretation hinges on an interpretation of Wittgenstein’s remarks on the “surveyability” of proofs, which is in my opinion mistaken. I shall indicate my reasons to believe that Wittgenstein’s remarks do not support such an interpretation. Although there are substantial differences, my treatment of the notion of “surveyability” in Wittgenstein writings is close to C. Wright’s (Wright 1980: 118-123).

One can say without any exaggeration that Wittgenstein’s philosophy of mathematics consists first of all in a reply to empirism (J. S. Mill), and to logicism, which revives it, with the writings of B. Russell (RFM: VI, § 23). The aim of the long discussions of RFM on the notion of proof is to show proofs not to be akin to experiences. It is precisely because he wanted to distinguish as radically as possible proofs from experience that Wittgenstein insisted on their being “surveyable”:

“Proof must be capable of being taken in” really means nothing but: a proof is not an experiment. We do not accept the result of a proof because it results once, or because it often results. But we see in the proof the reason for saying that this *must* be the result. (RFM: III, § 39)

(see also: (RFM: I, § 80; III, § 55; IV, § 41; VII, § 9, 18)). This requirement of surveyability must not be confused with the similar sounding but narrower requirement made by D. Hilbert. In a formalist spirit, Hilbert asked that we conceive mathematical proofs as arrays of strings of symbols, given in the spatio-temporal frame, for public inspection and ratification (Hilbert 1925: 381-2). Hilbert put the emphasis in his proof theory on the intuitive correctness of the concepts and principles used in the proofs. The more *simple* they are, the more *correct* they are. Therefore, Hilbert restricted the methods of his metamathematical investigations on proofs to the domain of finitism. This is to be contrasted to the simplicity of the “structure” of the proof, e.g. its length. Indeed, the use of finitist means often increases the length of the proof, rendering it more difficult to

understand. I agree with G. Kreisel, who insisted that Wittgenstein's *Übersichtlichkeit* should be interpreted in a broader sense than Hilbert's requirement (Kreisel 1984: 88). Wittgenstein used most frequently the German terms *übersehbar* and *übersichtlich*, usually translated by "surveyable". But he also used the expression *einprägsames Bild* (RFM : I, §80; III, § 9), which means "memorable", or "easy to take in". It is seldom mentioned that he also used the adjectives *anschaulich* (RFM : III, § 42), or "intuitive"; and *durchsichtig* (RFM : App. II, § 8), or "clear", "transparent".

We can well see that by the choice of his terms, Wittgenstein wanted to introduce a broader notion than Hilbert's: something like simplicity of structure is sought. Wittgenstein was asking is that proofs be simple enough to be taken in at a glance.<sup>1</sup> On the other hand, it should be clear from what follows that Wittgenstein isn't interested in the even broader but more vague aesthetic requirement of simplicity. Classical mathematicians sometimes insist on the fact that classical proofs often have a simplicity and *élégance* not to be found in their constructive equivalent. This is not the distinction stressed by Hilbert, nor by Wittgenstein.

In all due respect to Hilbert, it should be mentionned *en passant* that he spoke sometimes in terms quite similar to those of Wittgenstein, especially in relation to his first proof of the existence of a complete system of invariants for every algebraic form.<sup>2</sup> Although the proof was evidently non-constructive, Hilbert considered it as "satisfying our demand for simplicity and clarity" (Hilbert own words were, respectively, *Einfachheit* and *Durchsichtigkeit*) (Hilbert 1918: 154). This shows that when Hilbert spoke of "a criterion for the simplicity of mathematical proofs" (Hilbert 1918: 153),\* he had in mind not the simplicity of the concepts and principles in use, as in other more typical passages,

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<sup>1</sup> I think that S. Kripke is therefore mistaken if he is indeed interpreting Wittgenstein as asking, like Hilbert, that proofs be "visible (or audible, or palpable), concrete phenomena", that they be "short and clear enough for me to be able to judge of another person's proof whether I too would regard it as a proof" (Kripke 1982: 106).

<sup>2</sup> There is a brief discussion of this proof and objections to it in section 2.

but the simplicity of their “structure”. This is closer in spirit to Wittgenstein and to the theory of proofs of G. Kreisel and R. Statman (Kreisel 1976b, 1977)(Statman 1974).

According to Wittgenstein, that a proof must be surveyable also means that it should be a figure whose exact reproduction can be certain (RFM: III, § 1) (LFM: 37). He is therefore asking that the proof must be reproducible. This amounts to asking for a practically decidable concept of identity for proofs:

But the question is what is to count as the criterion for the reproduction of a proof—for the identity of proofs. How are they to be compared to establish the identity? Are they the same if they look the same? (RFM: III, §4)

This requirement has two different senses, depending on what is to be reproduced, i.e. the mere form (structure), or the reasoning. Indeed, Wittgenstein talks sometimes of the reproduction of a proof as if it is only a question of being able to write it out. Thus: “It must be easy to write down *exactly* this proof again” (RFM: III, §1). Or: “A proof must be capable of being reproduced by mere copying” (RFM: IV, §41). But he sometimes meant more than mere structural reproducibility, i.e. reproducibility in *essentials*. Wittgenstein was pointing not at mere copying of the proof but more: he asked that one also understands how the proof succeeds in convincing of its conclusion, i.e. he asked that one grasps its argumentation in totality in order to reproduce it:

A proof shews us what OUGHT to come out.—And since every reproduction of the proof must demonstrate the same thing, while on the one hand it must reproduce the result automatically, on the other hand it must also reproduce the *compulsion* to get it. (RFM: III, §55)

Wittgenstein was therefore also asking for identity in reasoning. It is not only a matter of being able to recognize that it is correct, but to grasp its reasoning *in toto*: it is indeed possible that one check all the steps one by one and see that they are correct, that there is no error, and still not understand how the proof works —therefore if there is an error one wouldn’t know what to do— while with a proper grasp of the reasoning employed, should there be an error one would know how to remedy it and go on with the



proof.<sup>1</sup> This requirement is definitely stronger than asking for mere structural reproducibility, because, as Wittgenstein would say, “the pattern is not the proof” (RFM: III, §12). It is indeed more difficult to see the collective soundness of many individual steps than to see that they have been correctly copied.

When Wittgenstein associates surveyability with reproducibility, it is a matter of the reproduction of the essential, of the reasoning. We can well see that formal reproducibility reduces to this stronger requirement:

‘A mathematical proof must be perspicuous’. Only a structure whose reproduction is an easy task is called a “proof”. It must be possible to decide with certainty whether we really have the same proof twice over or not. The proof must be a configuration whose exact reproduction can be certain. Or again: we must be sure we can exactly reproduce what is essential to the proof. It may for example be written down in two different handwritings or colours. What goes to make the reproduction of a proof is not anything like an exact reproduction of a shade of colour or a handwriting. (RFM: III, §1)

The requirement that we understand the structure of the proof is linked with the idea that an understanding of the mere verbal form, which would come from following the proof step by step without understanding how it works, doesn’t insure a proper understanding. Here I must quote again the following passage, previously cited in section 2:

Everything that I say really amounts to this, that one can know a proof thoroughly and follow it step by step, and yet at the same time not understand what it was that was proved.  
And in turn this is connected with the fact that one can form a mathematical proposition in a grammatically correct way without understanding its meaning. (RFM: V, § 25)

The requirement that the proof must be reproducible in its essentials goes along very well with the requirement that it has to be an image easy to understand (*einprägsames Bild*), and that we can see through it (*durchsichtig*). We can also see the anti-empirist motivation: If the proof consists in a reasoning easy to take in and to reproduce, it looses

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<sup>1</sup> Bourbaki made a similar claim: a proof isn’t understood if one just checks the inferences step by step “...without trying to conceive clearly the ideas leading to the construction of this chain of deductions instead of any other” (Bourbaki 1948: 37).\*

all causal character. If the proof was to be identified with experience, one would lose the compulsion to get the result, in which case the fact that the same result obtains would only be luck.

The best and most often mentioned example of a surveyable proof, certainly easy to take in, which ranges over the infinite sequence of natural numbers is Euclid's proof of his theorem on the infinity of prime numbers (*Elements*, Book IX, Proposition 20). It says:

*Prime numbers are more than any assigned multitude of prime numbers*

It is fitting to remark here that *Euclid did not use the notion of infinite in the formulation of his theorem!* The proof is easy. We start by supposing that there are a finite number of prime numbers, and that they form the sequence  $P_1, P_2, P_3, \dots, P_n$ . We define :

$$N = P_1 \cdot P_2 \cdot P_3 \cdot \dots \cdot P_n + 1$$

The number  $N$  is either prime, or composite. If it is prime, then we have a contradiction, since it would be bigger than all the prime numbers  $< n$ , and there would be more than  $n$  prime numbers.

If it is composite, it must be divisible (exactly) by a prime number. But this prime divisor cannot be  $P_1$ , nor  $P_2$ , ..., nor  $P_n$ , because they would all leave a remainder of 1. Therefore there must be another prime number, bigger than  $n$ . This again contradicts our initial hypothesis QED.

## 16. Dummett's Interpretation

It should be apparent by now that my reading of Wittgenstein is in contradiction with M. Dummett's. Let me now discuss his reading.

Following M. Dummett (and C. Wright, for that matter), one might be tempted to interpret Wittgenstein's overall strategy as trying to introduce sceptical doubts —of the strict finitist kind— about our conception of calculation in arithmetic in an effort to undermine our confidence in its certainty.<sup>1</sup> Dummett took the example of the sieve of Eratosthenes.<sup>2</sup> According to him, Wittgenstein would not admit that we have only one method to decide for all integers if they are prime or composite. Dummett considers the following case of a fanatic devoting his life to computing the primality of a very large number by means of the sieve of Eratosthenes, while it has already been proved prime by a more powerful method. If the fanatic's result is that the number in question is composite, our reaction, according to Dummett, would be that we would not give up our result, but rather claim that there must be an error in the fanatic's computation. This purports to show that:

... we are taking the "advanced" test, and not the sieve, as the *criterion* for primality here: we use the theorem as the standard whereby we judge the computation, and not conversely. The computation is of no use to us because it is not *surveyable*. (Dummett 1959: 180)

First of all, one must here distinguish between those cases where the sieve can and those where it can't be applied, because it requires too many steps (and we are limited physically). One can also distinguish, within the limits of possible applications of the sieve, between those that are surveyable from those that aren't. Dummett and Wright are interested in the former case, that of unsurveyable but still possible computations:

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<sup>1</sup> If we follow C. Wright, we see that this interpretation is made in conjunction with a so-called "sceptical" interpretation (close to S. Kripke's) of the rule-following considerations (PI : § 138-242).

<sup>2</sup> This method was conceived in order to establish tables of prime numbers: take any integer  $z \geq 2$ , and consider all the integers  $n$  in the interval

$$I_z = [z, z^2)$$

If we take out of  $I_z$  all the multiples of 2, 3, 5, ...and so on for all the prime numbers  $< z$ ; then all the integers of  $I_z$  surviving the sifting are prime numbers. Indeed, if  $n$  is an integer surviving the sifting and with at least two prime factors, then  $n$  would be at least as large as  $z^2$ , and would lie outside  $I_z$ . If we take the first  $n$  prime numbers:  $P_1, P_2, \dots, P_n$ ; this sieve enables us to give the list of all prime numbers up to  $(P_n + 2)^2$ . The sieve by itself cannot help us to prove the infinity of prime numbers, unless we can obtain a lower positive bound (with  $z \rightarrow \infty$ ) to the number of elements surviving the sifting. This is quite an effective means of obtaining prime numbers, but there are stronger (analytic) sieves, those of Brun, Selberg and Linnik & Renyi. For more details on these, see (Bombieri 1974).

Suppose we are concerned with numbers to which the application of the sieve has become unsurveyably but not *impossibly* long; that is, we are in a region of integers where we keep getting different results to each other, keep turning up errors in our own calculations, and where successive checks keep on giving different verdicts. (Wright 1980a: 126)

If the sieve of Eratosthenes is inadequate in some cases because the computations are unsurveyable, there are stronger methods to be used —there are many analytic sieves, such as Selberg's (Selberg 1952). As Dummett noticed, in those cases we will base our judgements on the stronger method and its surveyable result. But, for any new method adopted there will always be a number so large that the application of the new method will be unsurveyable. Therefore, according to Dummett's Wittgenstein "we should have no right to assert that every number is either prime or composite" (Dummett 1959: 181).

There is one passage of the LFM where Wittgenstein and Turing discuss the distinction between calculation and experiment, taking the example of multiplication and where Wittgenstein considered a case exactly similar to the one described by Wright:

Suppose that we make enormous multiplications—numerals with a thousand digits. Suppose that after a certain point, the results people get deviate from each other. There is no way of preventing this deviation; even when we check their results, the results deviate. (LFM: 101)

Wittgenstein immediately asks:

What would be the right result? Would anyone have found it? Would there be a right result? (LFM: 101)

To this last question, Dummett and Wright would be inclined to think that Wittgenstein would answer: No. But this isn't Wittgenstein's actual answer:

I should say, "This has ceased to be a calculation" (LFM: 101)

There is no discussion of a similar case of unsurveyable but not impossibly long calculations in RFM, apart the following passage, that I quote *in extenso*, where Wittgenstein gave the same answer:

If a calculation is an experiment and the *conditions are fulfilled*, then we must accept whatever comes, as the result; and if the calculation is an experiment then the proposition that it yields such and such a result is after all the proposition that under such and such conditions this kind of sign makes its appearance. And if under these conditions one result appears at

one time and another at another, we have no right to say “there’s something wrong here” or “both calculations cannot be all right”, but we should have to say: this calculation does not always yield the same result (*why* need not be known). But although the procedure is now just as interesting, perhaps even more interesting, what we have here *now* is no longer calculation. And this is of course a grammatical remark about the use of the word “calculation.” And this grammar has of course a point. What does it mean to reach *understanding* about a difference in the result of a calculation? It surely means to arrive at a calculation that is free of discrepancy. And if we can’t reach an understanding, then the one cannot say that the other is calculating too, only with different results. (RFM : VII, § 9)

According to Wittgenstein, if a calculation is unsurveyable it loses its character of being a proof, and becomes an experiment. It cannot therefore be properly called a calculation. So the case of a very long unsurveyable but still possible multiplication—or of a similarly complex application of the sieve—on which Dummett and Wright are resting their case would not even be recognized as a calculation by Wittgenstein. To say that when calculations with the sieve are unsurveyably long, they aren’t to be considered to be calculations any more doesn’t amount to say that there are cases where there is no correct application of the predicate “prime number”. It amounts only to saying that one shouldn’t conceive of such cases as counterexamples to the proposition: every integer is either prime or composite. After all, we aren’t dealing with calculations here: we are outside the domain of mathematics —we are missing a main ingredient of the language-game: agreement.

All this may be said to be rather unconvincing, however, because Dummett could still reply that in saying that an unsurveyably long calculation isn’t really a calculation, Wittgenstein implies that in that case there is no right answer. And if this is what Wittgenstein implies, then Dummett has a case.

Dummett’s reading turns out to rest on the following passage, from the following lecture, where Wittgenstein comes back to the same topic:

“For us human beings, the best thing we can arrive at, the nearest we can get, is that we always get it, or someone who had a lot of experience always got it.” As if only a God really knew.—Turing suggested this, and that is just where he and I differ. Actually there is nothing to stop us postulating that your result is right—so that in future all your children will

have to copy what is written on the blackboard. And then it is right.—  
There is nothing there for a higher intelligence to know—except what  
future generations will do. We know as much as God does in  
mathematics.(LFM: 103-104)<sup>1</sup>

This passage seems to confirm Dummett's reading, since Wittgenstein claims here that "there is nothing to stop us postulating that your result is right", and this in turn implies that according to him there is no right answer (Dummett 1978b: 65).

We seem to have two conflicting readings, both supported by textual evidence. Against Dummett's reading one could say that the only passage supporting it is a recording of what Wittgenstein apparently said in a lecture. We could also cut the Gordian knot by saying that there is no reason to think that Wittgenstein was consistent. But, I think this would be inappropriate. On the contrary, it is certainly better to assume him consistent if we want to make a decent job of understanding him. But maybe one can read the controversial passage (LFM: 103-104) differently than Dummett. A few things could be said here.

First of all, merely rejecting the Platonist's appeal to God—who knows the answer when we can't "medically" obtain it—is not enough to qualify Wittgenstein as a strict finitist, since this is also an intuitionistic criticism. Therefore, saying that there is nothing for a God to know that we don't know in mathematics is not something sufficient to distinguish Wittgenstein from the intuitionist. There is a bound to the complexity of computations we can in practice carry out. This is a fact of our mathematical practice, but the strict finitist wants to limit mathematics to this domain. The Platonist, defending the integrity of classical mathematics, would reply to the strict finitist that these so-called practical limitations are merely "medical" and not "logical." He would reject them as irrelevant, arguing that we can conceive of a God who could do what we can't. If I am

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<sup>1</sup> In conversation Prof. Dummett told me that he had in mind this passage which he knew from a copy of the Bosanquet set of notes of the 1939 lectures when he wrote his famous review of RFM. For my part, I was unable to find any other passage supporting unequivocally his interpretation.

right, Wittgenstein wants to reject this whole characterization as wrong-headed, not to take sides. Doesn't he say later on in the same lectures:

... we want to see the absurdities both of what the finitists say and of what their opponents say. (LFM: 111)

There is an interesting passage in the second half of **PI** in which Wittgenstein discusses the possibility of a dispute over the result of a long calculation. He first points out that these disputes are brief and easily settled—an indication that the emphasis put by Dummett on this aspect is rather alien to Wittgenstein:

There can be a dispute over the correct result of a calculation (say a rather long addition). But such disputes are rare and of short duration. They can be decided, as we say, 'with certainty'.  
Mathematicians do not in general quarrel over the result of a calculation. (This is an important fact.)—If it were otherwise, if for instance one mathematician was convinced that a figure had altered unperceived, or that his or someone else's memory had been deceived, and so on—then our concept of 'mathematical certainty' would not exist. (PI: p.225)<sup>1</sup>

But then, in his usual fashion, Wittgenstein let his imaginary opponent speak:

Even then it might always be said: "True we can never *know* what the result of a calculation is, but for all it always has a quite definite result. (God knows it.) Mathematics is indeed of the highest certainty—though we only have a crude reflection of it." (PI: p.225-226)

We know, of course, that Wittgenstein would claim against his imaginary opponent that in mathematics there is nothing for a God to know that we don't know.<sup>2</sup> Therefore,

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<sup>1</sup> See also (RFM: I, § 35).

<sup>2</sup> See our previous quotation (LFM: 103-104), and, *à propos* of the decimal expansion of  $\pi$ : "Even God can determine something mathematical only by mathematics. Even for him the mere rule of expansion cannot decide anything that it does not decide for us." (RFM: VII, § 41) Dummett would be inclined to think that Wittgenstein is here claiming that there is nothing outside the domain of the humanly feasible which would be known only by a God, and that therefore whenever we reach the domain of the unsurveyable any new application of the rule calls for a decision because there is no right answer. Wittgenstein's ambiguous expression could be interpreted differently, i.e. as the expression of his rejection of the viewpoint of the actual infinite of set theory. The following quotation from the Cambridge lectures could also be interpreted in this way: "You can ask what is the number of men or chairs, etc. in a group. You know how to get to know the number. But you cannot ask what is the number of cardinal numbers or points on a line. What would you do to find out? There is no number of them which God knows and we endeavour to find. There is only the general law which leads to their production" (LWL: 108). This does not imply a restriction to the domain of the feasible, outside of which there is no right answer. It is only the typical constructivist complaint against the argument for transfinite numbers. Indeed, to take the example of a well-known constructivist, E. Bishop would say: "If God has mathematics of his own that needs to be done, let him do it himself" (Bishop 1967: 2). For other remarks

he would simply reject the sentence put between inverted commas as the expression of a (Platonistic) confusion. This would leave him, according to Dummett's interpretation, only with the strict finitist alternative. But I tried to argue that there was a way out. Wittgenstein sensed the trap too and he avoided it with remarkable agility :

But am I trying to say some such thing as that the certainty of mathematics is based on the reliability of ink and paper? *No.* (That would be a vicious circle.) —I have not said *why* mathematicians do not quarrel, but only *that* they don't. (PI: p.226)

I take it that since Wittgenstein already avoided an epistemological characterization of the distinction between the finite and the infinite, he was not interested with the ensuing problem of the very long calculations. Moreover, it may be true that it is a fact of our mathematical practice that we are limited on the length of computations we can achieve — with relative, if any, certainty— but it is also true of our mathematical practice that mathematicians rarely quarrel over results and that such disputes are usually rapidly settled. Wittgenstein did not say more than that. This is the so-called “given”, the aspect of the *forms of life* to which his analysis regresses. Again, Wittgenstein seems to me to be avoiding at all costs the epistemological debate.

My second point is that when he said that “nothing can stop us postulating that your result is right”, Wittgenstein was not saying, as the strict finitist would, that there is no right answer for these unsurveyable calculations. I think it is the thrust of his remarks on “following a rule” that the result of applying the rule at each new step is not a matter of a decision.

It is part of the usual interpretation of the rule-following argument that our rule-governed responses are “informed not by an *intuition* (of the requirements of the rule) but a kind of *decision*” (Wright 1989: 240). Indeed, it is quite clear from, say, (PI: § 213) that Wittgenstein rejects any explanation of our rule-governed behavior in terms of

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about God and mathematics, see (PR: § 174) and especially (PI: § 426) where set theory is clearly rejected.



“intuition”. But he does not agree that an explanation in terms of decision is correct. In (PI: § 186) Wittgenstein did say (my italics): “*It would almost be more correct to say*, not that an intuition was needed at every stage, but that a new decision was needed at every stage.” This does not imply that it *is* correct to say that a “new decision was needed.” In A. Ambrose’s lecture notes, Wittgenstein said: “If any mental process is involved, it is one of decision, not of intuition” (AWL: 134). The point of the rule-following argument is precisely to show that explanations in terms of “mental processes” are deficient. This is precisely the reason why in the 1939 lectures Wittgenstein said of intuitionism that it was “all bosh”:

Intuitionism comes to saying that you can make a new rule at each point. It requires that we have an intuition at each step in calculation, at each application of the rule; for how can we tell how a rule which has been used for fourteen steps applies at the fifteenth?—And they go on to say that the series of cardinal numbers is known to us by a ground-intuition—that is, we know at each step what the operation of adding 1 will give. We might as well say that we need, not an intuition at each step, but a *decision*.—Actually there is neither. You don’t make a decision: you simply do a certain thing. It is a question of a certain practice.  
Intuitionism is all bosh—entirely. (LFM : 237)<sup>1</sup>

It should be clear that according to Wittgenstein in the case of previously uneffected calculations the result is never something that we postulate or decide. Now, if Wittgenstein argued that each new step in using a rule does not require a decision, was there still a right result beforehand according to him? I think that Wittgenstein’s answer was: yes. That is why we are inclined to think that the steps are all already taken and it is just a question of us writing them down (RFM: I, § 22). This is the image of the “rules as rails” of (PI: § 218-219):<sup>2</sup>

My question really was: “How can one keep to a rule?” And the picture that might occur to someone here is that of a short bit of hand-rail, by

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<sup>1</sup> Wittgenstein also had some critical comments on the intuitionistic “ground-intuition” in (PG: 322).

<sup>2</sup> See also (RFM: VI, § 31) and (RFM: I, § 21): “Thus Frege somewhere says that the straight line which connects any two points is really already there before we draw it; and it is the same when we say that the transitions, say in the series +2, have really already been made before we make them orally or in writing—as it were tracing them.” Here the connection is evident with (GA1: 88), which was quoted in section 8.

means of which I am to let myself be guided further than the rail reaches. [But there *is* nothing there; but there isn't *nothing* there!] For when I ask "How *can* one...", that means that something here looks *paradoxical* to me; and so a picture is confusing me. (RFM:VII, § 66)

What the rule-following argument purported to show was that there is something wrong with that image in that it carries with it the Platonist image that the result of previously uneffected calculations is already there and waiting to be discovered. This image must be seen only as the reflection of our decision to let ourselves be guided by the rule. It is not the rule that compels me to follow it—in some mysterious way such as "intuition", "interpretation", "inspiration", "decision", etc.—but I compel myself to follow it:

Why do I always speak of being compelled by a rule; why not of the fact that I can *choose* to follow it? For that is equally important. But I don't want to say, either, that the rule compels me to act like this; but that it makes it possible for me to hold by it and let it compel me. And if e.g. you play a game, you keep to its rules. And it is an interesting fact that people set up rules for the fun of it, and then keep to them. (RFM: VII, § 66)

Wittgenstein wanted to undermine the picture of the rule as "guiding" us. It is not the rule which determines its applications but we who determine *in our practice* what is to count as complying with the rule.

There is therefore a tension in Wittgenstein's thought—which is well illustrated by his own exclamation in (RFM: VII, § 66): "But there *is* nothing there; but there isn't *nothing* there!"—between, on the one hand the rejection of the Platonist conception of the steps of the rules being already taken and looking like the shadow of a reality waiting to be discovered, and on the other hand the necessity to recognize that at each new step there is only one correct way of following the rule, therefore one right result in its application. This almost amounts to saying that the steps are already taken like the Platonist says. But Wittgenstein definitely rejected the Platonist metaphor of the mathematician as a discoverer—see the introduction—and he made claims such as the following:

However queer it sounds, the further expansion of an irrational number is a further expansion of mathematics. (RFM : V, § 9)

which may be sounding strange, but which are meaning that, if we take the decimal expansion of  $\pi$  as an example, unless you have calculated up to a certain point there is still nothing to talk about. If one persists with the “false picture of a completed expansion”, one is forced to ask “unanswerable questions” (RFM : V, § 9). This is part of what I called Wittgenstein’s extreme anti-Platonism.

Borel —certainly a more moderate anti-Platonist— once made the following comment on the decimal expansion of  $\pi$ :

...methods to calculate... as many decimals as one wishes are to be found in all Treatises on elementary geometry. Such a computation would certainly be quite long, if one was to ask for a very big number of decimals; but we conceive that it could be done; we can in fact hope that the improvements in analysis will help shortening the length of the computations; but at any rate, it is beyond doubt that the value of the 1000th or the 3645th decimal of  $\pi$  is actually well determined, even if the computation isn't done so far. (Borel 1914: 168) \*

Under my interpretation, Wittgenstein would agree only in part with Borel. In Wittgenstein’s jargon, once we are in the possession of the rule, each new step is determined in advance, inasmuch as at every new step there is only one possible answer according to the rule. But, as I indicated in section 14, according to Wittgenstein there are things we cannot say about the decimal expansion of  $\pi$  unless the calculation up to the relevant point is already done, because there is no information given with the rule for the calculation of the expansion. I shall quote again the following crucial passage from RFM:

“But surely all members of the series from the 1st up to the 1,000th, up to the  $10^{10}$ -th and so on, are determined; so surely *all* the members are determined.” That is correct if it is supposed to mean that it is not the case that e.g. the so-and-so-many’th is *not* determined. But you can see that *that* gives you no information about whether a particular pattern is going to appear in the series (if it has not appeared so far). *And so we can see that we are using a misleading picture.* (RFM: V, § 11)

One understands why Wittgenstein would refuse to apply the Law of Excluded Middle to statements about the decimal expansion of  $\pi$ : *in advance of any calculation*, nothing in the information given to us by the rule to produce the decimal expansion of  $\pi$  helps us to answer a question about the appearance of a certain pattern in it. So unless we

have calculated as far as its appearance of a given sequence, the Law of Excluded Middle shouldn't apply. On the other hand, the Platonist would claim that the expansion is already all given, determined, therefore the question must have a yes or no answer even if we are not in a position to know in principle the result.

Here Wittgenstein's position is closer to that of the intuitionist than that of the strict finitist. It is important to see that he was trying to reject the Platonist imagery without, on the other hand, being left with strict finitism as the only alternative.

If the remarks about surveyability aren't to be understood as providing a strict finitist argument, then they were meant by Wittgenstein for another purpose. I would agree with C. Wright that Wittgenstein's insistence on the surveyability of proofs provided him with an argument against the pretence of the logicist or set-theoretical foundations (Wright 1980: 134).<sup>1</sup>

It was Russell's intention in **Principia Mathematica** to found the certainty of mathematical propositions on their logical counterparts. Wittgenstein was eager to point out the obvious fact that logical translation increases the complexity. Moreover, proofs in **Principia Mathematica** possess only a geometrical certainty (RFM: III, § 43). Logical proofs become quickly unsurveyable, and their certainty falls with their geometrical cogency (RFM: III, § 16) (WWK: 66-7). Once one is counting, then the decimal system is taking over, replacing the stroke system which is supposed to found it. Wittgenstein asked: "How can the proof in the stroke system prove that the proof in the decimal system is a proof?" (RFM: II, § 54). The proof in the decimal system is supposed to be an abbreviation of the one in the stroke system. But it can't be so:

I want to say; if you have a proof-pattern that cannot be taken in and by a change of notation you turn it into one that can, then you are producing a proof, where there was none before. (RFM: III, § 2)

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<sup>1</sup> The remarks about surveyability also lead to another important aspect of Wittgenstein's notion of proof which I shall not discuss here, i.e. that proofs modify concepts.

Obviously, a proof in stroke notation of “200+200=400” is unsurveyable (RFM: III, § 24). Therefore it isn’t a proof. At least, it should be obvious that it cannot serve to guarantee the cogency of the shorter, surveyable proof in decimal notation. If we look at a Russellian formula as representing an arithmetical formula, it is clear that its Russellian proof cannot fulfill its role of being the proof of the original arithmetical equation (RFM: III, § 14). On the contrary, Wittgenstein claimed that we need knowledge of the arithmetical formula to prove the Russellian formula convincingly, and even to understand the Russellian proof:

The correctness of an arithmetical proposition is never expressed by a proposition’s being a tautology. In the Russellian way of expressing it, the proposition  $3 + 4 = 7$  for example can be represented in the following manner:

$$(E3x)\phi x.(E4x)\psi x.\sim(Ex)\phi x.\psi x: \rightarrow: (E7x). \phi x\psi x$$

Now one might think that the proof of this equation consisted in this: that the proposition written down was a tautology. But in order to be able to write down this proposition, I have to *know* that  $3 + 4 = 7$ . The whole tautology is an application and not a proof of arithmetic. (WWK: 35) <sup>1</sup>

Wittgenstein was pointing out a major deficiency of formalisation. In short, as Kreisel aptly said: “arithmetic does more for logic than logic for arithmetic” (Kreisel 1978a: 102).<sup>2</sup>

This train of thoughts lead Wittgenstein to the idea that calculating in decimal notation isn’t dependent on calculating in stroke notation but *has a life of its own*, i.e. introducing the new notation amounts to introduce new numbers:

If there is something true about what I am trying to say, then—e.g.—calculating in the decimal notation must have its own life.—One can of course represent any decimal number in the form:

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<sup>1</sup> Similar remarks were made also by H. Wang in “Process and Existence”. Wang gave the following as an abbreviation for the theorem of logic corresponding to “7+5=12”:

$$(*) [ (\exists!_7 x) Gx \wedge (\exists!_5 x) Hx \wedge (\forall u) \sim (Gu \wedge Hu) ] \rightarrow (\exists!_{12} x) (Gx \vee Hx)$$

and then commented on the increase in conceptual complexity involved in the translation. An expanded proof of (\*) would indeed require that we count the variables, and this would defeat the purpose of the translation: “We are able to see that (\*) is a theorem of logic only because we are able to see that a corresponding arithmetic proposition is true, not the other way round” (Wang 1961: 335).

<sup>2</sup> (Steiner 1975: 41-54) contains a defence of Russell’s formalization against this particular critique by Wittgenstein. This is not the place, however, to assess Steiner’s arguments.



## VIII. The Continuum

Im Aufbau der Mathematik gibt es zwei offene Stellen, wo es möglicherweise ins Unergründliche geht: der Fortgang in der Reihe der natürlichen Zahlen und das Kontinuum.

H. Weyl

### 17. Cauchy Sequences

The discussion has so far centred around the notions of mathematical induction and infinity. It is now time to discuss another fundamental aspect of any sound philosophy of mathematics, i.e. its theory of the continuum. There are three main mathematical theories of the continuum,<sup>1</sup> two of them (Cauchy sequences, Dedekind cuts) were developed within classical mathematics and are equivalent. Constructive versions were provided, in particular by E. Bishop (Bishop 1967). In addition, Brouwerian intuitionism has its own theory, that of the choice sequences. Wittgenstein made a number of critical remarks on Cauchy sequences and on Dedekind cuts,<sup>2</sup> and although the intuitionistic theory of choice sequences wasn't fully developed in his days, he criticized early developments (I shall discuss these criticisms in the next section.) Since any view of the continuum has to do with the real numbers and therefore with the diagonal method—the latter being essential in Cantor's proof of the non-denumerability of the real numbers—I shall begin by discussing the few remarks Wittgenstein made in RFM on Cantor's *Diagonalverfahren*.

There exists a “diagonal” method which does not produce what appears to be a “new” number, as in Cantor's method. This “diagonal”, which was called “Cauchy's

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<sup>1</sup> I shall not discuss the multiple non-standard models of the continuum, since Wittgenstein had nothing to say on this topic. On non-standard analysis, see (Cutland 1988) and especially (Harthong & Barreau 1989) where an intuitionistic approach to non-standard analysis is presented. See also (Nelson 1977), the basis of the non-standard extension  $Q^*$  discussed at the end of section 9.

<sup>2</sup> I shall not discuss Wittgenstein's criticisms of Dedekind cuts here, since they are of lesser interest.

diagonal method” by E. Kamke (Kamke 1950: 9), is used in many proofs, such as the proof that the set of all rational numbers is countable, or the proof that the set of all algebraic numbers is countable (or enumerable.)<sup>1</sup> Cantor introduced another method which should properly be called the diagonal method in the proof of one of his most important results, which states that:

*The set of all real numbers in the interval  $0 \leq x \leq 1$  is uncountable.*

In Cantor’s proof, the reals are represented as an array of infinite (non-recurring) decimals:

0 .	a <sub>1</sub>	a <sub>2</sub>	a <sub>3</sub>	a <sub>4</sub>	a <sub>5</sub>	.....
0 .	b <sub>1</sub>	b <sub>2</sub>	b <sub>3</sub>	b <sub>4</sub>	b <sub>5</sub>	.....
0 .	c <sub>1</sub>	c <sub>2</sub>	c <sub>3</sub>	c <sub>4</sub>	c <sub>5</sub>	.....
0 .	d <sub>1</sub>	d <sub>2</sub>	d <sub>3</sub>	d <sub>4</sub>	d <sub>5</sub>	.....
0 .	e <sub>1</sub>	e <sub>2</sub>	e <sub>3</sub>	e <sub>4</sub>	e <sub>5</sub>	.....

(Here the letters represent digits 0 to 9.) Cantor observed that the array cannot be complete, since one can define a decimal which differs at the  $n$ th place from each  $n$ th decimal in the array.

Some very harsh words on Cantor’s diagonal method are to be found in Part II of **RFM**. For example, in an apparent comment on Cantor’s proof, Wittgenstein wrote:

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)

And in the following section, he wrote:

... one pretends to compare the ‘set’ of real numbers in magnitude with that of the cardinal numbers. The difference in kind between the two conceptions is represented, by a skew form of expression, as difference of extension. I believe, and hope, that a future generation will laugh at this hocus pocus. (RFM: II, § 22)

The immediate reaction of most readers is usually to dismiss these remarks as the expression of prejudice and ignorance. After all, this result of Cantor is well established

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<sup>1</sup> See the Chapter 1 of (Kamke 1950) for example.



and the diagonal method he used is a powerful tool, in the theory of transcendental numbers, for example. So, to use an expression due to Kreisel, anyone unaware of the importance of the diagonal method—and this description seems to fit Wittgenstein—is liable to be called a *logischer Krüppel* (Kreisel 1983: 301). But a closer look at Part II of **RFM** will indicate first that Wittgenstein did not oppose to the diagonal method itself, but to the interpretation of the result of its application. Of the diagonal method he said that “this kind of calculation is itself useful” (RFM: II, § 18). He also spoke of its “proper purpose” (RFM: II, § 13).

Rather, Wittgenstein wanted to question the idea that the new number produced by the diagonal is a “real”, as Cantor thought. I shall indicate my reasons for believing that Wittgenstein put his finger on one of the important problems in philosophy of mathematics, and that his answer deserves respect.

It is difficult indeed to oppose to the diagonal method itself. Indeed, it could be described as a rule to operate on rules, and as such it makes no commitment to completed infinities. Indeed, while Cantor simply observes that there is a decimal that differs in the  $n$ th place from each  $n$ th decimal in the array given above, one can restrict the diagonal to effectively computable infinite decimals, i.e. to decimals whose every  $n$ th place can be calculated. Moreover if for each  $n$  one can effectively determine what the  $n$ th decimal in the array should be, then one can effectively compute a decimal which is not part of the array. Hence the method becomes purely constructive.

Moreover, as pointed out by C. Wright, if we assume Church’s Thesis—and I shall indicate later in this section my reasons for believing that Wittgenstein would agree with the so-called “recursive analysis”—all the decimals in the array and each successive diagonal decimal will correspond to recursively enumerable sequences of numerals. And it is known that the totality of recursive functions is only countably infinite. Therefore the diagonal method as described cannot be interpreted as showing something about uncountability. The result now “shows that there is no recursive enumeration of all

recursively enumerable infinite decimals” (Wright 1985: 134). This is how Wittgenstein understood the diagonal: he spoke of it as showing that “it makes no sense to talk about a “series of all real numbers”” (RFM: II, § 16). This is precisely what he saw as the “proper purpose” of the diagonal:

Surely— if anyone tried day-in day-out ‘to put all irrational numbers into a series’ we could say: “Leave it alone; it means nothing; don’t you see, if you establish a series, I should come along with the diagonal series!” This might get him to abandon his undertaking. Well, that would be useful. And it strikes me as if this were the whole and proper purpose of this method. (RFM: II, § 13)

The result is about the impossibility of a recursive enumeration and not about uncountability, as Cantor thought it was: here lies the “hocus pocus”, the “Fata Morgana”<sup>1</sup> denounced by Wittgenstein.

The real numbers are usually defined in terms of Cauchy sequences in the following manner. The sequence  $(x_n) = (x_1, x_2, \dots, x_n, \dots)$  is a Cauchy sequence if the distance of the terms  $x_p$  and  $x_q$  of the sequence  $(x_n)$ , i.e. the number  $|x_p - x_q|$  is getting closer to 0 as  $p$  and  $q$  are getting bigger, i.e.

$$\lim_{p, q \rightarrow \infty} |x_p - x_q| = 0$$

The only necessary condition for a real sequence  $(x_n)$  to be convergent in  $\mathbf{R}$  is that it is a Cauchy sequence. This is a criterion of convergence, i.e. the so-called Cauchy criterion: for all real  $\varepsilon > 0$  there exists a natural  $A \in \mathbf{N}$  such that for any  $p, q > A$  there is:

$$|x_p - x_q| < \varepsilon$$

This criterion plays a useful role: one can say if a real sequence is convergent in  $\mathbf{R}$  without the need to provide its limit. With the following equality relation “ $=_r$ ” in the set of Cauchy sequences, here between  $(r_n)$  and  $(s_n)$  :

$$(r_n) =_r (s_n) \Leftrightarrow \lim_{n \rightarrow \infty} (r_n - s_n) = 0$$

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<sup>1</sup> This last expression is used in (RFM: V, § 16).

This relation identifies sequences which differ in a finite initial part and also those such as  $(\frac{1}{n})$  and  $(\frac{1}{n^2})$  (both converging to 0) with different convergence behavior. The set of reals is the set of equivalence classes of Cauchy sequences with respect with this equality relation. It is usually understood that the geometric line contains limits for all Cauchy sequences of rationals, therefore the set of reals is identified with the geometric continuum.

Both **PR** and **PG** contain extensive discussions of the theory of the real numbers, whose purpose is, according to P. Frascolla,

...to make clear what kind of law or prescription for the formation of convergent series of rationals can be legitimately considered a genuine generator of a real number. (Frascolla 1980b: 242)

Wittgenstein expressed two such prescriptions.<sup>1</sup> First, and this should come as no surprise, the prescription must be recursive:

A real number *yields* extensions, it is not an extension.  
A real number is: an arithmetical law which endlessly yields the places of a decimal fraction. (PR: § 186)

In quite a coherent fashion, *Wittgenstein defines here the prescription for the formation of real numbers in explicit conjunction with is rejection of the descriptivist viewpoint.*

This prescription is implicit in all of Wittgenstein's discussions in **PR** and in **PG**, for example in his typical example of  $\sqrt[7 \rightarrow 3]{2}$ , where "7→3" means that whenever 7 appears in the expansion of  $\sqrt{2}$  one must replace it by 3. According to Wittgenstein  $\sqrt[7 \rightarrow 3]{2}$  does not determine a real number, because it isn't an arithmetical operation:

Even if I wasn't familiar with the rule for forming  $\sqrt{2}$ , and I took  $\sqrt[7 \rightarrow 3]{2}$  to be the original prescription, I would still ask: what's the idea of this peculiar ceremony of replacing 7 by 3? Is it perhaps that 7 is tabu, so that we are forbidden to write it down? For substituting 3 for 7 surely adds absolutely nothing to the law, and in this system it isn't an arithmetical operation at all. (PR: § 186)<sup>2</sup>

<sup>1</sup> See (Frascolla 1980a: 666-667) and (Frascolla 1980b: 243).

<sup>2</sup> See also (PR: § 182) or (PR: § 179) where Wittgenstein discusses the example of successive throws of a coin as defining a point on the line by bisection; and asks "does this geometrical process define a

The problem with such an example is that it is difficult to see in what sense  $\sqrt[7 \rightarrow 3]{2}$  is not an arithmetic operation. I can only give Wittgenstein's own explanation, using the similar example of  $\sqrt[7 \rightarrow 3]{\pi}$  where the supplementary prescription "7→3" appears artificial:

This is how it is: the number  $\pi$  is expressed in the decimal system. You can't achieve a modification of this law by fixing on the specific expression in the decimal system. What you thereby influence isn't the law, it's its accidental expression. The influence does not penetrate as far as the law at all. Indeed it stands separated from it on the other side. It's like trying to influence a creature by working on a secretion that has already been discharged. (PR: § 188)

This first prescription seems also to follow from Wittgenstein's own understanding of the grammar of the "and so on" (see section 10 to 12):

The true nature of real numbers must be the induction. What I must look at in the real number, its sign, is the induction. —The "So" of which we may say 'and so on'. (PR: § 189)

Indeed, the general thrust of Wittgenstein's discussion of the infinite is that infinite sequences should not be seen as extension but as laws, or rules. *Real numbers being defined as infinite sequences of rationals, they must be introduced by a law, because only laws "reach to infinity"*. There seems to be no room for real numbers given in an "extensional" manner, no room for "random" real number. This is the gist of this rather crucial section of PR:

Now let's assume we have been given all the irrational numbers that can be represented by laws, but that there are yet other irrationals, and I am given a cut representing a number not belonging to the first class: How am I to tell that this is so? This is impossible, since no matter how far I go with my approximations, there will always also be a corresponding fraction.

And so we cannot say that the decimal fractions developed in accordance with a law still need supplementing by an infinite set of irregular infinite decimal fractions that would be 'brushed under the carpet' if we were to *restrict* ourselves to those *generated by a law*. Where is there such an infinite decimal that is generated by no law? And how would we notice that it was missing? Where is the gap it is needed to fill?

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*number?*" His answer is: "But the operation is not an arithmetical one. (And the point which I call to my aid in my endless construction can't be given arithmetically at all)".

If from the very outset only laws reach to infinity, the question whether the totality of laws exhaust the totality of infinite decimal fractions can make no sense at all. (PR: § 181)<sup>1</sup>

Wittgenstein argument seems to be that since only laws “reach to infinity”, lawless (or irregular) real numbers can only be given in a finite extension and that for any such finite extension there is a corresponding decimal expansion which in turn is given by a law. So to be a real is to be given by a law:

But can I be in doubt whether all the points of a line can actually be represented by arithmetical rules. Can I then ever find a point for which I can show that this is not the case? If it is given by means of a construction, then I can translate this into an arithmetical rule, and if it given by chance, then there is, no matter how far I continue the approximation, an arithmetically defined decimal expansion which is concomitant with it.

It is clear that a point corresponds to a rule. (PR: § 180)

This first prescription reduces therefore the real numbers to the so-called “recursive” real numbers, to the exclusion of the “random” real numbers, i.e. arbitrary decimal expansions (I shall discuss in a moment the rather obvious connections with constructivist versions of continuum such as E. Bishop’s.) Arbitrary infinite sequences are sequences generated not by a rule but by an arbitrary selection of one term after another. The usual example of such arbitrary sequences is that of a decimal expansion whose digits are obtained by successive throws of a die. Wittgenstein saw such numbers as “something empirical” (WWK: 83), and spoke in their case of an “arithmetical experiment”,<sup>2</sup> that is of something which is not in accordance with his fundamental view of mathematics as being essentially “calculus” or algorithms (the image of the abacus.) Wittgenstein stated clearly that reals should be defined in such a way (i.e. recursively) that one could not speak of an “arithmetical experiment”:

Is an arithmetical experiment still possible when a recursive definition has been set up? I believe, obviously not; because via the recursion each stage becomes arithmetically comprehensible. (PR: § 194)

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<sup>1</sup> A very similar passage is to be found in (PG: 473).

<sup>2</sup> “In this context we keep coming up against something that could be called an ‘arithmetical experiment’. Admittedly the data determines the result, but I can’t see *in what way* they determine it. (cf e.g. the occurrences of 7 in  $\pi$ .)” (PR: § 190).

Random numbers created by successive throws of a die or numbers defined laws such as

$\frac{7 \rightarrow 3}{\sqrt{2}}$  are not real numbers because one is simply not given a way of constructing them:

Put geometrically: it's not enough that someone should—supposedly—determine a point ever more closely by narrowing down its whereabouts; we must be able to construct *it*. (PR: § 186)<sup>1</sup>

To sum up *Wittgenstein is clearly asking that one possesses an effective rule of construction of every infinite series of rationals, i.e. to compute rational approximations.*

This rejection of random real numbers has ramifications in Wittgenstein's criticism of intuitionism and its peculiar notions of lawless and choice sequences. I shall discuss these in the next section.

The second prescription on the formation of real numbers is that any real number must be effectively comparable with every rational number, since "being comparable with other numbers is a fundamental characteristic of a number" (PG: 476):

It seems to be a good rule that what I will call a number is that which can be compared with any rational number taken at random. That is to say, that for which it can be established whether it is greater than, less than, or equal to a rational number.

That is to say, it makes sense to call a structure a number by analogy, if it is related to the rationals in ways which are analogous to (of the same multiplicity as) greater, less and equal to.

A real number is what can be compared with the rationals. (PR: § 191)<sup>2</sup>

This second prescription originates in Wittgenstein's attempts at avoiding the decriptivist image of the line. Commenting on his insistence on effective comparability, Wittgenstein added:

I want to say that this is precisely what has been meant or looked for under the name 'irrational number'.

Indeed, the way the irrationals are introduced in text books always makes it sound as if what is being said is: Look, that isn't a rational number, but still there is a number there. But why then do we still call what *is* there a 'number'? And the answer must be: because there is a definite way for comparing it with the rational numbers. (PR: § 191)

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<sup>1</sup> This passage is repeated in (PG: 477).

<sup>2</sup> See also (PR: § 195-7), (WWK: 72-73).

The second prescription also eliminates numbers not given by law (i.e. random numbers) since, simply, “you can compare a law with a law, but not a law with *no* law” (PR: § 181). But this second prescription eliminates some numbers given by a law which fulfills the first one. Frascolla gave the following example of a law satisfying the first but not the second prescription: <sup>1</sup>

The  $n$ th term of the expansion is  $\begin{cases} 0 & \text{if } 2n \text{ is the sum of two prime numbers} \\ 1 & \text{otherwise} \end{cases}$

More importantly, this second prescription also implies the rejection of Brouwer’s *duale Pendelzahl*, which is given recursively but not comparable in size to 0 from the domain of the reals (see section 14.) In Frascolla’s words:

One of the effects of this restrictions is the exclusion of the domain of the real numbers of the mathematical structures which were constructed by L. E. J. Brouwer to prove the inapplicability of the Law of Excluded Middle to the reasoning on infinite sets. (Frascolla 1980a: 667) \*

Wittgenstein was quite conscious of this fact:

The decisive thing about the construction of real numbers consists precisely in their comparability. It is only in virtue of this that the real numbers can be interpreted as points on a straight line. If, now, there are constructions that cannot be compared with rational numbers, then we have no right to find them a place among the rational numbers. Thus they simply are not on the number lines. (In Brouwer it appears as if they were real numbers about which we merely did not *know* whether they were larger than, or smaller than, or equal to another rational number.) (WWK: 73)

Wittgenstein’s annoyance with Brouwer’s *duale Pendelzahl* was already recorded in section 14. I contended, however, that Wittgenstein had sufficient grounds for rejecting the Law of Excluded Middle, without having to accept Brouwer’s counterexamples.

Many constructive versions of the definition of the real numbers *via* the Cauchy sequences are to be found in the literature. One example is to be found in E. Bishop’s **Foundations of Constructive Analysis** (Bishop 1967: Chap. 2). According to

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<sup>1</sup> (Frascolla 1980b: 243, note 3).

Bishop a constructive real is a pair  $((r_n), \mu)$ , where  $(r_n)$  is a constructively given Cauchy sequence and  $\mu$  a constructive rate-of-convergence function  $\mu: \mathbb{N} \rightarrow \mathbb{N}$  such that:

$$\forall k > 0 \quad \forall n, m \geq \mu(k) \quad [ |r_n - r_m| < \frac{1}{k} ]$$

Here, equality between reals is defined as:

$$((r_n), \mu) =_r ((s_n), \nu) \Leftrightarrow (r_n - s_n) \rightarrow 0 \quad ^1$$

Since the publication of Bishop's book, many logicians provided formal systems for his constructive analysis: P. Martin-Löf's "transfinite type theory", J. Myhill's "constructive set theory" and S. Feferman's constructive theory of functions and classes "T<sub>0</sub>" are some examples.<sup>2</sup>

Real numbers *à la* Bishop clearly give some flesh to Wittgenstein's insistence on effectivity. To use the intuitionistic terminology to be explained in the next section, Bishop is working in "lawlike" analysis, and his work corresponds to Brouwer's reconstruction of analysis without its idiosyncratic notion of choice sequences. In Feferman's "T<sub>0</sub>" there seems to be no clear way of distinguishing between "real" and "computable real" numbers--although there are different notions of recursivity, one of them corresponding to Bishop's (Feferman 1984: 152).

There is also another school of constructivism, which limits itself to "recursive" or "computable" analysis, a good representative of which is O. Aberth (Aberth 1970, 1980).<sup>3</sup> Here, the rather vague notions of "law" or "rule", of which Wittgenstein made ample use, are replaced by the precisely-defined concept of recursive function. All

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<sup>1</sup> I followed here the brief description given in (Feferman 1979: 167). See also (Feferman 1984). Bishop's constructivization could be seen as a strategy of "enriching data" (Kreisel 1976b: 124). According to Kreisel and Macintyre, this enrichment proved to be quite useful (Kreisel & Macintyre 1982: 238-239).

<sup>2</sup> In the following, I shall only mention the latter. For details, see (Feferman 1979) and (Feferman 1984).

<sup>3</sup> See also (Beeson 1985: Chap IV).



“objects” are given by numbers, and these are manipulated only by recursive functions.<sup>1</sup>

The opening paragraph of (Aberth 1970) is a good statement of his programme:

Computable analysis is an analysis restricted to the field of computable numbers where a real number is called computable if there is an algorithm for obtaining precise rational approximations. Algorithms are the basis also for the definitions of the functions and sequences of computable analysis, and in every instance of a number, function or sequence, an appropriate defining algorithm is assumed available. (Aberth 1970: 47)

The connections with Wittgenstein’s prescriptions for the formation of real numbers are obvious: both Wittgenstein and Aberth require an effective method of computation of the approximated rational values. There is in neither case any room for random real numbers. Aberth’s formulation also makes obvious the central role of algorithms, a role which corresponds perfectly to Wittgenstein’s insistence that mathematics is essentially of algorithmic nature (WWK: 106) (PR: § 151) (see section 1.)

According to M. Beeson, there are two different approaches to “computable” analysis, one using intuitionistic logic, which is essentially Markov’s constructivism without Markov’s Principle. The other school uses classical logic (Beeson 1985: 48-49). Although Wittgenstein rejected Brouwer’s counterexamples, his distrust of the Law of Excluded Middle leads me to believe that he would find the former approach more congenial.

## 18. Choice Sequences

One of the consequences of Wittgenstein’s prescription that all real numbers must be recursively defined is the exclusion of the random real numbers or arbitrary infinite

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<sup>1</sup> This approach, described by Beeson as “Living with Church’s Thesis”, seems to have resulted so far mainly in negative results. For example (Bishop 1967) mentioned the following problem: if  $f: [0,1] \rightarrow \mathbb{R}$  is uniformly continuous and for every  $x$   $f(x) > 0$ ; then must  $\inf f(x)$  be positive? In recursive analysis this result fails (Beeson 1985: 71-72). Since this is classically true, there is therefore no point in trying to solve the problem for Bishop’s analysis.

decimal expansions. Therefore, Wittgenstein must be classified with most constructivist approaches (such as Bishop's or Aberth's) to the theory of real numbers, in denying an important ingredient of the classical theory of the continuum. Brouwerian intuitionism, however, distinguishes itself from the other constructivist programmes by providing alongside such a theory of recursive real numbers a theory about sequences in which there is also free selections. M. Dummett provided the following justification:

Intuitionism aims, however, to reform mathematics, not to prune it; according to it, scarcely any of the ideas of classical mathematics is wholly spurious, but all are deformed by being systematically misconstrued. Hence intuitionism retains both fundamental ideas which go to form the classical continuum, admitting not only infinite sequences determined in advance by an effective rule for computing their terms, but also ones in whose generation free selection plays a part. (Dummett 1977: 62)

Such arbitrary sequences are now known as “lawless” and “choice” sequences, in contrast with “lawlike” sequences. The theory of choice sequences was of course rejected by other constructivists such as Bishop who said that it “makes mathematics so unpalatable to mathematicians, and foredooms the whole of Brouwer's program” (Bishop 1967: 6). Brouwerian intuitionism is also open to criticisms from Wittgenstein's standpoint, since according to the latter, “an irrational number isn't the extension of an infinite decimal fraction, ... it's a law” (PR: § 181).

Wittgenstein's knowledge of what was then called “freely developing sequences” came from reading the exposition of Brouwer's early remarks in H. Weyl's “Die heutige Erkenntnislage in der Mathematik” (Weyl 1925) and possibly from reading Weyl's **Philosophy of Mathematics and Natural Sciences** (Weyl 1927a). Wittgenstein made many remarks about sequences whose terms are determined by successive casts of a die<sup>1</sup> —an image which bear some analogy with the notion of lawless sequences— from which his critical view of choice sequences might be inferred.

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<sup>1</sup> (PR: § 179), (PG: 472, 484-485), (AWL: 220-221).

The notion of choice sequence was discussed by E. Borel, in a lecture attended by Brouwer in 1908 (Borel 1909).<sup>1</sup> Borel rejected such sequences as insufficiently defined to be legitimate mathematical objects (Borel 1909: 1268). Brouwer agreed with such criticisms, since according to his initial convictions the real numbers were identified with a (finite) law. Choice sequences were rejected in 1912 by Brouwer as a “formalist” creation:

Let us consider the concept: “real number between 0 and 1.” For the formalist this conception is equivalent to “elementary series of digits after the decimal point,” for the intuitionist it means “law for the construction of an elementary series of digits after the decimal point, built up by means of a finite number of operations.” And when the formalist creates the “set of all real numbers between 0 and 1,” these words are without meaning for the intuitionist, even whether one thinks of the real numbers of the formalist, determined by elementary series of freely selected digits, or of the real numbers of the intuitionist, determined by finite laws of construction. (Brouwer 1912: 134)

In these early stages, Brouwer could not admit convergent choice sequences as sufficiently well-defined to be arguments of real functions (van Stigt 1990: 358-359). Wittgenstein’s standpoint as expounded in the previous section, with its insistence that real numbers be given by a recursion, bears affinities with Borel’s rejection of choice sequences and these early statements by Brouwer. But both Borel and Brouwer already showed signs of admitting choice sequences, something Wittgenstein never did. In accordance with his views on the Axiom of Choice —see section 3— Borel had a greater willingness to admit countable sets of choices as compared to uncountable sets of choices, and Brouwer sounded as he might just be prepared to admit them:

... for the intuitionist can only construct denumerable sets of mathematical objects and if, on the basis of the intuition of the linear continuum, he admits elementary series of free selections... (Brouwer 1912: 134-135)

As soon as 1914, Brouwer liberalized his notion of constructive procedures in order to include choices, not just finite laws of generation:

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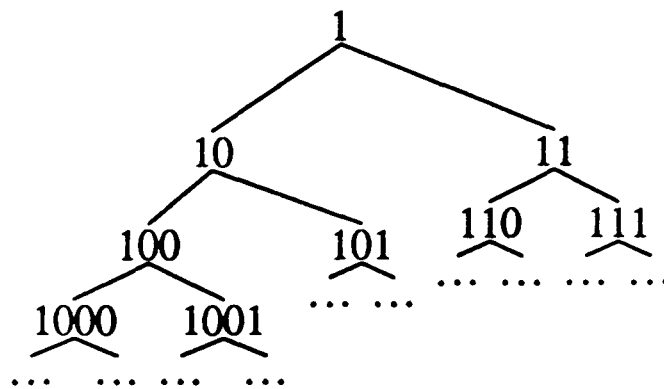
<sup>1</sup> For the genesis of the notion of choice sequence in (Borel and) Brouwer, see (Troelstra 1982).

... for the intuitionist only well-constructed infinite sets exist, which are assembled from a part of the first kind, constructed as a single fundamental sequence, and a part of the second kind, based on a fundamental sequences  $f$  conceived as a Frechet V-class, whose elements are determined by a sequence of choices from elements of a finite set or of a fundamental sequence in such a way that to every sequence of choices corresponds nested intervals of  $f$  whose widths converge to zero, and that there exists final segments of any two sequences of intervals, which are corresponding to any two sequences of choices, lying apart from each other. (Brouwer 1914: 140) \*

But, Brouwer never developed fully his ideas on the topic. One had to wait, among other, for the works of G. Kreisel and A. Troelstra for a more extensive development of the theory of lawless and choice sequences (Kreisel 1968)(Troelstra 1977). I shall elaborate on these intuitionistic notions before coming to Wittgenstein's criticisms.

In the current intuitionistic literature, a lawlike sequence is a sequence whose expansion is determined by a law, or rule, e.g. recursive primitive functions. A lawless sequence is defined by an expansion not defined by any law: at any stage the only knowledge available is about values previously obtained, and no information about future stages is available. The example of the tosses of a coin or the casts of a die is a good analogy here (Troelstra 1977: 12). A choice sequence is somewhat an intermediary between lawlike and lawless sequences. A choice sequence  $\alpha$  is a sequence of freely chosen values  $x_0, x_1, \dots$  such that we can determine it by introducing a new choice sequence  $\alpha_0$  defining:  $\alpha = \Gamma_0 \alpha_0$  — $\Gamma_0$  being a continuous operator— and then after a certain time one can determine  $\alpha$  again with  $\alpha = \Gamma_1 \alpha_1$ , and so on.

The real numbers are represented by the bisection of the line (or one-dimensional continuum). This process is purely combinatorial and can be given through an arithmetical law which gives the general schema of construction of its elements. The schema could be represented geometrically by an infinitely branching (binary) tree:



This process of subdivision determines a coordinate system within the continuum. One can then designate points by binary fractions. But since in the continuum no boundaries can be set to this subdivision, it always continues defining ever more precisely earlier points. But real numbers are not points, they are defined rather as “infinite sequence of nested division intervals of increasing level”:

Any two adjacent parts of the  $i^{\text{th}}$  division step may be joined into a ‘division interval of the  $i^{\text{th}}$  level’. The division intervals of the  $i^{\text{th}}$  level overlap in such a manner that for any approximately given number, as soon as the approximation is sufficiently accurate, a division interval of the  $i^{\text{th}}$  level can be found into which that number falls. Thus the individual real number will have to be defined as an *infinite sequences of nested division intervals of increasing level*. (Weyl 1927a: 53).

For Wittgenstein, as we shall see, this process of bisection corresponds to the tossing of a coin. In “Über die neue Grundlagenkrise der Mathematik” Weyl paid tribute to Brouwer for showing that number sequences created through a “free act of choice” (*freie Wahlakte*) could be objects of mathematical conceptualisation (Weyl 1921: 152-153). Following Brouwer, Weyl emphasized the importance of the *Wahlfolge*:

As the law  $\varphi$  determining an infinite sequence represents the isolated real number, so is the choice sequence limited by no laws in the freedom of its expansion representing the continuum. (Weyl 1921: 153) \*

Later, in “Die heutige Erkenntnislage in der Mathematik”, Weyl insisted that the most important characteristic mark of “choice sequences in becoming” (*werdenden Wahlfolgen*) is that they are closed under continuous operations:

That it is mathematically possible to operate with choice sequences in becoming, is adequately illustrated by the fact that one can provide an ordering between the choice sequences. E. g. the formula

$$n_h = m_1 + m_2 + \dots + m_h \quad (h = 1, 2, 3, \dots)$$

contains a law according to which a number sequence in becoming  $n_1, n_2, n_3, \dots$  is build from a sequence in becoming  $m_1, m_2, m_3, \dots$  given by free acts of choice, their coming into being keeping at the same pace. (Weyl 1925: 531) \*

This remark is still valid today. For example, in **Choice Sequences** Troelstra wrote:

...what is relevant from a mathematical point of view is ... the 'mathematical' fact that there exists many perfectly well-defined (lawlike) operations on sequences which can be carried out without assuming the arguments to be determined by a law. Example:  $\alpha + \beta \equiv_{\text{def}} \lambda x . (\alpha x + \beta x)$  constructs a new sequence out of two given ones. ... if  $\alpha$  and  $\beta$  are lawless,  $\alpha + \beta$  is a again a sequence, but not a lawless one. (Troelstra 1977: 12-13)

During a discussion with Schlick and Waismann, Wittgenstein made a direct reference to this very remark of Weyl, disagreeing entirely with the latter's recognition of the choice sequences as mathematical objects:

A freely developing sequence is in the first place something empirical. It is nothing but the numbers that I write down on paper. If Weyl believes that it is a mathematical structure because I can derive a freely developing sequence from another one by means of a general law, e.g.,

$m_1, m_2, m_3, \dots$

$m_1, m_1 + m_2, m_1 + m_3, \dots$

then the following is to be said against it: No, this shows only that I can add numbers, but not that a freely developing sequence is an admissible mathematical concept. (WWK: 83)

Here, Wittgenstein implies that the fact that one can operate with choice sequences, such as in the case of  $\alpha + \beta \equiv_{\text{def}} \lambda x . (\alpha x + \beta x)$ , is not a sufficient factor for their admissibility as genuine mathematical objects. One could reply to Wittgenstein by pointing out that, although his point is valid, he hasn't given any argument against the admissibility of choice sequences as mathematical objects. What is important here, however, is that with this critical remark about Weyl's exposition of Brouwer's notion of freely developing sequences, Wittgenstein was linking his remarks on arbitrary sequences (casts of a die) to the intuitionistic notions.

According to Wittgenstein, freely developing sequences are given only through a description, but "the description is not arithmetic" (WWK: 83). The very idea of such a description, i.e. of an "arithmetical experiment" was seen as nonsense:

A number as the result of an arithmetical experiment, and so the description of a number, is an absurdity.  
The experiment would be the description, not the representation of a number. (PR: § 196)

Wittgenstein insisted many times that “No law of succession is decreed by the instruction to toss a coin” (PR: § 179):

...the description “endless process of choosing between 1 and 0” does not determine a law in the writing of a decimal. Perhaps you feel like saying: the prescription for endless choice between 0 and 1 in this case would be a symbol like  $0 \begin{smallmatrix} 000 \\ 111 \end{smallmatrix} \dots \text{ad. inf.}$ . But if I adumbrate a law thus “ $0.001001001 \dots \text{ad. inf.}$ ”, what I want to show is not the finite section of the series as a specimen of the infinite series, but rather the kind of regularity to be perceived in it. But in  $0 \cdot \begin{smallmatrix} 000 \\ 111 \end{smallmatrix} \dots \text{ad. inf.}$  I don’t perceive *any* law, —on the contrary, precisely that a law is absent. (PG: 472)

The whole grain of Wittgenstein’s grammatical distinction between finite and infinite sequences is precisely that only laws, and not extensions, “reach to infinity”, and the idea of an infinite lawless sequence appeared as completely nonsensical to him (see (AWL: 221).)

Wittgenstein’s objection missed the mark: any intuitionist would agree that any particular choice sequence lacks the mathematical existence of a law, but the point of the exercise was not of justifying particular choice sequences as mathematical objects. It was rather, as P. Frasnquolla rightly pointed out, to provide a general schema, which would be studied for its own sake:

... Wittgenstein’s objection misses the mark because the tree representing the bisections or the spread-law are intended to represent not the prescription for the generation of a number but the general schema of construction of infinitely proceeding successions of noughts and ones. (Frasquolla 1980a: 673) \*

His complete rejection of such a distinguishing figure of Brouwerian intuitionism as the theory of the choice sequences in favour of a recursive model of the continuum places Wittgenstein definitely away from intuitionism, although he rests firmly in the tradition of Kronecker, Skolem and Bishop.

## CONCLUSION

“Hallo!” said Piglet, “What are you doing?”

“Hunting” said Pooh

“Hunting what?”

“Tracking something” said Winnie-the-Pooh very mysteriously.

“Tracking what?” said Piglet coming closer.

“That’s just what I ask myself. I ask myself, what?”

“What do you think you’ll answer?”

“I shall have to wait until I catch up with it” said Winnie-the-Pooh.

A. A. Milne, *Winnie-the-Pooh*,  
Chap. 3: In which Pooh &  
Piglet nearly catch a woozle.

I hope that any reader will have formed by now the opinion that Wittgenstein was undoubtedly a “revisionist.” In section 1, I have pointed out the incoherence of interpretations of Wittgenstein’s remarks as suggesting a “no-position” position, or of the claim of their incommensurability with traditional philosophy. The impression of neutrality comes from the fact that Wittgenstein’s attack set theory was from a different and much narrower angle than the critique leveled by the constructivists. Wittgenstein wanted a critique of more philosophical concepts —i.e. those with an everyday use— such as that of the infinite. The point of his critique was not the rejection of a theory on mathematical grounds, but, rather, to show its lack of interest. If Wittgenstein wrote remarks which were definitively implying a revision of classical logic and mathematics, to what extent was he ready to revise logic and mathematics?

In strict finitist programmes the epistemological argument about the practical limitations of humans to carry on operations plays a central role. Therefore not only the actual infinite does not exist for a strict finitist, but there is no difference between the large finite and the potential infinite, since it is practically impossible for us to reach some large finite numbers (see section 9.) To this, the Platonist would reply that these so-called practical limitations are merely “medical”: it is conceivable that God can do what we can’t do. I argued at length in sections 10 to 12 that Wittgenstein wanted to reject this characterization as wrong-headed.



A detailed study of Wittgenstein's remarks on the notion of infinite indicate that Wittgenstein wanted to replace the "epistemological" distinction between the humanly feasible and the mathematics of God by a distinction between "grammatical" and "empirical" possibility. The mathematical infinite corresponds to a feature of the laws or rules, not of their extension, i.e. it is a possibility of the symbolism. Therefore, while there may be a bound to the complexity of computations we can in practice carry out, Wittgenstein did not regard these empirical limitations as essential: only what is shown by the rules is the essence. Instead of putting forward some strict finitist doubts, Wittgenstein was trying to avoid the grammatical confusions embodied in the expression "infinite series" from which strict finitist epistemological doubts arise.

There is a danger that the arguments put forward by intuitionists such as M. Dummett to support their revision of classical logic can be used against intuitionism in favour of the more radical revisions of strict finitism. I have argued in section 12 that Wittgenstein is able to criticize the Platonist conception without having to face the challenge of the strict finitist. Instead, he can also criticize strict finitism by the same token —i.e. his critique of the epistemological characterization of the debate about the infinite.

Another attempt to characterize Wittgenstein as a strict finitist consisted in describing him as preoccupied with "feasibility", i.e. in an almagamation of the strict finitist insistence on "feasibility" with Wittgenstein's requirement that proofs must be surveyable. I hope to have argued conclusively in sections 15 and 16 against such an interpretation of Wittgenstein's remarks on surveyability. Therefore, I cannot find any ground to sustain the claim that Wittgenstein was a strict finitist.

There are affinities between Wittgenstein and Brouwerian intuitionism. For example, both Wittgenstein and the intuitionists criticized the unrestricted use of the Law of Excluded Middle (see section 14), but the arguments were different. One of Wittgenstein's prescriptions on the formation of real numbers is that they should be effectively comparable to any rational number. This prescription excludes a number such as Brouwer's *duale Pendelzahl*, which was used by the latter as a

counterexample. Both Wittgenstein and the intuitionists also reject the actual infinite, and accept only the potential infinite, but there was also major differences: while the intuitionists would in the end retain a minimum on quantification theory, Wittgenstein would simply shed it completely (see section 13.)

Moreover, there are many topics on which Wittgenstein disagreed straightforwardly with the intuitionists. I have pointed out, already in section 3, the contrast between Wittgenstein's complete rejection of the Axiom of Choice and the use of weaker versions, such as the Axiom of Countable Choices in intuitionism. Secondly, Wittgenstein's prescriptions on the formation of real numbers—to be discussed below—have as a consequence the rejection of the central notion of the intuitionistic approach to the continuum: the choice sequences (see section 18.)

Finally, Wittgenstein could not agree with the basic tenet of Brouwer's philosophy of mathematics, his "basic intuition" on which he founded the natural numbers series (see section 16.) Clearly, Wittgenstein could not be an intuitionist either. If Wittgenstein was neither a strict finitist, nor an intuitionist, he must have been a finitist of some sort, close to the work of T. Skolem or R. Goodstein. I think I have gathered in this study ample evidence to that effect.

The distinction between the "algorithmic" school of L. Kronecker or P. Gordan and the "axiomatic" and set-theoretic school of R. Dedekind, D. Hilbert, in the first four sections served as a good indication of the profoundly constructivist view of mathematics held by Wittgenstein. He upheld—as Gordan or all the constructivists did after Kronecker—the view that mathematics consists essentially in providing algorithms. Therefore he had, first, a strong bias in favour of constructive proofs against less informative existential proofs (see section 2.) This bias was given a philosophical justification: Wittgenstein linked understanding in mathematics not with the pure verbal understanding of the translation of the proposition in Russellian notation but with the possibility of applying the proposition. The constructive proof would give information leading to applications, while existential proofs are sometimes useless.

Secondly, Wittgenstein had a strong dislike for set-theoretical foundations such as those provided by Dedekind, Frege and Russell & Whitehead (described in section 4.) This is clearly indicated by his agreement with Poincaré's critique of logicist's treatments of mathematical induction as impredicative (section 5) and the fact that he himself proposed a constructivist definition of natural numbers in the TLP (section 6.)

Wittgenstein's severe condemnation of the Axiom of Choice (see section 3) indicate the strenght of his constructivist convictions, and brings him close to Skolem. Moreover, Wittgenstein gave arguments —which appear at first sight rather weak— against of the notion of numerical equality (see section 8.) Again, the rejection of such a basic set-theoretical notion is a distinguishing feature of the finitism of Goodstein.

Wittgenstein's views about generality and quantification, discussed in sections 7 and 13, clearly lead to a rejection of quantification theory: generality is shown by a proof by induction —shown by the proof— and cannot be described by the use of the universal quantifier. This, again, brings Wittgenstein close to Skolem and his work on Primitive Recursive Arithmetic or to R. Goodstein's equational calculus, both authors eschewing quantification theory in their systems.

Wittgenstein's remarks on real numbers and the continuum also bring him close to the tradition of "recursive analysis" (of which Skolem and Goodstein are pioneers.) Indeed, he insisted on two prescriptions on the formation of real numbers. Firstly, Wittgenstein was asking that one possesses an effective rule of construction of every infinite series of rationals, i.e. to compute rational approximations. Secondly, he was asking that any real number must be effectively comparable with every rational number. Put together, these two prescriptions reduce the continuum to "recursive" real numbers, to the exclusion of the "random" real numbers of the classical analyst or the choice sequences of the intuitionist. These prescriptions bring Wittgenstein in line with E. Bishop's reconstruction of classical analysis or, better still, with the "recursive analysis" of, say, O. Aberth where the notion of "law" is replaced by that of "recursive function" (see section 17.)

The link between Wittgenstein's view of the real numbers and his treatment of the notion of infinite (in sections 10 and 11) is clear enough: real numbers being defined as infinite sequences of rationals, they must be introduced by a law, because only laws "reach to infinity". There is no room for real numbers, such as "random" real numbers, given in an "extensional" manner.

I take it also that Wittgenstein's remarks on Cantor's diagonal method imply that he saw the diagonal as an indication of the impossibility of a recursive enumeration of the reals. Therefore he objected to the interpretation of the method leading to Cantor's proof of the non-denumerability of the reals (see section 17.)

Wittgenstein's remarks on the foundations of mathematics were certainly sketchy, often vague and too banale to be of use to the specialists. They bear the mark of writings which were not prepared for publication. But, one thing seems clear now: Wittgenstein was a finitist.

## Appendix 1

A good example of a non-constructive proof is Thue's proof of his famous theorem on Diophantine approximations. It was an improvement on a theorem of Liouville stating that if  $\alpha$  is an algebraic number of degree  $n$ , there exist a constant  $c(\alpha) > 0$  s.t. for all rationals  $p/q$  we have:

$$\left| \alpha - \frac{p}{q} \right| > \frac{c(\alpha)}{q^n}$$

Thue's improvement reads as follows: If  $\alpha$  is an algebraic number of degree  $n$  and if  $\kappa > 1 + (n/2)$ , then there exists a constant  $c(\alpha, \kappa)$  s.t. for every rational  $p/q$  we have:

$$\left| \alpha - \frac{p}{q} \right| > \frac{c(\alpha, \kappa)}{q^\kappa}$$

If  $n > 2$ , Thue's theorem is better than Liouville's. This is an obvious existence theorem, since it is only said that the constant  $c(\alpha, \kappa)$  exists. Thue's original proof was by *reductio ad absurdum*, and it gave no indication as how to calculate effectively  $c(\alpha, \kappa)$  with given values of  $\alpha$  and  $\kappa$ .

An improvement, by diminishing  $\kappa$  so that the conclusion still holds, in terms of  $n$  was obtained by C. Siegel, who has shown that we could take  $\kappa > 2\sqrt{n}$ . This result was further improved by F. Dyson with  $\kappa > \sqrt{2n}$  and finally by F. Roth who proved that we could take  $\kappa > 2$ , i.e. without conditions on  $\alpha$  (Roth 1955: 1-2). All these results are also non-effective.

So far the only constructive result obtained is by N. Feldman, who gave effectively calculable constants  $c(\kappa)$  and  $\kappa(\alpha)$  (the latter slightly smaller than 1) s.t.  $\kappa(\alpha) < n$  and s.t. for all rationals  $p/q$  we have:

$$\left| \alpha - \frac{p}{q} \right| > \frac{c(\alpha)}{q^{\kappa(\alpha)}}$$

(Baker 1975: 46). But no constructivisation of Thue's theorem is in sight. When this theorem is *applied* to the study of Diophantine equations, *its non-effectivity causes problems*. For example in the proof of this other theorem of Thue:

*If  $f(x,y) \in \mathbb{Z}[x,y]$  is an homogeneous form of degree  $n > 2$ , irreducible to  $\mathbb{Q}$ , then for all integer  $k$  the equation  $f(x,y) = k$  has only a finite number of integer solutions.*

I cannot go in all the details of the proof (see (Mordell 1969: Chap. 22)). After a few transformations from  $f(x,y)$ , we have:

$$\left| \alpha_1 - \frac{x_i}{y_i} \right| \leq \frac{A}{y_i^n}$$

with  $A$  being a constant depending only on  $f$  and  $k$ . Here one applies Thue's theorem on Diophantine approximations and one obtains that for all  $\varepsilon > 0$  there is a constant  $c = c(\alpha_1, n/2 + 1 + \varepsilon)$  s.t.;

$$\frac{c}{y_i^{n/2 + 1 + \varepsilon}} < \left| \alpha_1 - \frac{x_i}{y_i} \right| \leq \frac{A}{y_i^n}$$

This leads to a contradiction with  $y_i$  large enough (Baker 1975: Chap. 7). But the constant  $c$  is not effective, otherwise one could obtain from this inequality a calculable constant  $B$  s.t.  $|y| > B$  entailing  $f(x,y) \neq k$ . It would then be possible to calculate all solutions of  $f(x,y) = k$  by trying all values of  $|y| \leq B$ . One had to wait for the work of A. Gelfond and especially A. Baker to obtain effective results (Lang 1978: 176-180). Indeed, Baker was able to prove that all solutions of  $f(x,y) = k$  satisfy:

$$\max(|x|, |y|) \leq \exp((nH)^{10^5})$$

where  $H$  is the greatest element formed of  $|k|$  and the absolute value of the coefficients of  $f$ .<sup>1</sup> This is a good example of the application of a theorem, whose non-effective proof leads to another non-effective result. This shows why there is a need for effective results in mathematical practice, independently of philosophical considerations.

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<sup>1</sup> An extension of this result also due to Baker is discussed in section 9.

## Appendix 2

**p. 22, footnote 1, (Bouveresse 1988: 51):**

Le fait que Wittgenstein et les intuitionnistes aient des conceptions à peu près diamétralement opposées sur les relations de la philosophie et des mathématiques n'exclut évidemment pas *a priori* que le premier ait pu trouver un intérêt et même une raison d'être aux modifications et aux restrictions que les seconds proposent d'introduire dans les mathématiques.

**p. 27, (Kronecker 1882: 256-257):**

Die im Art. 1 aufgestellte Definition der Irreductibilität entberht so lange einer sicheren Grundlage, als nicht eine Methode angegeben ist, mittels deren bei einer bestimmten, vorgelegten Function entschieden werden kann, ob dieselbe der aufgestellten Definition gemäss irreductibel ist oder nicht.

**p. 28, (Molk 1885: 8):**

*Les définitions devront être algébriques et non pas logiques seulement.* Il ne suffit pas de dire: "Une chose est ou elle n'est pas. Il faut montrer ce que veut dire être et ne pas être, dans le domaine particulier où nous nous mouvons. Alors seulement nous faisons un pas en avant. Si nous définissons, par exemple, une fonction irréductible comme une fonction qui n'est pas réductible, c'est à dire qui n'est pas décomposable en d'autres fonctions d'une nature déterminée, nous ne donnons point de définition algébrique, nous n'énonçons qu'une simple vérité logique. Pour qu'*en Algèbre* nous soyons en droit de donner cette définition, il faut qu'elle soit précédée de l'exposé d'une méthode nous permettant d'obtenir à l'aide d'un nombre fini d'opérations rationnelles, les facteurs d'une fonction réductible. Seule cette méthode donne aux mots *réductible* et *irréductible* un sens algébrique.

**p. 28, (Kronecker 1901: vi):**

Er meinte, man könne und man müsse in diesem Gebiete eine jede Definition so fassen, darfs durch eine endliche Anzahl von Versuchen geprüft werden kann, ob sie auf eine vorgelegte GröÙe anwendbar ist oder nicht. Ebenso wäre ein Existenzbeweis für eine GröÙe erst dann als völlig streng anzusehen, wenn er zugleich eine Methode enthalte, durch welche die GröÙe, deren Existenz bewiesen werde, auch wirklich gefunden werden kann.

**p. 29, (Gordan 1893: 132):**

Der Beweis, welchen Herr Hilbert gegeben hat, ist materiell ganz richtig, jedoch empfand ich in seinen Ausführungen insofern eine Lücke, als er sich damit begnügte die Existenz der  $\varphi$  nachzuweisen und darauf verzichtete, ihre Eigenschaften zu erörtern.

**p. 35, (Borel 1905: 194):**

... il faudrait pouvoir donner un moyen au moins théorique de déterminer l'élément distingué  $m'$  d'un sous-ensemble quelconque  $M'$ ; et ce problème paraît des plus ardu, si l'on suppose, pour fixer les idées, que  $M$  coïncide avec le continu.

**p. 35, (Borel 1914: 156):**

... je vois parfois des difficultés aussi graves, à mon avis, dans des raisonnements où n'interviennent qu'une infinité dénombrables de choix que dans des raisonnements où il y en a une transfinité.

**p. 35-6, (Borel 1914: 154):**

La question revient à celle-ci, peu nouvelle: *Peut-on démontrer l'existence d'un être mathématique sans le définir?* C'est évidemment une affaire de convention; mais je crois qu'on ne peut bâtir solidement qu'en *admettant qu'on ne démontre l'existence d'un être qu'en le définissant*. A ce point de vue, voisin de celui de Kronecker...

**p. 38, (Peano 1906: 48):**

Nunc nos debe opina que propositione es vero, aut es falso? Opinione nostro es indifferente.

**p. 42, (Hilbert 1922b: 178):**

... der wesentliche dem Auswahlprinzip zugrunde liegende Gedanke ein allgemein logisches Prinzip ist, das schon für die ersten Anfangsgründe des mathematischen Schließens notwendig und unentberhlich ist.

**p. 54, (Frasquolla 1980a: 645):**

... nella definizione di Frege che in quella di Dedekind appare come essenziale il riferimento alla *totalità* delle classi induttive (Russell), ossia alla totalità delle classi ereditarie di cui è membro lo 0 (a parte l'uso da parte di Dedekind di 1 al posto di 0). Per chi dubitava dell'affidabilità o addirittura della legittimità del concetto classico di insieme questo riferirsi alla totalità delle classi induttive non poteva passare inosservato; ed infatti Poincaré non mancò di richiamare l'attenzione sulla natura impredicativa della definizione logica del numero naturale.

**p. 55, (Poincaré 1905-6: 154):**

...on devrait commencer par établir les propriétés des nombres cardinaux transfinis, puis distinguer parmi eux une toute petite classe, celle des nombres entiers ordinaires. Grâce à ce détournement on pourrait arriver à démontrer toutes les propositions relatives à cette petite classe (c'est à dire toute notre arithmétique et notre algèbre) sans se servir d'aucun principe étranger à la logique.

**p. 56, (Poincaré 1905-6: 26):**

... la démonstration fondamentale avait besoin d'être "complétée" et pour la compléter il fallait "s'appuyer sur la définition du nombre fini". Or cette définition elle-même sur



celle du plus petit infini, et celle-ci à son tour sur la démonstration en litige. Mais cela s'appelle un cercle vicieux.

**p. 56, (Poincaré 1912: 7):**

Ce sont encore des définitions par postulat, mais le postulat est ici une relation entre l'objet à définir et *tous* les individus d'un genre dont l'objet à définir est supposé faire lui-même partie (ou bien dont sont supposés faire partie des êtres qui ne peuvent être eux mêmes définis que par l'objet à définir)...Pour les Pragmatistes une pareille définition implique un cercle vicieux.

**p. 57, (Poincaré 1905-6: 316):**

C'est la croyance à l'existence de l'infini actuel qui a donné naissance à ces définitions non-prédicatives. Je m'explique: dans ces définitions figure le mot *tous*...Le mot *tous* a un sens bien net quand il s'agit d'un nombre fini d'objets; pour qu'il en eût encore un, quand les objets sont en nombre infini, il faudrait qu'il y eût un infini actuel. Autrement *tous* ces objets ne pourront pas être conçus comme posés antérieurement à leur définition et alors si la définition d'une notion N dépend de *tous* les objets A, elle peut être entachée d'un cercle vicieux, si parmi les objets A il y en a qu'on ne peut définir sans faire intervenir la notion N elle-même.

**p. 66, (Frascolla 1980a: 640):**

L'analisi dell'infinito aritmetico conduce così al cuore della prima teoria wittgensteiniana del linguaggio.

**p. 81, (Boutroux 1904: 916):**

Mais, ici encore, qu'ont de commun les éléments respectifs des deux classes considérées, sinon précisément leurs numéros d'ordres, dont nous nous sommes engagés à faire abstraction. Ou bien la correspondance que l'on postule implique déjà la notion de nombre; ou bien il nous est absolument impossible d'affirmer qu'elle ne change pas lorsque l'on passe d'un couple d'éléments à un autre.

**p. 83, (GA1: 88):**

Was thun wir denn eigentlich, wenn wir zum Zwecke des Beweises zuordnen? Offenbar etwas ähnliches, wie wenn wir in der Geometrie eine Hilfslinie ziehen... Wir bringen uns vielmehr in beiden Fällen nur zum Bewusstsein, fassen nur auf, was schon da war.

**p. 90, (Borel 1947: 979):**

Lorsque le fini devient très grand, il soulève les mêmes difficultés que l'infini.

**p. 90, (Borel 1927: 272):**

Une question plus sérieuse me paraît être celle de savoir si l'on doit considérer un nombre comme virtuellement connu lorsque son calcul théoriquement possible exige cependant un temps et une peine hors de proportion avec les possibilités humaines.

**p. 90, (Borel 1927: 272):**

Si nous continuons de la même manière seulement un millier de fois, nous arriverons à définir des nombres dont le calcul pratique n'exigerait pas seulement des myriades d'existences humaines, mais dont l'écriture seule, à supposer qu'ils fussent connus, nécessiterait un poids de papier de beaucoup supérieur au poids du globe terrestre. Doit-on considérer que le dernier des chiffres du millième nombre ainsi défini est pour nous calculable?

**p. 104, footnote 1 (Meschkowski 1967: 262):**

... die ganzen Zahlen sowohl getrennt wie auch in ihrer actual unendliche Totalität als ewige ideen in intellectu Divino im höchsten Gade der Realität existieren.

**p. 110, (Frasquolla 1980a: 641):**

La logica dell'infinito è, per Wittgenstein, la logica di questo "e così via"

**p. 124, (Brouwer 1919: 231, note 4):**

Meiner Überzeugung nach sind das Lösbarkeitsaxiom und der Satz vom ausgeschlossenen Dritten beide *falsch*...

**p. 124-5, (Weyl 1921: 155-156):**

Brouwer begründet seine Ansicht damit, dass man keinen Grund hat zu dem Glauben, jede derartige Existenzfrage lasse sich *entscheiden*; der Beweis der Gültigkeit des Satzes vom ausgeschlossenen Dritten müsste nach ihm in der Angabe einer Methode bestehen, die nachweislich für beliebige Eigenschaften  $F$  die Entscheidung der Existenzfrage im einen oder andern Sinne herbeiführt. Wie bekannt, ist dieser Standpunkt zuerst von Kronecker vertreten worden. In bewusstem Gegensatz dazu habe ich bei meinem Versuch der Grundlegung der Analysis die Meinung vertreten: es komme nicht darauf an, ob wir mit gewissen Hilfsmitteln, z. B. den Schlussweisen der formalen Logik, imstande sind, eine Frage zur Entscheidung zu bringen, sondern *wie sich die Sache an sich verhält*...

**p. 125, (Weyl 1921: 156):**

*Ein Existentialsatz* —etwa "es gibt eine gerade Zahl"—*ist überhaupt kein Urteil im eigentlichen Sinne das einen Sachverhalt behauptet*; Existential-Sachverhalte sind eine leere Erfindung der Logiker. "2 ist eine gerade Zahl": das ist ein wirkliches, einem Sachverhalt Ausdruck gebendes Urteil; "es gibt eine gerade Zahl" ist nur ein aus diesem Urteil gewonnenes *Urteilabstrakt*.

**p. 126, (Weyl 1921: 157):**

Ebensowenig ist das generelle "Jede Zahl hat die Eigenschaft  $F$ "—z.B. "Für jede Zahl  $m$  ist  $m+1=1+m$ "—ein wirkliches Urteil, sondern eine generelle *Anweisung auf Urteile*.

**p. 129, (Hilbert 1931: 131):**

Etwazu gleicher Zeit, also schon vor mehr als einem Menschenalter, hat Kronecker eine Auffassung klar ausgesprochen und durch zahlreiche Beispiele erläutert, die heute im wesentlichen mit unserer finiten Einstellung zusammenfällt.

**p. 149, (Brouwer 1928: 163):**

... ist nicht rational, trotzdem ihre Irrationalität absurd ist, und nicht mit Null vergleichbar, trotzdem ihre Unvergleichbarkeit mit Null absurd ist.

**p. 149, (Brouwer 1928: 161):**

Diese duale Pendelzahl ist weder gleich Null noch von Null verschieden, im Gegensatz zum Prinzip des ausgeschlossenen Dritten.

**p. 166, (Hilbert 1918: 153):**

... *Kriterium für die Einfachheit* von mathematischen Beweisen ...

**p. 168, footnote 1, (Bourbaki 1948: 37):**

...sans essayer de concevoir clairement les idées qui ont conduit à bâtir cette chaîne de déductions de préférence à tout autre.

**p. 178, (Borel 1914: 168):**

... dans tous les Traités de Géométrie élémentaire se trouvent indiquées des méthodes qui permettent ... de calculer autant de décimales qu'on veut. Ce calcul serait naturellement fort long, si l'on exigeait un très grand nombre de décimales; mais nous pouvons concevoir qu'il puisse être effectué; nous pouvons d'ailleurs espérer que le perfectionnement de l'analyse permette d'abrégier la durée des calculs; mais, quoi qu'il en soit de ce dernier point, il paraît incontestable que la valeur de la 1000e ou de la 3645e décimale de  $\pi$  est actuellement bien déterminée, même si le calcul n'a pas encore été effectué.

**p. 190, (Frascolla 1980a: 667):**

Uno degli effetti di questa restrizione è l'esclusione dal campo dei numeri reali di quelle strutture matematiche che L. E. J. Brouwer aveva costruito per dimostrare l'inapplicabilità del Principio del Terzo Escluso ai ragionamenti su insiemi infiniti.

**p. 195, (Brouwer 1914: 140):**

... für den Intuitionisten nur wohlkonstruierte unendliche Mengen existieren, welche sich zusammensetzen aus einem Teile erster Art, das sich als eine einzige Fundamentalreihe erzeugen läßt, und einem Teile zweiter Art, dem eine Fundamentalreihe  $f$  als Fréchetsche V-klasse zugrunde liegt, während seine Elemente in solcher Weise durch je eine Folge von Auswahlen unter den Elementen einer Endlichen Menge oder eine Fundamentalreihe bestimmt werden, daß jeder Folge von

Auswahlen eine Folge von einander einschließenden Teilgebieten von  $f$  mit gegen Null konvergierender Breite entspricht, und in den je zwei verschiedenen Folgen von Auswahlen entsprechenden Gebietsfolgen zwei außerhalb voneinander liegende Endsegmente existieren.

**p. 196, (Weyl 1921: 153):**

Repräsentiert das *Gesetz*  $\varphi$ , welches eine Folge ins Unendliche hinaus bestimmt, die einzelne Reelle Zahl, so die durch kein Gesetz in der Freiheit ihrer Entwicklung eingeschränkte Wahlfolge das *Kontinuum*.

**p. 196-7, (Weyl 1927: 531):**

Daß es mathematisch möglich ist, mit werdenden Wahlfolgen zu operieren, ist schon dadurch hinreichend belegt, daß man Zuordnungen zwischen Wahlfolgen stiften kann. Z. B. enthält die Formel

$$n_h = m_1 + m_2 + \dots + m_h \quad (h = 1, 2, 3, \dots)$$

ein Gesetz, gemäß welchem eine durch freie Wahlakte werdende Folge  $m_1, m_2, m_3, \dots$  eine werdende Zahlfolge  $n_1, n_2, n_3, \dots$  erzeugt, deren Entstehung mit ihr gleichen Schritt hält.

**p. 198, (Frasquolla 1980a: 673):**

... l'obiezone di Wittgenstein manca il bersaglio perché l'albero delle bisezioni o la legge di spiegamento vogliono rappresentare non la prescrizione per la generazione di un numero ma lo schema generale di costruzione delle infinite successioni infinitamente proseguibili di 0 e di 1.

## REFERENCES

### Abbreviations

#### 1) WITTGENSTEIN

The following abbreviations are used to refer to Wittgenstein's published works, listed in the chronological order of publication. References are usually made to the paragraph number, otherwise to the page number.

Published books and papers:

<b>TLP</b>	<i>Tractatus Logico-Philosophicus</i> , Intr. by B. Russell, tr. C.K. Ogden, London, Routledge & Kegan Paul, 1922
<b>PI</b>	<i>Philosophical Investigations</i> , Oxford, Blackwell, 1953
<b>RFM</b>	<i>Remarks on the Foundations of Mathematics</i> , revised sec. ed., Oxford, Blackwell, 1978
<b>B B</b>	<i>The Blue and Brown Books (Preliminary Studies for the "Philosophical Investigations", Generally Known as the Blue and Brown Books)</i> , Oxford, Blackwell, 1958
<b>N B</b>	<i>Notebooks 1914-1916</i> , revised sec. ed. Oxford, Blackwell, 1979
<b>PR</b>	<i>Philosophical Remarks</i> , Oxford, Blackwell, 1965
<b>Z</b>	<i>Zettel</i> , revised sec. ed., Oxford, Blackwell, 1981
<b>PG</b>	<i>Philosophical Grammar</i> , Oxford, Blackwell, 1974
<b>CV</b>	<i>Culture and Value</i> , Oxford, Blackwell, 1980
<b>RPP 1</b>	<i>Remarks on the Philosophy of Psychology</i> , Vol. I, Oxford, Blackwell, 1980

Letters:

<b>LO</b>	<i>Letters to C. K. Ogden with Comments on the English translation of the "Tractatus-Logico-Philosophicus"</i> , Oxford, Blackwell, 1973
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Lecture and discussion notes not made by Wittgenstein:

<b>M</b>	G. E. Moore, "Wittgenstein's Lectures in 1930-33" in G.E. Moore, <i>Philosophical Papers</i> , Londres, Allen & Unwin, 1959, pp.252-324
<b>LC</b>	<i>Lectures and Conversations on Aesthetics, Psychology, and Religious Belief</i> , from the notes of Y. Smythies, R. Rhees & J. Taylor, Berkeley,

University of California Press, 1967

- LFM** *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, from the notes of R. Bosanquet, N. Malcolm, R. Rhees & Y. Smythies, Ithaca, Cornell University Press, 1976
- AWL** *Wittgenstein's Lectures, Cambridge 1932-1935*, from the notes of A. Ambrose & M. McDonald, Oxford, Blackwell, 1979
- WWK** *Ludwig Wittgenstein and the Vienna Circle*, from the notes of F. Waismann, Oxford, Blackwell, 1979
- LWL** *Wittgenstein's Lectures, Cambridge 1930-1932*, from the notes of J. King & D. Lee, Oxford, Blackwell, 1980

## 2) FREGE

- B g** *Begriffsschrift, a formula Language, Modeled upon that of Arithmetic, for Pure Thought*, in (van Heijenoort 1967), pp. 5-82
- FA** *The Foundations of Arithmetic*, Oxford, Blackwell, sec. ed., 1980
- GA1** *Grundgesetze der Arithmetik, Begriffsschriftlich abgeleitet*. Vol. 1, Jena, H. Pohler, 1893.  
Partial English translation:  
Furth, M., ed., *The Basic laws of Arithmetic. Exposition of the System*, Berkeley, University of California press, 1964
- GA2** *Grundgesetze der Arithmetik, Begriffsschriftlich abgeleitet*, Vol. 2, Jena, H. Pohle, 1903  
Partial English Translation in:  
Black, M., Geach, P., eds., *Translations of the Writings of Gottlob Frege*, Oxford, Blackwell, 3rd ed., 1980; pp.
- CP** *Collected Papers on Mathematics, Logic, and Philosophy*, Oxford, Blackwell, 1984
- PW** *Posthumous Writings*, Oxford, Blackwell, 1979

## 3) WHITEHEAD & RUSSELL

References to A. N. Whitehead and B. Russell's *Principia Mathematica* are to the paragraph (\*) number or, as in the case of its introduction, to the page number of the second edition. All references are to the first of three volumes.

- PM** *Principia Mathematica*, Cambridge, Cambridge University Press, 3. Vol., sec. ed. 1927

## 4) ARISTOTLE AND KANT

Translations of Aristotle's *Physics* are by E. Hussey, published by the Clarendon Press, Oxford, in 1983; and the translation of Kant's *Critique Of Pure Reason* is from N. Kemp-Smith, published by Macmillan, London, 1934. References to these works are following the usual conventions.

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Where an English translation was already available, I used it, otherwise translations are mine and the quotations are followed by \*. The reader will find the original text in the Appendix 2. Where I quoted a passage for which a translation already existed, references are to the page number of the English translation, not of the French, Italian or German original. In order not to increase the length of the bibliography I decided not to give the original title and reference of any work whose English translation is used. Moreover, I shall only indicate the edition I used, not previous or even the more recent ones. But, in order to make more transparent the original year of publication, works are referred to by the year of their first publication, which, of course, may not correspond to the date of the edition used.

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