INTERVIEW

Wittgenstein's Philosophy of Mathematics

Felix Mühlhölzer in Conversation with Sebastian Grève

GRÈVE: Wittgenstein's philosophical remarks about mathematics have constituted a significant part of your academic work for over two decades now. But how, especially given your early background as a mathematician and philosopher of science, did you first get interested in Wittgenstein at all? And what do you think were the reasons behind the subsequent steady shift of focus in your academic work towards Wittgenstein and Wittgensteinian philosophy?

MÜHLHÖLZER: I studied mathematics, and also physics, in Mainz, Bonn and Heidelberg, specialised in algebraic topology and wrote my thesis with Albrecht Dold on sheaf cohomology. At that time, I wasn't working on philosophy at all. However, I began to hate the competitive spirit prevalent in mathematics, which also influenced my own attitude towards mathematics, and it became clear to me that, when I looked past this spirit, what really attracted me wasn't mathematics but philosophy. So I declined Dold's offer to write a dissertation in mathematics with him and in 1975 went to Wolfgang Stegmüller's large institute in Munich in order to work on a dissertation in philosophy. Eike von Savigny was there, and we agreed that I might write about later Wittgenstein's view of concepts, especially in his philosophy of mathematics. But then von Savigny went to Bielefeld and I wanted to stay in Munich. So I decided to write about conceptual problems in relativity theory, which had troubled me for a long time, with Stegmüller as my advisor, who had given seminars about this topic. This resulted in my dissertation about the notion of time in special relativity and also in my *Habilitationsschrift* of 1989 about Thomas Kuhn's notion of incommensurability, which I investigated in the case of the transition from Newtonian to Einsteinian gravitation theory. During this time, and also for several years afterwards, I became very involved in physics, but this made me more and more dissatisfied because it distracted me from genuine philosophy. So I eventually changed the course of my philosophical projects and finally and decisively turned towards the philosophy of the later Wittgenstein, especially his philosophy of mathematics since this corresponded with my background as a mathematician. This step led me to abandon the philosophy of science almost entirely (my little book entitled *Wissenschaft* of 2011 is only a sort of reverberation).

What had first attracted me to later Wittgenstein was his style, both philosophical and literary. (The *Tractatus*, on the other hand, despite being written by the same person, has remained foreign to me right up to the present day.) I'm fascinated by the inimitably down-to-earth manner of his philosophising, expressed in refreshing language, full of illuminating similes and interspersed with a characteristic sort of philosophical humour. At the same time, this philosophy is really deep and – certainly a sign of genius – inexhaustible. There are so many diverse echoes in his texts that every time one rereads them, new aspects rise to the surface. Now, this wonderful sort of philosophy is also extensively concerned with mathematics but, alas, not sufficiently appreciated in this domain, and even despised by many. So my aim is to show that it deserves more attention than it gets at present. Regardless of that, however, I'm happy to be working on his texts simply on their own merits, quite independently of their public appreciation. To me it is extremely satisfying to indulge in them. I hope the reasons why this is so will become sufficiently clear in the course of our conversation.

GRÈVE: "Does mathematics stand in need of a philosophical foundation?" This question can be seen to have played a major role in Wittgenstein's overall philosophical development from the very beginning; as when in 1911, following the advice of Gottlob Frege, he decided to abandon his studies in aeronautics at the University of Manchester and go to Cambridge instead, in order to study under Bertrand Russell. You first discussed this question in a short article about 15 years ago (Mühlhölzer 1998; cf. also 1999). Then, in 2010, you finally published your celebrated first 600-page volume of a commentary on Wittgenstein's *Remarks on the Foundations of Mathematics* [RFM], which bears this very question as its title: in German, "*Braucht die Mathematik eine Grundlegung"?*

You chose the German word "*Grundlegung*" for your title, rather than "*Grundlagen*" as in the German title of RFM, *Bemerkungen über die Grundlagen der Mathematik* [BGM]. As a gerund the former suggests activity, while the latter suggests passivity or even factivity. The former suggests that something is being grounded, to put it in contemporary terms, while the latter has it that there actually already exists a ground which we only have to dig out. On the face of it, this contrast could easily seem to be one between a constructivist account of the so-called "foundations" of mathematics and a realist one. However, considering Wittgenstein's unique approach to philosophising about mathematics, it would seem that you have yet another difference in mind when asking – in what would hence appear to be a somewhat more accurate translation – "Does mathematics stand in need of philosophical grounding?"

MÜHLHÖLZER: What I had in mind when distinguishing between "*Grundlegung*" and "*Grundlagen*" is nothing deep, really. *Grundlegungen* produce *Grundlagen*, and all *Grundlagen* are produced by *Grundlegungen*. I didn't entertain *Grundlagen* without *Grundlegungen*, but this does not point to "constructivism". You are right that the contrast between "constructivism" and "realism" is not at issue here. The title of my book is an allusion to the wonderful § 16 of RFM VII which I chose as its motto and which begins (in Anscombe's translation) as follows:

> What does mathematics need a foundation [*Grundlegung*] 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. | The *mathematical* problems of what is called foundations [*Grundlagen*] are no more the foundations of mathematics for us [*liegen für uns der Mathematik so wenig zu Grunde*] than the painted rock is the support of a painted tower [*wie der gemalte Fels die gemalte Burg trägt*].

As you see, in this translation the difference between "*Grundlegung*" and "*Grundlage*" is evened out: both German words appear as "foundation". In my book the whole first chapter of its Introduction is devoted to the expression "*Grundlagen der Mathematik*", and I start with the feeling of puzzlement that in several cases Wittgenstein uses it in order to point to his own endeavours (and that, furthermore, it occurs in the title of BGM), despite the fact that at the same time he disputes the need for any foundation of mathematics. How can this be reconciled? Now, it seems to me that in most cases he uses the expression "*Grundlagen der Mathematik*", or "foundations of mathematics", in order to name and to criticise existing attempts at founding mathematics, especially the attempts by Frege, Russell/Whitehead, Brouwer, Weyl and Hilbert. And very often he then puts this expression, or at least the word "*Grundlagen*/foundations", in quotation marks – as he should actually also have done in the quotation above, when he writes: "of what is called foundations [*Grundlagen*]". In RFM III in particular, which is the subject of my book, he explicitly investigates the foundations proposed by Frege, Russell and Hilbert, and the very end of this part of RFM, § 90, which comes along with the appearance of a specific weightiness, should be understood accordingly. It reads as follows:

> I have not yet made the role of miscalculating clear. The role of the proposition: "I must have miscalculated". It is really the key to an understanding of the 'foundations' ['*Grundlagen*'] of mathematics.

With this almost dramatic statement, Wittgenstein isn't referring to the key to his *own* understanding of mathematics, but to foundationalist endeavours like the Hilbertian one that aim at consistency proofs. This section (§ 90) is the culmination of

Wittgenstein's reflections about confusions produced by contradictions, and I comment on them in the last chapter of my book.

When "foundation" is understood as referring to the usual foundationalist programmes, then the title of RFM is misleading because this book contains much more than discussions of such programmes. Of course, one might also call Wittgenstein's own investigations "foundationalist" insofar as they aim at exposing important – and, as one might say, basic – structures and presuppositions of our actual mathematical practice. But I would prefer not to talk that way because what Wittgenstein does is too remote from normal foundationalist projects. When criticising the title of RFM, however, I should actually criticise the title of my own book as well, because only in the comments on the last fourteen sections of part III, where Wittgenstein discusses the alleged necessity of proving the consistency of mathematics, is the question as to whether mathematics *needs* a foundation really in the foreground. In all the preceding sections we merely find discussions of the foundationalist programmes by Frege and Russell as they happened to exist, with only very peripheral looks at the alleged necessity of such programmes. On the other hand, when Wittgenstein points out important failures of these programmes, he of course also casts doubts on all claims as to their necessity, and so my title might yet be justified in the end.

GRÈVE: Usually, the search for foundations is considered to be a central feature of the philosophy of mathematics. If Wittgenstein rejects that, what is his own view of this philosophy?

MÜHLHÖLZER: He says it in the passage quoted above: it aims at a clarification of the grammar of mathematical propositions, where what he has in mind are not the propositions, or pseudo-propositions, of foundationalist reconstructions of mathematics, but propositions as used within real mathematical practice. I have a tendency to call the foundationalist ersatz propositions "pseudo-propositions" because they are normally not really *used*, whereas it is precisely the way of being used which Wittgenstein considers to be essential, and this is what he means when talking about the "grammar" of mathematical propositions. And the clarification of this grammar is no end in itself but aims at solving, or dissolving, pertinent philosophical problems, problems regarding "infinity" in mathematics, mathematical necessity and apriority, the "formal" character of mathematics, what mathematical proofs consist in, and so on. In this way, Wittgenstein wants to get a better understanding of the most important features of this peculiar, iridescent practice called "mathematics".

One outstanding problem for Wittgenstein is the relation of mathematics to the empirical world. In § 23 of RFM VI he writes: "Not empiricism and yet realism in philosophy, that is the hardest thing", and the word "realism" is not meant here in its philosophical sense but in a very down-to-earth way, as when one says that someone behaves realistically and as Wittgenstein himself uses it, for example, in RFM III § 76 (extensively discussed in my book), where he writes:

> The conception of calculation as an [empirical] experiment tends to strike us as the only *realistic* one. | Everything else, we think, is moonshine. In an experiment we have something tangible. [...] | It looks like obscurantism to say that a calculation is not an experiment.

The challenge for him then is to show in what way a calculation – and a mathematical proof – is *not* an experiment, without falling victim to moonshine, to obscurantism, to a non-realistic mind-set. This problem pervades Wittgenstein's whole philosophy of mathematics and gives it its characteristic flavour.

GRÈVE: Perhaps continuing on the topic of representing Wittgenstein's whole philosophy of mathematics... The first volume of your commentary starts with what the editors published as part III of RFM. To many, it would probably seem more natural to start with part I; besides numeric order, there would seem to be another good reason to do so, since (after all) part I represents the intriguing set of remarks which Wittgenstein once intended as the continuation of the first 188 remarks of *Philosophical Investigations*, as is still reflected in the 1945 draft of a preface which has been published alongside the final version of the PI.

You, however, appear to think differently about this matter. Not only does the first volume of your commentary consist of the exegesis of part III, but you are currently working on the second volume which will contain the exegesis of part II. Also, you are about to finish a book, co-authored with Juliet Floyd, on Wittgenstein's annotations to Hardy's *A Course of Pure Mathemathics*, which will contain commentaries on much of part V and its underlying manuscripts. What are your reasons for proceeding in this rather unintuitive order, and when can we hope to receive your thoughts on part I?

MÜHLHÖLZER: The main reason why I started my project of publishing commentaries on BGM with part III is a purely pragmatic one (and I now talk mainly about "BGM", not "RFM", because it is the German original and not the translation that I am commenting on). I have been working on BGM for many years now, and I have actually written comments on every part, including the Appendices to part I, but in the end I felt that only my commentary on part III might be ripe for publication. But I can also give a reason for starting with part III related to its subject matter. It is there that, by rejecting foundationalist projects, Wittgenstein makes sure that he will not be bothered by foundationalist thoughts anymore, with the consequence that he can now freely turn to other questions, questions which he thinks to be the really worthwhile ones and which he discusses in the remaining parts of BGM. Therefore, starting with BGM III may be entirely the correct order.

As for my present work on part II and, together with Juliet Floyd, part V, I can offer similar reasons. I think that I have now given enough thought to them, and as a reason related to the subject matter I can refer to the fact that in these parts mathematics is discussed not only in elementary but also in more advanced forms. Here, we come across mathematics proper, so to speak, more than in other parts of BGM.

GRÈVE: Paul Bernays wittily remarked that Wittgenstein all too often writes as if mathematics existed solely for the purpose of housekeeping...

MÜHLHÖLZER: But that is not true of what we find in parts II and V. It might be profitable to look first at what Wittgenstein said there about specific issues of mathematics proper – about real numbers, Cantor's diagonal method, set theory, Dedekind cuts, limits, continuity, and so on – before turning to more general questions in the philosophy of mathematics. Incidentally, when Wittgenstein returned to Cambridge in January 1929, his very first note, written on 2 February 1929 in his first manuscript from this period, immediately starts with a remark about irrational numbers:

> Is a space conceivable that contains all rational points but not the irrational points? | And that means only: aren't the irrational numbers already prejudged in the rational ones? (MS 105, p. 1; my translation)

Thus, Wittgenstein himself started his post-*Tractarian* thinking, which then led to his mature philosophy of mathematics, with questions concerning specific issues in non-elementary mathematics. The idea of treating such questions before turning to more general ones is certainly not beside the point, and therefore, again, the publication of commentaries on later parts that precede those on earlier ones can appear to be justified.

For a long time, I thought that parts II and V of BGM were rather bad and that it would be better to ignore them, at least if one likes the philosophy of the later Wittgenstein, but I now see them differently. I'm impressed now by the thoroughness with which Wittgenstein develops his *non-extensionalist* view of mathematics and by the interesting insights this brings about. In my forthcoming commentary on BGM II, and the joint book with Juliet Floyd, which deals with much of BGM V, this view will be defended, as far as possible – not as a replacement of extensionalism in mathematics, but as an important addition which is more faithful to our actual practice than its extensionalist counterparts. Therefore, what will be presented is not merely an "exegesis". This is already true of my commentary on part III, which I called "critical and constructive" (see 2010a: 12), and will be even more true now with respect to what I will say about part II. Strictly speaking, I should not call this a "commentary" at all, but I do not know a better word. Maybe it might be called a "meditation" on part II if this didn't sound so highfalutin. But, of course, I will comment on every single section as in a genuine commentary.

What about BGM I? To my mind, this is the most difficult part, and I must admit being a bit daunted by it because right now there are still too many passages in it which I find confusing. But my plan is to write on it in a future monograph. After that, however, I expect I will not continue the project of commenting on BGM, if only for reasons of age. But isn't this project as such highly questionable? Strictly speaking, BGM is not the product of Wittgenstein himself – with the sole exception, perhaps, of part I – but a compilation of remarks which have been extracted by the editors from Wittgenstein's manuscripts, often with huge gaps and even with rearrangements. Can this be a reliable basis for a serious engagement with Wittgenstein's philosophy of mathematics? – Certainly not! But we now have straightforward access to Wittgenstein's *Nachlass* through the *Bergen Edition* which can be taken into account. So my commentaries are replete with quotations of *Nachlass* passages which are important, and sometimes indispensable, if one is trying to really understand Wittgenstein's thought (see my paper 2012b). At the same time, it is still advisable to comment on BGM and not on the *Nachlass* as such, because BGM is the text that most people who have written about Wittgenstein's philosophy of mathematics took as their basis. So my strategy is to accept this book as it is, but to complement it with relevant passages from the *Nachlass*. I comment on these passages as well – but not on more. Of course, it would be desirable to have available a publication of the complete *Nachlass*, preferably in a German-English edition, but these are dreams for the future.

Let me add one short remark on the issue of language. Many people have asked me why I do not write my commentaries in English. But this would imply, I think, that I do not comment on BGM itself but on its existing translation by Elizabeth Anscombe, that is, on RFM. This translation, however, cannot be taken at face value. It needs corrections in many places, and so preparatory steps would be needed throughout in order to present these corrections and also to explain them, and only after that could the actual commentary on Wittgenstein's text begin. A far too laborious and complicated procedure, to my mind, and so I feel practically forced to write these books in German, even though I write most of my articles in English. The book I have recently co-authored with Juliet Floyd is an exception: it will be in English.

GRÈVE: In connection with the question of the value of a commentary on this particular edition, there is the more general question of the overall value of Wittgenstein's remarks on mathematics in the wider context of the philosophy of mathematics. What, for instance, might have been the historical significance of "Wittgenstein's standpoint" in comparison with those of, say, Russell, Frege, Hilbert or Brouwer? Or, what, if anything, is there to learn from Wittgenstein from a contemporary perspective?

For example, some proponents of paraconsistent logic have claimed Wittgenstein to be the unexpected father of their theories. On the other hand, it has often been argued that Wittgenstein really did not have a sufficient understanding of any higher mathematics; and, more specifically, that he lacked a basic understanding of Gödel's incompleteness theorems (cf. Dummett 1959) or the Dedekind cut (cf. Putnam 2007).

MÜHLHÖLZER: It seems to me that Wittgenstein's thinking about mathematics is of barely any historical significance. This is already true of the *Tractatus*, which as a whole, of course, had an important impact on many philosophers, but the view of mathematics presented there is so narrow that one cannot even speak of a *Tractarian* "philosophy of mathematics". From 1929 onwards, Wittgenstein considerably widened his view on mathematics, actually created a totally new view, but this couldn't gain much significance either. Although Waismann had the opportunity to present the beginnings of this view in September 1930 at the second *Conference on the Epistemology of the Exact Sciences* in Königsberg, his contribution was printed only after his death and in a rather fragmentary form (see my *forthcoming (c)*). Hans Reichenbach, in a brief report on the conference, speaks of "[t]he strange views of Wittgenstein", and in the famous discussion at the conference, in which Gödel mentioned his undecidability result, Carnap expressed the opinion that Wittgenstein's ideas weren't yet ripe for debate. Judgements of this sort seem to prevail until the present day.

I myself am mainly interested in Wittgenstein's thoughts about mathematics in his later philosophy, and from now on I will talk only about that. To my mind they include much of his so-called "middle" period. There, mathematics is of particular importance, and several interesting investigations in this period – about metamathematics, say, or about inductive proofs – are not really continued later. It seems to me that the difference between "middle" and "later" Wittgenstein is not so important in the case of his philosophy of mathematics anyway, in contrast to the philosophy of language or mind.

What, then, are the reasons for the widespread rejection of his thinking about mathematics? The following two reasons immediately come to mind: there is, first, the fact that Wittgenstein could never bring himself to seriously consider the publication of his remarks about mathematics, or at least an appropriate selection of them. This sorry state of affairs, which from the outset may make them appear questionable, was in a sense made even worse when Wittgenstein's remarks were eventually published in BGM/RFM, because there – with the exception of part I – they are presented in a chaotic way which all too often results in serious contortions of Wittgenstein's actual trains of thought. It is precisely the aim of my commentaries, and also of some of my articles (see for example 1999, 2001, 2006, 2008 and 2012a, b), to adjust this situation by taking account of the *Nachlass* and by trying to give sensible interpretations. A second and presumably even more important reason for the widespread rejection of Wittgenstein's philosophy of mathematics lies in the fact that it intimately belongs to the characteristic new thinking of his later philosophy, which too many people simply cannot endure. In a way, his thinking about mathematics is a sort of training ground for this new way of practising philosophy. Wittgenstein's later philosophy in its entirety, of course, could only gain significance during the short time when so-called "ordinary language philosophy" was in flower, a time long gone, but even then his philosophy of mathematics certainly wasn't in the foreground. Today, ordinary language philosophy is almost dead. As of late, however, a certain tendency to reanimate it can be seen, including the philosophising of the later Wittgenstein, and I very much welcome this. My own writings about Wittgenstein can be understood as contributions to this tendency.

What about Wittgenstein's knowledge and understanding of mathematics? He had a diploma in mechanical engineering, and this certainly involved knowledge of mathematics, at least in its applied forms. When he was in Manchester in 1908–11, the mathematician Littlewood was also there and Wittgenstein attended some of his lectures on analysis. Later, Wittgenstein studied Hardy's *Course of Pure Mathematics*, at least the chapters about real numbers, functions of real variables and limits of sequences of real numbers. He read out extracts from the fifth edition of this book and critically discussed them in his lecture "Philosophy for Mathematicians" of 1932/33, and he made very interesting annotations to its eighth edition of 1941 which then flowed into his MSS 126 and 127, which are the basis of BGM V. As already mentioned, Juliet Floyd and I are currently finishing a book about these annotations and I hope it will be published soon.

So Wittgenstein got a basic education in analysis, and all his texts show that he had a solid understanding of it. What always remained foreign to him, however, were the very abstract tendencies of mathematics that have developed since Cantor and Dedekind, and one shouldn't conceal the fact that his philosophising about mathematics is pervaded by a subliminal resentment. He is suspicious throughout of what he considered to be superfluous abstraction or too highbrow. Regarding set theory, he often writes as if this theory were nothing but a craving for mathematical sensations – which is wrong for several reasons and clearly shows Wittgenstein's limitations. When reading his texts, it is important not to be dragged into this sort of resentment. Furthermore, the important topic of "axiomatics" is almost completely absent in his writings. It is touched upon when he discusses contradictions in mathematics, but all the other functions of axiomatisations, several of them really essential to mathematics, are left out. So when dealing with Wittgenstein's philosophy of mathematics, it is important to bear these defects in mind.

However, I do not think that this invalidates his remarks on mathematics. They offer an interesting perspective which is full of quite peculiar and philosophically important insights that are worth taking seriously. In this way, I try to interpret Wittgenstein's texts about mathematics charitably, while remaining aware of his limitations. In many cases, these limitations are simply not relevant because of the fact that mathematics – in contrast to physics, for example – exists in very elementary forms. Even as small children, we become acquainted with numbers and calculations, and this can be enough material for the philosophy of mathematics, as when one thinks, for example, about mathematical necessity or apriority. Accordingly, very often Wittgenstein can be quite content to remain on an elementary level, and this should not be a cause for wrinkling one's nose. On the contrary, it most clearly shows Wittgenstein's healthy tendency to avoid superfluous complications in order to carve out what is essential to the problems at hand.

As for Dedekind cuts and Gödel's incompleteness theorems, which you mention as specific targets of Wittgenstein criticism, I don't think that Wittgenstein didn't understand these things, nor does he reject them. But he also doesn't display the typical scientistic awe of them and asks probing questions. These are questions that any intelligent person can ask, even today in fact, and to detect in them a Wittgensteinian non-understanding is all too often a sign of not having understood Wittgenstein himself. This can be shown in detail, I think – I do not treat it as an a priori truth! – but not in this interview. As for the case of the Dedekind cuts, this will be a central subject in the book by Juliet Floyd and me. When, on the other hand, people feel inspired by Wittgenstein's texts, then this is very pleasant, of course. You mention proponents of paraconsistent logic, but I haven't carefully studied this sort of logic (my impression has always been that it is irrelevant to mathematics) and I cannot say anything substantial about it.

GRÈVE: You yourself make systematic use of Wittgenstein's texts in your "Wittgenstein and Surprises in Mathematics" (2002a) and also in "Mathematical Intuition and Natural Numbers" (2010b)...

MÜHLHÖLZER: That's right. "Wittgenstein and Surprises in Mathematics" is, as far as I know, the first paper at all about the topic of "surprises in mathematics", at least with respect to Wittgenstein, explaining his pithy statement on p. 111 of RFM: "If you are surprised [at a mathematical result], then you have not understood it yet." Since the paper was published, this topic seems to have gained a certain popularity. In "Mathematical Intuition and Natural Numbers" I investigate Charles Parsons' attempt at reanimating a Kantian notion of intuition in order to establish with its help the existence of numbers, for example in the perceptible form of strings of strokes which, according to Parsons, constitute an "intuitive model" of arithmetic. However, there is the difficulty that these objects do not have the precision and context-independence usually demanded of numbers. I argue that in order to overcome this difficulty it is necessary to consider the way we *use* these objects – the strings of strokes, say – but then "intuition" no longer seems to play an important role. This is a typically Wittgensteinian sort of argument, and in fact Parsons' approach is tailor-made for a Wittgensteinian criticism.

My main interest at present is expressed in two recent papers of mine, "On Live and Dead Signs in Mathematics" (2014) and "How Arithmetic Is About Numbers" (*forthcoming* (*b*)), which are continuations of what I wrote about metamathematics (2012a). In these papers, I address a problem that has occupied philosophers as well as mathematicians. It accrues from elementary results in model theory to the effect that many formalised theories, e.g., first-order Peano Arithmetic, have non-standard models, and it can be regarded as a dilemma, which I call the *aboutness dilemma*. Its first horn states that if one gives the aboutness of, say, arithmetic a precise formulation, as is done by the model-theoretic notion of interpretation, then one cannot capture it uniquely; there is a multitude of non-intended, non-standard interpretations. In its second horn, one observes that in the metalanguage, in which model theory is expressed, and typically expressed in a nonformalised way, one can very well single out the intended standard model, but then the aboutness of arithmetic is not made precise or transparent. I try to dissolve this dilemma by arguing that it is rooted in a blurring of the categorial difference between the notions of "interpretation" and "reference". The interpretations mentioned in the first horn are simply mathematical functions that do not involve any *use* of the so-called "signs" that are interpreted. These signs are "dead", *petrified* as I call it, and one shouldn't think that petrification is simply idealisation. Idealisation abstracts from non-essential aspects, while petrification abstracts from the essential ones, namely, use. In contrast to that, the second horn of the aboutness dilemma concerns "reference", a notion which according to Wittgenstein (see PI § 10) is essentially tied to the use of signs. This view adopts what I call the *full use thesis*, saying that there is *nothing more* to "reference" than what can be seen in the use of our terms, and rejects the *partial use thesis*, which is usually adopted in the literature and which only says that the use of our terms *contributes* to the necessary link between ourselves and the objects referred to. From this Wittgensteinian point of view one can accept the first horn of the aboutness dilemma, as asserting an uncontroversial mathematical result concerning interpretation, and at the same time the second horn as well, as presenting a perspective on "reference" that is sufficiently clear – albeit not precise, but this shouldn't be expected of "reference" from the outset. In the course of these considerations I also go into an interesting argument presented by Volker Halbach and Leon Horsten (2005), to the effect that the intended standard interpretation of arithmetic may be singled out with the help of Tennenbaum's Theorem, a seemingly surprising result in model theory. I show that this argument does not work. This becomes especially clear if one looks at the *proof* of Tennenbaum's Theorem – in the light of which, by the way, the theorem itself appears not at all surprising.

To my mind, considerations of this sort show that contemporary philosophy of mathematics could very well learn important lessons from a Wittgensteinian perspective. But what is, as you ask, the overall value of Wittgenstein's remarks on mathematics? It lies, I believe, in the perspective he adopts, the perspective on what we are *really doing* in mathematics, beyond any reconstructions, prejudices and one-sided ways of thinking, like the extensionalist ones for example. Precisely this realism – a realism in a non-philosophical sense, as called for in the passage in § 23 of RFM VI, which I quoted in my answer to one of your earlier questions – is an essential value of Wittgenstein's philosophy of mathematics and the main reason why I think that it should be taken seriously.

GRÈVE: In most of your written work about Wittgenstein's philosophy of mathematics you do not engage much with the literature on the topic, which is – although in proportion to existing literature on other parts of Wittgenstein's philosophical writings still quite under-represented – nevertheless immense. In all of your texts you display a tendency not to subsume Wittgenstein's approach to philosophical questions about mathematics under any existing ism. In your forthcoming paper about Hilary Putnam's philosophy of mathematics (*forthcoming (a)*) you especially disapprove of the realism/anti-realism controversy which lies at the centre of Putnam's thinking. Many commentators, if not the majority of them, continue to subsume Wittgenstein's philosophy under one ism or another. What precisely are your reasons for rejecting such attempts?

MÜHLHÖLZER: My attitude towards the secondary literature is very similar to that expressed by Robert Fogelin in the Preface of his book *Taking Wittgenstein at His Word*. I totally agree with him that discussions of the diverse isms mentioned in the literature will inevitably lead away from Wittgenstein's thinking itself, from his particular "manner of doing philosophy", as Fogelin writes, and my aim is to be in accord with this manner as far as possible. This implies the rejection of all philosophical isms because they stand in the way of Wittgenstein's non-philosophical realism which I mentioned before. In this rejection, I am in accord with Wittgenstein himself, of course, who always refused to pigeonhole his thinking. Such an attitude is also shared, for example, by Einstein who, in a nice letter of September 1918 to the "realist"

Eduard Study, refused to be called a "realist" (in the philosophical sense) himself and who remarked that any ism of this sort is only strong as long as it is fed by the weakness of its contra-ism, and that if such isms are cleared of their dirt they become identical. (I also refer to this on pp. 72f. of my commentary on BGM III.)

It is only in my forthcoming paper about Putnam that I explicitly discuss the secondary literature on Wittgenstein's philosophy of mathematics. (This paper is "forthcoming", as you rightly say, but I actually sent it to the publisher of the *Library of Living Philosophers* in December 2002 – two thousand two – and have never changed it since then. Nevertheless, I think that I can still accept its main assertions.) In this paper, I argue in particular against Putnam's obsession with the notion of truth and the realism/anti-realism issue, which he shares with Michael Dummett, and I try to show that this is an inadequate perspective for understanding Wittgenstein's philosophy. Of course, there are passages by Wittgenstein that may look like discussions of truth, but this appearance is deceptive. Let me quote one of these passages, which is rather prominent in this respect. At the end of § 41 of RFM VII, Wittgenstein writes:

> Suppose that people go on and on calculating the expansion of π. So God, who knows everything, knows whether they will have reached '777' by the end of the world. But can his *omniscience* decide whether they *would* have reached it after the end of the world? It cannot. I want to say: 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.
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> We might put it like this: if the rule for the expansion has been given us, a *calculation* can tell us that there is a '2' at the fifth place. Could God have known this, without the calculation, purely from the rule of expansion? I want to say: No.

This may suggest that for Wittgenstein *actually being proved until the end of the world* is a necessary condition for mathematical truth, and Wittgenstein has in fact been interpreted in such a way by Dummett, to whom the passage is expressing a particularly radical and unacceptable form of "anti-realism".

To my mind, this is a crass misinterpretation. The most important point with respect to Wittgenstein's passage is to see that the subject of it is not "Mathematical Truth" (with the message that mathematical truth must coincide with provability, or even with being proved until the end of the world), but rather: "Mathematical Theorems as Rules". Wittgenstein here fathoms the central insight of his philosophy of mathematics, the insight that mathematical theorems function like rules, and he wants to know what precisely this insight involves. Therefore, he asks, in the first paragraph of § 41 (which I haven't quoted), about the *purposes* which these rules might serve. He then states that a philosophy of mathematics that clings to the *form* of mathematical propositions instead of their *use* puts us on the wrong track. And the same is true, he then suggests, of a philosophy of mathematics that (explicitly or implicitly) imagines a God who, e.g., knows all the irrational numbers, can survey the entire decimal expansion of π, and so on. All these ways of thinking about mathematics miss the important insight expressed in the section which immediately follows § 41, namely: "[T]he expression of the rule and its sense is only part of the language game: following the rule."

In view of this insight, the end of § 41, which I quoted, should be interpreted as follows: when Wittgenstein talks about "mathematics" there, what he is referring to is *our* mathematical practice with *our* language games of following rules. This practice has an end with the end of the world (at the latest), and therefore it makes no sense even for God to go beyond it. God's omniscience may be able to achieve a lot, but when God deals with *our* mathematics he must take into account the limitations which are characteristic of it, and when he goes beyond them, he deals with something else and no longer with our mathematics. Or, to put it differently: when God uses the word "mathematics", he shouldn't mean by it something different from what *we* mean, because he is using *our* word. And so he should take into account the limitations inherent in our practice.

But suppose that, until the end of the world, we were not able to decide whether a certain pattern occurs in the decimal expansion of π. Shouldn't we say, then, that it is *not determined* whether this pattern does or does not occur in it? According to Dummett's interpretation, Wittgenstein would answer: in fact, in that case it is not determined. But Wittgenstein's actual answer would be: "The question contains a mistake", as he writes in PI § 189. The mistake lies in the assumption that it is clear what "determined" means here. That is certainly wrong, and Wittgenstein therefore goes on in PI § 189 as follows:

> We use the expression 'The steps are determined by the formula ... [e.g., a formula for the expansion of π]'. *How* is the expression used?

And when we face up to this question, we certainly find uses according to which the occurrence of the pattern has to be regarded as *not* determined (e.g., when "determined" is understood in the sense of "actually decided until the end of the world") and uses where the occurrence *has* to be regarded as determined. Within classical mathematics, for instance, it is regarded as determined, in the straightforward sense in which one accepts the law of excluded middle; i.e., it is the use of this law which gives sense to the term "determined" (where one has in the background all the techniques that classical mathematics provides).

When interpreted in such a relaxed way, § 41 does not express any "anti-realism" about mathematics, because it is far away from the obsession with the subject of "Truth" that is so characteristic of the realism/anti-realism issue. But then, neither does it make much sense to call Wittgenstein a "realist", in the philosophical sense of this term. The philosophy of the later Wittgenstein is simply at cross-purposes with the realism/anti-realism debate. Putnam, of course, does not adopt Dummett's anti-realist reading of Wittgenstein. On the contrary, he proposes a "realist" one, but this isn't really an improvement. He still reads RFM VII § 41 as concerned with the problem of truth and sees it as relevant to the realism/antirealism issue. This is the central mistake, a mistake quite characteristic of the secondary literature. Of course, there are exceptions, like Fogelin, whom I already mentioned, or like Juliet Floyd, with whose papers I generally agree, but in the majority of cases the distance to Wittgenstein is too great. Therefore, I prefer to avoid explicit discussions of the secondary literature.

GRÈVE: You just mentioned "mathematical theorems *as* rules", and you also called it "the central insight of Wittgenstein's philosophy of mathematics ... that mathematical theorems function *like* rules" and then added that Wittgenstein wanted "to know what precisely this insight involves". Please forgive me if I now try to press you a little on this point. You also once paraphrased this "insight" in the following way:

> the typical use of a mathematical proposition is much more similar to the use of propositions in order to determine concepts or to state rules ("The bishop in chess moves only diagonally") than to the use of propositions in order to report facts. (2001: 216)

And surely, this is a thought which many would be inclined to describe as being at the core of Wittgenstein's thinking about mathematics, and no doubt justifiably so. The status of this idea, however, is highly debated among commentators. For example: is it a "grammatical truth" – as one might say in the spirit of Peter Hacker's interpretation – that can easily seem to be problematic but that, when understood correctly, makes perfectly good sense and is indeed indisputable?; or is it merely (or, better perhaps: primarily) an analogy – as Gordon Baker once suggested – the usefulness of which is much less objective than the term "grammatical truth", for instance, would seem to suggest, but which is to a considerable extent person-relative?

MÜHLHÖLZER: This is an intricate question, and it will actually be at the centre of my commentary on BGM I. Let me try to sketch an answer based on the following statement by Wittgenstein in his *Remarks on the Philosophy of Psychology*: that "mathematical propositions are essentially akin to rules" *["ihrem Wesen nach mit Regeln verwandt"*] (RPP I § 266). To be *akin* to rules is more than to be "merely analogous" and more than to be merely subjective. Their kinship to rules consists in the fact that usually their sole function is to determine the concepts they invoke. Wittgenstein doesn't present this as a general thesis but as an observation with respect to characteristic language games involving mathematics, and one has to give surveyable representations of our use of the words and symbols in mathematics in order to confirm this.

In the paper from which you quote I try to show this in the case of a single example discussed by Wittgenstein himself in his *Lectures on the Foundations of Mathematics* of 1939: the concept of a regular heptagon as used in Euclidean geometry (i.e., in the Euclidean construction game with ruler and compass) and in Cartesian analytic geometry. The relevant proposition is: "There is no construction [in the sense of Euclidean geometry] of the regular heptagon". I try to make plausible that when belonging to analytic geometry this proposition is nothing but a partial determination of the concept of a regular heptagon as used in this sort of geometry and, furthermore, I argue that this proposition has no place in Euclidean geometry at all because there the concept of a regular heptagon has no mathematical use and therefore no mathematical meaning. Many people who read my paper think that this interpretation of Wittgenstein's considerations is too extreme, but I'd like to defend it. If one really takes Euclidean geometry as the construction game laid down in the *Elements*, then one discovers that there is no substantial use in the case of the term "regular heptagon" within this game (it isn't even mentioned in the *Elements*, nor is there any reference to regular n-gons in general!). Consequently, if one takes seriously Wittgenstein's view that "meaning" should be understood with reference to our use of the symbols at hand, "regular heptagon" mustn't be considered as being meaningful in Euclidean geometry. I regard this as an interesting insight following from Wittgenstein's focus on the use of our terms, and I find it rather liberating due to its avoidance of dubious conceptions of "meaning".

In an earlier paper (2002b) but also in section 6 of the Introduction of my commentary on BGM III, I sketch a general strategy for arguing in favour of Wittgenstein's view: consider typical properties or functions of paradigmatic rule formulations in the form of explicit explanations of concepts, and then show that such properties or functions can also be found in the case of mathematical propositions that *prima facie* do not appear to be rules. For example, one can state the following roles of explicit concept explanations: they can be causally responsible for our subsequent use of the concepts; they may be explicitly used as aids to memory or to orientation; they can be criteria for a person's conceptual understanding, as in the case of students who have to show, for example, that they know what "continuity" means. But precisely the same roles can also be played by mathematical theorems, and this may give good reasons to regard them as "rules" as well. – However, as already noted, this issue has to be discussed with great care, which I haven't yet done to a sufficient extent but plan to in my commentary on BGM I.

I also want to mention a certain complication that one must not ignore: Wittgenstein sometimes makes certain claims for therapeutic reasons only, and this in particular with regard to the issue of mathematical propositions as rules. So he has a strong tendency to consider mathematical propositions as being based on *decisions*, which then shows their kinship with rules. But in many places he also warns against this sort of talk because the word "decision" has inappropriate connotations, like the idea, for example, that we may actually choose one of several alternatives or that we might even struggle through such a choice. Dummett is famous for having taken this "decision" talk at face value, but the real Wittgenstein meant it only as a therapeutic manoeuvre in order to put us on the right track, as a ladder that should be thrown away in the end. In MS 123, p. 20r, he puts it thus (in my translation):

> Instead of saying "Mathematical propositions express decisions rather than insights", let us say: "Let us regard mathematical propositions as decisions instead of insights"!

One could in fact refer to countless places where Wittgenstein's approach is decidedly therapeutic. It is an important task for any interpreter to correctly identify these places.

GRÈVE: As for another controversial conception in recent Wittgenstein scholarship... In your written work, you have repeatedly stressed the significance of the notion of "surveyability" (German: *Übersichtlichkeit* or *Übersehbarkeit* or *Überschaubarkeit*) in Wittgenstein's later philosophising about mathematics (cf. esp. 2006 and chapter 1 of your commentary on BGM III). On the other hand, you also place stress on characterising Wittgenstein's later methods in terms of "perspicuity"; for example, when you write,

> [i]nvestigations of the sort envisaged by [Wittgenstein] remain on a purely descriptive level; they only aim at a 'perspicuous representation' (PU § 122) of our use of words and are abstinent with respect to any serious theorizing. (2001: 217–218)

The German original of the much-debated translation of PU § 122 has the adjective "*übersichtlich*" ("*übersichtliche Darstellung*"). So how does *Übersichtlichkeit* (or: surveyability) in mathematics, especially in mathematical proofs, relate to *Übersichtlichkeit* (or: perspicuity) as expressed in PU § 122? Is there not perhaps a risk of overshadowing potential insights into Wittgenstein's understanding of his own way of doing philosophy when, as seems fashionable nowadays, we continually emphasise the anti-scientistic character of his philosophy; insofar as that, on Wittgenstein's own view of mathematical proofs (as *übersichtliche Darstellungen*) his way of doing philosophy (as a practice of offering *übersichtliche Darstellungen*) did indeed bear some similarity to the mother of the exact sciences, mathematics (as understood by him)? In turn, would a clearer view of this relation not potentially help us see more clearly the significance that Wittgenstein's thinking about mathematics had for the development of his overall philosophical approach?

MÜHLHÖLZER: When using the expression "perspicuous representation" in my paper of 2001, I simply followed the Anscombe translation of PU § 122. Anscombe, however, also uses the word "perspicuous" in the context of mathematical proofs. Her translation of the beginning of BGM III, for example, is: "'A mathematical proof must be perspicuous'". But the word "perspicuous" suggests understanding (a "perspicuous proof" is one that "can be easily understood") and according to my interpretation this is not what Wittgenstein meant when using the word "*übersichtlich*" in BGM III. I agree with Anscombe that in both contexts the *same* word should be used in the translation, because Wittgenstein also used the same word: "*übersichtlich*"; but it must be a different English word. In my paper of 2006, I used "surveyable", and this seems to me the best choice. Therefore, I was pleased when I saw that, in the new translation of the PU by Peter Hacker and Joachim Schulte, Wittgenstein's "*übersichtliche Darstellung*" is now translated as "surveyable representation". This is fine, because it is sufficiently neutral.

Although he was using the same word, "*übersichtlich*", Wittgenstein meant rather different things when applying it to "*Darstellungen*" in philosophy and to proofs in mathematics. If I understand it correctly, the core of his talk about the "surveyability of proofs" is his observation that our most natural representations of natural numbers consist of strings of strokes, like |||||||||||, or of sums of 1s, like 1+1+1+1+1+1+1+1+1+1+1+1+1, which rapidly become unsurveyable in the simple sense that their identity as specific signs is blurred. As early as in manuscripts written from 1929 to 1931, one can find remarks like these (in my translation):

> A fundamental question: how can I know that |||||||| and |||||||| are the *same sign*? After all, it is not enough that they look similar? For it is not the rough sameness of the gestalt that should constitute the identity of the sign, but rather the sameness of numbers. (MS 106, pp. 22f.)

> [T]here is a certain difficulty with the numerals (1), ((1)+1), etc.: namely, that beyond a certain length we can no longer distinguish them without counting the lines, that is, without translating the signs into *different* ones. "||||||||||" and "|||||||||||" cannot be distinguished in the same sense – that is, they are not in the same sense different signs – as "10" and "11". (MS 111, pp. 156f.)

> The problem of the distinction between 1+1+1+1+1+1+1 and 1+1+1+1+1+1+1+1 is much more important/fundamental than appears at first/at first sight. (MS 112, p. 15)

Later, Wittgenstein elaborated these observations and used them in BGM III to argue against logicism and similar programmes of mathematical reductionism. He now talks about the surveyability of *proofs* which he considers to be an elementary and at the same time fundamental feature of all proofs in mathematics. As he immediately explains in § 1 of BGM III, by "surveyability" ("*Übersichtlichkeit*") he means that proofs can be reproduced with certainty and in the manner in which we reproduce pictures. With the help of this notion, he then presents two main arguments against foundational endeavours in mathematics. First, he shows that there are problems with the criteria of identity for the unsurveyable proof patterns that are at their bases, problems totally analogous to those observed with respect to 1+1+1+1+1+1+1+1+1; and second, he points out that by making these patterns surveyable we rely on concepts and procedures which go beyond the foundational frameworks. Space prevents me from explaining this in more detail, but it is extensively discussed in my paper of 2006 and in my commentary.

What does this have to do with the "surveyable representations" of PI § 122, described as being "of fundamental significance" and as "the way we represent things, how we look at matters"? Not much, I think. A Wittgensteinian surveyable representation is aimed at dissolving a philosophical problem that arises out of a characteristic "failure to understand", because "we don't have *an overview* of the use of our words", and it "produces precisely that kind of understanding which consists in 'seeing connections'". To this end, Wittgenstein very often designs simple language games that serve "as *objects of comparison* which, through similarities and dissimilarities, are meant to throw light on features of our language" (PI § 130) and "in which one can clearly survey the purpose and functioning of the words" (PI § 5). Now, this is obviously far away from the problems regarding the identity of signs like |||||||||| or 1+1+1+1+1+1+1+1+1+1 which are the core of Wittgenstein's discussion of the surveyability of proofs in RFM III. As I say in my papers (2010b) and (2014), this identity certainly depends on the use we make of the signs, but in their case the relevant use is of an extremely restricted and narrow sort, far away from what Wittgenstein has in mind when, with his notion of "surveyable representations", he refers to our new philosophical understanding through an overview of the use of our words in order to clearly survey the purpose and functioning of the words.

We can make surveyable representations of our mathematical practice, of course, including our practice of devising proofs, but these are endeavours *outside* mathematics in order to dissolve philosophical problems arising from it and from the way we often talk about it. In the nice § 273 of *Zettel*, Wittgenstein considers a statement by the mathematician Hardy about infinity, and he then describes his own way to dissolve the problem contained in this statement as follows:

> The aim is a synoptic comparative account of all the applications, illustrations, conceptions of the calculus. The complete survey of everything that may produce unclarity. And this survey must extend over a wide domain, for the roots of our ideas reach a long way.

Such a complete survey can include observations concerning the mathematicians' endeavour to construct surveyable proofs in the sense of § 1 of RFM III, but *this* endeavour is located *within* mathematics, while the survey itself aims at philosophical clarification *outside* mathematics.

As the quotations above from MSS 106, 111 and 112 show, for Wittgenstein the problems with notations like |||||||||| and 1+1+1+1+1+1+1+1+1 are *fundamental*, and his reason is quite simple: notations of this sort reflect our manner of dealing with the natural numbers and this is at the centre of our thinking and speaking. It is also essential for the mathematicians' concept of a proof, and again this is something central: now central with respect to mathematics itself. When talking about the surveyability of proofs in RFM III, Wittgenstein wants to highlight an essential feature of mathematics, and this shouldn't be lumped together with the "surveyable representations" aimed at in his philosophy in general. I would like to add that one can in fact distinguish many different notions of "surveyability". In my commentary (p. 95), albeit only in passing, I mention four of them, and I would welcome a systematic investigation of such notions.

So Wittgenstein really wants to understand this outstanding human practice called "mathematics". He is certainly no enemy of mathematics, as he is no enemy of real science in general. On the contrary, his aim is to get at the essentials of science, as opposed to any wool-gathering or narcissistic inflation. At the same time, he insists on the self-contained character of philosophy as an essentially non-scientific endeavour, and in this sense he is, as you say, anti-scientistic. To my regret, Wittgenstein rarely wrote about a respectable science like physics. I sometimes imagine something like Wittgensteinian "Remarks on Physics", and I wish that somebody would write a treatise like that.

GRÈVE: But haven't you already written that *yourself*?

MÜHLHÖLZER: Oh, well. Immediately after my commentary on BGM III, I wrote a short book entitled *Wissenschaft* in which I make the attempt to present a sort of philosophy of science that is not too far away from Wittgenstein's thinking, but I fear that I didn't succeed. Well, it would be ridiculous, anyway, to try to imitate him, because his ingeniousness is totally incalculable. But my hope is that in the future his thinking might nevertheless become more influential than it is today.

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