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The Certainty of Mathematics

A Philosophical Investigation

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For Agnes and Gunnar

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* * *

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Vasa, 28 October 2016

Kim-Erik Berts

Contents

1. Introduction

A traditional question in philosophy concerns what, if anything, we can know with certainty. Mathematics is the only field of knowledge concerning which there has been some rough consensus that it actually gives us certain knowledge. Even though some philosophers have preferred not to limit the range of certainty to mathematics alone, this discipline is often put forward as an ideal: 'this paragon of reliability and truth', as David Hilbert proclaimed.[1]

Certainty is an enticing concept: we want to be certain, we want to have certain knowledge. We want to be sure that we have not made any mistakes and to have a safeguard against error. This is a desire that is motivated by practical reasons: life is easier if one can avoid (at least sometimes) the consequences of error or of being wrong. It is motivated by social reasons too: being wrong can be embarrassing, and being in the know is deeply satisfying.

In many situations, the search for certainty makes us turn to mathematics. Mathematical techniques are often what brings certainty. They allow us to overview situations in such a way that we see clearly what must be the case. When we have settled something using mathematical tools, we can rely on that knowledge.

From a philosophical perspective, the question arises: Why is it that mathematics has this status? This question is raised already by the role that mathematics plays in our ordinary lives, and the urgency of it is only strengthened by the role that mathematics plays in the sciences. Stewart Shapiro gives voice to the need for such an explanation: 'It is thus incumbent on any complete philosophy of mathematics to account for the at-least apparent necessity and a priority of mathematics. … In the present climate, no one can rightfully claim that these notions are sufficiently clear and distinct.'[2]

However, what it means to say that mathematics is certain or that mathematics gives us certain knowledge is not sufficiently clear. Therefore, the why-question must yield to an investigation of what it means to say that mathematics is certain. One cannot determine why something is certain if it is not clear what calling it certain amounts to. Only when a greater clarity with regard to the concept of certainty in mathematics is achieved will it be possible to answer the first question – if it is indeed found to be a meaningful question.

The aim of this thesis is to explore the concept of certainty in mathematics in order to arrive at a clear view of what is meant thereby.

[1] David Hilbert. 'On the Infinite'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967, p. 375.

[2] Stewart Shapiro. *Thinking about mathematics: The philosophy of mathematics*. Oxford: Oxford University Press, 2000, p. 23.

The concept of certainty is not given much attention in contemporary philosophy of mathematics. By contrast, it was a popular topic in the beginning of the last century. During the nineteenth century, the confidence in mathematics had been shaken by the discovery of non-Euclidean geometry and by the unclarity with regard to what the proper methods of analysis were, but also by the discovery of paradoxes in set theory and logic at the turn of the twentieth century.

The foundational programmes – logicism, formalism, and intuitionism – that came to shape the philosophy of mathematics of the twentieth century grew out of a wish to establish the certainty of mathematics. This is an explicit goal in Bertrand Russell's and Hilbert's writings. However, towards the second half of the twentieth century, the concept of certainty attracted less attention. One reason for this shift of focus is surely to be found in technical results such as Kurt Gödel's incompleteness proofs, which showed the impossibility of carrying out the logicist and formalist programmes as intended. This in turn was taken to imply the impossibility of establishing certainty. As W. V. O. Quine's philosophical arguments for the revisability of mathematics gained wide acceptance, the philosophical discussion about mathematics came to focus on other topics. Questions about the ontological nature of mathematical objects and about the objectivity of mathematical truths became topics of frequent discussion. These are problems that permeate the contemporary discussion. It seems that objectivity is seen as a second best option if certainty is no longer studied, but even so – or precisely therefore – the concept of certainty looms in the background.

The aim of this thesis is thus to reawaken the interest in this concept and to show what it means for mathematics to be certain. The idea, however, is not to provide a new foundation for the certainty of mathematics. I shall not argue that mathematics is certain, but neither that it is not certain. An idea advanced here is that certainty does not stand and fall with the success of a foundational programme. Rather, certainty can be seen in the practice of mathematics and in the status that mathematics has for us. A preliminary discussion of the concept of certainty is undertaken in chapter 2.

The contemporary discussion with its focus on objectivity and on the ontological status of mathematical objects is, to a large extent, a debate between competing positions that fall under one of the overarching positions realism or anti-realism. It has become commonplace to distinguish between realism with regard to the existence of mathematical objects (i.e. ontological realism) and realism with regard to the truth of mathematical propositions. Ontological realism is the idea that mathematical objects exist independently of the mathematician. Such a theory is attractive since it seems to explain how it is possible for mathematics to give objectively valid knowledge. The independence of the objects guarantees that knowledge about them is objectively true. The obvious example of such a theory is Platonism.

Realism with regard to truth does not imply any position with regard to the ontological issue. This form of realism only claims that mathematical propositions have a definite truth value that is independent of the mathematician. Objectivity is taken to be guaranteed by the independence of truth. This kind of realism can be attractive if one feels that ontological realism amounts to bad metaphysics but that the truth of mathematical propositions cannot be influenced in any way by the mathematician.

Anti-realism amounts to a denial of the existence of mathematical objects. If the independence of mathematical truth is also denied, this implies an abandonment of the idea that mathematics provides us with objectively true knowledge. A position that could be placed under this heading is quasi-empiricism, which likens mathematical propositions to empirical ones in terms of their revisability. In many quasi-empirical texts, one finds the explicit denial of the certainty of mathematics.

Regardless of which position one favours in this debate, they all seem to be affected by the problem raised by Paul Benacerraf in his article 'Mathematical Truth'.[3] He argues that realist positions (in particular ontological realism) have trouble explaining how it is possible to gain knowledge of mathematical objects. Anti-realism (and possibly non-ontological realism) does not have this problem, but, according to Benacerraf, it is questionable whether one can call their version of mathematical truth, truth. The problem that Benacerraf identified is still discussed frequently. It is my impression, however, that the debate between realism and anti-realism has become troublesome.

My idea, and a central topic of chapter 3, is that Benacerraf's problem is, in part, due to a limited conception of mathematics. This is a view of mathematics as essentially consisting of a *body of true propositions*. It is usually not mentioned explicitly as a view that philosophers argue for, but a tacit assumption that guides one's thinking about mathematics. I will refer to this as the 'body of truths conception' of mathematics. Mathematical knowledge is, then, reduced to knowing which propositions are true and which false. An important aim which resonates with the discussion in chapter 2 is to show that this conception of mathematics does not take into account that mathematics is an activity and that knowledge of mathematics is, to a large extent, a skill. When this is taken into account, it is seen that Benacerraf's problem becomes acute because of this limited conception. The contemporary debate between realists and anti-realists in mathematics is troublesome because it concerns itself with solving a problem that involves many questionable assumptions. Another aspect of the body of truths conception that is vital to the overall aim of this thesis is that the certainty of mathematics takes the form of being certain *that a proposition is true*. This may,

[3]Paul Benacerraf. 'Mathematical Truth'. In: *The Journal of Philosophy* 70 (1973), pp. 661–80.

in turn, invite scepticism as to whether it is possible to attain such a position to a mathematical proposition. Focusing on mathematics as an activity shows that being certain in mathematics can also be described as being certain that one has performed an operation correctly, that one has followed the rules correctly.

A central feature of the contemporary philosophical outlook on mathematics is to view mathematics as a collection of formal systems that can be studied as objects on a metalevel. Ordinary mathematics is taken to be represented by the object systems and a theorem of mathematics by a theorem that is formally deducible in some system. This view goes well together with the idea of mathematics as a body of truths, and, if it is taken as a guide for the philosophical understanding of mathematics, it can conceal the importance of the ability to use the mathematical symbols. Working formally is often described as working with meaningless signs in a mechanical manner that requires no understanding of the signs. As I will show in chapter 4, already identifying a string of signs as a formula requires some kind of ability to use formula. This applies a fortiori to making inferences in a formal system.

This implies that there is no sharp divide to be made between formal and informal mathematics from a philosophical perspective. From a mathematical perspective, there is a clear difference; a theory is formal if the validity of the deductions performed in it does not depend on the interpretation of the terms used. This difference does not necessarily carry any philosophical implications, however. One conclusion to be drawn from this is that one cannot capture the essence of mathematics by turning a theory into a formal system. What makes mathematics into mathematics cannot be specified in any more detail than to say that it consists in using the techniques of calculation, of proof, etc. in accordance with an established practice.

Another conclusion that is of importance for the present project is that formality cannot be seen as a safeguard against error. It has often been thought that a greater formality allows for a greater certainty. As working formally is viewed as working with meaningless signs, the risk of errors associated with the intuitive or with meaning is supposedly eliminated. Once it is seen that not even purely formal expressions and deductions are free from the understanding that is associated with an ability to use them, it becomes clear that the certainty that is associated with formality is not alien to non-formal mathematics either.

Arguably, the most central concept in the philosophy of mathematics is the concept of proof. The contemporary discussion of proof is very disparate and this reflects the development of mathematics during the twentieth century. In the beginning of the twentieth century, Hilbert's formalism provided the definition of formal proof and that concept was often taken to capture the essence of mathematical proof. Gödel's incompleteness proofs – and especially the interpretation of these to the effect that any formal system complex enough to encompass arithmetic contains true propositions that are nevertheless not provable – suggest that either the extensions of the concepts 'provable' and 'truth' do not coincide, or otherwise 'formal proof' does not capture the ordinary notion of proof. Other notable features of twentieth century mathematics are the increasing complexity of traditional proofs and the use of computers in proofs.

All of these strands in twentieth century mathematics have led some philosophers as well as mathematicians to question that proofs offer conclusive evidence for theorems. At the same time, proof, being that which establishes theorems, plays a key role in the understanding of mathematical certainty. Traditionally, proofs have been regarded as that which brings certainty. Chapter 5 is devoted to the concept of proof. There it is argued that a particular view of proofs follows from the body of truths conception. Proofs are seen as devices that show that a proposition – which is taken to possess a clear meaning also before the proof – is true. It is, furthermore, common to stress the ability of proofs to confer conviction on the person who reads the proof. Proofs then appear to have a uniform role in mathematics and a philosophical problem readily announces itself: 'How can proofs convince us of the truth of a proposition?' It seems that the philosopher must uncover hidden logical functions that all proofs have in common that enable proofs to prove.

Against this view I contrast three aspects of proofs, or rather of our practice of working with proofs, that Ludwig Wittgenstein draws attention to. These aspects – (1) that grasping a proof involves shaping one's understanding of the concepts involved, (2) that proving differs from performing experiments, and (3) that a proof is surveyable – contribute to another perspective on proofs. They also contribute to forming another perspective on mathematical knowledge and certainty than viewing mathematics as a body of truths. Proofs do not function by exerting a convincing force on us, but proofs can be seen as devices that we use to determine our understanding of concepts. Accepting a proof can thus be described as being convinced that a concept must be used in a particular way.

This, in turn, is a way of making sense of Wittgenstein's comparison of mathematical propositions to rules. Instead of having a descriptive role, describing states of affairs that hold among mathematical objects, they can be seen as having a normative role. If viewed as descriptive statements about objects, it is evident that philosophical problems about the possibility of making such descriptions will arise. These problems do not arise with regard to norms. Other problems may arise, such as 'How is it possible that a proof can shape our understanding of mathematical concepts?' This problem must be countered with the observation that a proof, in a surveyable manner, shows us how a concept should be used, and accepting the proof means that one accepts that this is how one must use the concept. It will probably not be possible to give a general explanation of how proofs accomplish this; it must be studied case by case. Furthermore, it requires a prior understanding of the techniques that are employed in the proof, as will be argued in chapter 5.

All in all, what emerges is a picture of mathematics where there is room for certainty. It is not a justification, however, of the certainty of mathematics, nor is it an explanation of the certainty in terms of something external to the practice of mathematics. For example, I will not look for something underlying this certainty. Rather, I draw attention to central features of mathematics that should be obvious: that mathematical propositions often have a normative function; that a certain proficiency is required for an understanding of mathematics, not only on an everyday level but also in formal mathematics; and that proofs are grasped by surveying them. These observations contribute to a picture of how mathematical certainty manifests itself. It is not a justification of this certainty, but a description of our life with mathematics that allows us to understand in what sense mathematics is certain.

2. Certainty

> I wished to believe that some knowledge is certain and I thought that the best hope of finding certain knowledge was in mathematics.
>
> (Bertrand Russell, *Portraits from Memory*)[1]

> The kind of certainty is the kind of language-game.
>
> (Ludwig Wittgenstein, *Philosophical Investigations*)[2]

In philosophy, one speaks about 'certain knowledge' and about mathematical knowledge being 'certain', or simply about 'the certainty of mathematics'. What does one mean by such expressions? Is it clear how one should understand 'certainty' here? Still, it seems to be justified to claim that mathematics is certain, as is illustrated by Norbert Wiener in 1915: 'The place most people would look for absolute certainty is in pure mathematics or logic.'[3] A full century has passed since the publication of his article, but already Wiener remarks that he has 'become somewhat suspicious of the absolute certainty of mathematics through hearing it continually dwelt upon'. Instead of reflecting on the meaning of the phrase, however, he frames his critical point in a sceptical question: 'Is, then, mathematics absolutely certain?'[4] In this regard, John Stuart Mill, although opting for the sceptical alternative in the end, asked a crucial set of questions:

> [W]herein lies the peculiar certainty always ascribed to the sciences which are entirely, or almost entirely, deductive? Why are they called the Exact Sciences? Why are mathematical certainty, and the evidence of demonstration, common phrases to express the very highest degree of assurance attainable by reason? Why are mathematics by almost all philosophers … characterized as systems of Necessary Truth?[5]

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[1] Bertrand Russell. *Portraits from Memory: and Other Essays*. London: George Allen & Unwin, 1956, p. 4.

[2] Ludwig Wittgenstein. *Philosophical Investigations*. Ed. by G. E. M. Anscombe and Rush Rhees. 3rd ed. Oxford: Blackwell, 2001 (henceforth cited as PI), xi, p. 191.

[3] Norbert Wiener. 'Is Mathematical Certainty Absolute?' In: *The Journal of Philosophy, Psychology and Scientific Methods* 12 (1915), pp. 568–74, p. 568.

[4] Ibid. A contemporary of Wiener, E. T. Bell, even talks of a 'superstitious reverence for mathematical rigor and the "absolute *certainty*" of pure mathematics'. E. T. Bell. 'Mathematics and Credulity'. In: *The Journal of Philosophy* 22 (1925), pp. 449–58, p. 456.

[5] John Stuart Mill. *Collected Works of John Stuart Mill*. Vol. 7–8: *A System of Logic: Ratiocinative and Inductive. Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation*. Ed. by John M. Robson. Toronto; London: University of Toronto Press; Routledge & Kegan Paul, 1974, vol. 7, p. 224.

The present chapter aims to open up a discussion of the concept of certainty in mathematics. Certainty has been a major issue in modern philosophy and mathematics has often been considered a special case in this regard. Descartes, for example, famously took mathematics as a model for his method of guiding reason. The issue of the certainty of mathematics received a good deal of attention in the early decades of the twentieth century. During the years of the foundational crisis, the certainty of mathematics was taken to be threatened by the paradoxes in set theory and logic. It needed to be secured from this threat. As the foundational programmes logicism and formalism were quieted by the incompleteness results of Kurt Gödel, certainty appeared as too high a goal to work for, and the concept seems to have all but disappeared from the discussion. This is understandable if certainty is thought of as being achievable only if some kind of foundational programme is completed successfully. As the interest in certainty dwindled, its place was taken by *objectivity*. If certainty is not achievable, at least one can hope to show that mathematical knowledge is objectively valid. This concept is, in contrast to certainty, discussed frequently in contemporary philosophy of mathematics.[6]

However, it is by no means evident that the paradoxes actually put the certainty of mathematics into question. Many working mathematicians did not lose their confidence in the discipline although they were aware of the paradoxes. Another attitude is found among some philosophers who claim that the certainty of mathematics is only illusory, paradoxes or not. A third attitude could be one that does not equate certainty with what the foundational programmes sought to achieve. It is then seen that this concept is indeed puzzling and in the need of philosophical attention. Wittgenstein can be mentioned as an example of a philosopher who took this kind of interest in certainty, and the present investigation is carried out in this spirit.

In his book *Foundations without Foundationalism*, Shapiro argues that it is possible to work on the foundations of mathematics while abandoning foundationalism. By foundationalism he means the idea that mathematics needs to be put on an absolutely secure foundation (i.e. the idea behind the foundational programmes) while the former merely involves 'a reconstruction of its principles, either its truths or its knowable propositions'.[7] One could say that Shapiro gives up the concept of certainty while keeping foundations.[8] I shall, by contrast,

[6]See e.g. Penelope Maddy. *Defending the Axioms: On the Philosophical Foundations of Set Theory*. Oxford: Oxford University Press, 2011, sections III.4 and V.1.

[7]Stewart Shapiro. *Foundations without Foundationalism: A Case for Second-order Logic*. Oxford: Oxford University Press, 2000, p. 26.

[8]He writes: 'we have learned to live with uncertainty in virtually every special subject, and we can live with uncertainty in logic and foundations of mathematics'. Ibid., p. ix. His willingness to live with uncertainty is tied up with his insistence on using second order logic in his work on foundations. He notes that the preference for first order logic is a remnant of foundationalism, first

try to revive a discussion of certainty while detaching it from the attempts to provide a foundation.

When dealing with questions like Mill's, it is easy to search for some *quality* of mathematics, of mathematical knowledge or propositions, that will provide an *explanation of why* mathematics is certain. Such a quality is often located in the nature of mathematical propositions or in the nature of some kind of mathematical objects. One can think of the logicist thesis that true mathematical propositions rest solely on logic, or of the standpoint of Hilbert's in 'The New Grounding of Mathematics: First Report' that 'the objects of number theory are … the signs themselves, whose shape can be generally and certainly recognized by us'.[9]

Before searching for explanations for the certainty of mathematics, I believe it is important to consider first what is meant by calling mathematics certain. Therefore, I will begin this chapter by reviewing some suggestions as to how one can understand certainty. Interestingly, several of these suggestions can be extracted from various passages in Hilbert's writings. The fact that Hilbert was deeply committed to the attempts to establish the certainty of mathematics made him express his understanding of this concept on several occasions. It is telling of the unclarity of the concept that he gives voice to so many different ways of viewing it. Each of the suggestions discussed captures something of the concept of certainty, but it will be clear that none of them can constitute a final understanding of the certainty of mathematics. This overview, undertaken in sections 2.2–2.5, will be of a preliminary kind but it will serve to highlight problematic points in the concept of certainty.

Another perspective that may give valuable insights is to ask why certainty has such a strong appeal to us. One point of focus in dealing with such a question could be the need for certainty in practical situations. Somebody inquiring whether the medication of his ageing parent really is accurate might be given the answer: 'Yes, it's certain that he ought to take three pills a day, but not more than that!' In this case, the need for certainty is clear and unproblematic. Another focus could be the epistemological wish to find a discipline that gives us absolutely certain knowledge in order to establish the philosophical thesis that there is indeed certain knowledge to be found. In this second case, the issue is the po-

order logic being associated with a greater certainty due to its completeness. As Shapiro advocates abandoning foundationalism, the special preference for first order goes too. Shapiro's attitude is an echo of Russell who – disillusioned by the failure to overcome the paradoxes in a fully satisfactory way – wrote that 'an element of uncertainty must always remain, just as it remains in astronomy. It may with time be immensely diminished; but infallibility is not granted to mortals.' Russell, quoted in: Ivor Grattan-Guinness, ed. *From Calculus to Set Theory 1630–1910: An Introductory History*. Princeton: Princeton University Press, 1980, p. 234.

[9] David Hilbert. 'The New Grounding of Mathematics: First Report'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998, p. 202.

tential of a discipline to produce absolutely certain knowledge. It thus concerns an entire category of propositions.

One may wonder why it is so important to find certainty in this absolute sense. We are in many situations certain after all. Perhaps it is felt that unless we can establish the truth of the philosophical thesis that there is certain knowledge, one's ordinary certitude is not justified. This worry, which is akin to the sceptic's doubt about knowledge, is an ingredient in the philosophical discussions about mathematical certainty. The question is, however, if this need for a justification – in contrast to the justifications that one gives for being certain in a particular situation – is not due to a philosophical prejudice.

Bearing this in mind, I will end this chapter with a discussion of the role that being certain plays in some situations where basic mathematics is involved. The certainty that pertains to the mathematics that we learn as children is seen to be indistinguishable from the role that it plays in relation to other activities. They form, as it were, a backdrop for many situations and contribute to the way we judge them. They are not among the things that we judge on the scale of certainty or doubt but figure among that which allows us to form such judgements. Wittgenstein is famous for his insistence to view mathematical propositions as being *normative* and section 2.7 is an elaboration of this idea of Wittgenstein's. Their certainty is in this sense greater than the certainty that may pertain to other kinds of propositions, but it is greater because it is of a different *kind*, not because it is certain to a greater *degree*.

2.1 Hilbert on Certainty

In his work on the foundations of mathematics, Hilbert often mentions the goal of his efforts. In 'On the Infinite' he declares that the aim of his theory 'is to endow mathematical method with the definitive reliability that the critical era of the infinitesimal calculus did not achieve'.[10] In other places, he gives different and more detailed formulations, and his earnest (and rather pompous) attitude makes them into unusually clear expressions of the unclarity that pertains to our understanding of the certainty of mathematics.

I will quote some particularly striking passages, although this will not be a Hilbert exegesis. These passages will serve as a basis for the subsequent discussion of different aspects of certainty in mathematics. In 'Problems of the Grounding of Mathematics', Hilbert frames, what he calls, 'the generally held opinion about mathematics and mathematical thought' thus:

> The mathematical truths are absolutely certain, for they are proved on the basis of definitions through infallible inferences. Therefore they must also be correct everywhere in reality.[11]

[10] Hilbert, 'On the Infinite', p. 370.

[11] David Hilbert. 'Problems of the Grounding of Mathematics'. In: *From Brouwer To Hilbert. The*

The article 'On the Infinite' provides several suggestions:

> [H]as the contentual logical inference ever deceived and abandoned us anywhere when we applied it to real objects or events? No, contentual logical inference is indispensable. It has deceived us only when we accepted arbitrary abstract notions, in particular those under which infinitely many objects are subsumed. …
>
> One can claim … that [the science of mathematics] is an apparatus that must always yield correct numerical equations when applied to integers.[12]

In the already quoted 'The New Grounding of Mathematics: First Report', one finds the following passage which echoes the received view of Hilbert's work on the foundations of mathematics:

> Accordingly, a satisfactory conclusion to the research into these foundations can only be attained by the solution of the problem of the consistency of the axioms of analysis. If we can produce this proof, then we can say that mathematical statements are in fact incontestable and ultimate truths – a piece of knowledge that (also because of its general philosophical character) is of the greatest significance for us.[13]

The quotes reveal that the reason for the alarmed reactions to the paradoxes is that they indicate that there is a problem with mathematics, and the presumed existence of such a problem *contrasts* with the confidence that we (laymen and professional mathematicians alike) share with regard to the mathematics we use. When dealing with the concept of mathematical certainty this confidence is very important. If it were not for this spontaneous attitude towards mathematics, the paradoxes would probably not have been as upsetting as they were, nor would there be anything like securing mathematical certainty.

It is possible to distinguish four different strands in the quotes above: (1) mathematical methods are reliable, they give us the correct result; (2) mathematical methods are reliable in the sense that they do not lead to contradictory results; (3) mathematical methods are reliable (genuinely reliable) only when we have a proof of the consistency of them; and (4) the inferences of logic are reliable. I shall now turn to a discussion of these four strands, by turning them into suggestions as to how one could understand certainty. In addition to these, I will also consider the idea – not among the strands identified in Hilbert's writings – that mathematical truths are eternal. None of the suggestions discussed below constitutes a free-standing account of certainty in mathematics. None of them, taken in isolation, will be satisfactory. They will, however, point to aspects that contribute to our understanding of mathematical certainty. Furthermore, they

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Debate on the Foundations of Mathematics in the 1920s. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998, p. 228.

[12] Hilbert, 'On the Infinite', p. 376.

[13] Hilbert, 'The New Grounding of Mathematics: First Report', p. 202.

indicate that in discussions of certainty we can be talking past each other by talking about different things.

2.2 The Infallibility of Mathematical Methods

The first suggestion focuses on Hilbert's claim in the first and second quotes above that mathematical methods give us correct results. One can encapsulate it thus: *By the certainty of mathematical knowledge one means the impossibility of reaching erroneous results when using mathematical methods.* As is shown by the slightly differing emphasis in the two quotes, this may actually be seen as two different suggestions. The first could be that mathematical methods are infallible when applied in science, trade, construction, etc. and the second that the results of calculations and deductions in mathematics are always correct as such.

How should one understand the impossibility of reaching erroneous results when applying mathematics to empirical matters? Mathematical tools are useful in a great variety of situations and the advantages are of many different kinds: sometimes one saves time, sometimes the probability of making mistakes is smaller, sometimes a greater degree of accuracy is obtained, sometimes they enable one to achieve an overview of the problem, and sometimes there simply is no alternative to using a mathematical method in order to solve a problem. The infallibility under scrutiny here has – in contrast to the more practical benefits mentioned above – an absolute air to it. For practical purposes, the accuracy of the result of a calculation will be no better than the accuracy of the numbers entered into the calculation; if it proceeds from measurements, the inaccuracies of the measurements are reflected in the result. Nevertheless, this is not considered a shortcoming of the mathematical method, which may be taken to say only: 'If this is the correct value (of the length, weight, amount, voltage etc.), then this is the result.'

This infallibility, however, is not the same thing as a complete absence of erroneous results. One has to choose carefully which mathematical method to use in order to get relevant results. The wrong method will, if applicable at all, give incorrect results. A mathematical method does not by itself give either correct or incorrect results with regard to applications. The impossibility of erroneous results appears in a different light if one considers the fact that we do not use methods that do not work or that we have no use for.

Another, arguably more important, matter is that we often use basic mathematics as, so to speak, a measure or framework for ordinary experience. If measurements happen to diverge from what one has predicted through calculation, this kind of discrepancy is usually attributed to inaccuracies in the measurements, or to some feature of the thing measured that is not taken into account, e.g. thermal expansion. The discrepancies, unless they are taken to show that this particular mathematical method was inappropriate for the application, will not be assumed to be caused by flaws in the rules of arithmetic (whatever that means). On the contrary, a major incentive to look for inaccuracies in the measurements is the occurrence of such a discrepancy. The calculation is taken as the norm and it guides one in the search for errors in other places. This idea is found in Wittgenstein's writings, but it is also advanced by many of the logical empiricists.

A. J. Ayer discusses an example where one has five pairs of objects but only nine objects when one counts them one by one. With regard to the attempts to come to terms with this disagreement, he remarks:

> One would say that I was wrong in supposing that there were five pairs of objects to start with, or that one of the objects had been taken away while I was counting, or that two of them had coalesced, or that I had counted wrongly. One would adopt as an explanation whatever empirical hypothesis fitted in best with the accredited facts. The one explanation which would in no circumstances be adopted is that ten is not always the product of two and five.[14]

Ayer sees this as evidence for his claim that mathematical truths cannot be refuted by experience. Our reactions in this kind of case show that mathematical propositions are not analogous to empirical ones. In addition, I think Ayer's observation reveals that this difference is tied to the normative character of mathematical propositions. Thus, it might be worth considering if the infallibility of mathematical methods is not a consequence of our judging the world of experience through our mathematical methods. Cora Diamond contrasts the practice of mathematics with practices where one formulates descriptive propositions. Mathematical propositions play a normative role in relation to descriptive ones. 'Mathematics is integrated into the body of standards for carrying out methods of arriving at descriptive propositions, for locating miscounts (for example), or mistakes or inaccuracies of measurement.'[15]

I will return to the theme of normativity in the end of this chapter, but for now these comments will be enough to show that this first suggestion is not a straight forward way of understanding certainty.

If the infallibility of mathematical methods is considered while bracketing any possible applications outside mathematics, one has the second suggestion: *Mathematics will never produce an erroneous result.* To this and the previous suggestion, one has to add the proviso: the method will give a correct result only if one has calculated correctly. In the discussion of this suggestion, it becomes important to distinguish errors that are due to human mistakes from errors that are

[14] Alfred Jules Ayer. *Language, Truth and Logic*. Harmondsworth: Penguin, 1971, p. 101.

[15] Cora Diamond. 'Wittgenstein, Mathematics and Ethics: Resisting the Attractions of Realism'. In: *Cambridge Companion to Wittgenstein*. Ed. by Hans Sluga and David G. Stern. Cambridge: Cambridge University Press, 1996, p. 234.

due to the methods of mathematics. David Hume argues that '[i]n all demonstrative sciences the rules are certain and infallible', but he seizes on 'our fallible and uncertain faculties' and concludes that we 'are very apt to depart from them and fall into error'.[16] For Hume, this possibility of error licenses the conclusion that mathematical knowledge is, after all, not certain. However, Hume's sceptical conclusion does not bear on this suggestion since the fact that our capacities are limited does not infringe upon the value of mathematics. The certainty of mathematics should rather be associated with what Hume refers to as the rules being 'certain and infallible'.

If mathematical certainty is interpreted as an absence of errors that are due to the methods of mathematics, it becomes evident that we have no clear conception of what kind of error certainty excludes. This point applies to both of the suggestions considered so far. As mentioned above, the methods of mathematics have all kinds of advantages in comparison with other ways of determining a value, but the idea of absolute correctness leads one to expect that there is another kind of failure that is excluded by the mathematical methods. It is as if our mathematics *could* have been a mathematics where following the correct rules and procedures in some cases led to the wrong result – only, luckily, it happens not to be such.

When one says: 'mathematical methods cannot go wrong' – what is it that they cannot do? If one thinks about calculation, 'going wrong' could mean that a mistake is made; if one thinks of measurements, it could mean that one reads the wrong number from the calliper. We have in these cases a clear conception of what 'going wrong' could mean. The infallibility of mathematics, however, seems to ensure protection from yet another kind of error. This error would be one where correctly following the rules of calculation led to an incorrect result. What kind of situation is this? Consider first a case where one tries to apply a certain mathematical method and arrives at a result that is not usable or diverges strongly from what seems reasonable. One would probably assume that one has made a mistake and try again. If one is successful the next time, one would probably not think more about it. If, however, the same result occurs once more and one feels confident that no mistakes were made, the conclusion would probably be that this method was inappropriate for the application. These outcomes are not yet examples of the mystical error that I am trying to make sense of. Assume now that one calculates once more, checks the steps thoroughly, and a new, *different* result stands. Furthermore, the first calculation is also checked and no mistakes are found; the original result is, thus, confirmed too. This seems to be a description of a fallible mathematics where doing the same thing, following the same rules in the same way leads to different results at different times.

[16] David Hume. *A Treatise of Human Nature*. Ed. by David Fate Norton and Mary J. Norton. Oxford: Oxford University Press, 2000, I.4.1, p. 121.

This does not seem to be a *genuine* description, however. Is it not a criterion for 'having done the same thing', for 'having followed the rules in the same way' that one arrives at the same result? The two identical calculations would have to part ways at some particular step, be it the last one or some intermediate one. Having done the same thing at a particular step means writing the same thing. There seems to be no logical space for going wrong other than through deviation from the rules, i.e. through mistakes made by the calculator. Doing the same thing means getting the same result, otherwise one has not done the same thing. That one does not have a clear idea of the nature of this kind of error is also seen in the fact that the above description (of doing the same thing, getting different results) can hardly be called a genuine description of *mathematics* at all.

The above remarks apply both to the first and to the second suggestions, but, if one restricts the discussion to the second (i.e. to pure mathematics), the issue of how one is able to distinguish the correct from the incorrect becomes a pressing matter. One is faced with the question of whether there exists some standard of correctness against which to weigh the result of a calculation. That is to say, is there some standard for the correctness of the result other than the calculation itself and the rules one has followed in completing it?

For a Platonist, this presents no problem at first sight. If mathematics produces truths that correspond to what can be truthfully said about the entities in the mathematical realm, then it produces correct results, otherwise incorrect results. Even so, the impossibility of somehow describing mathematical objects without actually doing mathematics, makes it highly questionable that one could establish true descriptive propositions against which to judge the ones reached through calculation or inference. As I shall argue in the next chapter, the Platonist, like everybody else, is left with the ordinary techniques of mathematics in order to determine what is correct and what is not – and this is no shortcoming.

Sometimes the term *intuition* is proposed to compensate for the lack of a mathematical counterpart of sensory experience.[17] Nevertheless, if intuition is regarded as a way of apprehending mathematical truths that is parallel to deducing and calculating, one may wonder which of them that is the more reliable one. Gödel suggested that the reliability of this intuition is no less than that of sense perception, but if the problem under consideration is whether intuition can furnish a criterion of correctness for the results arrived at through deduc-

[17] C.f. Kurt Gödel's well-known defence of intuition as a kind of perception: '[T]he objects of transfinite set theory … clearly do not belong to the physical world … But, despite their remoteness from sense experience, we do have something like a perception also of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception.' Kurt Gödel. 'What is Cantor's Continuum Problem?' In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983, pp. 483–84.

tion or calculation, then surely intuition is not sufficient. The reliability of our mathematical methods must be greater.

To be sure, one develops an ability, a skill – often called intuition – which allows one to judge what sounds plausible, to judge which method of proof, which rule, or what theorem one should rely on in order to solve some particular problem. This intuition allows one to judge, among other things, if the outcome of a calculation seems reasonable. This is, however, not the same as judging if it is *correct* by means of intuition. As William Tait remarks: 'What we call "mathematical intuition", it seems to me, is not a *criterion* for correct usage.'[18] In applied mathematics, the correspondence between results of calculations and measurements might make sense as an external criterion of correctness but this is not an option in pure mathematics. Proofs are the supreme courts in mathematics, and if a proof is correct, then what it proves is *eo ipso* correct.

2.3 Consistency

The third suggestion is that certainty in mathematics is to be understood as freedom from contradictions: *Mathematics, to the extent that is certain, will never produce a contradiction.* This suggestion is a natural one in face of the paradoxes that arose in set theory and logic in the late nineteenth and early twentieth centuries. The clearest expression of this attitude is found in Hilbert's writings, the fourth of the above Hilbert quotes being a good example. Marcus Giaquinto's *The Search for Certainty* can be given as a contemporary example. He asks: 'When we cannot be certain of the reliability, hence consistency, of some mathematics, can we none the less have a high degree of confidence in it?'[19]

As it was pointed out in the introduction, many have felt that one is not entitled to claim that mathematical knowledge is certain after all, since (1) paradoxes did arise, and (2) Gödel proved that consistency is impossible to prove for central areas of mathematics. In this context, it is remarkable that Gödel's proof made use of such techniques that Hilbert called *finitary* and that according to Hilbert were the most reliable ones. The paradoxes, he thought, arose because techniques involving the concept of infinity were employed. Thus he wrote: 'It is necessary to make inferences everywhere as reliable as they are in ordinary elementary number theory, which no one questions and in which contradictions and paradoxes arise only through our carelessness.'[20]

This third suggestion – as is seen in the quotes from Hilbert and Giaquinto – indicates a distinction between certainty in the sense 'the confidence that I feel'

[18] W. W. Tait. 'Truth and Proof: The Platonism of Mathematics'. In: *Synthese* 69 (1986), pp. 341–70, p. 346.

[19] Marcus Giaquinto. *The Search for Certainty: A Philosophical Account of Foundations of Mathematics*. Oxford: Oxford University Press, 2002, p. 222.

[20] Hilbert, 'On the Infinite', p. 376.

and 'the impossibility of reaching contradictory results'. It is the latter that is important according to this suggestion. An expression of this contrast is the strong confidence in set theory among a large part of the mathematicians working at the turn of the twentieth century, despite the fact that set-theoretical antinomies could be derived.

The fact that the paradoxes were considered a threat to the certainty of mathematics is, I think, due to their unexpected appearance. One has the feeling that one might be doing mathematics in the ordinary fashion and – through the correct application of rules of inference or calculation – arrive at contradictory results. Perhaps the result contradicts an earlier one. Perhaps one does not even notice that it does. Georg Henrik von Wright expresses this uneasiness that the paradoxes gave rise to: 'Could one be confident that one would not one day run into contradictions in arithmetic, algebra, or geometry too?'[21] Uncertainty acquires an air of *distrust* of mathematics.

The idea that the set theoretical paradoxes remove certainty from mathematics, lives within a fear that contradictions might appear where one would not expect them to – and not because of an error on the part of human beings, but on the part of mathematics itself. This idea can be accompanied by the further worry that one would not necessarily recognise a contradictory result at once or fail to realise that the result contradicts another that one depends on. It is as if mathematics produced results in the manner of a machine, and if certainty is found in mathematics one can trust it not to produce a contradiction.

Of course, mathematics is not a machine that produces results independently of mathematicians. If a paradox arises, the usual response is to review the calculations or the proof once more to make sure no mistakes have been made. If none are found and the contradiction remains, a layman will perhaps ask a mathematician or somebody more skilled for an explanation of the unexpected result. A mathematician might turn to colleagues for advice or conclude that a contradiction arises if one follows certain rules in a certain way and perhaps make this publicly known by publishing it. There is much prestige in finding a contradiction. It is then up to the community of mathematicians to decide what to do about it.

Now, the occurrence of contradictions, the set-theoretical ones in particular, is seldom seen as a reason to abandon mathematics or even a part of it, as the malfunctioning of a machine might.[22] Assume that a machine, e.g. a computer,

[21] Georg Henrik von Wright. *Logik, filosofi och språk*. 2nd ed. Lund: Doxa, 1965, p. 77, my translation from Swedish.

[22] Haskell B. Curry comments on Hilbert's insistence on consistency proofs that inconsistency does not make a theory useless for applications and '[e]ven if an inconsistency is discovered this does not mean complete abandonment of the theory, but its modification and refinement.' Haskell B. Curry. 'Remarks on the Definition and Nature of Mathematics'. In: *Philosophy of mathematics.*

produces strange results. The same commands give one output at one time and a different output at another when they are supposed to give the same output every time. There seem to be two kinds of problem to look for. Either a part of the machine is worn down or the machine was not properly constructed from the start. If the mathematical machine produces contradictions, the possibility of parts being broken is, naturally, excluded. If it produces contradictions this must be due to poor construction. If the machine analogy is followed strictly, the natural solution would be to abandon the machine or to reconstruct it. However, nobody abandoned the machine of mathematics. (Although the intuitionist response to abandon the law of the excluded middle could be viewed in analogy with reconstructing the machine.) Both logicism and formalism tried to solve the problems by *mathematical* means.[23]

In conclusion, one could say that the existence of contradictions does not seem to introduce uncertainty into mathematics, even though it does call for clarification. In this context, 'clarification' can mean two things, and the paradoxes of set theory necessitated both. Firstly, there is room for philosophical clarification of the situation where the paradox arises and worries us. This is, arguably, what Wittgenstein sought to do in his remarks on the subject as is brought forward in the following passage: 'It is the business of philosophy, not to resolve a contradiction by means of a mathematical or logico-mathematical discovery, but to make it possible for us to get a clear view of the state of mathematics that troubles us: the state of affairs before the contradiction is resolved.'[24] Secondly, it becomes necessary to overview the mathematics where the contradiction arises in order to see if and how it can be avoided, but also to see what mathematical results it possibly endangers. This is mathematical work, and it is what Wittgenstein refers to as 'resolving the contradiction'. That paradoxical results call for such clarification accords well with the reaction to them displayed in the history of mathematics. In this sense, much of the foundational work in the early twentieth century could also be called clarificatory. Nevertheless, I am inclined to think that what gave work on the foundations of mathematics much of its

Selected readings. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983, p. 206.

[23] A more serious problem with this picture is perhaps the nature of 'wrong answer'. A machine is usually associated with a purpose. If it functions properly, there is some kind of job it will accomplish. 'Giving out a result/product that it is supposed to' becomes meaningful in light of the purpose of the machine. This purpose is, moreover, determined by matters external to the machine. The output of the machine of mathematics, on the other hand, cannot be judged in the same manner. True, the purpose of mathematics is, among other things application, but 'the result it is supposed to give' cannot be understood extra-mathematically. The assessment of the result is also a piece of mathematics and would accordingly belong to the working of the machine. An absurd consequence of the analogy would be that one simply has to accept whatever it pleases to give out.

[24] PI, § 125.

drive was precisely the idea that one thought of it as attempting to establish the certainty of mathematics.

2.4 Eternal Truths

The fourth suggestion is similar to the second: *Mathematical propositions are always true; they express eternal truths.* It is frequently stated that mathematical truths, in contrast to other truths, are timeless and everlasting. In Plato's *The Republic*, Socrates states that 'the knowledge at which geometry aims is knowledge of the eternal, and not of aught perishing and transient'.[25]

Again, this is not sufficiently clear as it stands, and the choice of the word 'eternal' gives the suggestion a mystical air. Still, there is something to the idea that mathematics is not dependent on particular situations and the change of circumstances. The validity of an inference based on a rule of calculation does not depend on the time the conclusion is drawn. In this respect, mathematics is timeless. However, to understand this non-temporality, one must distinguish between something being extended in time, possibly indefinitely, and something where references to time does not enter. An example of the first case could be that Finland has been an independent state since 1917. A sentence stating as much has been true for almost a hundred years. The timelessness of mathematics must not be understood in analogy with this example, with the addition that the truth of mathematical propositions extends indefinitely in time. As Sören Stenlund notes:

> The expression 'timeless' is used as though it made sense to relate mathematical truths to time at all, as though it made sense to talk about a mathematical fact as being a fact, before, after or simultaneous with something else, which it obviously does not. We do not ask questions like: 'When did 2 + 2 = 4 become true and who made it true?'[26]

With mathematical truths, time is not an issue. If the students, who are told that it is possible to calculate the volume of a certain body by using techniques from integral calculus, responded by asking their teacher: 'Since when is this possible?' – the teacher would probably recount some relevant parts of the history of calculus. There seems to be no room for taking the question as being analogous to the question 'Since when is Finland an independent state?' Recounting the historical background of the integral calculus is in these cases not analogous to recounting the events that led to Finland's independence.

Consequently, there is an important sense in which mathematics is timeless. There is nothing about this understanding of non-temporality, however, that implies the eternal existence of mathematical objects. One could say that

[25] Plato. *The Republic*. New York: P. F. Collier, 1901, p. 527b.

[26] Sören Stenlund. *Language and Philosophical Problems*. London: Routledge, 1990, p. 126.

a grammatical or logical remark about the nature of mathematical propositions is mistaken for a remark about the nature of mathematical objects. Anyway, the claim that mathematical knowledge is certain does not seem to be given a proper illumination by it.

Another way of understanding 'eternal' in this context could be that the results of past mathematicians are as good today as they were thousands of years ago. Bell remarks: 'Euclid's Proposition I, 47 stands, as it has stood for over 2,200 years. Under the proper assumptions it has been rigorously proved.'[27] In view of the fact that many ideas of antiquity have been revised since then, it is indeed remarkable that their mathematics still stands. However, as Bell notes, they have been proved 'under the proper assumptions': considered as a theory of physical space, the geometry of Euclid is no longer true. Its timelessness is therefore restricted to the inferences from the assumptions stated in the axioms and postulates of Euclid, that is, restricted to Euclidean geometry considered as a system of pure mathematics. This is, of course, in line with the suggestion that mathematics is eternal, since it did not concern physical theories after all. However, that we still view the inferences of Euclid as valid can simply be a consequence of the relative stability of human reasoning. We still reason in the same way as the Greeks of antiquity, or our reasoning is at least sufficiently close to theirs. We can also appreciate the philosophy of Plato and the plays of Sophocles. This is another sense of 'timeless' but it is not related to the concept of certainty in any direct way.

2.5 Deductive and Empirical Sciences

The claim that mathematics is certain can be a reminder that there is a difference between sciences. Perhaps one wants to say that, whereas there is an inherent uncertainty in the empirical sciences, we can have certainty in mathematics. Sometimes this difference is underlined with a reference to proof. The fact that in mathematics we prove things deductively distinguishes it from other disciplines. Shapiro points to this difference: 'Unlike science, mathematics proceeds via *proof*. A successful, correct proof eliminates all rational doubt, not just all reasonable doubt.'[28]

The distinction between rational and reasonable doubt is ambiguous, however. One possible interpretation is that rational doubt is to be equated with Descartes's methodical doubt, i.e. any logically possible (however far-fetched) doubt. Yet, this is not a kind of doubt that one needs a proof to eliminate. It is better countered by philosophical work on scepticism, since it is a doubt that arises from a philosophical confusion. Anyhow, I will not argue this issue here.

[27] E. T. Bell. *The Development of Mathematics*. 2nd ed. New York: McGraw-Hill, 1945, p. 12.
[28] Shapiro, *Thinking about mathematics*, p. 22.

The most likely interpretation (and one that is in line with the reminder about the difference between science and mathematics) is that reasonable doubt can be eliminated by experimental corroboration of a hypothesis, whereas rational doubt can only be eliminated by a mathematical proof. Accordingly, one can in science reach certainty (only) to such a degree that doubt is no longer *reasonable*. Yet, since there is a possibility that future experiments can, in principle, falsify any hypothesis, it may still be *rational* to doubt the hypothesis.

Nevertheless, this construal of the difference between science and mathematics is potentially misleading. There is a risk that one understands the function of proofs as merely one of *increasing* conviction. This, however, seems to leave out the most important aspects of proofs, i.e. that understanding a proof involves the exclusion of doubt, involves changing the status of the theorem in relation to other propositions. (I shall return to these features of proof in chapter 5). It connects with the discussion of certainty since it invites the idea that the difference in certainty is one of *degree*. One thinks – mistakenly, I believe – that one has a clear grasp of what certainty is and that certainty is only partly attainable in science, but fully attainable in mathematics, and that this is what separates mathematics from science. That certainty in mathematics differs from other kinds of certainty not in degree but in kind will be the point of section 2.7.

2.6 Uncertainty

Among these different suggestions concerning how one should understand the certainty in mathematics it is also worth mentioning the possibility of denying it altogether. There is a line of thought, running from Mill, claiming that mathematics, contrary to common belief, rests upon empirical generalisations and therefore cannot reach a higher degree of certainty than those generalisations. This is argued by, for example, Harold Smart:

> It would seem incontestable, therefore, that mathematical reasoning, like other reasoning, when examined objectively, presents features or aspects which can only be consistently described as inductive in nature. The characterization of mathematics as a purely deductive science accordingly reveals itself as the result of an uncritically accepted tradition, coupled with certain 'metaphysical' prepossessions, such as that of the ability of 'pure' thought to generate out of itself, wholly apart from 'experience', results of real significance.[29]

A contemporary form of this criticism departs from the fact that the length of many proofs implies a substantial risk of error. Philip Kitcher argues that if there is a reasonable probability of error, the theorem proved cannot be said

[29] Harold R. Smart. 'Is Mathematics a "Deductive" Science?' In: *The Philosophical Review* 38 (1929), pp. 232–45, p. 241.

to be known a priori.[30] This line of thought is reinforced by the introduction of computer assisted proofs, the length of which makes it impossible for any human being to assess them. In both cases, proofs are found not to live up to the demands of certainty.

Another line of thought takes as its starting point the observation that the standards of proof have changed throughout history. Bell claims: 'The standard of mathematical proof has risen steadily since 1821, and finality is no longer sought or desired. … It is clear that we must have some convention regarding "proof".'[31] Since these standards of proof have changed, there are no guarantees that they will not change in the future. That is, we may have to revise what we now consider proved conclusively. This implies that the mathematics we know does not live up to absolute certainty, although we feel confident about it.

A third line of thought, which in many respects is similar to the second, is found in Lakatos's quasi-empiricism. In *Proofs and Refutations*, he gives examples of proofs that have been found inconclusive and later improved to overcome the errors of the earlier version. Lakatos explicitly criticises a dogmatism that he finds in formalism and in the metamathematical tradition. This dogmatism, he claims, manifests itself in a neglect of the historical development of mathematics, which, according to Lakatos, displays a series of conjectures, proofs, and refutations. The history of mathematics is, therefore, not a steady cumulative progress, but a process where conjectures are made and proofs are suggested and refined, but where no proof reaches a final state of absolute certainty. There is a resemblance between empirical and mathematical propositions in the sense that both are *revisable*.[32] Lakatos summarises: 'We never *know*: we only guess. We can, however, turn our guesses into criticizable ones, and criticize and improve them.'[33]

These lines of thought have had a substantial influence on contemporary philosophy of mathematics. This influence has been valuable in that it has directed the attention of philosophers to the history of mathematics but also to the

[30] Philip Kitcher. *The Nature of Mathematical Knowledge*. New York: Oxford University Press, 1984, pp. 40, 42.

[31] Bell, *The Development of Mathematics*, p. 10.

[32] See the author's introduction in: Imre Lakatos. *Proofs and Refutations: The Logic of Mathematical Discovery*. Ed. by John Worrall and Elie Zahar. Cambridge: Cambridge University Press, 1976. Similar considerations are also voiced by Hilary Putnam, although he is not associated with the quasi-empiricist philosophy. He writes that 'mathematical knowledge resembles *empirical* knowledge – that is … the criterion of truth in mathematics just as much as in physics is success of our ideas in practice, and that mathematical knowledge is corrigible and not absolute.' Hilary Putnam. 'What is Mathematical Truth?' In: *Philosophical Papers*. Vol. 1: *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975, p. 61.

[33] Imre Lakatos. 'Infinite Regress and Foundations of Mathematics'. In: *Philosophical Papers*. Vol. 2: *Mathematics, Science and Epistemology*. Ed. by John Worrall and Gregory Currie. Cambridge: Cambridge University Press, 1978, p. 10.

practice of mathematics. Quasi-empiricism, in particular, has also had a considerable impact on the didactics of mathematics. William Aspray and Kitcher describe the current situation as a debate between the *mainstream* and *maverick* philosophers of mathematics.[34] The dissatisfaction among the mavericks concerning the present state of the philosophy of mathematics (i.e. the dissatisfaction with the mainstream) is pointedly expressed by Reuben Hersh: 'Our inherited *and unexamined* philosophical dogma is that mathematical truth should possess absolute certainty. Our actual experience in mathematical work offers uncertainty in plenty.'[35]

Since the aim of this thesis primarily is to work out how we can understand the certainty of mathematics, rather than to argue for its certainty or uncertainty, I will not devote much attention to the debate between the mainstream and the maverick traditions. I will, however, make a brief comment towards the end of this thesis about the possibilities of arguing for one or the other of these positions. At present, I will only make a short comment about the difference between research mathematics and the mathematics shared by anybody who has learnt mathematics in school.

It lies in the nature of research that it moves on the frontiers of our knowledge. There will in such cases often be an uncertainty concerning the correctness of the results or the interpretation of the results. Mistakes that have been made may not have been spotted yet by the research community. This goes for any science, mathematics not excluded. The history of mathematics has shown that new methods of proof have been met with suspicion and that it takes a while to settle the range of applicability of such methods. That there exists a degree of openness in the new results of research, however, should not be taken as a defect of established mathematics. When thinking about the certainty of mathematics, the starting point should rather be our ordinary mathematics, which is shared by most human beings – not only by research mathematicians.

2.7 Certainty and Being Certain

At this point, having surveyed these suggestions, I shall turn to a more general discussion of the concept of certainty – of *being certain*, and of *attributing certainty to something*. I make no claims to a complete discussion of this concept, but I shall in particular explore the contrast between things we do claim to be certain *about* and things we attribute certainty *to*. In addition, I will consider the difference between facts to which we explicitly attribute certainty and mat-

[34]William Aspray and Philip Kitcher, eds. *History and Philosophy of Modern Mathematics*. Minneapolis: University of Minnesota Press, 1988, p. 17.

[35]Reuben Hersh. 'Some Proposals for Reviving the Philosophy of Mathematics'. In: *New Directions in the Philosophy of Mathematics*. Ed. by Thomas Tymoczko. 2nd ed. Princeton: Princeton University Press, 1998, p. 17, emphasis in the original.

ters the certainty of which is tacitly presupposed. The aim of this section is to argue that certainty is not a homogenous concept that we use the same way in all the situations in which it occurs. One has to be open to the possibility that certainty, when used with regard to mathematics, is a different concept than in, say, everyday empirical contexts.

To begin with, it is worth distinguishing between expressions such as 'I'm convinced that …'; 'I'm certain of …'; 'It's certain that …'; and '… is certain'. Spontaneously, one would say that there is a difference between *being* certain of and attributing certainty *to* something. The phrase '… is certain' seems to be a stronger claim than merely saying that *I* am certain of something. Here is a way of understanding the difference between the above expressions: the declaration that one is certain of something or convinced of something seems to be a report of a subjective state of conviction (which implies that there is a possibility of a mistake), whereas the attribution of certainty to something seems not to involve the person behind the utterance. One wants to say that certainty somehow clings to what is labelled certain (e.g. a mathematical proposition) and that this makes the certainty in such a case into an objective fact.[36] Consequently, it seems to make sense to look for some quality of the thing labelled certain which explains why it is certain. This understanding of the difference between the expressions misses important aspects of 'certainty'. A problem with this characterisation of the difference lies in locating the source of the objectivity of 'It's certain' in the mathematical proposition. I would rather say that the certainty of it is indistinguishable from its use in mathematical activity, and indistinguishable from the relation that mathematical activity bears to other activities.

Still, the expressions 'I'm convinced' and 'It's certain' surely point to some, arguably important, difference. In order to come to terms with this difference, let me begin by considering Norman Malcolm's contrasting view that there is no logical difference between them. He notes that if someone utters any of the above phrases, there is, naturally, someone behind the utterances, regardless of how the claim is phrased. From this he concludes that 'it is an individual who asserts that something is certain. If I am certain about the truth of something it is *I* who am certain.'[37] Accordingly, saying that something is certain implies that the speaker is certain thereof; hence, both 'are "expressions" of the certainty of the speaker'.[38]

Expressions of certainty are, of course, *somebody's* expression, but the choice of words signifies a difference in the speaker's attitude to the certainty at hand.

[36] Interestingly, the distinction between certainty as a psychological state of a person and as a quality of an object is also found in the *Oxford Dictionary of English* entry for 'certainty'.

[37] Norman Malcolm. *Nothing Is Hidden: Wittgenstein's Criticism of his Early Thought*. Oxford: Blackwell, 1986, p. 234.

[38] Ibid., p. 206.

One difference between 'I'm certain …' and 'It's certain that …' has to do with the urgency of the will to silence the doubts of the other. The latter expression constitutes a stronger claim. It is one we may use in the face of persistent scepticism: 'Don't worry, it's certain that …' By contrast, the claim 'I'm certain' uttered as a reaction to someone's doubt is often a report of a conviction that one does not necessarily expect the other to share: 'Well, I am certain, you believe what you please.' The phrase 'It's certain', by contrast, can be taken as a guarantee that it is *trustworthy*; the person voicing the certainty takes on a responsibility for what is being labelled certain: 'You can trust me, this is certain.' Saying 'It's certain' thus involves the further claim to be in a position where one can rightfully take that responsibility.

As an illustration of this, one can consider the following two examples of situations when expressions of certainty occur. If a friend and I have decided to attend a lecture given by a visiting philosopher and we find that we are the only persons waiting in the room at the announced time, my friend may ask me: 'Are you sure it's supposed to be here?' I might reply: 'Yes, I'm certain, I checked the venue just before I left.' My friend might insist: 'Was it really today? Could it have been Thursday next week?' Perhaps annoyed at this questioning (and perhaps I checked the time and place carefully beforehand), I may reply: 'It's certain, it is today. They're probably just late.' If, on the other hand, I realise that the doubts will not be put to rest by anything short of showing the letter with the time and the place, I may say 'Well, *I* am certain that this is the right time.'

The other example is one where the professed certainty concerns a mathematical statement. Two builders are building a purlin roof on a small cottage. They are about to put the rafters in place and one of them insists on using the Pythagorean theorem to calculate the length of them. The one doing the calculations says: 'The ridge rises 1.2 metres above the wall plate and the distance between the ridge and the walls is 2.5 metres. According to the calculation, the distance between the wall plate and the ridge board is 2.8 metres. The eaves overhang should be 40 centimetres. Let's make the rafters 3.2 metres.' The other one asks: 'Are you sure?' – not too sure of his capacity to judge the calculations. 'Yes, I'm quite certain.'

The other might be satisfied with this assurance and they go along, sawing off the planks at 3.2 metres. Alternatively, the other builder may not be happy with this assurance, because, say, a mistake was made in the calculations last time and a rafter ended up too short. He then insists: 'Are you really sure? I'd like to get it right this time!' The first one, proud of his calculating abilities, answers: 'It's perfectly certain that 3.2 metres is the correct length. I double-checked the calculations, they're just fine.'

In both of these examples, the choice of words 'It's certain' signifies that the person claiming to be in the know takes responsibility for what is claimed. It is implied that he has grounds for his claim. In a mathematical context, this can mean that one has done the requisite calculations or produced a proof. In most cases, this is what justifies certainty in mathematics. In the second example, the one who has done the calculations is in a position where he can rightfully say that it is certain that the length is correct. Thus, the upshot of this discussion is that there are important differences between the choice of words that we use for expressing certainty.

Malcolm's reason for attributing a similar status to 'It's certain' as to 'I'm certain' is probably the possibility of being mistaken. Regardless of how a person expresses his certainty, it appears that there is always room for mistakes, and in this sense there is a similarity between the two kinds expression. That there actually is a possibility of mistakes when somebody claims that 'It's certain' becomes clearer if one considers the difference between the first person and third person perspectives. If one says: 'He is certain' or 'He says that it's certain' it is perfectly intelligible that one may later discover that he was mistakenly convinced.

Now, this possibility is unsettling and it is particularly unsettling in the case of mathematics. If one follows traditional discussions on absolute certainty, it is as if the possibility of mistakes would have to be ruled out beforehand. Only then can something be said to be absolutely certain. This can be seen in the way Descartes introduces his method.[39] It is also the requirement Hume puts on knowledge. Realising that the demands are too high, however, Hume opts for scepticism. Are we forced to draw a sceptical conclusion too?

I will now discuss the rather puzzling distinction between *subjective* and *objective* certainty that Wittgenstein introduces in *On Certainty*:

> With the word "certain" we express complete conviction, the total absence of doubt, and thereby we seek to convince other people. That is *subjective* certainty. But when is something objectively certain? When a mistake is not possible. But what kind of possibility is that? Mustn't a mistake be logically excluded?[40]

Could the difference between subjective and objective certainty allow for a way of accommodating the possibility of making mistakes while at the same time making room for something which is logically excluded from doubt? Malcolm takes this distinction to signify a difference between, on the one hand, things which I am certain of, but about which I could nevertheless imagine myself be-

[39] See e.g. Descartes's first rule for 'rightly conducting one's reason': 'The first [rule] was never to accept anything as true if I did not have evident knowledge of its truth: that is, carefully to avoid precipitate conclusions and preconceptions, and to include nothing more in my judgements than what presented itself to my mind so clearly and so distinctly that I had no occasion to doubt it.' René Descartes. 'Discourse on the Method'. In: *The Philosophical Writings of Descartes*. Ed. by John Cottingham, Robert Stoothoff, and Dugald Murdoch. Vol. 1. Cambridge: Cambridge University Press, 1985, p. 120.

[40] Cf. Ludwig Wittgenstein. *On Certainty*. Ed. by G. E. M. Anscombe and G. H. von Wright. Oxford: Blackwell, 1969 (henceforth cited as OC), § 194, emphasis in the original.

ing mistaken – and, on the other hand, things that I am certain of and about which I could not even imagine myself being mistaken. Among subjective certainties Malcolm counts the kind of conviction the two friends waiting for the lecture to start give voice to, while objective certainties are the ones Wittgenstein is struggling with in *On Certainty*. These include the truisms that G. E. Moore claims to know in his articles 'A Defence of Common Sense' and 'Proof of an External World': 'There exists at present a living human body, which is my body'; 'the earth had existed also for many years before my body was born';[41] 'Here's one hand, and here's another' (while holding up one's hands), etc.[42] To these, Wittgenstein adds other examples, but the common puzzling feature is that they appear to be of an empirical nature but, nonetheless, exempt from doubt. Furthermore, and importantly for this investigation, Malcolm also counts elementary mathematical facts which we do not need to prove – such as '2 + 2 = 4' – among the basic certainties.

Might this distinction be a key to absolute certainty? If one follows Malcolm, it is not. Firstly, objective certainty is, when thinking of mathematics, restricted to very basic operations which we know by heart. If I prove something more complex, my certainty is of the subjective kind, since I could, after all, picture myself having made a mistake in proving it. Secondly, this difference is, for Malcolm, merely one in *my* apprehension of things. In the case of objective certainty, I cannot imagine any kind of mistake, but that does not, in principle, eliminate the possibility that I am mistaken. 'Being perfectly certain (i.e. objectively certain) of something … is an attitude, a stance, that we take towards various matters; but this attitude does not necessarily carry *truth* in its wake.'[43] On this view, there would be no infallible attributions of certainty, *if* by absolute certainty one understands the philosophical notion of something beyond every conceivable doubt. Still, may there not be something more to saying that mathematics is certain, than (merely) professing my conviction about it?

Malcom is perhaps not doing justice to the above quoted passage from *On Certainty*.[44] Criticising Malcolm's discussion of these matters, Elizabeth Wolgast writes that when we claim to know something or to be certain of something – regardless of how weak or strong this claim is – we are thereby drawing them into 'the certainty language-game'. She remarks: 'When we say that something is certain, we bring it forward into the certainty language-game, into the arena which is also that of doubt, presentation of evidence, challenge of evidence and

[41] George Edward Moore. 'A Defence of Common Sense'. In: *Contemporary British Philosophy: Personal Statements. (2nd series)*. Ed. by J. H. Muirhead. London: George Allen & Unwin, 1925.

[42] George Edward Moore. 'Proof of an External World'. In: *Proceedings of the British Academy* 25 (1939), pp. 273–300.

[43] Malcolm, *Nothing Is Hidden*, p. 216.

[44] Lars Hertzberg has suggested that it may even be read as an ironical remark about how we tend to view certainty (personal conversation).

so on.'[45] Thus, she comments on the example '2 + 2 = 4' that what we want to say when philosophising – that '2+2 = 4' is absolutely certain – does not make sense. Claiming that something is certain brings the meaningfulness of questioning it in its wake:

> So long as a thing is asserted or said to be known or to be certain, its denial and debate will necessarily be appropriate. This is a logical fact. For this is how 'I know' normally functions. It is how 'It's certain' functions too. And it is how 'It is absolutely certain, certain beyond any question and certain in the highest degree' functions as well. … We cannot close debate with asseverations of certainty.[46]

According to the above discussion of the difference between 'I'm certain' and 'It's certain', however, the latter expression does often serve to close a debate. It does by no means close a debate simply by being uttered, it carries no such force on its own – but it constitutes an invitation to the other to leave her doubts aside and trust what is being said. Wolgast's conclusion thus seems too drastic. Still, she leaves room for the certainty of such sentences as '2 + 2 = 4' by saying that the strongest candidates for certainty are those that we do not say that we know or that they are certain. She writes: 'The sentence "I know that 2 + 2 = 4" is an exceedingly odd one to actually use. The oddness of it has to do with the fact that no one questions whether 2+2 = 4, and that reflects in turn its perfect certainty.'[47] However, it is not difficult to imagine a situation when the sentence could be used meaningfully: when talking about counting with a child, for instance. That the sentence 'I know that 2 + 2 = 4' is an odd one to use would, accordingly, not have to do with it becoming an object of doubt if one says that one knows it or that it is certain, but with the fact that we do not in general state things that are obvious.

Thus, it is perhaps not a supposed exclusion from use together with 'I'm certain' or 'It's certain' that is the key to understanding the certainty of our basic rules of arithmetic, but the fact that they are tacitly presupposed in virtually all of our activities. Perhaps Wolgast's point could be given a better formulation if one focused on the word 'revisable' instead of on 'certain'. Hilary Putnam makes a remark which bears similarities to Wolgast's line of thought:

> [I]f we cannot *describe* circumstances under which a belief would be falsified, circumstances under which we would be prepared to say that -B had been confirmed then we are not presently able to attach a clear *sense* to 'B can be revised.' In such a case we cannot, I grant, say that B is 'unrevisable,' but neither can we intelligibly say 'B can be revised.'[48]

[45] Elizabeth Wolgast. 'Whether Certainty Is a Form of Life'. In: *The Philosphical Quarterly* 37 (1987), pp. 151–65, p. 156.

[46] Elizabeth Wolgast. *Paradoxes of Knowledge*. Ithaca NY: Cornell University Press, 1977, pp. 198–99.

[47] Ibid., p. 194.

[48] Hilary Putnam. 'Rethinking Mathematical Necessity'. In: *Words and Life*. Ed. by James Conant.

I shall elaborate on this 'tacitly presupposed' a little further. In *On Certainty*, Wittgenstein devotes attention to such propositions which – although one has taken no particular measures to ascertain them – seem to be beyond reasonable doubt, although they would traditionally be classified as empirical. It is puzzling that we do not learn them explicitly, but they still seem to be taken for granted in much that we do, and there does not normally arise any need to check whether they are true or not. Still, claiming to know such truths does not seem to be analogous to knowing ordinary empirical propositions, e.g. that birch wood is harder than pine. In the context of this discussion, Wittgenstein also discusses simple mathematical propositions like '2 + 2 = 4'. They, too, are beyond doubt, but in contrast to the other certainties, they are mathematical and one learns them explicitly.[49] Moreover, they are not proved like other mathematical propositions.[50] The basic rules of arithmetic (e.g. '2 + 2 = 4') are learnt as one learns to count, add, subtract, etc. Once one has learnt these techniques, the simple rules fall into the background. We use them frequently, sometimes consciously, sometimes almost unconsciously.

Concerning Moore's statements (but I think it captures our relation to simple rules of mathematics too), Wolgast writes: 'These certainties characterize our lives, and we learn them (if that term can be applied here at all) in the very process of growing to adulthood. We acquire these truths as we learn to act, learn the procedures of science and history, learn to express ourselves.'[51] This status, perhaps one could call it their certainty, is seen in the fact that they are withdrawn from the evaluation that other propositions undergo, and instead participate in the background apparatus that is involved in judging the things people say to each other. That they have this status is not something we ordinarily express in words, but it shows in the way we act. They 'have no expression in language; they belong to behaviour. Actions, not words, are their expression.'[52] In the case of arithmetic, this is not entirely true since they are, obviously, often expressed. However, they too have an impact on our lives in much the same way as the Moorean certainties in the sense that they also belong to our behaviour and find their expression in behaviour.

In order to illustrate this, I will explore the example with the builders a bit further. The first time they try to determine the length of the rafters, they end up

Cambridge MA: Harvard University Press, 1994, pp. 253–54.

[49] This is a clear difference to the certainties that form the main theme of *On Certainty*. Cf. § 152: 'I do not explicitly learn the propositions that stand fast for me.'

[50] Although it is possible to prove them within an axiomatic system such as set theory, this is something that, on the whole, very few people are aware of; yet, they have the unshakeable status for everyone who has learnt them. One might on these grounds argue that, although they in some axiomatisations appear as derived propositions, they are in practice more fundamental than the axioms.

[51] Wolgast, 'Whether Certainty Is a Form of Life', p. 152.

[52] Ibid., p. 153.

too short. The Pythagorean theorem will tell them how long to make them when they know the distance between the wall plate and the centre of the cottage, and the height of the ridge compared with the wall plate. These two distances will form the legs, while the rafters form the hypotenuse of a right triangle. Now, imagine that they double-check the calculations but arrive at several differing results. When they finally settle for what they think is the right answer, the rafter is too short once more. When trying to fill out the details of their discussion as they try to come to terms with this problem, several alternatives are conceivable.

At first, they will probably assume that some mistake was made in the calculations and go over them again. If no errors are found, they may wonder if they measured wrongly, so they measure once more. If they still cannot explain why the rafter did not fit, they might suspect that the building is leaning unnoticeably and that, therefore, they are not working with a right triangle. This would mean that the formula is not suitable for the application, although the calculations as such are correct. If there is no problem at this point either, they may wonder: 'Isn't this a suitable problem to apply the Pythagorean theorem to after all? But this is a right triangle and that's what the theorem is about.' At some point they will probably give up their attempts to solve the problem and look for another way to determine the length of the rafters.

If they are very keen on finding out why their approach did not work, they might at some point doubt their memory: 'Did we not remember the measures or did they perhaps change? Was the weather extreme in some way that could have caused the planks to expand or bend?' The last two alternatives are highly unlikely, but one thing they will probably not question is the validity of the basic rules of arithmetic. In trying to locate the error they may even use the calculations as a guide. That is, they will probably not try calculating with other rules (unless they are aware of some completely different method which would also serve the purpose). However, they would not try to calculate the square of the lengths differently, whereas they might use another measuring tape. One does revise one's opinion of many things, but not of what the correct rules of adding are. Importantly, the builders would not have to mention this in their search for errors: it shows in the way they behave.

It was indicated above that the rules of elementary arithmetic were beyond doubt, and this example shows one aspect of what it means for them to be indubitable. When the builders noted a discrepancy between the calculated length of the rafter and what was actually needed for the roof, they concluded that they had made a mistake somewhere. In their attempts to locate the mistake, they assess various steps where mistakes are likely to be made, but they do not consider the elementary rules of arithmetic among these possible sources.[53] Is this

[53]In a similar vein, Putnam states: 'I cannot imagine *finding out* that [the law of the excluded contradiction] is false.' Putnam, 'Rethinking Mathematical Necessity', p. 250.

a consequence of the impossibility of being mistaken concerning these rules? It is important for the problem at hand that this absence of mistakes is correctly understood.

As mentioned, Malcolm writes that the distinctive feature of what he calls objective certainty is that the person being certain cannot imagine any kind of mistake about the object of the certainty. According to Malcolm, however, this provides no guarantee that other persons will not say that she made a mistake. He thus concludes that the notion of objective certainty, although describing our strongest convictions, does not lead us to the philosophical concept of metaphysical certainty. There is, however, an important difference between *my* not being able to conceive myself as being mistaken and *our* not being able to *treat something as a mistake*.

I am suggesting that if someone seemed to calculate '2 + 2 = 5' we would not treat this as a false belief that ought simply to be corrected, but rather as a joke, as a slip of the tongue, as a test of our attention, or, perhaps, as a sign that the person in question has not learnt to count properly. One might ask: what would a mistake about '2 + 2 = 4' be like? Would it be analogous to a mistake where one believed that a flower was blue when in fact it was red (perhaps one turned one's head quickly to have a look and there was a bright blue object next to it)? One possible mistake in the case of '2 + 2 = 4' would be a slip of the pen that resulted in '2 + 2 = 9' (or a careless keystroke which resulted in '2 + 2 = 5'). However, there seems to be no room for a false belief about elementary arithmetic – no room for believing that '2 + 2 = 5'. If someone tries to calculate '2 + 2 = 5', and we notify her of this, imaginable answers include: 'Oh, right, I meant "2 + 2 = 4' of course!' and 'You're paying attention, that's good!' If she insists that she is correct in calculating '2 + 2 = 5', we may assume she is putting on a show. Perhaps we play along if we find it amusing, or then we may try to make her stop. If she does not show any signs of playing a role and grows irritated by our attempts to make her stop pretending, we will not know what to make of it. We may conclude that she had not been taught to calculate correctly (especially if we are talking to a child) or that she is suffering from a mental illness,[54] or we may try to make her see the inconsistencies that follows from using her rule. Thus, we *can* imagine mistakes in the case of basic arithmetic, but the mistakes that we are willing to call mistakes, e.g. a slip of the pen, are not of the kind that introduces the uncertainty that worries us.

In her study of Wittgenstein's *On Certainty*, Danièle Moyal-Sharrock distinguishes *mistakes* from *anomalies*.[55] The example with the person who insists

[54]Cf. OC, § 71: 'I should not call this a *mistake*, but rather a mental disturbance, perhaps a transient one.' The mistake in question concerns a friend who believes he lives somewhere he does not.

[55]Danièle Moyal-Sharrock. *Understanding Wittgenstein's* On certainty. New York: Palgrave

upon using '2 + 2 = 5' would surely count as an anomaly, not as a proper mistake. For something to be a mistake, there has to be a certain intelligibility to it, Moyal-Sharrock comments with reference to the following passage from *On Certainty*: 'Can we say: a *mistake* doesn't only have a cause, it also has a ground? I.e., roughly: when someone makes a mistake, this can be fitted into what he knows aright.'[56] When we perceive something as an anomaly, she remarks, 'it isn't the truth-content of my statement that would be under investigation, but my ability to understand the words I am using or, more sadly, my sanity – *I* would be under investigation.'[57] One can see this illustrated in the above example, in the attempts to make sense of the mistake as a mistake (e.g. as a slip of the tongue), or as an anomaly (as a joke, as a symptom of idiosyncrasy or insanity). If we cannot make sense of it as a mistake, it seems that we cannot treat it as a mistake. We would, for instance, be at a loss regarding how to correct it. Importantly, we cannot make sense of it as a mistake qua false belief.

It seems to me that when discussing the certainty of mathematics, the kind of mistake which would have philosophical consequences is the possibility of being mistaken about mathematical propositions in the sense of *holding false beliefs*. At least in the case of the simple rules under consideration now, this is not a possibility, not a move in the language-game at all. In this sense, one could say that mistakes are logically excluded, to use a phrase from the quote from § 194 of *On Certainty*. I would be hesitant to call it objective certainty, though, since I doubt that it would be meaningful (or even possible) to try to delineate such a concept.

The discussion of mistakes has up till now assumed that the person we are talking to has been someone to whom we attribute full understanding of the counting and calculating procedures that the vast majority of people learn. With children, the situation is different, naturally, and this shows something interesting about the status of, for instance, '2 + 2 = 4'. Children learning to count often leave out a number in the natural number series, put them in the wrong order, or the like. When counting objects, they sometimes point to the same object twice saying '4, 5', thus counting it twice. In trying to mimic the counting of an adult, it is, naturally, not easy to understand which features of the procedure that are essential for *counting*. Adults demonstrating a counting procedure often count rhythmically, and the child who has noticed the rhythmic features of the adult's counting will perhaps find it easier to maintain that rhythm by counting an object twice if it is difficult to find the next one to be counted. Learning to count involves, among other things, to see which features that are essential to the procedure (counting every object exactly once) and which that are merely practical

Macmillan, 2004, p. 73.

[56] OC, § 74.

[57] Moyal-Sharrock, *Understanding Wittgenstein's* On certainty, p. 74.

(maintaining a rhythm in order to facilitate following the number series). The point is, when one is thinking of someone learning to count, add, and so forth, the notion of mistake is a different one from that of adults. In the case of children learning to count, it does not sound strange to talk about a mistake with regard to the rules of arithmetic. In some cases, we might want to call such a mistake a false belief; in other cases, we might say instead that the child has not yet learnt the rule correctly (which may be different from a false belief, since there are perhaps no beliefs present about the rule yet). Anyhow, already the learning of elementary arithmetic involves a training process and in this training it is possible to make mistakes. It is not self-evident for the learner what the correct procedure is.

All this means that when somebody makes a slip of the pen in doing a calculation and thereby accidentally applies the wrong rule, this does not make the rules fallible, since the rules were not followed correctly – indeed the rules *were not* followed (cf. p. 15). Furthermore, we only ascribe the ability to follow the rule to someone who does not deviate from the correct procedure (or to be precise: only to someone whose deviations can be made sense of as simple slips).

This section has, so far, focused on the simple rules of arithmetic, but there does not seem to be any room for doubt about theorems that have been proved either. How should we understand the certainty of conclusions of proofs? If one has read a proof and settled that the proof is correct – i.e. that each step is valid and it forms a sound argument – there is no room for any doubt about the theorem.

This stands in stark contrast to, for instance, the possibility of doubt concerning the correctness of a sentence given in a trial. The court may have closed a case and passed judgment in accordance with correct procedures, but there may still be doubts about the correctness of the judgement. It seems to me that the philosophical use of 'is certain' with regard to mathematics is pointing to this absence of alternatives once correct procedures have been followed. This kind of certainty, which is related to being convinced that one has done something accurately, is in turn only possible against the background of a firmly established practice. Otherwise, it would not be possible to distinguish correct from incorrect procedure.

This may be a way of making sense of the claim that mathematics gives us certain knowledge, but one has of course not pointed to some quality of mathematical propositions or objects, only to how we relate to mathematics. Importantly, one has not given an explanation of why mathematics holds this place. In a related discussion, Wittgenstein emphasises that he has 'not said *why* mathematicians do not quarrel, but only *that* they do not'.[58]

[58] PI, part II, xi, p. 192.

In closing this chapter, I shall introduce a theme that is found Wittgenstein's philosophy of mathematics and which can provide an interesting shift in perspective on mathematics. This is the comparison between mathematical propositions and rules, and I will return to it in several discussions below.

The simple rules of arithmetic are learnt and used in everyday applications, but also to calculate more complicated things. In this respect, they are properly called rules, since they guide our activities. This, in turn, means that knowing them is more closely related to using them correctly than to holding proper beliefs. Wittgenstein emphasises this analogy with rules, for instance when he remarks: 'Mathematics and logic are part of the *apparatus* of language, not part of the application of language.'[59] As Diamond has pointed out, there are connections also with his remarks about mathematical propositions as paradigms: that things proved in mathematics 'are put in the archives'. This expression derives from his comparison of rules and propositions proved to the standard metre rod, which is kept in an archive.[60] Simon Friederich argues for an understanding of mathematics as being normative by pointing to the implicit definitions of the kind of axiom system favoured by Hilbert. That an axiom system is taken to implicitly define the concepts of a theory can also be seen as a laying down of the norms that govern their use.[61]

Stuart Shanker takes this theme as a key to understanding mathematical certainty. Indeed, Wittgenstein writes: '*What* is unshakably certain about what is proved? To accept a proposition as unshakably certain – I want to say – means to use it as a grammatical rule: this removes uncertainty from it.'[62] Shanker's understanding of Wittgenstein is that the latter claims that mathematical propositions are *rules of syntax*, not merely that they can be fruitfully compared to rules. Being rules of syntax, Shanker remarks, means that they do not express knowledge; they are not empirical propositions. Since doubt, according to Shanker, is associated with empirical propositions, doubt is logically excluded from the mathematical domain.[63] While I am hesitant to Shanker's equating mathematical propositions and rules of syntax, I do think that the difference between

[59]Ludwig Wittgenstein. *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*. Ed. by Cora Diamond. Ithaca NY: Cornell University Press, 1976 (henceforth cited as LFM), p. 250.

[60]Cora Diamond. 'How Long Is the Standard Meter in Paris?' In: *Wittgenstein in America*. Ed. by Timothy McCarthy and Sean C. Stidd. Oxford: Clarendon Press, 2001. Cf. also the Diamond quote on p. 13.

[61]Simon Friederich. 'Motivating Wittgenstein's Perspective on Mathematical Sentences as Norms'. In: *Philosophia Mathematica* 19 (2011), pp. 1–19.

[62]Ludwig Wittgenstein. *Remarks on the Foundations of Mathematics*. Ed. by G. E. M. Ansombe, Rush Rhees, and G. H. von Wright. 3rd ed. Oxford: Blackwell, 1978 (henceforth cited as RFM), III § 39.

[63]Stuart G. Shanker. *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*. New York: State Universtity of New York Press, 1987, p. 71.

mathematical propositions and empirical ones is important for the understanding of mathematical certainty. As Shanker writes, 'the certainty which characterises mathematical truth is *categorically* as opposed to *quantitatively* different from that which applies to empirical knowledge, and mathematics is not the "most certain" of the sciences but rather, certain in a completely different manner.'[64] The certainty of mathematics is not of the same kind as the certainty in empirical disciplines. And the above discussion has been an attempt to highlight preliminarily some important features of this certainty.

Lars Hertzberg discusses the uncertainty that one sometimes feels when trying to figure out what another person is feeling or thinking. This uncertainty is often invoked as an argument in favour of a scepticism about other minds. As Hertzberg shows, however, this occasional uncertainty and the occasional mistake when judging other people's emotions do not warrant scepticism. They do not undermine our talk of emotions although they are part of it.

> Here we see what room there is for the notion that uncertainty might belong to the character of the language game. The uncertainty does not reside in a relation between the language game and something external to it; it is not, as it were, a comparative notion at all. It consists, rather, in the manner in which discussions about the right and wrong application of words are carried on, in the forms that disagreement and criticism may take.[65]

One could compare this uncertainty with the certainty of mathematics. In this thesis, I will present a picture of mathematics where certainty is not 'a comparative notion', and where certainty 'does not reside in a relation between the language game and something external to it'. In the end I hope I can say: 'Here we see what room there is for the notion that *certainty* might belong to the character of the language game.'

[64]Ibid., p. 287.

[65]Lars Hertzberg. '"The Kind of Certainty is the Kind of Language Game"'. In: *Wittgenstein: Attention to Particulars. Essays in Honour of Rush Rhees (1905–89)*. Ed. by Dewi Z. Phillips and Peter Winch. London: Macmillan, 1989, p. 101.

3. Knowledge

> Yet anybody who has the least acquaintance with geometry will not deny that such a conception of the science [that it compels the soul to turn her gaze towards that place, where is the full perfection of being] is in flat contradiction to the ordinary language of geometricians. … They have in view practice only, and are always speaking in a narrow and ridiculous manner, of squaring and extending and applying and the like … whereas knowledge is the real object of the whole science.
>
> (Plato, *The Republic*)[1]

> Of course, in one sense mathematics is a branch of knowledge, – but it is also an *activity*.
>
> (Ludwig Wittgenstein, *Philosophical Investigations*)[2]

In the previous chapter, the certainty of mathematics was portrayed as belonging to the practice of mathematics rather than being a feature of mathematical objects or propositions. This portrayal is not to be understood as an explanation of why mathematics is certain, but rather as an alternative outlook on the issue. The question '*Why* is mathematical knowledge certain?' was rejected in favour of an inquiry into the meaning of the notion 'mathematical certainty'. However, the phrase 'mathematical knowledge' was not subjected to scrutiny. That is the aim of this chapter.

What kind of knowledge does mathematics give us? It seems that one answer announces itself readily: mathematics gives us knowledge of numbers, functions, sets, etc. Yehuda Rav even labels this the *standard view*: 'Indeed the *standard view* in philosophical writings seems to be that mathematical knowledge resides in a body of theorems (propositions, sentences), whereas the function of proofs is to derive theorems from first principles, true axioms, and thus confer truth on the theorems.'[3] According to this view, this knowledge is expressed in theorems and other mathematical propositions and to have such knowledge is to be able to form true propositions. A contemporary expression of this view is found in Michael D. Resnik who writes: 'Taken literally and seriously, mathematics affirms truths about numbers, functions, sets, spaces and other entities, which are as real as rocks and yet inhabit neither space-time nor our minds.'[4] A neat picture

[1] Plato, *The Republic*, p. 527a.

[2] PI, xi, p. 193.

[3] Yehuda Rav. 'Why Do We Prove Theorems?' In: *Philosophia Mathematica* 7 (1999), pp. 5–41, p. 15.

[4] Michael D. Resnik. 'Proof as a Source of Truth'. In: *Proof and Knowledge in Mathematics*. Ed.

of mathematics and mathematical knowledge emerges: the discipline appears as a collection of true propositions about numbers, functions, sets, algebras, etc. I will refer to this as the 'body of truths conception'. Interestingly, these truths may be described in different ways: as being a priori, necessary, eternal, about mathematical objects, etc. Accordingly, a wide range of philosophical positions in the philosophy of mathematics seem to be reconcilable with this picture. Perhaps one could go so far as to say that it has given rise to a large part of the philosophy of mathematics from the *Grundlagenstreit* onward.

If one approaches this question by considering instead what a person who is knowledgeable in mathematics knows, the above answer will probably still be a good description. However, it will not be the whole truth. In addition, one will find a host of other descriptions that fit the picture. It will also be correct to say that she or he knows a set of calculating techniques and proof strategies, how to apply certain techniques to solve practical problems, how to solve exercises, how to formulate exercises, and how to judge what is beautiful mathematics. He or she will also have some idea about what the major problems in contemporary mathematics are and which the important books and journals are.

In philosophy, concise and general answers or theories are often favoured and this preference may result in a temptation to view the items on this list as *mere practical consequences* of the knowledge of numbers, functions, etc. The strong normative flavour of Resnik's choice of words 'literally and seriously' may serve as an example of this attitude. It is implied that other approaches to mathematical knowledge will be dismissed as not being *serious* about mathematics.

The point of discussing the body of truths picture is that I see it as an unarticulated assumption in much philosophy of mathematics. It need not be problematic – after all, the impression that university level textbooks in mathematics gives is one of a body of truths. However, if it guides one's thinking on the notion of certainty and of knowledge, it is potentially misleading. If knowledge is manifested in propositions about abstract objects, answers to the question about the nature of the peculiar certainty in mathematics will likely focus on such things as the nature of its propositions (e.g. necessary propositions), on the nature of its objects (objects that allow for infallible knowledge), on our relation to these objects (postulating a special faculty like intuition), and so on. Indeed, this is how much of contemporary philosophy of mathematics is done – one approaches the problem through these openings, abstractly so to speak, while forgetting about everyday aspects of learning mathematics, about solving mathematical problems and reaching results.

The present chapter will, firstly, discuss this picture of mathematical knowledge: its historical roots, and how it has come to shape the contemporary dis-

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by Michael Detlefsen. London: Routledge, 1992, p. 7.

cussion. Secondly, I will try to nuance the picture of knowledge in mathematics by criticising two central features of the contemporary discussion: (1) the idea that truth in mathematics is established through reference to mathematical entities, and (2) the attempts to achieve a philosophical aim in the philosophy of mathematics through mathematical means. In the end, there emerges a picture of mathematics that will be more in line with the notion of certainty that the ended the previous chapter.

3.1 From the Science of Quantity to a Body of Truths

I shall begin by sketching a historical background to the body of truths conception. The point of this sketch is to highlight some developments in mathematics and in philosophy that make this picture a natural outlook. This will also serve to indicate in what way philosophy of mathematics tends to look upon mathematics as a body of truths.

Mathematics was labelled 'the science of quantity' by Aristotle, and this description seems to have been part of the discipline's self-image until, roughly, the nineteenth century. Moritz Epple summarises: 'This science was understood to consist of the geometric and algebraic study of numbers and continuous magnitudes such as lengths and weights or their "abstract" counterparts'.[5] Another noteworthy feature of mathematics' self-understanding during the centuries before the nineteenth is that it was seen as part of the general project of understanding the physical world. During the nineteenth century, several lines of development necessitated a re-evaluation of these two features. It seems natural that mathematics of earlier centuries, understood as the science of quantity, would be regarded as a body of truths. That the axiomatic structure of Euclidean geometry was considered as an ideal for the discipline (and for science in general) probably added to this impression. Be that as it may, what is important for the present chapter is that while mathematics underwent major changes, the image of the discipline as a body of truths did not. Only, it was not a body of truths about *quantities* anymore, and this new absence of a natural subject matter must only have spurred philosophical bewilderment.[6] Russell, writing in his *Introduction to Mathematical Philosophy* some decades after these changes, expresses this philosophical unclarity clearly:

> It used to be said that mathematics is the science of 'quantity.' 'Quantity' is a vague word, but for the sake of argument we may replace it by the word 'num-

[5] Moritz Epple. 'The End of the Science of Quantity: Foundations of Analysis, 1860–1910'. In: *A History of Analysis*. Ed. by Hans Niels Jahnke. Providence RI: American Mathematical Society and London Mathematical Society, 2003, p. 291.

[6] It may be worth mentioning that describing mathematics as a body of truths is potentially misleading even when one is considering mathematics of the time before the label 'science of quantity' fell into disrepute.

> ber.' The statement that mathematics is the science of number would be untrue in two different ways. On the one hand, there are recognised branches of mathematics which have nothing to do with number … On the other hand … it has become possible to generalise much that used to be proved only in connection with numbers. … It is a principle in all formal reasoning, to generalise to the utmost, since we thereby secure that a given process of deduction shall have more widely applicable results …
>
> We are thus brought face to face with the question: What is this subject, which may be called indifferently either mathematics or logic?[7]

I will review some of these changes and the way they affected the conception of mathematics.

Accounts of the changes that took place in the nineteenth century commonly mention the development of non-Euclidean geometry by Carl Friedrich Gauss, János Bolyai, and Nicholai Lobachevsky. The fact that alternative geometries could be formulated challenged the idea that mathematics dealt with the geometric features of physical space. If there were different geometries that seemed internally consistent but contradicted each other, what were the ones *not* describing physical space about? This contributed to the separation of mathematics from the natural sciences, as at least geometry was concerned with more than just physical space. A second prominent theme is the suspicion against geometrical intuition that led mathematicians such as Bernhard Bolzano, Augustin Louis Cauchy, and Karl Weierstrass to develop definitions of, for example, *limit* and *continuity*. These definitions enabled a symbolic treatment of analysis which previously had used such notions as *infinitesimals* and relied on a visual intuition of the functions studied. A third theme is the change in the conception of the real numbers sometimes referred to as the *arithmetisation of analysis*.[8]

The new definitions are often described as arising out of a need for a greater degree of rigour. The focus on rigour would be interesting to investigate further, but would lead too far astray. Jesper Lützen comments that one could also view the work on the definitions as a natural continuation of the developments that had taken place earlier.[9] Mathematics evolved and the hitherto established techniques did not give any clear-cut answers to the questions that it became possible to ask, and, therefore, one had to extend the conceptual toolbox.

One example of these technical developments is Bolzano's proof of the intermediate value theorem of 1817. John Stillwell emphasises this proof as an im-

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7 Bertrand Russell. *Introduction to Mathematical Philosophy*. London: George Allen & Unwin, 1919, pp. 195–96.

8 Cf. Giaquinto, *The Search for Certainty*; John Stillwell. 'Logic and the Philosophy of Mathematics in the Nineteenth Century'. In: *Routledge History of Philosophy*. Vol. 7: *The Nineteenth Centuty*. Ed. by C. L. Ten. London: Routledge, 1994.

9 Jesper Lützen. 'The Foundation of Analysis in the 19th Century'. In: *A History of Analysis*. Ed. by Hans Niels Jahnke. Providence RI: American Mathematical Society and London Mathematical Society, 2003, p. 155.

portant reason for the growing interest in the nature of the real numbers. He remarks that, in his proof, Bolzano made use of the *least upper bound* property.[10] Bolzano's reliance on this assumption made mathematicians aware of the fact that there was more to the real numbers than was obvious in the notion of continuous quantity, which, in turn, relied upon the geometric visual imagery of a line segment. Was the least upper bound property something that needed a proof? Geometrical intuition tells one that the least upper bound property is self-evident. Lützen, for his part, emphasises that *Fourier series* spurred such investigations. He also mentions that several of the mathematicians involved in the attempts to give new definitions of the real numbers, such as Cauchy, Richard Dedekind, Charles Meray, and Weierstrass felt that teaching the foundations of analysis was awkward when a technically satisfactory definition of the real numbers was lacking.[11]

The development of mathematics thus forced mathematicians to inquire into the nature of the real numbers, for technical as well as for philosophical reasons. Work on the real numbers further estranged mathematics from the science of quantity view. Epple distinguishes three different kinds of attitude towards the clarificatory work on the real numbers: some felt that the notion of quantity had to be retained as a basis for the concept real number (e.g. Hermann Hankel), while others tried to define the real numbers in terms of natural numbers and rational numbers (e.g. Dedekind, Georg Cantor, Weierstrass, in general the arithmetisation of analysis). The third attitude, which Epple labels 'formalistic', lets the properties of the real numbers emerge implicitly from a collection of axioms. It is associated with Johannes Thomae and Hilbert.[12] This third stance avoids the problem by not taking a stand on the issue of the nature of the real numbers, and it has become the standard way of introducing analysis in introductory courses. This meant that the meaning of the notion of real number changed in a way that was foreign to the view of mathematics as a science of quantity.

Through his work on the real numbers and functions in analysis, Georg Cantor was led to study sets of points or sets of real numbers. As he developed set theory, mathematics had yet another kind of objects that did not suit the science of quantity view.

In consequence, mathematics could no longer be thought of as the science of quantity. In general, the nineteenth century was a period of growing self-

[10] Stillwell, 'Logic and the Philosophy of Mathematics in the Nineteenth Century', p. 255. A set of real numbers that has an upper bound also has a least upper bound. In modern courses in analysis, this is sometimes taken as an axiom of the real numbers and sometimes proved as a theorem if it is replaced by an axiom expressing the completeness of the real numbers in some other way, e.g. through postulating the convergence of bounded sequences. Cf. Colin W. Clark. *Elementary Mathematical Analysis*. 2nd ed. Pacific Grove CA: Brooks/Cole, 1982.

[11] Lützen, 'The Foundation of Analysis in the 19th Century', p. 155.

[12] Epple, 'The End of the Science of Quantity', pp. 301, 314.

awareness among the sciences. It became important to be able to circumscribe the objects and methods of investigation of one's discipline. For mathematics, in contrast, this coincides with a greater unclarity precisely with regard to these matters. What are the objects of investigation for mathematics? This general unclarity with regard to the subject matter is reflected in other and more general investigations into the nature of number too. Dedekind discussed the question of the nature of the *natural* numbers in his *Was sind und was sollen die Zahlen?*, and, in *Grundlagen der Arithmetik*, Gottlob Frege expressed his worry: 'Yet, is it not a scandal that our science should be so unclear about the first and foremost among its objects, and one which is apparently so simple?'[13] In the 1930s, Bernays comments on the situation:

> The problematic, the difficulties, and the differences of opinion begin rather at the point where one inquires not simply about the mathematical facts, but rather about the grounds of knowledge and the delimitation of mathematics. These questions of a philosophical nature have received a certain urgency since the transformation of the methodological approach to mathematics experienced at the end of the nineteenth century.[14]

Towards the end of the nineteenth century, there emerged several attempts at axiomatising different parts of mathematics. Dedekind and Giuseppe Peano presented axiomatic systems for arithmetic, Hilbert for Euclidean geometry and analysis. In order to carry out his logicist programme, Frege provided an axiom system for logic. The systems of Dedekind, Peano, and Hilbert were of the formalistic kind mentioned above, in that they did not try to define beforehand the objects that the systems treated, but rather let the properties of the objects be given through the axioms, implicitly. As this became something of a standard view of mathematical theories, the worry about the objects of study could be forgotten for a while. Mathematics dealt with whatever objects that satisfied the axioms. Thomae expresses this clearly: 'The formal standpoint relieves us of all metaphysical difficulties, this is the benefit it offers to us.'[15] Increasing attention was given the systems as such, however. An expression of this is Hilbert's attempts to prove the consistency of axiomatic systems, and it also enabled a view like the one expressed by Curry: 'mathematics is the science of formal systems.'[16]

When viewing mathematics thus changed with a philosophical eye, it will, even more than before the changes described above, appear as a structured system of propositions, as a body of truths. This new self-image also appeals to

[13] Gottlob Frege. *The Foundations of Arithmetic: A Logico-Mathematical Enquiry into the Concept of Number*. 2nd ed. Oxford: Blackwell, 1953, p. ii.

[14] Paul Bernays. 'The Philosophy of Mathematics and Hilbert's Proof Theory'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998, p. 234.

[15] Thomae, quoted in: Epple, 'The End of the Science of Quantity', p. 301.

[16] Curry, 'Remarks on the Definition and Nature of Mathematics', p. 204.

philosophically minded mathematicians who want to retain the picture of mathematics as a science among others with its own objects and methods of investigation. In this way, the body of truths picture allows for a continuity in the way one regards mathematics, although so much about mathematics changed during the nineteenth century.[17] In addition, the foundational work of logicism (in particular Russell's and Whitehead's *Principia Mathematica*) and axiomatic set theory made it clear that mathematics could be reduced to an axiomatic system that is based on relatively few concepts. These discoveries also strengthened the view of the totality of mathematics as a unified structure.

It seems that the problems about the nature of the objects of investigation are best described as having been postponed during the time of the foundational programmes. These questions again attracted attention towards the middle of the twentieth century. Perhaps it is fair to say that the newly discovered paradoxes took most of the attention and that there was an anticipation to see what would come out of the foundational programmes that channelled efforts into technical work on the programmes. Had this reduction fulfilled all the philosophical demands that were advanced at the turn of the twentieth century, it is possible that much of the philosophical problems about the foundations of mathematics would have ceased to worry philosophers. However, the reduction of mathematics to logic, although technically possible, had to make use of axioms that could not be regarded as self-evidently true propositions of logic. Moreover, as Gödel proved, there were propositions that were neither provable, nor disprovable in the systems employed by logicists and formalists. Finally, it was not possible to prove that the axiom systems employed in the reduction were consistent as Hilbert had requested.

The discussions of the decades following Gödel's results were, in part, occupied with sorting out the heritage from the foundational programmes. Two issues that are central to the dynamic of the body of truths picture are worth highlighting: (1) the difference between the truth and the provability of a proposition and (2) the successful reduction of mathematics to set theory.

Before the formalist programme in the foundations of mathematics, there were no reason to regard the concepts 'provable' and 'true' to be different. By focusing on provability, the formalists were able to continue the formalistic approach to the mathematical objects discussed above. However, the example supplied by Gödel, and also by Alfred Tarski, opened a gap between the concepts. If

[17] The emphasis with which the term 'classical mathematics' is often uttered is telling. What is labelled 'classical' is, as Stenlund remarks, the mathematics that resulted from the changes that took place in the nineteenth century, and it is thus not classical in the sense that it is old. The choice of adjective, however, signals a desire to connect with mathematics of earlier centuries. Sören Stenlund. 'Hilbert and the Problem of Clarifying the Infinite'. In: *Logicism, Intuitionism and Formalism: What Has Become of Them?* Ed. by Sten Lindström et al. Dordrecht: Springer, 2009, pp. 495–96.

there were propositions that could neither be proved nor disproved in a formal system, and the principle of excluded middle entails that either the proposition or its negation is true, then the two concepts do not coincide. Tarski mentions this as an explicit reason for his devising an account of the concept 'truth'.[18]

The new branch of model theory that emerged from Tarski's investigations added a new flavour to the body of truths picture that has proved to be very seductive. Mathematics appears as a collection of formal axiomatic systems whose propositions are true in various models. The theories are about these models. Even if the formalistic view of mathematics in the beginning had the benefit of eliminating abstract objects, it actually invited this problem in a new form by stressing that the theories could be true about anything satisfying the axioms.

The reduction of mathematics to set theory also raises philosophical problems because the ontological status of sets was (and still is) unclear. Adding to this puzzle, Gödel speaks of a special faculty, an *intuition*, by which we apprehend sets (see the quote on p. 15). Thus, he puts forward a possibility of gaining knowledge in mathematics which is fundamentally different from the one implicit in the formalist and logicist programmes, which emphasised proofs.

From these two issues, there emerges a picture of mathematics as a body (or a collection of bodies) of structured propositions, and they are true about certain abstract things. This is a suggestive picture of mathematics, and it appears almost self-evident from a contemporary perspective. Nevertheless, it tends to leave other aspects of mathematics in the shadow. Questions of practice, skill, and judgment are, if not forgotten, at least pushed aside to the category of non-essential side-effects. Moreover, as philosophy tends to disregard complexities arising out of the commonplace – perhaps claiming that such particularities are not relevant for the general understanding that philosophers aspire to – there is a strong incentive to continue this neglect.

I will end this section by giving two quotes from the 1950's that express the atmosphere that I am trying to describe. In the article 'Remarks on the Definition and Nature of Mathematics' from 1954, Curry makes the following remark: '[Mathematics] is a body of propositions dealing with a certain subject matter; and these propositions are true insofar as they correspond with the facts.'[19] Morris Kline gives the following characterisation in his *Mathematics in Western Culture*: 'Mathematics is a body of knowledge. But it contains no truths.'[20] The addition that it contains no truths is a consequence of the view that there are no mathematical objects, and hence nothing that the propositions of mathematics

[18] Alfred Tarski. 'The Concept of Truth in Formalized Languages'. In: *Logic, Semantics, Metamathematics. Papers from 1923 to 1938*. Trans. by J. H. Woodger. Oxford: Clarendon Press, 1956, p. 186.

[19] Curry, 'Remarks on the Definition and Nature of Mathematics', p. 202.

[20] Morris Kline. *Mathematics in Western Culture*. Oxford: Oxford University Press, 1953, p. 9.

could be true or false *about*.

In the body of truths picture as it has been sketched above, there are two strands, one stressing the view of mathematics as a *structure* of propositions and the other stressing mathematics as a collection of *truths* about abstract objects. There is certainly something correct about both of these views; they draw our attention to features that are central to mathematics. The body of truths conception becomes troublesome, however, when it is allowed to guide one's thinking as one is trying to come to terms with philosophical problems about mathematics and one thereby forgets other features that are equally central. It is due to the presence of both of these two strands that realism as well as anti-realism are reconcilable with the body of truths picture. Arguably, the debate between realism and anti-realism can also be seen as *consequence* of this picture.

3.2 Benacerraf and the Contemporary Discussion

When it comes to shaping the contemporary discussion, two philosophers are worth mentioning. In 1951, Quine's 'Two Dogmas of Empiricism'[21] questioned the division of truths into analytic and synthetic, and, as a consequence, the logical empiricist view that mathematical propositions were analytically true.[22] In 1965, Paul Benacerraf's 'What Numbers Could Not Be' added to the philosophical worry concerning the reduction of mathematics to set theory. He argued that since there are several different ways of defining the natural numbers as sets, numbers must be something other than sets.[23] Thus, Quine and Benacerraf both contributed to the renewed interest in the problems of the nature of mathematical knowledge and mathematical entities.

Even more important, however, is Benacerraf's 'Mathematical Truth', published in 1973. When trying to understand the reason for its impact on subsequent philosophy of mathematics, the above mentioned shift of focus from foundational to ontological and epistemological issues provides an important clue. Benacerraf's article diagnoses a worry that runs through much of the discussion in the decades following Gödel's incompleteness proofs and the relinquishment of the logical empiricist view of mathematics following Quine's writings.

[21] Willard Van Orman Quine. 'Two Dogmas of Empiricism'. In: *Philosophical Review* 60 (1951), pp. 20–43.

[22] A clear expression of this view is found in Carl G. Hempel: '[T]he validity of mathematics rests neither on its alleged self-evidential character nor on any empirical basis, but derives from the stipulations which determine the meaning of the mathematical concepts, and that the propositions of mathematics are therefore essentially "true by definition".' Carl G. Hempel. 'On the Nature of Mathematical Truth'. In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983, p. 380.

[23] Paul Benacerraf. 'What Numbers Could Not Be'. In: *Philosophical Review* 74 (1965), pp. 47–73.

Benacerraf argues that a satisfactory account of 'truth' in mathematics has to meet two conditions: (1) It has to be consistent with the possibility of *gaining knowledge* of such truths, and (2) it has to be an account of *truth* and not something else. Most accounts give priority to one of these conditions at the expense of the other, he writes. The accounts that purport to explain how we can have knowledge in mathematics usually equate truth with *derivability in an axiomatic system*. On such an account, it is easy to see how one comes to know the truths of mathematics – we can prove many things and thus we have knowledge about them. Benacerraf considers it questionable, however, that we actually want equate derivability in a system and truth.[24]

He thinks that 'any satisfactory account of truth, reference, meaning, and knowledge must embrace them all and must be adequate for all the propositions to which these concepts apply.'[25] Benacerraf is thus requesting an account of truth that at the same time explains what reference, meaning, and knowledge is, not only in mathematics but in other contexts as well. He admits that there is no such general account of truth for all of language. Still, he considers it a minimum requirement that any account should explain truth in mathematics in a way that is analogous to truth in other regions of language. The account he prefers is Tarski's. This is in essence a 'referential semantics' – according to Benacerraf – in that it explains the truth of a proposition as a correspondence between what is expressed by it and the domain of discourse about which it states something.[26]

Whether the above description is a fair rendering of Tarski's work on the concept of truth is a question that I will return to in section 3.5. Still, in 'The Concept of Truth in Formalized Languages', Tarski gives truth conditions for any formula of the (first order) calculus of classes. He shows that this is possible for predicate logic so long as the predicates are of finite order (the number of variables of the predicates, however, may be infinitely many). Since it is commonly held that it is possible to express the logical structure of propositions of various kinds of discourse in the language of predicate logic, there is a certain plausibility to the idea that Tarski's account could provide truth conditions for all kinds of propositions.[27] This seems to be Benacerraf's reason for preferring

[24] Benacerraf, 'Mathematical Truth', pp. 661, 666–67.

[25] Ibid., p. 662.

[26] Ibid., pp. 661, 667. The thought that a referential semantics puts mathematics on a par with the sciences with regard to truth is clearly expressed by Benacerraf and Putnam in their introduction to the collection *Philosophy of mathematics: Selected readings*: 'A referential semantics exhibits the propositions of physics as being "about" rigid bodies, fields, electrons; those of number theory as about numbers; set theory about sets. They are true if and only if the relevant entities have the properties ascribed to them' (p. 22).

[27] Tarski, however, rejects this. See p. 164 of 'The Concept of Truth in Formalized Languages'. The impossibility of defining 'truth' for ordinary language (due to the liar-paradox) is the reason

an account of mathematical truth in terms of reference and, furthermore, the reason that he hopes for a unified semantics for all of language. That, however, may not be a reasonable thing to wish for, but I shall not argue the issue here.

Still, an account that proceeds from a referential semantics finds difficulties when it comes to explaining how it is possible to have knowledge about mathematical objects. We do not, after all, have any causal interaction with such abstract objects that mathematics deals with. For Benacerraf, having knowledge is a matter of the objects of knowledge somehow interacting with our sensory system.[28] This problematic is often referred to as 'Benacerraf's dilemma': accounts that give priority to the possibility of having knowledge fail with regard to 'truth', while accounts that live up to Benacerraf's requirements on accounts of truth fail to explain how it is possible to know such truths.[29]

The impact of Benacerraf's 1973 article lies, not so much in that it convinced philosophers to adopt some one particular position, but rather in how it has shaped the subsequent discussion. After Benacerraf, many writers on the subject have felt it necessary to address his dilemma by arguing for one of its horns. The view is then defended in such a manner as to overcome the dilemma.

This has put the focus of contemporary philosophy of mathematics on the ontological status of mathematical objects. Since Benacerraf sides with realist views (or standard as he calls them), the problem of gaining access to mathematical objects appears as the main problem. In contemporary philosophy of mathematics we find positions such as Platonism, naturalism, structuralism, and nominalism.

These are often identified and distinguished according to the answer they give to the question about the nature of mathematical objects. Their respective approaches to such problems have many similarities, and, taken together, they constitute the mainstream in contemporary philosophy of mathematics.

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he restricts his attention to the first order calculus of classes.

[28]'I favor a causal account of knowledge on which for *X* to know that *S* is true requires some causal relation to obtain between *X* and the referents of the names, predicates, and quantifiers of *S*.' Ibid., p. 671. Interestingly, the problem Benacerraf draws attention to does not depend on this particular account of knowledge. Even if one has another idea of how one gains knowledge about mathematical objects than the causal account favoured by Benacerraf, it may still be a problem to account for the possibility of *referring* to mathematical objects. See Leon Horsten. 'Philosophy of Mathematics'. In: *The Stanford Encyclopedia of Philosophy*. Ed. by Edward N. Zalta. Spring 2015. URL: http://plato.stanford.edu/archives/spr2015/entries/philosophy-mathematics/.

[29]Benacerraf labels the kinds of account 'standard' and 'combinatorial' views, respectively. An example of a standard view would be Platonism or realism, whereas some kind of anti-realism (formalism, constructivism or conventionalism) could pass for a combinatorial position. A peculiarity of this labelling is that Benacerraf gives Quine as an example of a philosopher who holds a combinatorial view because of his naturalist position on mathematics. However, Quine's *indispensability argument* (i.e. that mathematical objects exist because they are indispensable to science and science offers the best explanation of the world) is considered one of the strongest arguments in favour of realism in mathematics.

In Resnik's article 'Mathematics as a Science of Patterns: Ontology and Reference', we find him introducing his argument for structuralism with the following passage:

> I seek an account of mathematics in which the logical forms of mathematical statements are taken at face value and their semantics is standardly referential, say, in the manner of Tarski. This together with fairly uncontested assumptions entails that mathematics is a science of abstract entities, that is, immaterial and nonmental things which do not exist in space and time. So I am a platonist.[30]

Immediately following this passage, Resnik reviews the problem that Benacerraf's dilemma poses for a realist (and a fortiori for a Platonist).[31] Then he goes on to explain how his structuralism avoids the problems.

Stewart Shapiro, too, lets Benacerraf's two problems influence the presentation of his version of structuralism. The arguments that address the problems have a prominent position in his *Philosophy of Mathematics: Structure and Ontology*, and Shapiro asserts that his version of structuralism is a realist position.[32]

Mark Balaguer claims that Bencerraf's dilemma presents the most serious argument against Platonism. His argument for what he calls 'full-blooded platonism' (i.e. the view that all mathematical objects that are *possible* actually exist) in *Platonism and Anti-Platonism in Mathematics* is constructed around countering the problem of gaining knowledge of causally inert abstract objects. He reviews earlier attempts to meet this challenge and concludes that they are not successful, while full-blooded platonism is:

> [K]nowledge of the consistency of a mathematical theory – or any *other* kind of theory, for that matter – does not require any sort of contact with, or access to, the objects that the theory is about. Thus, the Benacerrafian objection has been answered: we can acquire knowledge of abstract mathematical objects *without* the aid of any sort of contact with such objects.[33]

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[30] Michael D. Resnik. 'Mathematics as a Science of Patterns: Ontology and Reference'. In: *Noûs* 15 (1981), pp. 529–50, p. 529.

[31] The position *structuralism* is also conceived as an attempted solution to the problem put forth in Benacerraf's 1965 paper. A conclusion reached in this paper is that one cannot determine the objects of mathematics any closer than up to isomorphism. The example Benacerraf gives is the definition of the natural numbers in terms of sets using either Ernst Zermelo's or John von Neumann's definition. Since the numbers that the definitions give rise to are isomorphic, there is no way to tell which definition correctly captures the natural numbers (if 'correct' is even appropriate in the context).

[32] Stewart Shapiro. *Philosophy of Mathematics: Structure and Ontology*. New York: Oxford University Press, 1997. Shapiro calls his position *ante rem* structuralism. In structuralism, mathematical objects are identified with positions in a structure, and the features that mathematical objects have are due to the relations that they have to other positions in the structure. A question that Shapiro discusses is whether structures exist as such even if no concrete objects instantiate the structure or if a structure can be said to exist only if there exists a collection of objects forming such a structure. The label '*ante rem*' signifies the view that structures exist before concrete instantiations.

[33] Mark Balaguer. *Platonism and Anti-Platonism in Mathematics*. New York: Oxford University

Mary Leng discusses so called *algebraic* views of mathematics and contrasts these with *assertory* views (the terms are borrowed from Geoffrey Hellman). The label 'algebraic' applies to different kinds of positions in the debate about the ontology of mathematics; it signifies the view that the objects of mathematics and their qualities emerge implicitly from the axiom systems of the mathematical theories. This is a continuation of the formalistic approach to the natural numbers mentioned above in connection with Hilbert and Thomae. Leng finds examples of algebraic views within all the major positions: structuralism (e.g. Shapiro's *ante rem* structuralism), Platonism (e.g. Balaguer's full-blooded version), and nominalism (e.g. Hartry Field's fictionalism), and she claims that the motivating factor behind these views is the epistemic horn of Benacerraf's dilemma.[34]

That Benacerraf's article gained so much attention is, I think, a consequence of the renewed interest in ontological and epistemological issues that followed upon Gödel's, Wittgenstein's, Quine's, and Benacerraf's own writings on the subject. Many felt that the dilemma spoke to them with such an urgency that it was allowed to shape a large part of the subsequent discussion. That the dilemma was regarded as such an important issue is, I would argue, a consequence of the fact that the body of truths picture runs as a chorus through so much of twentieth century philosophy of mathematics. In hindsight, it seems that Benacerraf only cemented its grip.

3.3 Two Perspectives on Mathematics

The perspective that concluded chapter 2 – that certainty is not to be viewed as something resulting from the properties of objects or propositions, but rather as a feature of the way we relate to mathematical propositions, rules, and methods – suggests a break with the picture of mathematics as a collection of true propositions. If the fact that learning mathematics also involves learning a technique, acquiring a certain proficiency is taken into consideration, the notion of mathematical knowledge will be seen to include such notions as *skill* and *ability*.

We tend to think of mathematics, on the one hand, as a body of truths and, on the other hand, as a technique, a doing. I suspect that the first alternative would be predominant among persons trained in mathematics or logic, while the other probably is more common among persons with no formal education in mathematics. Both of these associations pick out important features of mathematics.

Press, 1998, chapter 2, quote on pp. 48–49.

[34] Mary Leng. '"Algebraic" Approaches to Mathematics'. In: *New Waves in Philosophy of Mathematics*. Ed. by Otávio Bueno and Øystein Linnebo. Houndmills, Basingstoke: Palgrave Macmillan, 2009. Interestingly, many of the articles in the collection *New Waves in Philosophy of Mathematics* deal with the Benacerraf problem, and this shows that it is still considered an important issue.

The body of truths conception accords with the fact that this is how the majority of books in mathematics are written: propositions are stated briefly, followed by a, possibly elaborate, proof. In textbooks, these propositions are also arranged hierarchically starting with the most basic ones, advancing to more and more remote consequences of these as the text continues. This has been the standard since Euclid's *Elements* and is the language of the *science* mathematics.

Nevertheless, if this aspect of the discipline is allowed to dominate, the doing of mathematics appears to happen alongside this body of truths, as a constant relating or connecting to it. Calculating and inferring correctly becomes the successful adhering to these truths. There is a risk that one, as Wittgenstein expresses it, 'nourishes one's thinking with only one kind of example'.[35] Mathematics is constantly thought of as a body of truths, and 'a piece of mathematics' is exemplified, not by, say, the concrete struggle with a difficult proof, but by the theory of, say, Hilbert spaces.

When doing mathematics, on the other hand, there is no perceiving of mathematical truths but a constant assessing: 'Is this in accordance with this rule?', 'Is this a possible strategy?', 'Is this a correct application of this theorem in this case?', 'Is this analogous to that?', 'Can I do like this?', etc. One makes decisions to try certain techniques, to go along in a particular fashion, to accept certain results; one realises that one has made mistakes and tries to correct them, etc.[36]

An objection to this description could be that, while it aptly portrays the practice of proving and calculating, it is not part of mathematics proper but merely of the struggle to find the true propositions of mathematics. In analogy, it could be argued that the carpentry that produced the shelf is not part of the shelf; indeed, we can understand what a shelf is and make use of it without bothering about the toil behind its coming into being. Now, while it may be true that we can have full benefit of the shelf without knowing anything about carpentry, this is not true in the case of theorems. Understanding a mathematical theorem involves knowing the work behind it, most importantly the proof of it.

We find Wittgenstein and G. H. Hardy agreeing on this point (although they disagreed on many things in the philosophy of mathematics): 'If you want to know *what* is proved, look at the proof' writes Wittgenstein;[37] and Hardy, with an air of self-evidence, briefly remarks: 'in the theorems, of course, I include the proofs'.[38] (I shall return to these aspects of mathematical proof in chapter 5.) This understanding is furthered still if one knows what problems the theorem

[35] PI, § 593.

[36] These features of the practice of mathematics are illustrated by the dialogue in the first part of Lakatos's *Proofs and Refutations*.

[37] Ludwig Wittgenstein. *Philosophical Grammar*. Ed. by Rush Rhees. Oxford: Blackwell, 1974 (henceforth cited as PG), p. 369.

[38] G. H. Hardy. *A Mathematician's Apology*. Cambridge: Cambridge University Press, 1967, p. 113.

and the particular details of it have been devised to solve.[39] Not only the proof, but also the specific needs for a particular proof of a theorem show its place in the mathematical theory and to what use it can be put. It may be added that what possible future meaning and significance a theorem might have cannot be settled now.

A look at the difficulties involved in understanding what propositions in mathematics mean – in particular for the learner but also to some extent for mathematicians – may illustrate what I am after. A greater sensitivity to the historical development may also serve to cast doubt on the body of truths conception.

I shall consider a simple example like the equation '$x^2 + 1 = 0$'. Seen from the perspective of a pupil, it may be taken as an exercise. Possibly, it looks like other tasks the pupil has performed, so she assumes that she understands it and that she only needs to do some calculations to find its roots. If imaginary numbers have not yet been introduced, she will have to revise her understanding of the equation. Her teacher might have wanted to introduce the notion of equations without solutions by allowing the pupils themselves to draw the conclusion that some equations lack roots.

The students' broadening of the concept of equation from something that has a solution to something that may or may not have a solution has consequences for the truth of claims about equations. It would be unfair to say that the pupils' first belief, that equations have solutions, was false. Rather, they are widening the meaning of 'equation'. This widening of the concept can only be accomplished when the first stage – equations have one or several roots – is passed. One of the pupils may even object to the teacher's new notion of equation: 'That is not an equation because the two sides will never be equal.'

Later, the teacher introduces imaginary numbers and shows the pupils how this extension of the concept of number allows one to solve equations like '$x^2+1 = 0$'. Again, they have to revise old truths about equations and roots, and, again, I do not think it would do justice to the teaching of mathematics to say that the pupils were *wrong* when, prior to their learning imaginary numbers, they claimed that such equations lack a solution. The teacher may, of course, have been careful and advised them to say that the equation does not have any *real* roots.

A teacher may perhaps introduce a lesson about imaginary numbers by saying: 'What I taught you earlier about equations lacking a solution was wrong.' Perhaps he wants to catch the attention of the pupils in this way. However, he will not *correct* his earlier teaching – only introduce a new concept, a new way

[39] In *Proofs and Refutations*, app. 2.2, Lakatos discusses the introduction of certain definitions in nineteenth century mathematics. He shows that knowing the background of a definition, knowing what issues the phrasing of it is supposed to address, furthers the understanding it.

of calculating. This will have consequences for how they see their old knowledge about equations. Even after this lesson, however, as long as one limits the discussion to real numbers only, nothing will have changed.

This development of the pupils' knowledge of mathematics finds parallels in the history of the subject too, e.g. in the discovery or invention of imaginary numbers by Girolamo Cardano in the sixteenth century. He considers the possibility of 'dividing' 10 in two (e.g. 3 and 7) in such a way that these two numbers multiplied produce 40. If one was interested in finding a division, such that the product of the two numbers was a number less than 25, this would be a straight forward problem. By deliberately choosing a number which is greater than any possible product of two numbers whose sum is 10, a peculiar problem arises. The problem has a contemporary counterpart in finding the roots to the polynomial equation:

$$\begin{aligned}(10-x)x &= 40 \quad \text{or}\\ x^2-10x+40 &= 0.\end{aligned}$$

Although Cardano states that it is impossible, he solves the problem in a way analogous to the solution of a problem that is possible to solve.[40] He divides 10 into equal parts, i.e. 5 and 5, whose product is 25. From this product he subtracts the requested product, 40, leaving −15. The numbers into which 10 should be divided are obtained by subtracting, and adding, respectively, the square root of −15 from 5. In contemporary notation the roots are: $5-i\sqrt{15}$ and $5+i\sqrt{15}$. The product $(5-i\sqrt{15})(5+i\sqrt{15})$ is of course 40 since the imaginary parts even out.[41]

It would be absurd to say that Cardano's predecessors were simply wrong in supposing that certain problems did not have solutions. Even the word 'suppose' is inappropriate here: they did not suppose anything, they saw clearly that the problem mentioned by Cardano could not have any solutions. If one considers the graphical representation of the equation corresponding to the problem in a Cartesian coordinate system, this is also clear. A parable which is completely located above the x-axis, does not intersect it.

Cardano did not discover that earlier mathematicians had been wrong; he extended the concept *solution*, and he did so by introducing a new technique, not by discovering a new fact. The controversy that surrounded the introduction of complex numbers lasted, roughly, until the time of Gauß, that is, almost

[40] A problem that is possible to solve could be if the product sought is 21. If one divides 10 into 5 and 5 and subtracts 21 from the product of 5 and 5, i.e. from 25, one gets 4. The numbers into which 10 should be divided are $5-\sqrt{4}=3$ and $5+\sqrt{4}=7$. The number 10 should therefore be divided into 3 and 7.

[41] Girolamo Cardano. 'Cardan's Treatment of Imaginary Roots'. In: *A Source Book in Mathematics*. Ed. by David Eugene Smith. New York: McGraw-Hill, 1929, pp. 201–02.

three hundred years. This, I believe, is telling of the difficulties involved in determining the meaning and place of mathematical concepts and propositions in the surrounding theory.

It is easy to dismiss the example of the students learning about imaginary numbers as not being a relevant criticism of the body of truths picture. It is obvious that the development of the student's understanding may have to go through certain steps that, although not plain wrong, do not match the body of true propositions about polynomial equations. It may be argued that their understanding is progressing slowly, but steadily towards the body of truths. The historical development of the imaginary numbers, however, shows that the position of the mathematician making new discoveries can be similar to the one of the students in many respects. Cardano, too, found that a new way of solving polynomial equations was possible. This new technique put the old truths about equations into a new perspective. It introduced new truths and at the same time it altered old ones. I would not say it *falsified* old ones, and in this sense, I do not see the example as an argument in favour of fallibilism, i.e. the idea that mathematical truths may turn out to be false although proved. Lakatos takes the development of the theory of polyhedrons to involve a series of proofs and refutations – old proofs being refuted and followed by new proofs. His dialogue is a nice example of a conceptual development in the history of mathematics.[42] However, I do not see proofs being refuted, only concepts being changed and therefore requiring new proofs and techniques. The proofs supposedly refuted are still valid proofs if the concept of polyhedron that the earlier proof worked with is retained. It is another matter that a proof working with a certain concept of polyhedron – or number for that matter – may make it obvious that there is another way of viewing the concept, but it does not necessarily refute the old proof (unless of course it became obvious that some mistake had been made in the proof, but that is another matter).

One must also be aware of the false impressions created by reading a greater continuity into the historical development than it displays. Bell warns: 'Nothing is easier … than to fit a deceptively smooth curve to the discontinuities of mathematical invention.'[43] Afterwards, it is easy to get the impression that past mathematicians were dealing with the same concepts as we are and that they viewed them in a similar manner. The example of imaginary numbers is especially striking since these were probably invented out of a will to unify the treatment of polynomial equations. When considered from the perspective of real numbers, such an equation has anything from zero to n roots if its degree is n. Once imaginary numbers are introduced, one can state the fundamental

[42] Lakatos, *Proofs and Refutations*, ch. 1. In his dialogue the parallel situation of a student and a research mathematician is also implicitly shown.

[43] Bell, *The Development of Mathematics*, p. viii.

theorem of algebra: a (single-variable) polynomial equation of degree n has n roots, imaginary or real, some possibly appearing more than once. Thus, the concept of polynomial equation is radically changed as the imaginary numbers are introduced.

3.4 Studying a Mathematical Object

From the perspective of Platonism, the above discussion may appear absurd. Abstract entities existing independently of us do not admit of any changes in concepts (unless the change is a result of correcting a mistake in one's apprehension of them). Since Platonism and the body of truths picture go especially well together, the present section will deal with the issue of studying a mathematical object. If, as I have suggested in the previous section, the body of truths picture is a misleading view of mathematics, what does the study of mathematical objects amount to?

If one is studying an ancient coin, there are many kinds of question one can ask in relation to the coin. One kind is related to the physical aspects of it; for example, the measurements of the coin and the material it is made from may be of interest. Of another kind are questions that concern the coin's place in the monetary system. One could be interested in knowing whether people in general used this kind of coin or if it was a privilege of higher classes, what kinds of goods it could be used as payment for, etc. The answers to these questions do not depend on the answers to the first kind of questions, and one could certainly not find an answer to them by looking at the answers to the first ones. In mathematics, I would say, there is trade but no coins. The questions one asks about mathematical objects have more in common with the second category than with the first.

Similarly, if one wants to investigate the features of a piece of chess that are of relevance for the game, one is not interested in the physical aspects of the piece. To be sure, it is of importance for the game that, for instance, the pawns are all of identical shape and that they differ clearly from other pieces, but the shape does not determine that they can move only one step at a time. If chess was played by drawing or writing down the positions of the pieces, it would be a game without physical pieces and the same questions about the features of the pieces could still be asked.

As is shown by these two examples, one can distinguish two senses of 'feature' or 'quality'. The first of these could be called 'physical features', whereas the second relates to the role something plays in a specific context. In some cases, as in a monetary system or chess, features can be attributed to the objects in question in both of these senses. With mathematical objects, however, only the second sense seems applicable: the only features that we discover about mathematical objects are of the kind that relates to the role that these objects have to other mathematical objects. In conclusion, it seems that if there were some kind of abstract mathematical objects existing independently of us, it is unclear what importance they would have. From this perspective they fall out of the picture as superfluous, and the ontological status of mathematical entities is seen to be a pseudoproblem.

Does this lead to the conclusion that mathematics is in some sense unstable? This fear of uncertainty is probably a reason for the postulation of abstract objects in the philosophy of mathematics.[44] Still, a piece of chess is thoroughly determined by the rules for its movement and its relations to the other pieces. Furthermore, there seems to be no problem discussing possible moves and outcomes in the game of chess. Yet, we do not feel the need to postulate abstract chess pieces. Why would this not be possible in mathematics too?

To gain knowledge of a function, class of functions, theorem, or theory, one must use it actively according to the established practice where it occurs – calculate, compare, deduce, conjecture, etc. To study it is to spend time with it in this way – there is no studying the function as such in isolation from this practice. To learn something about, for example, the *sine function*, one could not simply read a list of its features and thereby know it. If one is familiar with similar functions, the list may well make sense, but if one is not, such a list would hardly be of any use. Instead, one needs to see how it applies to right angled triangles and to the unit circle; one needs to study its values, its periodicity, and how these aspects of the sine function are related to each other. In practice, this is accomplished by reading examples and doing exercises, by gaining an ability to use the function in various situations.

That the function in question is *periodic*, for instance, means something only when one realises that the function values must recur at a certain interval of values of the argument, and that this is a consequence of how the function is defined. A graphical representation of the unit circle or of the function graph adds to the understanding of periodicity. Its meaning is also enriched if one tries to determine the maximum (or minimum) of this function, perhaps as part of an attempt to make use of it in application. As one realises that it has an in-

[44] Jody Azzouni discusses what he calls 'Morton's challenge': that mathematicians without the guidance of mathematical objects would lose their ability to discover new interesting results and to distinguish interesting results from uninteresting ones. This challenge was pointed out by Adam Morton as an answer to Azzouni's thought experiment about what would happen if mathematical objects ceased to exist at a certain point in history. Azzouni's guess was that nothing would change, but the gist of his paper is still that mathematics needs something stable, something that underlies mathematical practice to give it the stability needed for objectivity. His suggestion is that there are derivations in some algorithmic system that underlie the proofs that usually appear in mathematics. Jody Azzouni. 'The Derivation-Indicator View of Mathematical Practice'. In: *Philosophia Mathematica* 12 (2004), pp. 81–105, pp. 81–83. I will discuss this idea in chapter 4.

finite number of periodically recurring maxima, either one has to give up the idea of determining a maximum or to restrict oneself to a certain interval of the function's domain. This would lead to a better understanding the sine and other trigonometric functions.

The prominent role of solving exercises in mathematics education, from the first grades in schools to university level courses, is a clear expression of the fact that a central part of learning mathematics is acquiring an ability to use its techniques and concepts in the practice of mathematics. The fact that assessing mathematical knowledge is done with tests almost exclusively consisting of problems that the student solves also shows that to know mathematics is to be able to use it meaningfully. It is common to view exercises and examples as being merely illustrations of theorems. As such, they are considered to be of secondary importance. According to this view, proofs occupy a middle position – more important than examples and exercises but not as important as theorems. However, from the perspective I am advancing, proofs occupy the, perhaps, most central role. If one is speaking of textbooks, examples and exercises often have as important a role as proofs. If the theory is presented without giving proofs, the examples that accompany theoretical parts take over the role that proofs play: they tell the reader what the theorem means and how it is to be understood (as far as this is possible without giving the proofs). The major difference between proofs and examples is that many different examples could do the job, whereas there are limitations as to which proof will do. Rav captures this nicely:

> There is a way out of the foundational difficulty, and it consists of realising that *proofs rather than the statement-form of theorems are the bearers of mathematical knowledge*. Theorems are in a sense just tags, labels for proofs, summaries of information, headlines of news, editorial devices. The whole arsenal of mathematical methodologies, concepts, strategies and techniques for solving problems, the establishment of interconnections between theories, the systematisation of results – the entire mathematical know-how is embedded in proofs.[45]

This quote brings out a clear contrast between the body of truths picture and the perspective put forth here. Viewing mathematics as a corpus of truths and viewing mathematical knowledge as being about these truths downplays the role of proofs, whereas a perspective on knowledge that stresses the ability to use the techniques and concepts involved will emphasise proofs. (I will discuss this perspective of proofs in chapter 5.) In addition, Rav's remark contains a clue as to why the body of truths picture so easily announces itself as the proper way to understand mathematics. In order to facilitate remembering the know-how in mathematics it is structured into theorems and collections of theorems. At first sight, the theorems will appear to take the centre stage, whereas they, on a closer look, only serve to point towards the know-how involved.

[45] Rav, 'Why Do We Prove Theorems?', p. 20, emphasis in the original.

Herbert Breger highlights a change of style in the writing of textbooks, a change that strengthens the impression that propositions rather than techniques are important.

> The general expression is that the older books do not aim at a maximal formal elegance on a higher level of abstraction, but rather try to stimulate a certain know-how on a lower level of abstraction in the mind of the reader, for example the knowledge of how to deal with real numbers, variables, infinite sequences, equations of the fifth degree and the like. The know-how necessary for the understanding of the modern textbook seems to be omitted on purpose, whereas the older books try to draw one's attention to the fact that there is a know-how. This is most obvious in the different attitudes towards problems and methods. Whereas the older books perform the act of solving, the modern books just give an appendix of exercises at the end of each chapter.[46]

Jeremy Avigad also stresses the importance of an *ability to use* in understanding mathematical knowledge. He remarks that this way of thinking about understanding distances itself from 'the traditional view of mathematics as a collection of definitions and theorems', and that it 'challenges us to view mathematics in dynamic terms, not as a body of knowledge, but, rather, as a complex system that guides our thoughts and actions.'[47] As Avigad remarks, this take on mathematics is a step away from the body of truths picture. To avoid this picture is, I believe, one of the most important parts of the philosophical work towards a greater clarity in the philosophy of mathematics.

These comparisons between mathematical knowledge and knowledge of a monetary system or chess have similarities with the structuralist position. Indeed, structuralism treats mathematical objects as places in a structure and does not necessarily bother about whether there is a collection of objects instantiating this structure or not. I am sympathetic towards the structuralist idea that the features of mathematical objects are determined by their place in a larger pattern or structure. However, as will be argued below, I do not see structuralism as an answer to the main problems of this thesis.

3.5 Truth and Referential Semantics

I have tried to point to certain features of doing mathematics that show in what way the body of truths conception can be misleading. These features also imply that the referential semantics called for by Benacerraf is ill-suited for an understanding of mathematical truth. The causal theory of knowledge favoured by Benacerraf with regard to physical objects as well as mathematical objects is no

[46] Herbert Breger. 'Tacit Knowledge and Mathematical Progress'. In: *The Growth of Mathematical Knowledge*. Ed. by Emily Grosholz and Herbert Breger. Dordrecht: Kluwer, 2000, p. 225.

[47] Jeremy Avigad. 'Understanding Proofs'. In: *The Philosophy of Mathematical Practice*. Ed. by Paolo Mancosu. Oxford: Oxford University Press, 2008, p. 327.

longer the dominating one. As was mentioned above, the dilemma presented in that article still affects realist theories although they may depend on another theory of knowledge. What seems to have survived in contemporary theories, however, is the referential semantics that Benacerraf also proposed as a requirement for a theory that purports to explain truth in mathematics (and not dress up something else as truth). In practice, referential semantics amounts to explaining truth via Tarski's definitions of truth in formalised languages. Because of the status that Tarski's definition enjoys[48] and because it is often interpreted as explaining truth via reference to mathematical objects, it adds to the prima facie plausibility of the body of truths picture.[49] However, I shall argue that referential semantics is inappropriate for understanding truth in mathematics. This will, in turn, lessen the appeal of the body of truths picture.

What I am criticising is the idea that truth is to be understood in terms of reference to a particular domain of objects that have certain features and that truth in mathematics should be seen as a special case of this. I am not questioning Tarski's theory qua mathematical theory. However, I see no necessary connection between Benacerraf's referential semantics qua philosophical theory of truth and Tarski's mathematical theory, while Benacerraf equates them. Benacerraf and the philosophers troubled by his dilemma are interested in a philosophical understanding of mathematical knowledge and truth in mathematics. Tarski's definition of truth as it appears in 'The Concept of Truth in Formalized Languages' is a mathematical device applicable to certain formalised languages. Tait criticises Benacerraf for drawing illegitimate philosophical conclusions: 'It is difficult to understand how Tarski's "account" of truth can have any significant bearing on any issue in the philosophy of mathematics. For it consists of a definition *in* mathematics of the concept of truth for a model in a formal language *L*, where the concept both of a formal language and of its models are mathematical notions.'[50]

Tarski's theory becomes a philosophical theory of truth – for mathematics or language in general – only together with a substantial philosophical interpretation. Whether Tarski himself supplied it with such an interpretation is, strictly speaking, not relevant, but it seems that he did not intend to do that. 'There will

[48] John Etchemendy has described its status in logic: 'The highest compliment that can be paid the author of a piece of conceptual analysis', he writes, 'comes when the suggested definition is no longer seen as the result of conceptual analysis … and the definition is treated as common knowledge.' John Etchemendy. *The Concept of Logical Consequence*. Cambridge MA: Harvard University Press, 1990, p. 1. Tarski's definition of truth is indeed treated as common knowledge among analytic philosophers.

[49] Shapiro notes: 'Philosophical realism is well served by a bivalent, model-theoretic framework, sometimes called "Tarskian".' He remarks that the preference for realism, as a by-product, prompts a model-theoretic semantics. Shapiro, *Structure and Ontology*, pp. 46–48, quote on p. 46.

[50] Tait, 'Truth and Proof', p. 347.

be no question at all here of giving a single general definition of the term', Tarski writes.[51] However, even though he aims *not* for a 'thorough analysis of the meaning current in everyday life of the term "true"', he is still interested in 'grasping the intentions which are contained in the so-called *classical* conception of truth ("true – corresponding with reality")' – with regard to the formalised languages which occupy him. Although he makes no claims to generality regarding the predicate 'true', he still attempts to construct his definition on the model of correspondence. His preference for this particular outlook on 'truth' thus makes it intelligible that Benacerraf sees a referential semantics in Tarski's work. This preference of Tarski's is, in turn, manifested in his *Convention T*, which, according to Tarski, forms the touchstone for any 'materially adequate' definition of truth.[52]

The path to a formal definition of 'truth' goes via the concept of *satisfaction*. But neither Convention T nor a truth definition in terms of satisfaction need be interpreted so as to imply realism (nor any causal interaction with mathematical objects). That it is taken to imply realism is, however, not surprising. When truth is defined in terms of satisfaction of a formula by sequences of objects – whether by all sequences or as relativised to the sequences of some particular domain – the picture readily announces itself of formulas expressing facts about a collection of objects, being true if these facts do indeed obtain, and otherwise false. Accordingly, one has the theory which consists of a collection of formulae (i.e. axioms and theorems) and what it describes (i.e. models) which is external to the theory. The idea of a model easily takes the role of a Platonic realm (a model in the sky, to use Tait's phrase), although it is obvious that *model*, too, is a mathematical concept (an ordered pair, i.e. a set), and not something that mathematics refers *to*.

If the domain of a model is seen as something external to mathematics, it seems that one must understand the relation between the formulas of a formalised theory and the elements of this domain in terms of reference, and hence a referential semantics seems to be the proper philosophical answer to the problem of truth. Still, the domain of a model is, of course, a set, a mathematical object. Finally, satisfaction means the assignment of values to the variables of a function.[53] As Tait observes, this makes the term 'reference' misleading: 'The model assigns values to the constants of *L* [the language of the formalised theory]; but this, like the notion of valuation, is expressed in terms of the notion

[51] Tarski, 'The Concept of Truth in Formalized Languages', p. 153.

[52] Ibid., pp. 187–88; Alfred Tarski. 'The Semantic Conception of Truth: and the Foundations of Semantics'. In: *Philosophy and Phenomenological Research* 4 (1944), pp. 341–76, § 4.

[53] Cf. Definition 22 (p. 193) of 'The Concept of Truth in Formalized Languages': 'The sequence *f* *satisfies* the sentential function *x* if and only if *f* is an infinite sequence of classes and *x* is a sentential function and these satisfy one of the following four conditions …'

of function, and the concept of reference does not enter in. … It is the more misleading when Benacerraf goes on to advocate a causal theory of reference.'[54]

What would it be like if reference entered into the assignment of values to a function? Would it not be rather awkward to say that a function referred to its domain? Assigning values to variables can be thought of in two ways: (1) One has a concrete function and substitutes for the variable (or variables) some particular value (or values) and then calculates the value of the function for that value of the argument. (2) One states that a certain function takes its values from a certain set (e.g. the function $f(x) = \sqrt{x}$ defined for the positive real numbers or the function $f(n) = 2n$ defined for the natural numbers). Sometimes, it is defined on a particular set because it is not meaningful to assign other values to the variable (as in the case with the square root). Sometimes, one is interested in studying the function for a limited domain (as in the case with the function from natural numbers to even numbers). In all of these cases, calling the assignment of values to a variable 'referring' is out of place. Regarding the function $f(n) = 2n$, the meaning of 'refers' would, if anything, be that the function refers to the even numbers. One might, of course, give the expression 'the function *refers*' the special meaning 'picking out its domain'. Then, however, it is no longer analogous to cases in ordinary speech such as answering the question 'what do you mean by "my precious"?' by saying: 'Oh, I was referring to my ring.'

Tarski devised his mathematical construction in order to capture the (as he wrote) ordinary meaning of 'true', so naturally one thinks of it as a way of formulating the concept true. However, one could treat it simply as a mathematical device, symbolically and forget about the philosophical motivations. It then becomes obvious that it is indeed a mathematical concept and that its connection with the ordinary concept true is loosened. Thus, although it has been of great importance as a piece of mathematics, this does not imply that it gives us the solution to the general problem of truth in mathematics. The appeal that Tarski's definition has had depends to a large extent that it fits into the body of truths conception so neatly, and as is the case with Benacerraf's article on mathematical truth, it has come to strengthen the hold that this picture has on the philosophy of mathematics.

Another matter that may be added to the criticism concerns Benacerraf's motivations for his preference for a referential semantics. A supposed merit of it is that the referential picture gives hope of a unified account of meaning that would incorporate all areas of human discourse (or at least 'all the propositions to which [truth] applies'). He describes his preference thus: 'My bias for what I call a Tarskian theory stems simply from the fact that he has given us the only viable systematic general account we have of truth. So, one consequence of the

[54]Tait, 'Truth and Proof', p. 347.

economy attending the standard view is that logical relations are subject to uniform treatment: they are invariant with subject matter.'[55]

To be sure, we do speak of objects around us, especially if we are trying to do something with an object, e.g. when making use of an ingredient in a stew, painting the walls of a house, or analysing a blood sample. However, even if we do refer to things, *referring* is not always the same activity. This can be seen if one compares such examples as speaking of a house, a feeling, a piece of music, or simply mentioning something with the word 'this'. Moreover, if one thinks of the discussions and exchanges we often have, a great deal of them cannot be understood in terms of reference to things. The charm of the referential view may thus be the result of a false appearance.

3.6 Structuralism

Above, the thought of abstract objects that have features in themselves independently of their relations to other objects and independently of what we do with them was rejected. The discussion of what it might mean to study a mathematical object, even in the absence of such abstract entities, suggested a similarity between the perspective proposed here and structuralism. In this section, I will indicate sympathies and differences with some contemporary views, structuralism in particular.

All of the positions mentioned in section 3.2 succeed in accounting for some part of mathematics as it is done in practice. None of them manage to tell a story that incorporates all features in a natural and intuitive way. Platonism or ontological realism seems to be a good explanation of the general agreement about results in mathematics and of the fact that the search for solutions to problems or for proofs proceeds as if there is a clear, unambiguous solution. Maddy notes yet another reason for the preference for realism among many mathematicians, namely, that it corresponds to the experience of actual mathematical activity.[56] Olle Häggström gives a clear expression of this experience: 'To anyone who has experienced the inescapable force and logical necessity of a mathematical proof, the Platonic existence of numbers and their properties – independently of us humans who think and argue about them – is obvious.'[57]

Thus, realism claims that there are mathematical entities independent of the mathematician, and that explains the stability of the practice. Fictionalism supplies an explanation of the freedom of the development of mathematics. Mathematics progresses in so many different directions, utilising so many different

[55] Benacerraf, 'Mathematical Truth', p. 670.

[56] Penelope Maddy. 'Set Theoretic Naturalism'. In: *The Journal of Symbolic Logic* 61 (1996), pp. 490–514, p. 492.

[57] Olle Häggström. 'Objective Truth versus Human Understanding in Mathematics and in Chess'. In: *The Montana Mathematics Enthusiast* 4 (2007), pp. 140–53, p. 140.

concepts and techniques that the comparison to an author's freedom in creating fictional characters appears apposite. Structuralism, as was mentioned above, manages to provide an account of the fact that the features of mathematical objects very often – perhaps always – emerge through their relations to other mathematical objects, thus making their position in the structure the decisive issue for learning about them.[58]

In the end, these positions turn out to be simplifications that proceed from some observation that the proponents find particularly important to incorporate into the position. When it comes to features of mathematics that do not fit the observations of preference, they are forced to incorporate these other features in more or less awkward ways. The attempts to explain how it is possible to make contact with abstract objects and thereby solve the problem that Benacerraf raises for ontological realism may serve as an example of this.

While I think that structuralism's focus on the relations between objects as a way to overcome the worry about the ontological status of mathematical objects is promising, their accounts do not escape ontological qualms. This is evident in the debate within structuralism about the nature of structures themselves. Structuralism views mathematical objects as places in a structure.[59] With regard to the status of structures, Shapiro distinguishes three stances. The first is that a structure can be said to exist if there are objects that instantiate the structure. That such a structure can be said to exist is clear if there is a finite collection of objects instantiating the structure. However, arithmetic, analysis, and set theory deal with an infinite number of objects, and, therefore, already the structure of arithmetic requires for its existence an infinite number of objects that can be arranged according to the structure. This does not seem to place the structuralist in any better position than the ordinary Platonist. An alternative to postulating a collection of objects that instantiate a structure (usually sets), would be to claim that structuralism deals with *possibly existing* collections of objects. The use of modal logic in formulating this view renders it the name *modal* structuralism, and this view is often associated with Geoffrey Hellman. This is the second stance that Shapiro mentions. Following Charles Parsons, he calls these two alternatives *eliminative structuralisms*.[60] The term 'eliminative' signifies that

[58] Saunders Mac Lane writes in a similar vein: 'All mathematics can indeed be built up within set theory, but the description of many mathematical objects as structures is much more illuminating then [sic] some explicit set-theoretic description.' Saunders Mac Lane. 'Structure in Mathematics'. In: *Philosophia Mathematica* 4 (1996), pp. 174–83, p. 182.

[59] In Resnik's words: 'The objects of mathematics, that is, the entities which our mathematical constants and quantifiers denote, are themselves atoms, structureless points, or positions in structures. And as such they have no identity or distinguishing features outside a structure.' Michael D. Resnik. *Mathematics as a Science of Patterns*. Oxford: Clarendon Press, 1997, p. 201

[60] Shapiro, *Structure and Ontology*; Charles Parsons. 'The Structuralist View of Mathematical Objects'. In: *Synthese* 84 (1990), pp. 303–46.

structures existing as objects in their own right are eliminated from this view. To overcome the difficulty with the existence of objects instantiating the structure, Shapiro suggests that structures are objects in their own right and that these can be studied regardless of whether they are instantiated by any objects. This forms the third stance on structures, and he dubs this view *ante rem* structuralism in analogy with the similar position in the debate about universals. Resnik's approach is similar. This, in turn, begs the question about the ontological status of structures.

Hellman lists four alternative structuralisms: (1) structures are models and, through the standard definition of model, sets; (2) structures are *categories*; (3) structures are a primitive kind of object not reducible to anything else; and (4) modal structuralism.[61]

In each of these cases, some particular concept is taken as a starting point which cannot be explained in terms of some other more fundamental concept. In (1) this is sets. Taking sets as the primitive concept allows for a mathematically attractive theory because one can use already established set theoretic definitions of concepts such as model. If one has philosophical ambitions with structuralism, it will be a major drawback that the structures that mathematics supposedly studies are in the universe of sets. Unless one assumes that there is one structure that is not a set, this means that set theory itself cannot be viewed as a structure with sets as places in this structure. In (2) and (3), categories and structures, respectively, are introduced as primitive concepts, and thus they allow for the possibility of viewing all of mathematics (set theory included) as the study of structures. The status, however, of these new objects (at least in the case of *ante rem* structures) is not clear, so whether or not anything is gained, from an ontological perspective, remains uncertain. Hellman even suggests the label 'hyperplatonism' for the *ante rem* variant.[62] In modal structuralism (4) it seems necessary to take the modal concepts as primitive. Otherwise, a definition of them through set theory lies close at hand, and, in that case, it would probably be better to aim for set theory from the beginning. Even if one takes modal concepts as primitive, it is not clear that much is gained. Shapiro notes that 'It is unfair to reject set theory, as our antirealists do, and then claim that we have a pretheoretic grasp of modal notions that, when applied to mathematics, exactly matches the results of the model-theoretic explication of them.'[63]

[61] Geoffrey Hellman. 'Structuralism'. In: *The Oxford Handbook of Philosophy of Mathematics and Logic*. Ed. by Stewart Shapiro. Oxford Handbooks in Philosophy. New York: Oxford University Press, 2005. This list roughly coincides with the above that follows the exposition in Shapiro's *Structure and Ontology*. Shapiro, however, mentions the category alternative under the heading of *ante rem* structuralism.

[62] Ibid., p. 542.

[63] Shapiro, *Structure and Ontology*, p. 17.

In all of these alternatives, the status of the primitive concept is a cause for concern. They may be able to solve the problem raised by Benacerraf's 1965 article, which was a major inspiration for structuralism, but the problem posed by the 1973 article – about the possibility of gaining knowledge of abstract objects – remains, even for the anti-realist option modal structuralism. This is because, although reference to abstract objects is seemingly avoided, so much is moved in under the modal concepts that a similar question arises: 'How can we know what is possible?' To sum up, structuralism seemingly solves the problem of referring to the mathematical objects as they are places within a structure. Nevertheless, a similar problem appears anew in the case of the structures themselves. It seems impossible to avoid this problem as long as one postulates new abstract objects.[64]

In addition to these points of criticism that concern structuralism generally, I shall consider some issues that apply to Resnik's and Shapiro's view. They both have a promising way of solving the access problem that pertains to structures. They both start with the fact that we encounter finite surveyable patterns in everyday life. Through a process of abstraction, we learn about number series that extend beyond what is immediately recognisable. The step in this picture that both consider to be the most troublesome – the step to infinite structures – is taken through implicit definition, through the axiomatic method. We learn about infinite structures through the axioms that implicitly define them. Resnik observes that learning mathematics may be thought of in analogy with learning language and music; the comparison between learning mathematics and learning language is found in Shapiro's structuralism too.[65] I find these analogies apt, but the analogies would do equally well if one was speaking, not of structures, but directly of numbers or sets (and probably other mathematical concepts as well). That is, although I find this rough picture correct in many ways, it does not speak exclusively in favour of structuralism.

Ante rem structuralism has come under criticism in matters concerning the individuation of places in structures. Shapiro seems to accept Leibniz' principle of the *identity of indiscernibles*.[66] In structuralism, this means that if two objects

[64]Yet another form of criticism is voiced by Mac Lane. He remarks that, while many areas of mathematics may be described as a study of structures, there are also areas that do not fit this description. He mentions (among other) the study of partial differential equations, and number theory. These areas cannot, he claims, be understood as the study of axiomatically defined structures.Mac Lane, 'Structure in Mathematics', p. 177. As long as one is thinking of group theory or arithmetic as an axiomatic system, the structuralist view seems apt; however, if one wants to maintain that mathematics is about studying structures, it seems that a substantial reinterpretation of some areas of mathematics is required (if it is even possible).

[65]Resnik, *Mathematics as a Science of Patterns*, p. 202; Shapiro, *Structure and Ontology*, pp. 139–40.

[66]Ibid., section 4.5.

can occupy the same place in the structure without any changes in the structure, they ought to be considered the same object. John P. Burgess and Jukka Keränen have both pointed out that from a structural point of view, the imaginary unit i and its additive inverse $-i$ are indistinguishable, and therefore the same object.[67] This, however, seems absurd, since they are, from the point of view of mathematical practice, different numbers.

The criticism of Burgess and Keränen has been met by Shapiro by modifying the principle of identity of indiscernibles.[68] I am mentioning these criticisms not because they necessarily provide conclusive evidence that structuralism is false, but rather because the situation is discouraging from a philosophical perspective. If one wants to understand the certainty of mathematical knowledge (or if one wants to explain of the apriority and objectivity of mathematics as Shapiro requests), what are small tweaks to a theory worth?

From my point of view, there are more serious objections to structuralism, however. It is common to assume a second-order logic as the background theory for structuralism. There are several ways of justifying this choice. Philosophically interesting is Shapiro's thought that second order logic is needed to do justice to mathematical practice. The reasons he gives are of a technical nature and I will not go into them here.[69] More generally, he states: 'I assume that formal languages more or less accurately render the languages and logical forms of mathematics.'[70] Shapiro's structuralism – which is a mathematical theory – depends on this possibility of capturing mathematical practice in a formal language. Yet, unless one thinks that logic is, somehow, on a different level than the rest of mathematics, this seems too strong a claim. Moreover, Shapiro remarks that logic should be viewed as part of mathematics: '[T]here is no sharp distinction between logic and mathematics. The study of correct inference, like almost any other science, involves some mathematics and some mathematical presuppositions.'[71] Now, if logic is a form of mathematics, doing logic is a form of mathematical practice, and, in conclusion, there seems to be a potentially vicious circle.

[67] John P. Burgess. 'Review of Stewart Shapiro. *Philosophy of Mathematics: Structure and Ontology*'. In: *Notre Dame Journal of Formal Logic* 40 (1999), pp. 283–91; Jukka Keränen. 'The Identity Problem for Realist Structuralism'. In: *Philosophia Mathematica* 9 (2001), pp. 308–30.

[68] Stewart Shapiro. 'Identity, Indiscernibility, and *ante rem* Structuralism: The Tale of i and $-i$'. In: *Philosophia Mathematica* 16 (2008), pp. 285–309. A related criticism is voiced by Tim Räz who gives detailed examples of groups of symmetry that are isomorphic to each other. He remarks that if it is accepted that isomorphic structures are identical, this entails that some structures that are actually considered to be different would be identical. Tim Räz. 'Say My Name: An Objection to *Ante Rem* Structuralism'. In: *Philosophia Mathematica* 23 (2014), pp. 116–25.

[69] For details see Stewart Shapiro. 'Second-Order Languages and Mathematical Practice'. In: *The Journal of Symbolic Logic* 50 (1985), pp. 714–42; Resnik, *Mathematics as a Science of Patterns*, chapter 10.4.

[70] Shapiro, *Structure and Ontology*, p. 48.

[71] Shapiro, 'Second-Order Languages and Mathematical Practice', p. 716.

A similar circularity also occurs in the claim that mathematics studies structures. It is obvious that *structure* is a mathematical concept, at least in Shapiro's structuralism, so it seems that he is trying to understand mathematics in terms of yet another mathematical concept.[72] This is troublesome in the same way as the definition of truth in mathematics via another mathematical concept, satisfaction, as was pointed out in section 3.5. In short: if one wants to achieve a greater *philosophical* understanding of mathematics, this cannot be done simply by doing more mathematics.[73]

Can one not understand mathematics by doing mathematics? Probably the best way to learn what mathematical thinking is, is by doing mathematics and being attentive to what one is doing. Similarly, if one wants to understand physics, one must do physics. However, presumably any kind of mathematics would do in that case, and the understanding thus gained is probably not the kind of understanding that is sought if one sets out to construct a structuralist theory. Such a theory may, of course, be a good mathematical theory; what I am critical of is the idea that there are some special philosophical lessons that can be learnt from it.

In a discussion about Carnap's distinction between questions internal and external to a linguistic framework, Shapiro gives as an example of an internal ontological question the following: 'Is there a prime number greater than one million?' and of an external one: 'Do numbers exist?' However, Shapiro's rephrasing of the external question shows that he considers the formalised theory to be, in essence, the same as the unformalised one: 'In present terms, the traditional question is whether the envisioned natural-number framework accurately describes an intended domain of discourse.'[74] Furthermore, he describes the philosophical study of mathematics as studying an object language in a metalanguage. He devotes some effort to discussing whether this metalanguage that we employ in studying mathematics should be formalised or not and also what its ontological status is. In contrast, the possibility of equating the mathematics that we try to understand (i.e. ordinary mathematics) and formal systems passes without comment in Shapiro's discussion. This possibility is seemingly taken for gran-

[72] At least within mathematics, Shapiro, too, considers this troublesome: 'From the present point of view, the major shortcoming of ω-languages is that they assume or presuppose the natural numbers. Therefore, such a language cannot be used to show, illustrate, or characterize how the natural number structure is itself understood, grasped, or communicated.' Shapiro, 'Second-Order Languages and Mathematical Practice', p. 733.

[73] Resnik does not consider his theory to be mathematical. 'I do not think of my view as a mathematical theory.' Resnik, 'Mathematics as a Science of Patterns: Ontology and Reference', p. 542. However, it is not mathematical only in the sense that it is not mathematical *yet*. This becomes clear when he remarks that 'the main motivation for developing [a mathematical theory of patterns] would be to show that my informal theory of patterns can be made mathematically precise'. Resnik, *Mathematics as a Science of Patterns*, p. 258.

[74] Shapiro, *Structure and Ontology*, p. 58.

ted, as is indicated by Shapiro's claim that 'one major purpose of axiomatizing a branch of mathematics is to codify the practice of that branch.'[75] This silence is unfortunate since the claim that mathematical practice can be captured in formal systems involves a substantial philosophical interpretation, which is by no means innocent. Among other things, it involves disregarding the issues of skill and technique that I emphasised above. This disregard is, however, symptomatic of much of contemporary philosophy of mathematics. I will return to the relation between formal systems and ordinary mathematics in chapter 4.

Finally, if one considers structuralism from the perspective proposed above, where ability and skill are seen as vital to an understanding of mathematical knowledge, the structuralist theory itself, as much as any other mathematical theory, stands in need of philosophical explanation. Sören Stenlund discusses various attitudes to formal work in the philosophy of mathematics:

> In the philosophy of mathematics, as elsewhere, conceptual clarification conceived as reconstruction never gets at the real foundations. In its efforts towards mathematical progress, it passes too quickly over the points where the conceptual problems can be resolved absolutely. … [T]hey are not developed in a vacuum but within the language of ordinary mathematics and on the basis of the 'trivial mathematics' which is erroneously considered to be conceptually unproblematic (on the grounds that it is mathematically trivial). Many of the original conceptual puzzles which motivated the foundational studies therefore still arise *within* the formalizations as much as in ordinary mathematics.[76]

Perhaps it is fair to say with Putnam: '[I]t seems clear that what is needed in philosophy of mathematics is work that is *philosophical* and not primarily technical.'[77] In this respect, the increased focus on practice in recent philosophy of mathematics is laudable. The criticism voiced above is primarily directed at the positions that develop structuralism as a foundational programme, e.g. Shapiro's structuralism with second order logic as the background theory. However, the principal idea behind structuralism – that mathematical objects need not be thought of as objects existing in their own right, but rather as places in a structure and defined through the relations that they have to other objects – is a sound observation that is not touched by this criticism, and, furthermore, one that is in accord with the perspective advanced here.

The present state of the debate between realism and anti-realism is unsettling. As was argued above, this debate is the outgrowth of the body of truths picture and of the form that this picture took in Benacerraf's article on mathematical truth. From the 1980's onwards, one finds several arguments that, taken together, point towards the insight that the different positions boil down to the

[75] Shapiro, 'Second-Order Languages and Mathematical Practice', p. 716.

[76] Stenlund, *Language and Philosophical Problems*, p. 136.

[77] Hilary Putnam. 'Philosophy of Mathematics: Why Nothing Works'. In: *Words and Life*. Ed. by James Conant. Cambridge MA: Harvard University Press, 1994, p. 510.

same thing. Resnik has argued that Hartry Field's nominalism does not succeed in eliminating abstract objects.[78] From a structuralist point of view, it is clear that the space-time points Field invokes to nominalise analysis can be seen as places in a mathematical structure even though they have a physical location. Thus, they still have the character of mathematical objects.[79] His criticism is valid from a perspective of mathematical practice too. Although it may be possible to find physical counterparts of the numbers, it is still the *use* within mathematics that makes them into mathematical objects.

Balaguer, for his part, argues that structuralism and Platonism are not that different after all: 'it's not clear why positions *shouldn't* be considered objects. We can refer to them with singular terms, quantify over them in first-order languages, ascribe properties to them, and so on. What else is needed?'[80] Considered from a point of view of practice, this seems right. Structuralism may claim that no Platonic objects are needed, that they are places in a structure, but from the view of mathematical practice there would be no difference.[81] Have we come full circle?

These observations suggest that there is no substantial difference between these positions after all. Balaguer's point is that there is 'no fact of the matter' which could settle the question in favour of either metaphysical realism or anti-realism. In the choice between the different kinds of structuralism, Shapiro remarks in a similar fashion that they 'are each, under certain (plausible) conditions, definitionally equivalent to a standard, "realist" theory. Thus, the intended structure – and the ontology/ideology of each theory is the same as that of the corresponding realist theory.'[82] He continues by saying that which kind of structuralism one chooses depends on how well it accords with mathematical practice.

All in all, this indicates that this debate is not fruitful. To be sure, it may be fruitful in the sense that there is much to be learnt while working on the different positions, but they will not solve the philosophical problems that they discuss. Penelope Maddy is writing in a similar spirit: '[I]f questions of ontology and truth are red herrings … then I can at least hope to shift attention away from a misplaced worry and to focus it instead on the challenge of understanding the phenomenon that in fact drives the practice of pure mathematics.'[83]

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[78] Michael D. Resnik. 'How Nominalist Is Hartry Field's Nominalism?' In: *Philosophical Studies* 47 (1985), pp. 163–81.

[79] Shapiro voices similar criticism. Cf. Shapiro, *Structure and Ontology*, pp. 76–77.

[80] Balaguer, *Platonism and Anti-Platonism*, p. 9.

[81] Similar points are raised by Krzysztof Wójtowicz. 'Object Realism versus Mathematical Structuralism'. In: *Semiotica* 188 (2012), pp. 157–69.

[82] Shapiro, *Structure and Ontology*, p. 242.

[83] Maddy, *Defending the Axioms*, p. 117.

Interestingly, this bears similarities to Hilary Putnam's resigned criticism in 1967 of the foundational programmes of the earlier half of the twentieth century. 'The much touted problems in the philosophy of mathematics seem to me, without exception, to be problems internal to the thought of various system builders', Putnam claims and continues, 'the various systems of mathematical philosophy, without exception, need not be taken seriously'.[84] While I do think that there are genuine and troubling problems in the philosophy of mathematics, it is questionable whether the debate between realism and anti-realism will further our understanding of them.

3.7 Concluding Remarks

Mathematical knowledge is knowledge of theorems and axiomatic systems; it is also the ability to prove things and make fruitful conjectures. It is, furthermore, the ability to make use of mathematical techniques and calculi in order to make predictions and solve problems in everyday life, in economics, in engineering, and in science. Knowing things in mathematics is not reducible to a single thing (e.g. justified true belief regarding what theorems say); it encompasses a range of insights, memorised formulas, abilities to prove and apply, and generally to 'see' when a certain calculus, theorem, rule, etc. is applicable to a certain case. The idea or, perhaps, the tacit assumption that one could understand mathematical knowledge along the lines of a single formula is, as I hope to have shown, likely to lead to philosophical problems.

Stenlund has suggested that the concept of knowledge as such leads us to think in terms of knowledge *about* something and that, therefore, the phrase 'mathematical knowledge' can be misleading.[85] I agree with him, although, in this chapter, I have tried to see what sense one could make of the notion, rather than avoid it. What emerges is a picture of mathematical knowledge where mathematics as an activity plays a central role. Having knowledge in mathematics is seen to include knowing how to use the concepts of mathematics, and it cannot – as Stenlund remarks – be understood (exclusively) as a knowledge about something.

Wittgenstein remarks that 'mathematics as such is always measure, not thing measured',[86] and this indicates a way of understanding Stenlund's suggestion. One can compare mathematics to a ruler, and if knowledge is the result of measuring, then mathematics does not give us knowledge (at least not in the same sense). Mathematics, then, acquires a different status, as it is involved in our

[84]Hilary Putnam. 'Mathematics Without Foundations'. In: *Philosophical Papers*. Vol. 1: *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975, p. 43.

[85]Personal correspondence.

[86]RFM, III § 75.

tools for gaining knowledge about other things. From this perspective, work in pure mathematics could be seen as a work on our tools for gaining knowledge, as a work on our conceptual framework. If mathematics was 'the thing measured', then the propositions of mathematics would be thought of as being true or false (analogous to descriptions of facts). Mathematical knowledge would take the guise of knowing the *content* of the propositions that are true. Furthermore, *certainty* will appear as certainty that the proposition is true.

If mathematics is instead compared to the measures we have in language, one could (taking the risk of pushing the analogy too far) say that knowledge becomes a skill in using these measures and in developing new ones. Its certainty also takes another form, and it can be seen along the lines of the comparison to norms sketched in chapter 2. What is judged on the scale of certainty and uncertainty are the things measured, while the measure itself is exempt from it, being involved as it is in making such judgments. Wittgenstein gives a description of this status with regard to logical inferences, but the same goes, I would say, for mathematical propositions too.

> The steps which are not brought in question are logical inferences. But the reason why they are not brought in question is not that they 'certainly correspond to the truth' – or something of the sort, – no, it is just this that is called 'thinking', 'speaking', 'inferring', 'arguing'. There is not any question at all here of some correspondence between what is said and reality; rather is logic *antecedent* to any such correspondence; in the same sense, that is, as that in which the establishment of a method of measurement is *antecedent* to the correctness or incorrectness of a statement of length.[87]

That the results of mathematics are regarded as perfectly certainty reflects this status of being withdrawn from evaluation in terms of certainty and uncertainty. As they are, instead, part of such evaluations of other propositions, their relation to such propositions is normative. This is a kind of certainty, but it is not of the same kind as the certainty of propositions describing matters of fact. It can be said to be greater than the certainty of descriptive propositions, but only because mathematical propositions are exempt from the evaluation that such propositions undergo. Does this perspective imply that one cannot doubt the truth of a mathematical proposition? This seems as an absurd consequence because in many cases it seems possible to doubt a mathematical proposition, especially if one is not that well acquainted with it. It is true that not all of mathematics can have this status for a particular person. There will always be many mathematical results that one is not prepared to use in judging the plausibility of descriptive propositions. However, if one takes into account that having knowledge of a mathematical result involves an ability to use it, it is seen that

[87] RFM, I § 156. The perspective brought up in this quote will be central to the discussion of proofs in chapter 5.

the results that one is certain of are precisely the ones that one is sufficiently familiar with. The results that have this normative role, that for me are exempt from evaluations in terms of certainty and uncertainty and that I am prepared to make use of in important situations where I must not make a mistake, are the ones that I am at ease with. Which results that are certain in this sense varies greatly from person to person, since we do not, in general, ascribe certainty to something that we have no relation to. The certainty of mathematics, if one by this means mathematics in general and not merely my certainty, can be taken to signify this possibility of ascertaining results by gaining a working knowledge of them – in addition to the fact that a vast amount of techniques and results have this status for any rational person, as was seen in chapter 2.

One could, with the risk of simplifying matters, capture the main point of this chapter in the remark that knowledge in mathematics is more a question of knowing *how* than knowing *that* – knowing how to prove a theorem, rather than knowing that matters are as the theorem says. Fully understanding and knowing a theorem amounts to knowing how to prove it, and that involves having an eye for what works (or as one often says, 'having an intuition for it'). 'Having an eye for what works' is not an innate capacity to perceive facts, but a certain handiness one gains through persistent practice, much like a carpenter having an eye for wood.

Thus, the gist of this chapter is mainly negative. The problems that led to the unfruitful debate between realism and anti-realism are all but unavoidable if one approaches mathematics from the body of truths perspective. The historical development has, furthermore, made this perspective appear self-evidently true to such a degree that it is forgotten that it, in fact, involves a substantial philosophical construal of mathematics. This construal captures some of the characteristics of mathematics, but it does not do justice to how mathematics is done in practice – and, therefore, it becomes a misleading starting point for philosophy. It is thus important to nuance the understanding of mathematical knowledge. Since the body of truths picture also implies a particular take on certainty – i.e. that mathematical certainty should be understood as a certainty that a mathematical proposition correctly describes the mathematical objects – it is vital for the aim of this thesis not to let this picture guide one's thinking on mathematics.

An issue that has been repeatedly touched upon in this chapter but not been discussed explicitly is the relation between formal systems and ordinary mathematics. It was mentioned that one guise of the body of truths conception was to view mathematics as a collection of formal systems. In the discussion of structuralism, Shapiro's claim that ordinary mathematical reasoning is captured in formal languages was criticised briefly. However, the idea that the object languages of metamathematics can be taken to represent ordinary mathematics is a common assumption. Related to this assumption is also one that relates to certainty and rigour. It is commonly held that formal systems are superior to informal mathematics in terms of their certainty, precisely because they are formal and free from meaning. Therefore, chapter 4 will problematise the relation between formal and informal mathematics.

4. Formality

> [A]ll of pure mathematics can be imbedded in formal systems.
>
> (J. M. Henle, 'The Happy Formalist')[1]

> There is a conflict between mathematical practice and the formalist doctrine.
>
> (Georg Kreisel, 'The Formalist-Positivist Doctrine of Mathematical Precision in the Light of Experience')[2]

The discussion of knowledge in mathematics in the last chapter touched upon the distinction between mathematics as it is done in practice and mathematics seen as formal axiomatic systems. This distinction is the focus of the present chapter and there are two reasons for this.

Firstly, there is the question that grows out of the previous chapter, namely, 'Which part of the divide should be given conceptual priority when a philosopher is trying to understand what mathematics is?' Another question, not to be conflated with the former, is: 'Is it correct to say that mathematics, deep down, is nothing but formal systems?' In chapter 3, the emphasis on mathematics as an activity naturally suggests that the answer to this second question is that mathematics is more than just formal systems. It is also a natural consequence of that discussion that ordinary mathematics ought to be the focus of a project aiming for a greater philosophical understanding of mathematics and its certainty. This is not, however, to say that the label 'mathematics' is better suited for one rather than the other. Moreover, 'mathematical practice' has become something of a buzz phrase lately, but simply taking account of the way mathematics is done does not by itself solve philosophical problems. One has to consider carefully what the problems at hand require.

Secondly, the distinction is central to the problem of certainty. A greater formality is often associated with a greater degree of reliability, rigour, certainty, etc. Hence, it is important for the present investigation to consider the role of formality in the certainty of mathematics. Is the certainty of mathematics a consequence of its status as a formal science? Is it the possibility of proving theorems in formal axiomatic systems that warrants certainty? These questions also makes it obvious that one needs to consider the relation between formal and

[1] James Henle. 'The Happy Formalist'. In: *The Mathematics Intelligencer* 13 (1991), pp. 12–18, p. 13.

[2] Georg Kreisel. 'The Formalist-Positivist Doctrine of Mathematical Precision in the Light of Experience'. In: *L'Âge de la Science* 3 (1969), pp. 17–46, p. 39.

non-formal or intuitive mathematics mentioned above.[3]

The purpose of this chapter is to place certain ideas related to the divide between the formal and the informal under scrutiny. In particular, I am sceptical of the idea that there is a purely syntactic or formal modus, free from ambiguity. Moreover, I also want to question the idea that intuitive mathematics would be compromised by uncertainty simply due to its informal nature or due to the fact that one takes into account the meanings of the symbols occurring in the expressions. Furthermore, I will argue that some form of intuitive understanding is also present when dealing with purely formal mathematics and that there seem to be no distinguishing features that allow for a sharp divide between formal and informal mathematics – when it comes to certainty. The features of formality that allow for a greater certainty (I am not denying that there are such) are not foreign to informal mathematics either. An important conclusion of this is that the certainty of mathematics cannot be explained by particular features found exclusively in formal mathematics. Its relation to how we deal with symbolic expressions is, once more, found to be important. These ideas are of importance for the investigation as such but they also prepare the ground for the discussion of proofs in chapter 5.

4.1 The Formal and the Intuitive

In many ordinary discussions, the labels 'formal', 'informal', and 'intuitive' are used as casual descriptions: 'Ah, you're using that textbook? I found it a bit too formal' or 'I like the way she writes, very intuitive.' It may refer to which style of writing one prefers in mathematics. A book that is formal in this respect does not necessarily contain any material on formal systems. However, in the philosophy of mathematics the *formal* and the *intuitive* are made into something more than casual descriptions. 'Formal' becomes the name of a realm where everything is rigorous, neutral, and mechanically checkable; where meaning (and presumably distracting associations) is completely absent and where the only source of error is the wanting capacities of human beings to remember and overview the formal strings of symbols. This formality is supposedly realised in formal axiomatic systems, where everything is explicitly specified – from what is to be considered a formula to what is to be considered a permissible deduction.

If one considers the attitude towards the purpose of formal systems from an *epistemological* point of view, one can distinguish several possible alternative views.[4] Firstly, one may think that formal systems provide an ideal that ordin-

[3]What is to be understood by 'non-formal', 'informal', or 'intuitive' is not clear and varies with context, and what is to be regarded as the proper counter-part of formal systems is not clear. Moreover, 'formal' is also often used ambiguously. The issue will be given some attention below.

[4]Within mathematics, formal systems have an established role as object theories in mathem-

ary mathematical practice should strive towards in order to maximise its certainty and reliability. This attitude may be understood in (at least) two ways. One may think that mathematical theories do not live up to proper standards of certainty unless they are transformed into formal counterparts. Another, less radical, stance would be that ordinary mathematics is good enough, but if a greater confidence is desired, one can try to formalise, say, a proof and verify it in a formal system. A contemporary expression of this attitude is found in an interview with Alan Hajek: '[Formal methods] often provide a safeguard against error: by meticulously following a set of rules prescribed by a given system, we minimize the risk of making illicit inferences.'[5]

Secondly, Azzouni has argued that some kind of formal system underlies ordinary mathematical thinking although this is not obvious on the surface. Because of this underlying derivation, ordinary proofs are objectively valid. '[A] mathematical proof of *B* from *A* indicates that there is a mechanically recognizable derivation from (a proxy of) *A* to (a proxy of) *B* in an algorithmic system.'[6]

Thirdly, some maintain that formal systems have no epistemological role to play, that they are misleading. We find this attitude in Lakatos's writings. A more recent scepticism towards the use of formal systems has been voiced by Carlo Celluci.[7]

From a *practical* point of view, the benefits of increasing formality are obvious, especially if one thinks of the risks of making an error when making leaps in an argumentation. Sometimes it is also of importance for the perspicuity of a proof or a calculation to consider symbols merely as symbols and not take into account what they refer to. It remains to be settled, however, whether or not these advantages are tied exclusively to a neutral, basic level of formality, if one can even speak meaningfully of such a basic level. Moreover, mistakes are not necessarily eliminated through formality. Indeed, an increase in formality and explicitness may be achieved at the expense of overview and comprehensibility. Chains of reasoning are often kept informal to allow for a greater readability, to allow the reader to form an overarching understanding of a proof. The risk of losing sight of the working of the proof naturally grows with the number of steps and details. Also, the risk of making a mistake increases with the number of steps and calculations. That is, a greater degree of formality may, on the one

atical logic. Formalising ordinary mathematics in formal systems has also found an application in automated proof checking with computers. These uses of formal systems need not be related to any specific philosophical-epistemological aims.

[5]'Interview with Alan Hajek'. In: *Masses of Formal Philosophy*. Ed. by Vincent F. Hendricks and John Symons. Automatic Press / VIP, 2006.

[6]Jody Azzouni. *Tracking Reason: Proof, Consequence, and Truth*. New York: Oxford University Press, 2006, p. 119.

[7]Carlo Cellucci. 'Why Proof? What is a Proof?' In: *Deduction, Computation, Experiment: Exploring the Effectiveness of Proof*. Ed. by G. Corsi and R. Lupacchini. Berlin: Springer, 2008.

hand, allow us to avoid one kind of error, but, on the other hand, make us more prone to another kind of error.

In philosophical discussions, the notion of formality is often used ambiguously. Indeed, Jan Woleński highlights several dichotomies each pointing to a possible contrast with regard to the concept complex 'formal – informal'. He stresses the importance of not conflating them:

> Three contrasts applied to languages are relevant to our problem: (A) natural – artificial; (B) informal – formal; (C) unformalized – formalized; (D) interpreted – uninterpreted. For the first look, it might seem that members of the sequence 'natural, informal, unformalized, interpreted' express the same property, which can be also pointed out as 'ordinary, colloquial', etc. Consequently, the words 'artificial', 'formal', 'formalized', and 'uninterpreted' seem to refer to the same feature. However, a closer inspection shows that these extensional identifications are dubious.[8]

In particular, 'formal' is often associated with 'formalised'. I will, in this chapter, use the phrase 'formal system' to mean 'formal axiomatic system', but I will also discuss the more loosely circumscribed 'formal' and 'logical form', which need not imply 'formalised' (i.e. paraphrased into a particular symbolism). This sense of 'formal' is tied to the structural properties of a proposition that emerge in the use of it in accordance with the rules of the practice.

The counterpart of formal mathematics is referred to as 'informal', 'contentual', and 'intuitive' mathematics. In many cases, these are only ways of referring to mathematics as it is done in practice as opposed to formal systems. In some cases, however, a normative attitude is involved in the use of 'informal'; it may carry the connotations 'sloppy' or 'unreliable', and a connection can be seen with the first of the attitudes to formality mentioned above.

The concept 'intuitive' is a particularly complex one. It is used to denote widely diverging phenomena, and what adds to the complexity and importance of the intuitive is that the elusive notion of *understanding* in mathematics seems to dwell somewhere among the meanings of the word. This is evident in the use that, for example, Gödel has made of the term (see the quote on p. 15).

The concepts 'intuition' and 'intuitive' has traditionally been strongly associated with Kant's theory of knowledge. For him, *Anschauung*, translated as 'intuition', is what allows the knowing subject to have mathematical knowledge and also metaphysical knowledge. Kant, thus, assigns an important role to intuition.[9]

The association with Kant's philosophy finds a continuation in the use of 'intuition' to refer to visual imagery and the forming of pictures to illustrate math-

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[8] Jan Woleński. 'What is Formal in Formal Semantics?' In: *Essays on Logic and its Applications in Philosophy*. Frankfurt am Main: Peter Lang, 2011, p. 81.

[9] Kant's view is that in mathematics we make synthetic judgements that provide us with knowledge a priori. What makes them synthetic is precisely that one needs intuition (*Anschauung*) to apprehend them. This is contrasted with logical truths which are analytical.

ematical facts. This kind of intuition is sometimes called *geometric intuition* although it may arguably occur also in other subdisciplines of mathematics than in geometry. This kind of intuition is frequently frowned upon as being unreliable, and this suspicion has its roots in the development of mathematics during the nineteenth century. This development is often portrayed as the abandonment of geometrical intuition as a standard of correctness in favour of a reliance on rigorous, formal definitions (see section 3.1).

A use of 'intuition' that has its roots in the Kantian tradition can be found in L. E. J. Brouwer's philosophy of mathematics. For Brouwer, intuition of time provides the basic stuff that underlies all mathematical theorising. The influence of Kant's use of intuition is also evident in Hilbert's thought. In his *The Foundations of Geometry*, (for which he chose as a motto a quote on intuition from *The Critique of Pure Reason*) he describes the task of geometry as 'the logical analysis of our intuition of space'.[10]

To these philosophically flavoured senses of 'intuition' one may add the use common among mathematicians: to say that one has an intuition for a theory, or starting go get an intuition for something. This usually means that one has or is on the way towards a thorough understanding of it. William Thurston's comment about his work in mathematics illustrates this use: 'I gradually built up over a number of years a certain intuition for hyperbolic three-manifolds, with a repertoire of constructions, examples and proofs.'[11] In this respect, having an intuition is synonymous to being knowledgeable about the theory.

In addition to the above mentioned senses, Solomon Feferman mentions 'sudden flashes of insight' where the solution to a problem is found, but also 'hunches': the vague feeling that this is the path to take in order to make progress in attempting to prove something.[12] Of these two, hunches in particular, may be considered part of the mathematician's intuition in the sense mentioned above, as being knowledgeable about a theory. Being versed in a theory manifests itself in, among other things, having a hunch about what the next fruitful step would be.

There is a large literature on the subject of intuition in mathematics, but it is not my intention to dwell on the issue. For now, I want to show merely that since the counterpart of formality is so loosely circumscribed, our understanding of

[10] David Hilbert. *The Foundations of Geometry*. La Salle IL: Open Court, 1950, p. 1. Michael Detlefsen argues that the Kantian influence is important for the understanding of his formalism too. Michael Detlefsen. 'The Kantian Character of Hilbert's Formalism'. In: *Proceedings of the 15th International Wittgenstein-Symposium*. Vol. 1: *Philosophy of Mathematics*. Ed. by Johannes Czermak. Wien: Hölder-Pichler-Tempsky, 1993.

[11] William P. Thurston. 'On Proof and Progress in Mathematics'. In: *Bulletin of The American Mathematical Society* 30 (1994), pp. 161–77, p. 174.

[12] Solomon Feferman. 'Mathematical Intuition vs. Mathematical Monsters'. In: *Synthese* 125 (2000), pp. 317–32, pp. 317–18.

formality may not be as straightforward as the definition of a formal system may lead one to think. There is also a risk that these diverging senses of intuition are conflated, and as a result that a philosophical confusion as to what intuition is arises. This confusion is, in part, due to the unclarity of this notion, in part, due to the power that it seems to have. Before passing on to a discussion of the concept of formality, I shall sketch a brief background to the notions *formal system* and *logical form*.

4.2 Historical Background to Formal Systems

From a historical perspective, it is clear that one purpose of increasing formality has been to gain a greater rigour in proofs and deductions. The development of the algebraic notation in the sixteenth century by François Viète, Pierre Fermat, and others opened a conceptual space for favouring the formal. The advantages of a system of notation that allowed one to focus on the schematic relations between what the symbols represented were enormous: it lessened the risk of making mistakes, but it also facilitated new discoveries as it increased the surveyability of expressions in comparison with mathematical expressions written in prose.[13]

The discoveries of the late eighteenth and nineteenth centuries discussed in chapter 3 also display a turn towards a more formal approach. This time, however, the contrast is found in visual thinking and not in prose. In many historical overviews, the counterintuitive discoveries of non-Euclidean geometry and the existence of continuous, but nowhere differentiable functions have been mentioned as points of conflict between the judgments of geometric intuition (i.e. visual thinking) and the conclusions reached by a more formal, algebraic procedure.[14] Due to the successful development of non-Euclidean geometries especially by Bernhard Riemann and the definitions of concepts central to analysis by Weierstrass and others, the formal approach emerged as the more reliable one. It is understandable that the intuitive aspects of mathematics were sometimes looked upon as a source of error and therefore something to be avoided. A consequence was that the philosophical views of Kant were put to scrutiny, notably by Frege. Thus, at the end of the nineteenth century, one finds several turns towards a more formal approach (e.g. Ernst Schröder's algebraic logic and the axiomatisations by Frege, Peano, and Hilbert, mentioned in section 3.1). All of these, in their own way, influenced the emergence of the modern concept *formal*

[13]Stenlund stresses the importance of the development of the algebraic notation in the seventeenth century. Sören Stenlund. *The Origin of Symbolic Mathematics and the End of the Science of Quantity*. Uppsala: Department of Philosophy, Uppsala University, 2014, Ch. 3.

[14]See e.g. Bell, *The Development of Mathematics*, ch. 13; Uta C. Merzbach and Carl B. Boyer. *A History of Mathematics*. 3rd ed. Hoboken NJ: Wiley, 2011, pp. 533–37; or Stillwell, 'Logic and the Philosophy of Mathematics in the Nineteenth Century', pp. 246–51.

axiomatic system. It is also with thinkers like Frege and Hilbert that the idea of formality as a neutral and infallible sphere becomes more common.

This idea, however, has an early proponent in G. W. F. Leibniz, who also influenced the above mentioned thinkers. He writes about finding the accurate number for each thing or concept. This will enable secure and fruitful reasoning about things. His intention was, however, to apply a mathematical method to areas outside mathematics and thus make use of the certainty that he already found in mathematics in other, less certain, spheres. The advantages he envisages are compared to what a microscope or a telescope offer visual perception.[15]

The comparison to a microscope can be found in Frege's preface to his 1879 *Begriffsschrift* too.[16] In this book, Frege anticipated the discussion of *logical form*, although he did not discuss logical form but rather *conceptual content* (*begriffliche Inhalt*) – hence the name *Begriffsschrift* (concept script). Frege's reason for inventing a concept script was that he wanted to find out if arithmetic could do without references to facts of experience, that is, if the laws of logic – 'those laws upon which all knowledge rests' – were sufficient as a basis for all of arithmetic.[17] In order to find an answer, he tried to see how far logical inferences alone would take him in arithmetic. In attempting this deduction, he found ordinary language to be inappropriate, as it did not allow him to see clearly what was presupposed in each step in the chain of inferences, and in particular if something intuitive (*Anschauliches*) entered into the premisses. Thus, he devised the concept script 'to provide us with the most reliable test of the validity of a chain of inferences and to point out every presupposition that tries to sneak in unnoticed'. Ideally, the concept script should express nothing other than what is of 'significance for the *inferential sequence*'.[18]

Frege does not mention the intuitive as a source of error, but if he is to succeed in his project of showing that mathematics fits the slot 'analytic a priori' in Kant's framework, intuitive elements must not enter into the chain of inferences in the deduction of arithmetic from the axioms of logic.

As mentioned in section 3.1, Hilbert's axiomatisation of geometry in *Grund-*

[15] Leibniz vividly describes the benefits that his method will bring: 'Once the characteristic numbers of a majority of our concepts are determined, mankind will be in possession of a new instrument that will enhance the capacities of the soul far more than optical lenses improve the visual acuity of the eyes, and that will surpass the microscope and telescope to the same extent that reason is superior to visual perception.' Gottfried Wilhelm Leibniz. *Philosophische Schriften*. Vol. 4: *Schriften zur Logik und zur philosophischen Grundlegung von Mathematik und Naturwissenschaft*. Ed. by Herbert Herring. Darmstadt: Wissenschaftliche Buchgesellschaft, 1992, p. 53, my translation from German.

[16] Gottlob Frege. 'Begriffsschrift'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967, p. 6.

[17] Ibid., p. 5. In other words, Frege wanted to see into which slot of the Kantian metaphysical framework arithmetic fit: synthetic or analytic a priori.

[18] Ibid., p. 6.

lagen der Geometrie was formalistic in that it implicitly defined the objects of geometry rather than put down their meaning in definitions. They did not purport to express indubitable *truths* about objects known beforehand, as axioms in the tradition of Aristotle were supposed to. Hermann Weyl testifies to the novelty of Hilbert's conception: 'Before Hilbert constructed his proof theory everyone thought of mathematics as a system of contentual [*inhaltliche*], meaningful [*sinnerfüllte*], and evident [*einsichtige*] truths; this point of view was the common platform of all discussions.'[19]

In his reactions to Hilbert's axiomatisation of geometry, Frege shows himself to be very traditional. In a letter to Hilbert dated 27 December 1899, Frege accuses him of conflating definitions and axioms. Axioms should not contain any words whose meaning has not yet been precisely defined. For Frege, an axiom was still a self-evident truth.[20] In his reply on 29 December, Hilbert gives the following oft-quoted characterisation of his view:

> It is self-evident that every theory is only a scaffolding (schema) of concepts together with their necessary relations, and the basic elements of the theory can be thought of in any way one likes. For example, instead of points, a system: love, law, chimney sweep … for which all the axioms hold, then the Pythagorean theorem also holds for these. Every theory can always be applied to infinitely many systems of basic elements. […] The aforementioned fact is thus no shortcoming (rather a tremendous advantage) of a theory.[21]

With Hilbert's proof theory, the association between meaning and lack of certainty is established. With regard to his request for a proof of the consistency of arithmetic, he explicitly mentions the *contentual* as a source of *uncertainty*: 'if we use contentual axioms as starting points and foundations for the proofs, then mathematics thereby loses the character of absolute certainty. With the acceptance of assumptions we enter the sphere of what is problematic.'[22] What is problematic is, in this context, the paradoxes of set theory. However, Hilbert's aversion to the contentual only concerned proof theory. The point of the consistency proof was, after all, to secure classical mathematics so that research in mathematics could be continued as before. His verdict on the contentual is telling, however.

This shows another contrast between Frege and Hilbert (and the metamathematical tradition that can be seen as a continuation of Hilbert's programme).

[19]Hermann Weyl. 'Comments on Hilbert's Second lecture on the Foundations of Mathematics'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967, p. 482.

[20]Gottlob Frege. *Nachgelassene Schriften und Wissenschaftlicher Briefwechsel*. Ed. by Hans Hermer, Friedrich Kambartel, and Friedrich Kaulbach. Vol. 2. Hamburg: Felix Meiner, 1976, pp. 62–63.

[21]Ibid., p. 69, my translation from German.

[22]Hilbert, 'Problems of the Grounding of Mathematics', p. 228.

The idea behind Frege's concept script was not to provide a means for investigating *meaningless* symbolic expressions. On the contrary it was important that the symbolic formulas expressed truths and falsities. Formalisation was, for him, an attempt to clarify thoughts, and thoughts are not meaningless. The Hilbert school, by contrast, disregards the meaning of particular sentences and signs and devotes itself to the structure that emerges through the implicit definitions that the axioms constitute.[23] The advantage that Hilbert mentions in the quote above is that the structure, the theory, emerges more clearly when one does not at the same time think about possible applications. Interestingly, the applications of mathematics – within mathematics as well as to practical problems – now fall into the background. In the metamathematical tradition, a possible application of a theory is seen as an *interpretation* of the formal theory.

To sum up, we may in the development of the modern notion of formality see two different strands: (1) the emphasis on freedom from meaning, and (2) the emphasis on symbolism. As the concept logical form is taking shape in the early twentieth century, there is also a growing awareness of the fact that this concept is not sufficiently understood.

Frege does not, in his *Begriffsschrift*, explicitly define 'conceptual content'. It is simply 'that which influences [the judgement's] *possible consequences*.' Thus, in the concept script '[e]verything necessary for a correct inference is expressed in full, but what is not necessary is generally not indicated; *nothing is left to guesswork*.'[24] This, according to Frege excludes features of ordinary language such as *how* a proposition is uttered, whether passive or active form is used, etc.

Russell shares Frege's suspicion of ordinary language: 'Because language is misleading, as well as because it is diffuse and inexact when applied to logic (for which it was never intended), logical symbolism is absolutely necessary to any exact or thorough treatment of our subject.'[25]

He also states – in a manner of giving a definition – what he takes logical form to be, namely: 'that, in it, that remains unchanged when every constituent of the proposition is replaced by another.'[26] The logical constants are what remains unchanged and these are equated with logical form; 'in fact, they are in essence the same thing.'[27] This focus on the logical constants when searching for the form of expressions becomes commonplace in the first half of the twentieth century; in combination with the distrust of everyday language, it cements the tight association between logical form and freedom from meaning, on the one hand, and between logical form and symbolic expression, on the other hand.

[23] I am grateful to Martin Gustafsson who helped me to spell out this difference.
[24] Frege, 'Begriffsschrift', p. 12.
[25] Russell, *Introduction to Mathematical Philosophy*, p. 205.
[26] Ibid., p. 199.
[27] Ibid., p. 201.

The discussions concerning the nature of logical form seem to have faded into the background in the 1930s. First order logic seems to have acquired a status of 'the received view' of logical form. This is, to a large extent, a consequence of Gödel's proof of the completeness of first order predicate calculus in 1930 and his of incompleteness proofs of 1931. Together, these proofs pointed towards a divide between first order predicate calculus and other formal axiomatic systems. A consensus emerged to the effect that what is expressible in first order predicate logic is regarded as the basic level of formality. A formal system where the underlying machinery is first order logic comes to be regarded as the basic kind of formal system.

The philosophical bewilderment did not thereby disappear as can be seen in von Wrights inaugural lecture for the Cambridge chair in 1949. He describes logical form in much the same way as Russell: 'We shall say that the variables give to the syllogism its content, and that the [logical] constants give to it its form.'[28] He then shows that the form of a proposition from the perspective of propositional logic will not be enough to recognise what is, from the perspective of predicate logic, a *formal* truth. Consequently, concentrating on the logical constants will not give us any definite answer as to what *form* is. 'The distinction between form and content … is far from clear'.[29] His assessment is echoed in the *Encyclopedia of Philosophy* article on 'Logic, Modern': 'there is still no satisfactory account of logical form'.[30]

I shall now proceed to discuss these three issues regarding formality: the emphasis on freedom from meaning, the emphasis on symbolism, and the unclarity that affects the notion of logical form. It will emerge that formal mathematics (just as much as any other kind of mathematics) is grounded in a practice that involves the ability to use the signs according to established rules, and that no *philosophically* significant line can be drawn between formal and non-formal mathematics.

4.3 Meaningless Signs

To do mathematics formally is often described as a dealing with *meaningless signs*. A striking example is found in Hilbert's claim that 'the objects of number theory are … the signs themselves, whose shape can be generally and certainly recognized by us'. He continues: 'These number-signs … are themselves the ob-

[28] Georg Henrik von Wright. *Form and Content in Logic: An Inaugural Lecture*. Cambridge: Cambridge University Press, 1949, p. 6.

[29] Ibid., p. 13.

[30] Albert E. Blumberg. 'Logic, Modern'. In: *Encyclopedia of Philosophy*. Ed. by Paul Edwards. Vol. 5. New York: Macmillan, 1967, p. 13.

ject of our consideration, but otherwise they have no *meaning* of any sort.'[31]

The idea that we deal with meaningless signs that we furthermore clearly see on a paper is an important feature of many characterisations of formality. A clear example is found in Rudolph Carnap's *The Logical Syntax of Language*:

> A theory, a rule, a definition, or the like is to be called *formal* when no reference is made in it either to the meaning of the symbols (for example, the words) or to the sense of the expressions (e.g. the sentences), but simply and solely to the kinds and order of the symbols from which the expressions are constructed.[32]

Sometimes the formal, symbolic expressions are referred to as *concrete objects*, as in Hilbert's assertion that 'a formalized proof, like a numeral [Zahlzeichen], is a concrete and surveyable object [überblickbarer Gegenstand].'[33]

But what does 'meaningless' mean here? Already in 1923, Aloys Müller voices the objection against the formalists that if they were really dealing with meaningless signs, then the particular form of the signs would have no significance – not the form of individual signs nor the form of series of signs.[34] What Müller seems to be saying is that meaningless signs would not be enough to found a mathematical theory on. Bernays's reply to Müller is, roughly, that number theory disregards the meaning of the individual signs but not the meaning that arises through relations that hold between the signs: '[S]enseless figures are equally capable of such meaning, because of the external properties that are found in them and of the external relationships that can be observed between them.'[35]

What seems to be overlooked in this discussion is that there are two different senses of 'meaningless' in play (and, in consequence, two different senses of 'meaning', but I shall return to that below). In order to spell out this difference, I shall make use of the distinction between *sign* and *symbol* which is found in Wittgenstein's *Tractatus Logico-Philosophicus*.[36] A sign is a physical mark whereas a symbol is 'determined by its place and use in the symbolic system', to use Stenlund's formulation.[37]

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[31] Hilbert, 'The New Grounding of Mathematics: First Report', pp. 202–03.

[32] Rudolf Carnap. *Logical Syntax of Language*. New York: Humanities Press, 1951, p. 1.

[33] Hilbert, 'On the Infinite', p. 383. Cf. also the textbook Joseph R. Schoenfield. *Mathematical Logic*. Reading MA: Addison-Wesley, 1967, p. 2: 'A sentence … is a concrete object, we approach the abstract through the concrete.'

[34] Aloys Müller. 'Über Zahlen als Zeichen'. In: *Mathematische Annalen* 90 (1923), pp. 153–58.

[35] Paul Bernays. 'Reply to the Note by Mr. Aloys Müller, "On Numbers as Signs"'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. by Paolo Mancosu. New York: Oxford University Press, 1998, p. 225.

[36] Ludwig Wittgenstein. *Tractatus Logico-Philosophicus*. London: Routledge & Kegan Paul, 1922 (henceforth cited as TLP), § 3.32.

[37] Sören Stenlund. 'Different Senses of Finitude: An Inquiry into Hilbert's Finitism'. In: *Synthese* 185 (2012), pp. 335–63, p. 352. Cf. also the following description of 'symbol': 'It is the *operational* aspect of a symbol, its function in the calculus, its role in the manipulation and transformation of expressions, which *constitutes it as a symbol*.' Stenlund, *The Origin of Symbolic Mathematics and*

Stenlund claims that these two notions (signs and symbols) are conflated in the tradition of metamathematics.[38] A consequence of this conflation is a very suggestive picture of formal work: the meaninglessness of the signs of the formal system makes them into something pre-mathematical. Dealing with such signs is, therefore, taken to be immune to the distortions and misunderstandings associated with human understanding. This meaninglessness would, supposedly, set such a practice apart from informal mathematics with regard to rigour, reliability, and certainty. It may, therefore, contribute to the wide acclaim that formal work won in the twentieth century and still enjoys. However, this appearance depends on taking 'meaningless' to mean signs that are isolated from a practice, isolated from their place and use in a symbolic system.

The discussion of the meaninglessness, naturally, evokes questions about the concept 'meaning' too. As in the case of 'intuitive', what should be understood by 'the meaning of an expression' is also unclear. I shall not go into a discussion of this problem, but I will mention three different ways in which meaning can enter into the context of the present discussion. One may think of the meaning of a symbol in terms of its reference, e.g. the symbol π can be taken to refer to a particular number. This is also related to the sense in which a formal system can be applied to a particular model. The model is then the interpretation of the system. Another sense of 'meaning' can be seen in the possible practical applications of a mathematical theory (not necessarily a formal system). A third and for this discussion central sense of 'meaning' is the one associated with understanding the use of an expression. This sense can be regarded as the same as Frege's 'conceptual content' or, perhaps, 'logical form'.

In what sense, then, can a symbol be meaningless? In what sense does this differ from signs? One can think of a symbol as meaningless if one disregards whatever reference it has, if one does not pay attention to what the symbol stands for. The symbol π may refer to a particular number, but one can bracket this for the moment and treat it formally, as a meaningless symbol. This does not mean, however, that no understanding is involved in our grasping the symbols or in our using them. We do need a mastery of a technique to be able to use them in accordance with the rules of the system or theory, and even to see them as the symbols they are. Without this understanding, could we even distinguish the signs from, say, accidental marks? The understanding required is, thus, not an understanding of what the symbols may be used to refer to, but of their use in the system, of their meaning in the sense 'logical form'. In order to achieve this skill, a good deal of successful training is of the essence. Moreover, a logical prerequisite for the learning of such a skill is the existence of an established practice

the End of the Science of Quantity, p. 22, emphasis in the original.

[38] Stenlund, 'Different Senses of Finitude', p. 352.

of working with the symbols, a system of rules recognised by the participants of that practice.

In the criticism mentioned above, Müller seems to be talking of 'meaningless' in the sense that a sign, a physical mark, is meaningless. His criticism is correct, I think, to the extent that one cannot ground a mathematical theory in such signs alone. However, what Bernays refers to by 'meaningless' is the meaninglessness that one can connect with symbols, i.e. they are indeed mathematical symbols but one does not at the moment pay attention to what they refer to, or may be applied to. Nevertheless, although treating symbols as meaningless in this way does in many cases have enormous benefits, I do not think that *this* kind of meaninglessness sets formal systems apart from non-formal mathematics in a way that would indicate a qualitative difference in rigour between them.

Within the context of formal systems, this sense of meaningless has a technical, intra-mathematical sense. It means that we have not (yet) provided an interpretation, a semantics. We have not yet specified an interpretation function. However, as noted above, we still understand them as mathematical symbols, otherwise there would be no talk of providing a semantics in the first place. This is aptly formulated by Stenlund: 'To give an interpretation of a sign is to introduce and employ other signs, so there has to be an understanding of signs that is not given by an interpretation. To possess that understanding is to be able to master a certain use of the signs.'[39]

The previous discussion comes to the following: *Formality* cannot be understood to arise from dealing with meaningless signs if this is the meaninglessness of physical marks, because what we deal with are indeed symbols. However, this kind of symbolic work is not unique to formal mathematics but something that is characteristic of all of mathematics. This means that in so far as rigour and reliability are actually present to a greater degree in formal mathematics than otherwise, this cannot be due to its dealing with pre-mathematical physical marks that are passively taken in by the mind.

I shall elaborate on this by discussing Azzouni's comparison between diagrammatic proofs and what he calls 'language proofs': 'We tacitly use our visualization faculties to appreciate the proof-theoretic elements in diagrammatic proofs. What goes unremarked is that we use exactly the same visualization faculties (tacitly) to appreciate the proof-theoretic elements in language proofs as well. What, therefore, makes us persist in seeing diagrammatic proofs as somehow "more intuitive" than language ones?'[40]

Azzouni's rhetorical question is, as I understand it, an expression of the fact

[39] Ibid., p. 355.

[40] Jody Azzouni. 'That We *See* That Some Diagrammatic Proofs Are Perfectly Rigorous'. In: *Philosophia Mathematica* 21 (2013), pp. 323–38, p. 333.

that understanding is needed in order to deal with symbols. That we need an understanding to grasp the gist of a diagram is evident (and they have precisely therefore often been regarded with suspicion), but the same goes for proofs expressed in symbols too.[41] Now, Azzouni describes this as a matter of *visual* recognition. 'Mechanical recognizability … is rooted in our visual powers.' This, together with the term 'mechanical', suggests that Azzouni regards this recognition as something passive. In this respect, there is a similarity between, on the other hand, Azzouni and, on the other, Hilbert and Bernays regarding the attitude towards this recognition. However, the following passage complicates matters: 'the properties of the language-entities I'm describing our recognizing are *conventionalized* ones: they're not the actual physical properties of those entities, or not entirely those properties anyway.'[42]

Obviously, the conventionalised properties that we *see* are features which may be ascribed to a symbol, not to a sign, if one uses the above distinction. This seeing is not a passive perceiving of physical marks. The term 'mechanical' still suggests that this seeing is free from the openness that we associate with interpretation, although what we are recognising are conventionalised properties. These properties are, I believe, their use in the symbolic system.[43] What makes the seeing of this kind of properties 'mechanical' is the background of an established practice. Being fluent in this practice manifests itself in, among other things, the fact that there is one way of dealing with the symbols which is the correct way according to the rules of the practice. Felix Mühlhölzer makes a similar observation: '[E]vidently, pictures and signs (considered as visual shapes) as such do not contain their own *rules* of use – something must be additionally present, so to speak. What is this something? According to Wittgenstein, it is the *use itself* in form of a certain established *practice*.'[44]

The use of the word 'see' is common in ordinary discussions about mathematics, and perceptual metaphors abound. If what 'seeing' refers to is a possible use of the symbols rather than physical features of the signs, a possible reaction might be that 'seeing' is an inappropriate term. While restricting the use of perceptual vocabulary would be an overreaction, I think that this is an indication

[41] One might put this differently: diagrams too are symbols in this respect – a similar kind of understanding is needed in order to benefit from them in a proof, as is needed in order to be able to use a symbol in accordance with the rules of the system it occurs in. That is, one must be able to use the elements of the diagram in accordance with the practice of drawing such diagrams.

[42] Azzouni, 'That We *See* That Some Diagrammatic Proofs Are Perfectly Rigorous', p. 330, emphasis added.

[43] I would, however, be hesitant to call them 'conventionalised', since this term gives the impression that there is room for making changes at will.

[44] Felix Mühlhölzer. 'Mathematical Intuition and Natural Numbers: A Critical Discussion. Review of Charles Parsons' *Mathematical Thought and Its Objects*'. In: *Erkenntnis* 73 (2010), pp. 265–92, pp. 283–84.

of the fact that seeing (in mathematics and, arguably, also otherwise) is indistinguishable from *understanding*, especially in the sense of 'being able to use'.

The following example is intended to show the connection between seeing and a particular usage:

$$2 \mathbin{\&} 4 \text{ make } 6 \tag{4.1}$$

$$3 \mathbin{\&} 6 \text{ make } 9 \tag{4.2}$$

$$2 \mathbin{\&} 4 \text{ make } 6 \text{ qr } 8 \tag{4.3}$$

$$1 \mathbin{\&} 2 \text{ make } 3 \text{ qr } 4 \text{ ip } 5 \tag{4.4}$$

If one considers the expressions (4.1) and (4.2) only, the pattern '$a + 2a = 3a$' will perhaps strike one as the form they share, i.e. one will see them as additions. When (4.3) and (4.4) are added to the array, however, one may get the impression that the two latter expressions are nonsensical. Indeed, they are nonsensical, given the use of (4.1) and (4.2) that first announced itself. A closer study of all four expressions taken together shows another meaningful way of seeing them. They could be the first elements of three different arithmetic progressions which in ordinary notation could be written: '$1, 2, 3, \ldots$' (4.4); '$2, 4, 6, \ldots$' (4.1) and (4.3); and '$3, 6, 9, \ldots$' (4.2).

This example is presented to make plausible the connection between seeing (a logical form) and an ability to use the symbols in a certain way. It shows that what form one sees in a proposition is not determined by the physical signs as such but by my understanding of them, by the way I see myself using them. The example can thus be seen as an illustration of Wittgenstein's claim that '[i]n order to recognize the symbol in the sign we must consider the significant use. The sign determines a logical form only together with its logical syntactic application.'[45] It also shows that it is not possible to build a mathematical theory on meaningless signs.

The example shows that there may be several different meaningful uses of the same signs: we may see different symbols in the signs. How they should be treated is a matter of an established practice. Without this practice it is not even clear that we should read the signs from left to right. Stenlund comments on this: 'Even if we follow explicit, formal rules, this rule-following is rooted in something which is not an explicit rule at all, but a practice of calculation.'[46]

An objection which might be raised against this emphasis on practice is that one could do away with practice if only everything was explicitly stated. The

[45] TLP, §§ 3.326–3.327.

[46] Sören Stenlund. 'The Limits of Formalization'. In: *Logic and Philosophy of Science in Uppsala: Papers from the 9th International Congress of Logic, Methodology and Philosophy of Science*. Ed. by Dag Prawitz and Dag Westerståhl. Dordrecht: Kluwer, 1994, p. 370.

rules of the practice could perhaps be set down in a further set of explicit rules and one could thereby free the symbolism from the particularities of mastering a certain technique. A passage in Azzouni's article can be read in this way: 'In both cases [diagrammatic and language proofs] substantial mathematical content lurks in the proof-theoretic presuppositions that can become explicit when the proof procedure itself is fully characterized.'[47]

However, I do not see this as a possible way of freeing the formal system from its context-dependence. For these new rules would in turn require a similar understanding as the one we just tried to set down in explicit rules. We would arrive at a situation where – to speak with Lewis Carroll – the tortoise could continue to ask for further explications of every new rule that we gave.[48]

Saul Kripke, in his discussion of Wittgenstein's remarks on rule following, argued that Wittgenstein had invented a new kind of scepticism. A scepticism to the effect that since we can never hope to spell out everything involved in the following of a particular rule, there is always room for disagreement concerning what counts as the correct application of a rule.[49] I do not agree with the conclusion Kripke draws. Wittgenstein's remarks could, instead, be understood as showing that the demand for absolute explicitness is misconceived. Juliet Floyd discusses rule following through the example of continuing a number series such as '2, 4, 6, . . . '. Wittgenstein remarks that if there is a philosophical problem concerning our ability to continue that series, then the same problem must also apply to the series '2, 2, 2, . . . '.[50] This could be interpreted in accordance with Kripke's reading. Floyd, however, argues that Wittgenstein wanted to point to the absurdity in the need for further explanations of how we can follow simple rules. Since there is no problem of understanding how to continue '2, 2, 2, . . . ', there need not be any in the case of '2, 4, 6, . . . ' either.[51] Far from resulting in an instability with regard to rule-following, the impossibility of explicitly stating everything involved points towards the fact that some things will always depend on the existence of an established practice of following rules.[52] Furthermore, I do think that such a practice is compatible with the complete certainty that we associate with mathematics. The idea that there could be a form

[47] Azzouni, 'That We *See* That Some Diagrammatic Proofs Are Perfectly Rigorous', p. 332.

[48] Lewis Carroll. 'What the Tortoise Said to Achilles'. In: *Mind* 4 (1895), pp. 278–80.

[49] Saul A. Kripke. *Wittgenstein on Rules and Private Language: An Elementary Exposition*. Oxford: Blackwell, 1982.

[50] RFM, I § 2.

[51] Juliet Floyd. 'Wittgenstein on 2, 2, 2 . . .: The Opening of *Remarks on the Foundations of Mathematics*'. In: *Synthese* 87 (1991), pp. 143–80.

[52] Miriam Lipschütz-Yevick comments on the need for a context in the dealing with formal systems: 'Contrary to the claims of the formalists, *the formal system is no more formal and context-free than are the systems that are to be imbedded in it.*' Miriam Lipschütz-Yevick. 'The Happy (Non-formalist) Mathematician'. In: *The Mathematics Intelligencer* 14 (1992), pp. 4–6, p. 4.

of mathematics which deals with purely physical signs and where certainty is guaranteed by the 'sterile' character of the objects of study would not even get off the ground.

4.4 Advantages of Symbolism

The above discussion concerned the idea that formal mathematics is qualitatively different from informal mathematics. This idea stems from the thought that the former deals with meaningless signs while the latter takes account of their meaning. The point was to show that the difference between formal and informal mathematics is not a difference of kind but rather of degree – at least from an epistemological perspective. In mathematical logic, by contrast, the notion 'formal system' is clearly circumscribed through such definitions as that of *well-formed formula* and of *proof*. Thereby, 'formal' and 'meaningless' acquire a clear, albeit technical, meaning. The difference between formal systems and other axiomatic systems is one *within* mathematics, but it is not a difference that can act as a foundation for the certainty of mathematics.

Why does it seem plausible to turn to formal mathematics for an increase in reliability and rigour? I believe that it is important for a greater clarity with regard to the concept of certainty that one addresses this, as it were, natural reaction. One reason is, surely, the one mentioned by Frege: to make proofs more transparent. Stenlund argues for such an understanding: 'The method of disregarding the traditional content of signs was an efficient tool for making explicit properties that were tacitly used, but not explicitly stated, in contentual reasoning. Hilbert is here using the same feature of the axiomatic method that he had already employed successfully for making explicit "hidden assumptions" in his axiomatization of Euclidian geometry.'[53]

It is also possible to understand the quote from Hajek in a similar spirit. In light of the above discussion, however, this advantage is not exclusive to formal mathematics. It may, perhaps, be said that in formal systems this surveyability is, in one sense, refined. In different sense, however, it is not, as the length of the formal expressions and proofs quickly cancels out the advantage gained through explicitness.[54] Detlefsen also discusses this tension in the concept of rigour and suggests a way of understanding it that is similar to Stenlund's:

[53] Stenlund, 'Different Senses of Finitude', p. 357.

[54] Kreisel also mentions the risk of mistakes in long formal proofs: 'Hilbert sometimes speaks of the reliability (*Sicherheit*) of finitist reasoning. As Bernays has pointed out …, realistically speaking, almost the opposite is true, the chance of an oversight in long finitist arguments of metamathematics being particularly great.' Georg Kreisel. 'Hilbert's Programme'. In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983, p. 211.

> [I]t seems at least possible to think of rigor as linked to explanatory transparency – an inference being rigorous to the extent that its premises can be seen to *explain* its conclusion. The greater such explanatory transparency, the more confident we can be that unrecognized information has not been used to connect a conclusion to premises in ways that matter. To the extent, then, that formalization decreases explanatory transparency, it also decreases rigor.[55]

The advantage that Stenlund identifies in Hilbert's approach was not new nor exclusive to the formal systems that Hilbert started developing in the 1920's. Stenlund observes that Hilbert already utilised the method of making assumptions explicit in his axiomatisation of Euclidean geometry. His *Grundlagen der Geometrie* was published in 1899, and while it is an axiomatic treatment of geometry, it did not develop Euclidean geometry as a formal axiomatic system. It may, however, be described as being more formal than Euclid's *Elements*. Thus, this way of increasing rigour – i.e. making explicit such properties that are otherwise tacitly assumed by disregarding the meaning of the signs – is not exclusive to formal systems. It is a common strategy in modern mathematics when unclarity arises. While it is mainly associated with formal systems and more generally with post nineteenth century mathematics, the possibility of increasing transparency by disregarding the meaning of the signs employed has – I would say – been present at least since the development of the algebraic style of notation in the sixteenth century, by Viète, Fermat, and Descartes.

Mühlhölzer discusses another perspective on the apparent rigour of a formal system such as Hilbert's. It is possible that the system in itself does not allow for any greater rigour, but that the rigour that we are used to in ordinary arithmetic (but not aware of because it is always before our eyes) is *projected* onto the formal symbols. The rigour that we already are in possession of then strikes us as something new. Mühlhölzer remarks that:

> we understand what natural numbers are, and how to deal with them, and we accept the sort of precision and sharpness that reigns in the domain of natural numbers, long before we get acquainted with the Hilbertian 'visual numbers' |, ||, |||, …. And when we then meet this domain of visual numbers, we automatically project our antecedent understanding of numbers into it and measure what is happening in it against what we know from the numbers we are familiar with. In this way we unconsciously also project the latter's sharpness into it.[56]

The two quotes from Stenlund and Mühlhölzer seemingly contradict each other – one speaking of the success of formality and one pointing to the illusory character of this success. But the conflict is only on the surface. One can see

[55] Michael Detlefsen. 'Proof: Its Nature and Significance'. In: *Proof and Other Dilemmas: Mathematics and Philosophy*. Ed. by Bonnie Gold and Roger A. Simons. Spectrum. Washington DC: Mathematical Association of America, 2008, p. 19.

[56] Mühlhölzer, 'Mathematical Intuition and Natural Numbers', p. 283.

both of these observations as consequences of the same observation, namely that the sharpness and rigour that is commonly associated with formal systems is present already in ordinary mathematics and that this is indeed a prerequisite for recognising rigour in formal systems. This, again, means that there is no sharp division between rigorous and not quite rigorous mathematics. The success of Hilbert's axiomatisation of Euclidean geometry shows a genuine possibility of increasing reliability but this possibility is not exclusive to formal systems.[57]

The discussion in this chapter has so far been concerned with refuting one, as it seems, common way of understanding formality, and the notion of rigour associated with it. I shall now draw attention to certain features of logical form that provide a different perspective. One may identify a meaning of 'formal' and 'logical form' which is not tied exclusively to formal systems, and with this understanding of logical form in mind, one may account for the rigour of formality without involving notions such as meaningless signs or formal systems. I am thinking of form as it emerges in the following example where the expressions can be said to share a common form.

$$2+1 \quad = \quad 3 \tag{4.5}$$

$$\bullet\bullet \;\&\; \bullet \quad \text{make} \quad \bullet\bullet\bullet \tag{4.6}$$

Another example is the form, different from the above, shared by the following pair of expressions:

$$2+1 \;=\; 3 \tag{4.7}$$

$$\{\varnothing,\{\varnothing\}\} \;\cup\; \{\{\varnothing,\{\varnothing\}\}\} \;=\; \{\varnothing,\{\varnothing\},\{\varnothing,\{\varnothing\}\}\} \tag{4.8}$$

That these expressions share logical forms is, however, evident only to persons who know the use of them in the relevant context – the examples (4.5) and (4.6) to anyone who has learnt to count and perform simple additions, (4.7) and (4.8) to people who are familiar with the successor function in formal arithmetic and how this function is used to define the natural numbers in set theory.[58] Logical form, in this respect, is aptly described by Frege's 'that which influences the possible consequences' (see the quote on p. 81), although this is not particularly specific. I would say that logical form in this respect is what can be seen in the expressions qua symbols, and this cannot be separated from an understanding

[57] Philip Kitcher has argued that demands for rigour must be understood in the light of advances in mathematical knowledge. What is considered fully rigorous, he claims, is something that changes with the development of new concepts and techniques. Philip Kitcher. 'Mathematical Rigor – Who needs it?' In: *Noûs* 15 (1981), pp. 469–93.

[58] The ordinal '2', for instance, can be defined as the set $\{0,1\}$. Generally, for any ordinal α, its successor $\alpha+1$ can be defined as $\alpha\cup\{\alpha\}$. This is von Neumann's definition of the ordinal numbers in set theory. If 0 is defined as the empty set $\varnothing$, the successor of 0, i.e. 1, is $\{\varnothing\}$, and the successor of 1 is $\{\varnothing,\{\varnothing\}\}$. The expression (4.8) then follows and forms a counterpart of (4.7).

of their use within the system. Wittgenstein writes: 'It characterizes the logical form of two expressions, that they can be substituted for one another.'[59] Form is an essential part of realising when something is interchangeable, equivalent. In this respect, *logical form* is an essential notion in mathematics. Furthermore – to tie this discussion to the previous chapter – this understanding is aptly described as a skill, a 'knowing how'.

It is worth emphasising that form in this respect is not tied to a particular symbolism. One of the ideas of formal symbolisms was, originally, that logical form and logical symbolism should correspond. This is seen in the following remark by Russell:

> Assuming – as I think we may – that the forms of propositions *can* be represented by the forms of the propositions in which they are expressed without any special word for forms, we should arrive at a language in which everything formal belonged to syntax and not to vocabulary. In such a language we could express *all* the propositions of mathematics even if we did not know a single word of the language. The language of mathematical logic, if it were perfected, would be such a language.[60]

The attitude that Russell expresses gives reason for high expectations on formalisation. When formalising in a proper symbolism, ideally, one isolates the features relevant for deduction and expresses them fully and unambiguously. This idea becomes troublesome if one envisages the construction of a symbolic language which is in some sense immune to misunderstandings, a perfect language. The idea that it is possible to express logical form completely in a formula combines easily with the idea that formality is about meaningless signs. One would thereby arrive at a neat picture of the benefits of formality: it is possible to isolate what is essential to a mathematical theory – logical form – and to express this form in mere physical signs. However, as was seen in the above discussion, even if we had a privileged mode of notation which lends itself to expressing logical form better than others, one would still not avoid the need for an ability to use these signs in order to realise what form they express. This applies to any notational system.

Stenlund calls the notion of form exemplified by Russell *the mechanical notion of form*, and he distinguishes this from logical form. The idea behind the mechanical notion is summarised thus: 'It is supposed to be possible to give (at least in principle) a specification of all the external features of the expressions of a language that are relevant to their meaning without referring to or presupposing the meaning or the use of the expressions in this specification.'[61] In contrast, logical form is *not* something that one can isolate from the expressions that show

[59] TLP, § 6.23.

[60] Russell, *Introduction to Mathematical Philosophy*, pp. 200–01.

[61] Stenlund, *Language and Philosophical Problems*, p. 4.

this form and point to in separation from these expressions. Realising that an array of expressions share a common form is about identifying a common use of the expressions, and this means that all one can point to is examples of expressions sharing this form. 'There is no non-circular way of defining it. The form "itself", so to speak, is determined as an existing form of use of [the] symbols', as Stenlund comments.[62]

There are further critical points worth raising with regard to the conception of form under discussion here. In an article with the same title, Etchemendy questions what he calls 'the doctrine of logic as form'. He summarises this doctrine as follows: 'Two sentences cannot differ logically if they do not also differ *formally* or *structurally*.'[63] Etchemendy mentions examples and arguments that speak in favour of the doctrine but he questions the general validity of the idea that logical form and syntactical form coincide.

In addition, Quine's idea of the *indeterminacy of translation* has consequences for the notion of formalisation. In *Word and Object*, he writes that there are no reasons for assuming that formalising a proposition *captures* the form of the original one.[64] This does not, of course, rule out possible benefits of formalisation if the formalised expression comes reasonably close to the original proposition. The virtue of the symbolic expression is, however, not that it is synonymous with the first one, and what is 'reasonably close to the original proposition' is related to the needs that prompted the formalisation in the first place.

I will now return to the question about the possible benefits of formalisation, or increasing formality in mathematics. The first kind of benefit is tied to the insights that can be gained in trying to express something in another (more formal) notation. In struggling with the 'translation', questions about how one should understand a proposition are likely to arise. These benefits, then, result from a conceptual clarification of the concepts involved, but they are not necessarily gained from the end product (the formal expression), but from the process of formulating it. The second kind of benefit is of a more pragmatic nature. This kind has to do with what one is trying to achieve by expressing something more

[62]Ibid., p. 160. Max Black, who otherwise follows Russell, distances himself from what seems to be a consequence of Russell's view on logical form, namely that logical form is equated with a variable propositional function. He quotes Russell, who in the paper 'Philosophy of Logical Atomism', claims: 'I mean by the form of a proposition that which you get when for every single one of its constituents you substitute a variable.' Black then gives the following alternative which is more in line with the one portrayed above: 'the correct view is that the form is what the proposition has in common with the variable propositional functions derived from it by changing all its constituents into variables.' Max Black. *The Nature of Mathematics: A Critical Survey*. London: Kegan Paul, Trench, Trubner, 1933, p. 49.

[63]John Etchemendy. 'The Doctrine of Logic as Form'. In: *Linguistics and Philosophy* 6 (1983), pp. 319–34, p. 320.

[64]Willard Van Orman Quine. *Word and Object*. Cambridge MA: The MIT Press, 1960, § 33.

formally (be it in a formal system or otherwise). Formalisation (or increasing formality in general) usually involves: (1) disregarding traditional content, (2) fixing the use of the symbols employed, and (3) creating surveyable expressions (in the literal sense of surveyable).

All these features can be found in traditional mathematics as well as in formal systems. The increase in rigour that they allow for is genuine, I believe, but it is not of an absolute kind, rather of a pragmatic kind. It is often easier to work with expressions that are formal in the sense of (1)–(3). The first of these was already touched upon above, but it is aptly described by Yehuda Rav as 'squeez[ing] out the sap of meanings in order not to blur focusing only on the logico-structural properties of proofs.'[65] The second – fixing the use of a particular symbol – is similar: one does not have to think about what the symbols refer to. Moreover, when the use of the symbols is fixed, the use of them becomes effortless. In particular, such problems are eliminated as when one is reading a proof and is struck by the question: 'Is this *a* the same symbol as the *a* there?' The third point, naturally, connects with the possibility of surveying expressions and lessening the risk for making mistakes.

However, there seems to be yet another important kind of benefit of formal expressions compared with writing ordinary sentences. When one substitutes symbols for words one advantage lies in a shift from *reading* to *perceiving*. When reading and writing words, the written letters are of secondary importance to the meaning which is expressed. One's attention is turned to what is expressed, away from the letter signs. When looking at formulas, by contrast, the signs – or rather the symbols – come to the fore. It becomes possible to treat the letter and operator symbols in a way that is reminiscent of diagrams in geometry. They are viewed in a manner similar to pictures or diagrams, and the constants and variables become mere placeholders.[66] This idea will also be discussed in chapter 5 in the context of the surveyability of proofs.

Interestingly, the point in the history of mathematics where the greatest increase in formality occurred seems to have been the introduction of the algebraic notation by Viète in the sixteenth century – not the introduction of modern formal axiomatic systems.[67]

[65] Rav, 'Why Do We Prove Theorems?', p. 12.

[66] However, the surveyability of diagrams in geometry are of a different kind from that of algebra, and the risk of making mistakes in judging a diagram is obvious. The kinds of mistake are also different from those common in algebra, and they may be described as a difficulty of deciding whether a property seen in a diagram is essential to any object of the kind pictured or if it is restricted to the particular instance drawn. Cf. Erik Stenius. '*Anschauung* and Formal Proof: A Comment on *Tractatus* 6.233'. In: *Critical Essays*. Ed. by Ingmar Pörn. Vol. 2. Helsinki: Societas Philosophica Fennica, 1989.

[67] Cf. the discussion in Stenlund, *The Origin of Symbolic Mathematics and the End of the Science of Quantity*, Ch. 1 and 3.

4.5 Where is Genuine Mathematics?

The concept 'formal axiomatic system' makes it possible to distinguish between the system and the reasoning about the system – the object language and the metalanguage. Mathematical systems are treated mathematically, but the mathematics that is done on the systems belongs to the informal side. Interestingly, what is taken to represent real mathematics – the object languages – is not where mathematical activity goes on. This distinction also makes the truth definition of Tarski possible and at the same time indispensable. Tarski's definition and the discipline of model theory also cement this distinction. For many research mathematicians in, say, analysis this distinction does not play any important role and research is fruitfully carried out in a fashion that is not formal in the sense of formal axiomatic systems, but still rigorous in light of the established practice of the field.

If one thinks of the informal part of mathematics, it is much easier to become aware of the alternative perspective advanced in chapter 3. If one concentrates mainly on object languages, however, there is no room for the practical matters of mathematical activity. From that perspective, these matters become extra-mathematical, something that we humans with limited capacities struggle with when trying to reach mathematics proper – the axiomatic systems. On this view, then, the insights that can be gained from a study of this struggle become unimportant for the understanding of genuine mathematics; they take the form of psychological and anthropological peculiarities.

To make the thought that mathematics can be reduced to formal systems more plausible, its proponents argue that the different kinds of reasoning that one actually finds in mathematics are nothing but long leaps in the chain of inferences. We find this line of argument already in Frege: 'In proofs as we know them, progress is made by jumps, which is why the variety of types of inference in mathematics appears to be so excessively rich; for the bigger the jump, the more diverse are the combinations it can represent of simple inferences with axioms derived from intuition.'[68]

These different kinds of reasoning are often referred to by opponents of formalism as irreducible and therefore as falsifying the formalist thesis (if one by formalism means the idea that all of mathematics can be reduced to formal systems).[69] The question of whether or not it is possible to reduce every proof technique and method to basic steps in a formal deduction is a complex one. If it is indeed impossible to capture all different kinds of mathematical reasoning in formal deductions, the formalist thesis seems wrong. That *some* kind of formal counterpart to ordinary mathematical reasoning is available seems uncontro-

[68] Frege, *The Foundations of Arithmetic*, § 90.

[69] See Rav, 'Why Do We Prove Theorems?', pp. 14–15 and p. 21 for an example.

versial, but, even though some kind of translation is possible, it is not clear that it is enough to justify the claim that all mathematics is, deep down, formal mathematics. Moreover, even if ordinary mathematics and formal mathematics actually do coincide extensionally, it is not clear what philosophical conclusions that can be drawn from this. That there exists a formal counterpart of some mode of reasoning does not mean that one, for example, could manage with the formal counterpart only.

One may question the conceptual priority that formalists claim these formal counterparts would have over the more complex leaps in reasoning. A certain proof strategy has its point within a proof because it advances the proof in a certain way. (Often, it also makes use of the meanings of the concepts involved, as Rav notes.) If a proof depends on the application of a certain proof technique (which may depend on the meanings of the concepts involved), is it then possible to understand the proof simply by following the logical steps in the formal version of it? That is, would it be possible to understand it, without understanding it as the formal counterpart of a proof employing a certain mode of reasoning? Nicholas Bourbaki makes an important point about the understanding of proofs:

> Indeed every mathematician knows that a proof has not really been 'understood' if one has done nothing more than verifying step by step the correctness of the deductions of which it is composed, and has not tried to gain a clear insight into the ideas which have led to the construction of this particular chain of deductions in preference to every other one.[70]

Moreover, would it be possible to learn a certain proof strategy merely by learning its formal counterpart? I would rather say that it becomes possible to see something as a formal counterpart only when one has mastered the proof technique or strategy in informal mathematics, i.e. when one knows how to use it and understands its working in proofs.

These aspects of informal mathematics are, I take it, an essential part of our mathematics and show that a reduction of mathematics to formal systems is not possible. Mathematics may be reducible to formal systems in the sense that it is possible to find a formal counterpart or translation of every theorem (although this will probably not happen in practice), but this does not necessarily carry any philosophical consequences for our understanding of mathematics.

Azzouni, for instance, sees underlying derivations as a guarantee for the objectivity of mathematics. He claims that it is not possible to explain the important fact that mathematicians tend not to disagree about whether a proof is correct or not unless one assumes the existence of an underlying, mechanically checkable

[70] Nicholas Bourbaki. 'Architecture of Mathematics'. In: *The American Mathematical Monthly* 57 (1950), pp. 221–32, p. 223. I will discuss the significance of the difference between verifying a proof step by step and grasping the overall working of a proof in chapter 5.

derivation.[71] Now, if it was true that the mathematics that we do is the implicit – although not obvious – working with formal systems (or algorithmic systems as preferred by Azzouni) it would seem natural that we would be able to understand theories and proofs by studying formal systems directly. As it is now, we do not. We have to take the 'detour' via our common informal mathematical understanding. That this is the case is obvious to anyone who has learnt (or taught) formal systems.

Moreover, formal deductions very quickly grow out of hand as one tries to formalise more complex (and interesting) theorems. Bearing in mind one of the original goals of formalisation – the desire for a greater rigour – the increased risk for errors is troublesome. It is even more so since increasingly complex deductions are almost impossible to overview and check for accuracy. This is a problematic fact, if one, like Azzouni, claims that underlying derivations are the source of the unanimity of mathematicians. What epistemological gains can these derivations have when it comes to judging the correctness of a proof if the informal proof is generally easier to judge than the supposed underlying derivation?

In a discussion about the relation between ordinary arithmetic and Russell's and Whitehead's *Principia Mathematica*, Wittgenstein makes a comment which captures the above point: 'If I give you a calculation to do, you say that you will do it by Principia. But what if I do it in the ordinary way and get a different result? How do we decide which calculation is correct?'[72] Thus, even if every proof actually had a formal counterpart, what role the formal derivation could play with regard to the objectivity (or certainty) of mathematics is unclear if it is the ordinary counterpart that establishes the result.

I have so far discussed Azzouni's derivations as if those who perform proofs are, in some sense, aware of the derivations that underlie their proofs. Azzouni, however, denies that mathematicians need to be aware of them, at least if this is understood as conscious awareness. Still, in what sense ordinary proofs and derivations correlate does not seem to be properly accounted for. What is clear is that he denies that the derivations are performed on some neurological level. He also explicitly denies that the derivations are abbreviations of proofs, that they capture the logical form of ordinary proofs, and that ordinary proofs are reducible to derivations.[73] Fenner Tanswell remarks that an ordinary proof often has several formal counterparts, e.g. in automated proof checking by computers. Tanswell argues that if the relation between proof and derivation is agent-independent, as Azzouni seems to suggest, then the fact that a proof has several

[71] Azzouni, 'The Derivation-Indicator View of Mathematical Practice', p. 83.
[72] LFM, p. 261.
[73] Azzouni, *Tracking Reason*, pp. 169–173.

formal counterparts makes the link between proof and derivation questionable. Which of the possible derivations is indicated by the informal proof?[74]

These considerations indicate the implausibility of formalism as a theory in the philosophy of mathematics. Moreover, our ordinary practice of mathematics is what philosophy should take as its starting point. In this respect I agree with Shapiro who remarks: 'It is at least prima facie plausible that the language and techniques active in mathematical understanding and explanation are good indicators of the nature of mathematics itself. Indeed, how a given subject matter is grasped should have something to do with what it ultimately is.'[75] This criticism of formalism can also be seen as extending the criticism of the body of truths picture, in particular if mathematics is seen as a collection of formal systems.

The point of this chapter is, again, partly negative. The common view that formal systems are superior to ordinary mathematics because they work with meaningless signs is found to be problematic. In all of mathematics, an understanding which amounts to an ability to use the symbols employed is essential. Thus, the difference between formal and informal mathematics cannot be explained by claiming that one deals with meaningless signs while the other takes their meaning into account. In conclusion, the certainty of mathematics cannot be restricted to formal mathematics. This is important because locating the certainty of mathematics in formal mathematics would remove it from the practice of mathematics.

4.6 Formal and Informal Proofs

I shall end this chapter with a discussion of the relation between formal proofs and ordinary proofs. This will, again, emphasise the necessity of focusing on ordinary, informal proofs in the philosophy of mathematics. If one wants to understand the certainty of mathematics and in what sense proofs contribute to the certainty of theorems, the starting point should be our practice of proving.

The contemporary understanding of proof is to a great extent influenced by the notion of *formal derivation* or *formal proof*. This concept is associated with formal systems and is usually given a strict definition in relation to such a system, as in Stephen Cole Kleene's classic textbook *Introduction to Metamathematics*:

> A *(formal) proof* is a finite sequence of one or more (occurrences of) formulas such that each formula of the sequence is either an axiom or an immediate consequence of preceding formulas of the sequence. A proof is said to be a

[74] Fenner Tanswell. 'A Problem with the Dependence of Informal Proofs on Formal Proofs'. In: *Philosophia Mathematica* 23 (2015), pp. 295–310.

[75] Shapiro, *Structure and Ontology*, p. 186. As was seen in chapter 3, however, Shapiro holds that second-order logic captures mathematical practice in the way I am criticising here.

> proof *of* its last formula, and this formula is said to be (*formally*) *provable* or to be a (*formal*) *theorem*.[76]

If there was an awareness of the difference between more and less strictly regulated proofs before the modern notion of formal systems, there is now a sharp divide between formal and informal proofs. Since proofs of these two kinds often differ considerably from each other, there arises a question of the relation between the two. This has been the subject of controversy for the last fifty years. We can distinguish a range of possible answers to this question.

1. Informal proofs are the genuine kind of proof, but we can sometimes paraphrase an informal proof as a formal one, and this may be useful in proof theory.
2. Formal proofs are the genuine kind of proof, whereas informal ones only gesture towards or abbreviate their formal counterparts. Informal proofs, it is conceded, are easier to read and understand, but these advantages are gained at the expense of rigour. Furthermore, an informal proof can be accepted as a proof only insofar as it can be transformed into a formal one.
3. There are formal proofs and informal proofs, but these are used for different purposes and neither is given priority over the other. Furthermore, informal proofs often, but not necessarily, translate into formal counterparts.

These positions roughly coincide with the view on the question of where to find genuine mathematics that was discussed above. It is interesting that explicit proponents of the second attitude are not too easy to find in the contemporary literature. This is slightly odd since (2) is often alluded to as the traditional conception, suggesting that it is a common view.[77] It is often taken as self-evident by many philosophers. The concept 'formal proof' was first put forward by the formalist school in the debate about the foundations of mathematics in the early twentieth century. For Hilbert, the purpose does not seem to have been to extract the essence of the concept of proof, but rather to allow for a consistency proof of classical mathematics.[78] It seems, however, that the notion of formal proof gradually came to be seen as a correct analysis of proof. Tarski does not

[76] Stephen Cole Kleene. *Introduction to Metamathematics*. New York: D. van Nostrand, 1952, p. 83.

[77] Indeed, Detlefsen labels it 'the common view'. Detlefsen, 'Proof: Its Nature and Significance', p. 17. Resnik calls it 'a popular view'. Resnik, 'Proof as a Source of Truth', p. 12. Hannes Leitgeb comments that this is the way the 'prevailing tradition' treats proof. Hannes Leitgeb. 'On Formal and Informal Provability'. In: *New Waves in Philosophy of Mathematics*. Ed. by Otávio Bueno and Øystein Linnebo. Houndmills, Basingstoke: Palgrave Macmillan, 2009, p. 263.

[78] The view (2) is, however, referred to as the 'Hilbert thesis' in Rav, 'Why Do We Prove Theorems?'; and as the 'Hilbert-Gentzen thesis' in Cellucci, 'Why Proof? What is a Proof?' It is not clear, however, that (2) was Hilbert's view, although he is the main influence behind the notion of formal proof. Sometimes (2) is called the 'formalist' view, and this may be correct if one by formalism means the idea that mathematics is like a game with meaningless signs, but this was, arguably, not Hilbert's view. See e.g. Detlefsen, 'The Kantian Character of Hilbert's Formalism'.

prevaricate in his description of the success of the formalists: 'they had … succeeded in reproducing in the shape of formalized proofs all the exact reasonings which had ever been carried out in mathematics.'[79] A decade later he wrote: 'Due to the development of modern logic, the notion of mathematical proof has undergone a far-reaching simplification.'[80] A contemporary example of (2) could be Jörgen Sjögren who maintains that the notion of formal proof is an explication (in Carnap's sense) of the informal counterpart.[81] Another view close to (2) is Azzouni's that was discussed in the previous section.

During the last forty years or so, the attitude (1) has gained much popularity among philosophers who emphasise the practice of mathematics. The idea that formal derivations correctly capture the notion of proof has, accordingly, received much criticism. This movement partly coincides with quasi-empiricism, and it finds an important source of inspiration is Lakatos's work on proof. As was seen in section 2.6, a noteworthy standpoint of quasi-empiricism is that proofs do not prove conclusively. The title of Lakatos's *Proofs and Refutations* refers to Lakatos's idea that proofs are fallible, but constantly improved through what he calls 'the logic of proofs and refutations'. This idea, however, is not shared by all who emphasise the need for paying attention to mathematical practice. Before Lakatos's work in the 60's and 70's, not many spoke explicitly in favour of (1) – Wittgenstein being a noteworthy exception. This is odd because informal proofs have always been the kind of proof found in the mathematics literature – also after the concept of formal proof became generally known. This is true of the metamathematical literature too.

Formal proofs are a rare species, and occurrences are almost exclusively illustrations of the feasibility of deriving something in a particular system. In other words, these proofs are not used to establish theorems but to show that a theorem that has been established informally is derivable in the system in question. Now, this would seemingly indicate that (1) has always been the common view of proofs. One can say that the discrepancy between practice and the, so called, prevailing view was what led to the increased interest in (1). This has led to the contemporary situation where it seems that most philosophers recognise the importance of informal proofs for the philosophical understanding of mathematics. This attitude ranges from Shapiro's moderate acknowledging that 'with respect to practice formalization is unnatural',[82] to Celluci's dismissing of formal proofs, but also of axiomatic proofs in general. Instead, Celluci favours what he

[79] Alfred Tarski. 'On the Concept of Logical Consequence'. In: *Logic, Semantics, Metamathematics. Papers from 1923 to 1938*. Trans. by J. H. Woodger. Oxford: Clarendon Press, 1956, p. 410.

[80] Tarski, 'The Semantic Conception of Truth', p. 372, n. 17.

[81] Jörgen Sjögren. 'A Note on the Relation Between Formal and Informal Proof'. In: *Acta Analytica* 25 (2010), pp. 447–58.

[82] Shapiro, *Structure and Ontology*, p. 185.

calls 'analytic proofs': 'Axiomatic proof is no viable alternative to analytic proof since it is inadequate.'[83] The reason for the inadequacy of axiomatic proofs, he argues, is that there is no way of ascertaining the axioms, nor any 'non-circular way of proving that deduction from primitive premisses is truth-preserving'.[84] Celluci stresses that advances in mathematics seldom have the form of deducing new consequences from axioms and previously established theorems, and this is valuable criticism. However, his conception of axiomatic proofs seems to be too simplistic, and his criticism therefore misses its mark.

In order to understand the notion of proof and its connection to certainty better, one must start with ordinary, informal proofs. However, I would not give priority to any one of these kinds of proof in terms of 'being a genuine proof', or 'providing certainty'. Whether or not a proof shows its conclusion with certainty is not a matter of its being of a particular kind but a characteristic that is inseparable from its being a proof. Still, in one sense, informal proofs are prior to formal ones: the point of a formal proof can be seen only when the idea of proof in general (i.e. of informal proofs) is grasped. As became clear in the discussion above, one should not put too much weight on the difference between formal and informal mathematics. This means that a formal proof will be recognised to be formal, and possibly more rigorous, only when contrasted with an informal one. From a mathematical point of view, there is a difference to be sure, but the philosophical idea of a purely formal endeavour where no intuitive meaning enters is a chimera. The idea of a greater rigour associated with formal proofs (correctly or not) is only meaningful in contrast with informal proofs, in which steps are often deliberately left out. While this may lead to a mistake, such mistakes are probably exceptions. That steps are left out does not entail that mistakes arise, not even that mistakes are more probable. As noted above, mistakes are not excluded by the formal approach either. That is, whether the gaps need to be filled in, is a matter of what one wants to accomplish with the proof. This turns the attention to the *point* of proving things in mathematics. A philosophical understanding of proofs must focus on informal proofs, but whereas there is a strong consensus as to the nature of formal proof, this is not the case with informal proofs. As Sjögren notes, informal proof can be seen as a *family resemblance* concept.[85] A discussion of this more general concept of proof will therefore be the topic of the next chapter.

[83] Cellucci, 'Why Proof? What is a Proof?', p. 12.

[84] Ibid., p. 12.

[85] Sjögren, 'A Note on the Relation Between Formal and Informal Proof', p. 449. He, however, sees this as an indication that it needs to be made exact through explication.

5. Proof

> Proofs make claims about mathematical objects.
>
> (Michael D. Resnik, 'Proof as a Source of Truth')[1]

> We may make meaning, thought, inference, proof into mysterious achievements that indeed call for philosophical explanation. Seeing them as they are in our life and giving up the desire for such explanations go together.
>
> (Cora Diamond, *The Realistic Spirit*)[2]

The practice of proving propositions deductively is often cited as evidence of the superior certainty of mathematics. In this spirit, the mathematician Hyman Bass writes: 'The characteristic that distinguishes mathematics from all other sciences is the nature of mathematical knowledge and its *certification* by means of mathematical proof … it is the only science that thus pretends to claims of absolute certainty.'[3] This feature of mathematics is indeed interesting, not only because it is limited to mathematics and related disciplines, but also because proofs are philosophically intriguing in their own right. How is it possible to prove something conclusively? What is the relation between understanding and conviction with regard to proofs? What is the relation between truth and provability in mathematics? Thus, 'proof' is a concept that occupies a central place in the problem field of the present investigation. At least prima facie, proofs grant to mathematics 'the peculiar certainty' that puzzled, among others, Mill (cf. the quote on p. 7). The role of proof in the mathematical enterprise is also of great importance for the understanding of mathematical knowledge as the discussion in chapter 3 indicated.

The discussion of proofs will begin by focusing on our need for proofs. What are the reasons to ask for a proof? The need for proof is often associated with a need for conviction and thus a need for certainty. Another central reason to ask for a proof is the need for an explanation of why a theorem holds. As will be seen below, the need for conviction is given too strong an emphasis by some writers. In this chapter, I shall, firstly, discuss the tension between the convincing and the explaining roles of proofs.

[1] Resnik, 'Proof as a Source of Truth', p. 16.

[2] Cora Diamond. *The Realistic Spirit: Wittgenstein, Philosophy and the Mind*. Cambridge MA: The MIT Press, 1991, p. 13.

[3] Hyman Bass, quoted in: Joseph Auslander. 'On the Roles of Proofs in Mathematics'. In: *Proof and Other Dilemmas: Mathematics and Philosophy*. Ed. by Bonnie Gold and Roger A. Simons. Spectrum. Washington DC: Mathematical Association of America, 2008, p. 64.

The discussion of proofs would not be complete, however, without considering the perspective that was already indicated in the Wittgenstein quote on p. 50. This is the idea that a proof is what allows one to understand the proposition, that the proposition regarded in isolation from its proof does not suffice for a genuine understanding of what it says. This idea will be elaborated in the latter part of this chapter.

When this idea is taken into account, we can discern two different conceptions of proof. According to the first of these, a proof is a verification of the truth of a proposition which is understood independently of the proof, as it were, beforehand. The role of proofs thus becomes to show why the proposition is true or to convince that it is true. With this conception of proofs in the background, it is easy to stress the ability of proofs to convince one of the truth of a proposition. This take on proofs, thus, goes hand in hand with the body of truths conception of mathematics.

The other conception sees proofs as integral to the meaning of the proposition. What the theorem says becomes clear only when we grasp how it is proved and what axioms, other theorems, and techniques are employed in the proof. We may have an understanding of the proposition prior to having a proof of it, but this is often a shallow understanding. The proper understanding of the theorem that allows one to use it, for instance in proving other theorems, cannot (in most cases) be achieved without studying a proof. The emphasis on proofs in the teaching of mathematics is an also an expression of this fact. These remarks are a natural continuation of the perspective on mathematical knowledge that was presented in chapter 3. Understanding a theorem is intimately connected to an ability to make use of it, and this ability is furthered by (and often presupposes) understanding a proof, which, in turn, involves a familiarity with the techniques employed.

5.1 The Role of Conviction

As mentioned in section 4.5, the idea that the true kind of proof is captured by the notion of formal derivation has been criticised extensively. Contemporary criticism often gains momentum from questions concerning the roles that proofs play in mathematical practice. Formal proofs are found not to live up to these roles, and, therefore, it is claimed that formal proofs cannot constitute the essence of proofs. One of these roles is that proofs ought to be *convincing*. I will mention some philosophers who emphasise this function of proofs before discussing the question of whether or not this function should be given such a central role.

In one of the articles in the debate that followed upon Rav's article 'Why Do We Prove Theorems?', John W. Dawson Jr. states that 'we shall take a proof to be an *informal* argument whose purpose is to convince those who endeavor to follow it that a certain mathematical statement is true (and, ideally, to explain *why* it is true)', and he continues, 'despite their rigor [formal derivations] are often *unconvincing*'.[4] Dawson's conception of proofs is echoed by Andrzej Pelc in another contribution to that debate: 'we prove theorems to convince ourselves and others that they are true'.[5]

The focus on conviction that is associated with a critique of formal derivations goes back at least to the dialogue 'The Ideal Mathematician' in Philip J. Davis's and Reuben Hersh's *The Mathematical Experience*. When asked for a definition of proof the *Ideal Mathematician*, unable to give a satisfactory answer, in the end claims that 'it's an argument that convinces someone who knows the subject'.[6]

All of the quoted philosophers acknowledge that proofs have other roles in addition to being convincing. Dawson explicitly relates the importance of conviction to proofs' ability to convey *understanding of why* a proposition is true. Hersh also contends that, in research mathematics, the task of proofs is to convince fellow experts, while, in teaching, the point is to explain why theorems are true.[7]

Resnik also emphasises conviction, although his emphasis stems from a different line of thought. Resnik's realist position involves viewing mathematical propositions as being true if they correspond to mathematical reality. As was remarked in chapter 3, the realist position involves the possibility that the extensions of the concepts 'provable' and 'true' do not coincide. This opens a conceptual gap between proved propositions and true propositions, and it becomes unclear how proofs can establish the truth of propositions. He describes the role of proofs thus: 'proving p establishes, shows, or demonstrates p only in the *epistemic sense* of providing us with good reasons for believing p.'[8] Resnik's idea, however, begs the question of how it is possible for a proof to accomplish this. He frames the problematic in two different questions: '[W]hy does proving p induce us to believe p?' and 'Why are the reasons a proof provides good reasons?'[9]

Regardless of the route leading up to the emphasis on conviction, it highlights a philosophical problem: 'What is it about a valid proof that allows it to convince

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[4] John W. Dawson, Jr. 'Why Do Mathematicians Re-prove Theorems?' In: *Philosophia Mathematica* 14 (2006), pp. 269–86, pp. 270–71.

[5] Andrzej Pelc. 'Why Do We Believe Theorems?' In: *Philosophia Mathematica* 17 (2009), pp. 84–94, p. 84.

[6] Philip J. Davis and Reuben Hersh. *The Mathematical Experience*. Boston: Birkhäuser, 1981, p. 40.

[7] Reuben Hersh. 'Proving Is Convincing and Explaining'. In: *Educational Studies in Mathematics* 24 (1993), pp. 389–99.

[8] Resnik, 'Proof as a Source of Truth', p. 10.

[9] Ibid., p. 11.

somebody who has understood it that a proposition is true?' This problem is of particular interest for the present investigation, due to the natural association between being convinced and being certain. Do we in mathematics find a particular form of conviction that is due to proofs and that differs from conviction in other contexts?

From the perspective of formal proofs, there seems to be a straight forward answer to the question about the convincing ability of proofs: 'In a valid proof each step is a sound deduction according to the rules of the system, and its premisses are true.' This does not solve the problem, however, since it remains to be answered how the deductions manage to convince in each step. In addition, the problem still remains for informal proofs. The problem must therefore be handled in some other way.[10]

It should be noted that none of the above mentioned philosophers thinks that proof reduces to mere persuasion. This is seen in their additions to the remarks about the convincing nature of proofs: it should convince *by explaining why* (Dawson), convince *experts* (Hersh), or provide *good reasons* (Resnik). An interesting expression of this is found in Keith Devlin's remark that 'being a proof means having the capacity to completely convince any sufficiently educated, intelligent, rational person.'[11]

The demonstration that a proof provides is important because it gives insights into why the proposition is true and into why the conviction is *justified*. Even though those who stress the role of conviction do not claim that proof is merely about conviction, there seems to be a tacit idea that the main task of proofs is to produce conviction and that they accomplish this task by explaining why. It is as if the role of proofs is merely to put one in a certain position – epistemologically speaking – to the proposition proved. However, the convincing power of proofs become something of a mystery through this approach.

In the didactics of mathematics, there has been a discussion about the role that proofs may play in mathematics. This discussion was motivated by the difficulty of teaching proofs. It is often only a few pupils in each class that manage to understand proofs and the point of proving things. This speaks in favour of

[10] Resnik is also critical of the idea that formal derivations grant a greater degree of certainty to the theorems than ordinary, informal proofs, which he calls 'working proofs'. Shapiro, although noting the unnaturalness of formal derivations with regard to mathematical practice (see the quote on p. 100), seems to hold a different view. He sees formal derivations as 'the ultimate standard of justification' and one can sense that he regards them as superior in terms of convincing power: 'Were one interested in establishing a theorem beyond the doubts of all but the most obstinate skeptic, one would present it as the result of a deduction from (agreed on) axioms or previously established theorems.' Informal proofs, by contrast, are associated with understanding and explanation. Shapiro, *Structure and Ontology*, p. 186.

[11] Keith Devlin. *Mathematics: The Science of Patterns: The Search for Order in Life, Mind, and the Universe*. New York: Scientific American Library, 1994, p. 38.

omitting proofs and spending time on things that benefit a greater share of the pupils. Still, it seems vital to achieve some kind of familiarity with the practice of proving, since this is an essential part of mathematics. This problematic has probably troubled mathematics teachers a long time due to the presence of Euclid's *Elements* on the curriculum, and it probably grew more acute due to the so-called New Math in the United States and similar movements in other countries with their emphasis on proof and other more abstract parts of mathematics.

In response, didactics researchers have discussed the functions of proofs in order to gain insight into *how* and *why* proofs should be taught. If one assumes that proof is about convincing someone of the truth of a proposition, the point of teaching proofs become somewhat obscure. This is due to the fact that the proofs that are simple enough to be accessible for school children often concern matters that are more or less obvious, and this, in turn, makes the point of proving them unclear for the pupils. This is problematic especially in a classroom setting, since it is usually the case that pupils accept things simply because it is their teacher who says them. Acceptance is in most cases based on authority and only partly on critical assessment.

Another reason for the fact that conviction seems out of place when discussing the teaching of proofs is that the pupils are learning to follow a mathematical argument and to understand how one step leads to the next. They are learning which inferences that are possible. They are not (in general) trying to determine *if* what is inferred *actually* follows, as one would if reading a proof with a critical eye. This speaks in favour of Hersh's claim that proofs in the classroom are explaining (while conviction is reserved for proofs in research mathematics). However, what the term 'explaining' involves is not clear. There is a genuine explaining role for proofs to perform at any level of mathematical activity as will be clear shortly. Nevertheless, when proofs are studied in school for the sake of learning mathematical reasoning and presented as examples of such reasoning, their role is not explaining in the same sense as it is for someone who wants to understand why a theorem holds. Another important aspect of learning proofs and learning to prove is that one has to learn to bracket the things that appear as evident at first glance. To the extent that learning proofs actually is concerned with convincing students, it is about learning them a new way of becoming convinced. This learning of a new conception of evidence is vital for the successful entering into the practice of proving.

In one of the first articles in the discussion of proofs from a didactical perspective, A. W. Bell writes:

> Some teachers have said that proof, for a pupil, is what brings him conviction. Although this is a valuable remark, in that it directs attention to the need for classroom explanations to have meaning for the pupil rather than be formal rituals, it is perhaps dangerous in that it avoids consideration of the real nature

> of proof. Conviction is normally reached by quite other means than that of following a logical proof.[12]

One can see in Bell's text the same kind of criticism of formal proofs that was seen in Dawson's article. Formal proofs do not live up to the demands of practical situations. Unlike Dawson and Pelc, however, Bell does not see the function to convince as the primary demand of such practical situations. In order to open up the consideration of the nature of proof, he distinguishes three different functions of proofs: *verification* or *justification*, *illumination*, and *systematisation*.[13] Michael de Villiers adds to these the function of *facilitating new discoveries* and *communication*.[14]

This kind of differentiation of the roles of proofs is a valuable reminder that undermines the idea that the primary role of proofs is to convince the reader of the truth of a proposition. I fear that this idea may be a major obstacle to a sound understanding of proofs and their role in the problem complex surrounding mathematical certainty. I will now turn to a criticism of this idea. It will be seen that different motives may lie behind a request for a proof and that some of them do not necessarily stem from a need to be convinced of the truth of a theorem. Furthermore, proofs are found to perform different tasks and cannot be understood exclusively in terms of conviction.

The situations in which we request proofs vary greatly. Sometimes a proof is needed to reassure oneself that a method one has found is correct, sometimes it is needed to prove a conjecture, sometimes to silence the doubts of others, and sometimes it is needed in order to learn a theorem during a mathematics course.

The need to convince oneself of a proposition is surely a common reason for a mathematician searching for a proof. A mathematician may be working on a proof and realises that the proof could be simplified by referring to a certain lemma. In order to make certain that the main proof is still correct if the lemma is used, she must convince herself that the lemma is valid, and thus she tries to prove it. However, one can distinguish between (1) being convinced that something gives correct results when applied, (2) being convinced that a theorem is correct or true, (3) being convinced that it is possible to prove something, and (4) being convinced that a proof is correct. In light of the distinction between

[12] A. W. Bell. 'A Study of Pupils' Proof-explanations in Mathematical Situations'. In: *Educational Studies in Mathematics* 7 (1976), pp. 23–40, p. 24.

[13] Ibid., p. 24.

[14] Michael de Villiers. 'The Role and Function of Proof in Mathematics'. In: *Pythagoras* 24 (1990), pp. 17–24. For other similar differentiations of the roles of proofs, see Gila Hanna. 'Proof, Explanation and Exploration: An Overview'. In: *Educational Studies in Mathematics* 44 (2000), pp. 5–23, p. 8; Nicolas Balacheff. 'Bridging Knowing and Proving in Mathematics: A Didactical Perspective'. In: *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives*. Ed. by Gila Hanna, Hans Niels Jahnke, and Helmut Pulte. New York: Springer, 2010, p. 130.

'provable' and 'true', one could perhaps add: (5) being convinced that what is proved is true.

In the first case, the conviction may consist in a readiness to use the formula or technique for solving problems, e.g. building a bridge that has to bear the weight of the vehicles that will cross it or giving the correct amount of change to customers. In the second case, it could be about not expecting any contradictions in mathematics if one makes use of the proposition one is convinced of. The third case might concern one's readiness to embark on proving something, or that one is expecting to find a proof in the literature. One is convinced that it will be possible to accommodate the method or proposition within existing mathematics. In this case, the conviction cannot be understood as a result of having read a proof. The fourth case can be an expression of one's confidence in one's ability to judge the correctness of the proof, but it may also be a conviction that the proof follows good mathematical practice: that valid methods of proof have been used, that they are used in the correct way, that the conclusions actually follow, etc. The fifth case is probably limited to controversial cases where there actually is a possibility that one understands the argument of a proof but still is sceptical of what is proved. If one does not accept a divide between 'provable' and 'true', the fifth case does not seem to be a genuine possibility. However, perhaps one could see a possibility for this in new branches of mathematics that are still searching for a common practice regarding the accepted methods of proof. An example that might illustrate this tension is Cantor's response to his proof that the set consisting of ordered pairs of real numbers have the same cardinality as the real numbers, or as this is often expressed, that the number of points in a plane is the same as the number of points on a line. In a letter to Richard Dedekind he wrote: 'I see it, but I don't believe it.'[15]

Conviction may thus play many different roles in mathematics, and only in the second case does one find the kind of conviction that a proof brings about. Even in this case, I would be hesitant to look for a uniform convincing ability of proofs. It is also important that one does not too often say that a mathematical proposition is *true*, nor that one is convinced of its truth. Instead, common expressions include that something is proved, valid, gives the right result, and that it is correct. Using an umbrella expression like 'the proposition is true' makes our dealings with mathematical propositions appear more uniform than it actually is and, moreover, encourages the view of a uniform function of proofs. I claim that the conviction about the correctness of propositions, methods, or calculi must be understood according to the specific context.

To these considerations one can add the observation that the need to be convinced can be fulfilled by something other than a mathematical proof. In certain

[15] Cantor, quoted in: Giaquinto, *The Search for Certainty*, pp. 26–27.

cases, it may suffice that a more experienced colleague tells one that it is correct. This may be the case when one is applying simple methods for practical purposes but perhaps also in more advanced mathematics if one knows that the person one is asking has devoted much time and effort to studying something. In teaching situations, this is probably the most common form of accepting a new piece of mathematics, as was mentioned above. In high school mathematics, techniques and theorems are commonly taught without proof. It may, however, be misleading to speak of conviction when discussing teaching situations because in an ordinary teaching situation conviction or doubt usually do not come into question. The pupils simply try to understand and memorise what their teacher tells them. When doubt arises, the teacher has two options: either the teacher gives a mathematical justification (a full proof or a sketch) for accepting the result or he may simply ask the pupils to take his word for it. Which option he chooses is a matter of the difficulty of the proof and the pupils' ability. In these cases of trusting another, more knowledgeable colleague or teacher, this other person is taking over the responsibility for the proposition or method. In becoming convinced one is so to speak putting one's trust in the other person's ability to prove it.

One may feel that it is only a *superficial* kind of certainty if somebody is convinced by a colleague's or a teacher's authority. It cannot be the rigid kind of certainty that results from having understood a mathematical proof. It seems to me that such an objection conflates *conviction* and *understanding*. It is true that the one who knows a proof of a method knows something else, knows more, than the one who only uses it because, say, a colleague has assured her or him that it works. I believe it is a mistake to assume that the degree of conviction increases with knowledge. The feeling of conviction, the degree of certainty, need not be different in the person who has read and understood a proof from the one who only trusts the teacher. Even one who is wrongly convinced may be as convinced as the one who has read a proof. Wittgenstein comments on the status of conviction: 'The proof convinces us of something – though what interests us is, not the mental state of conviction, but the applications attaching to this conviction.'[16] It is not the feeling of conviction that is decisive for whether someone can use a method or proposition correctly. Being convinced is not the same as having knowledge or knowing how. It seems, however, that this difference easily falls into the background when one is interested in *certain knowledge* and thinks of this as knowledge where one's feeling of conviction is at its peak. This is another indication that – as was argued in chapter 2 – the certainty of mathematics is not a comparative notion. It is not the kind of certainty that is increased by the piling up evidence. This reaction (that there has to be a difference between

[16] RFM, III § 25.

the conviction that proofs bring about and other kinds of conviction) shows that conviction cannot be the central task that proofs perform. Proofs provide something other than a senior colleague's assurance but it is not a greater degree of conviction. That ought to be the moral, otherwise a kind of psychologism would enter into our description of proofs.

The main reason for rejecting the idea that the primary task of proofs is to convince one of the truth of the proposition, however, has to do with the fact that it presupposes the view on proofs that was mentioned in the beginning of the present chapter: that the theorem has a clear meaning as such and we can understand what it would be like if it was true without having read the proof of it. On this view, the proof only lets us understand that it is indeed true and possibly also why it is true, but it does not affect the meaning or our ability to understand the meaning of the proposition. As will be seen later in this chapter, however, not even in the case when a proof is required primarily for the purpose of verifying a theorem can this function be understood as solely convincing somebody of the truth of a theorem the meaning of which is known in advance. I shall return to and elaborate on this idea shortly, but, in order to situate that discussion, I will briefly consider another reasons to ask for a proof or study an already existing proof.

5.2 The Role of Understanding

An important reason to ask for a proof is the need to place a (new) method or proposition in its proper mathematical context and to understand its place in the mathematical theory. It is, arguably, a common reason among mathematicians. When facing a new theorem, especially if one wants to build on it and develop the theory further, it may become important to ponder such questions as: 'What kind of proof has been used to prove the theorem?', 'Are there other similar problems that could be solved by an analogous proof?', and 'Can the theorem be utilised to simplify other known proofs?' This reason to ask for a proof does not necessarily arise from a need to ascertain whether a proposition is correct or not. When it became known that Andrew Wiles had proved Fermat's last theorem in the 1990's, a common reaction among mathematicians was probably a wish to read the proof – not out of a need to convince themselves that the equation $x^n + y^n = z^n$ had no roots for $n > 2$ – but to find out in which parts of mathematics the theorem should be located.

It is also possible to view the re-proving of theorems in this light. If a certain theorem is established by the existence of a proof, it is still considered valuable to find another proof of the same theorem. One of the values of re-proving theorems lies, I would say, in that it opens up a new perspective on the theorem and the concepts involved by placing it in a new context, by establishing new connections to other concepts. This is particularly striking if the proof utilises methods from a different theory than the first. One can, for example, think about the elucidation that different proofs of the Pythagorean theorem gives. Another example is non-standard analysis, where a new understanding of the concept *infinitesimal* is enabled through its embedment in set theory. Furthermore, an aspect of the arithmetisation of analysis and the definitions of such concepts as continuity and convergence in the nineteenth century is that it situates them in one neat theory. It is not only about increasing the rigour with which they are treated (by ruling out talk of infinitesimals) but also about clarifying the meaning of 'real number' and 'continuous function'. In examples like these, the *systematising* role of proofs is highlighted (see the list by Bell on p. 108).

If one considers an open problem, such as Goldbach's conjecture, it is obvious that one can be convinced of its correctness even though there is no proof to study. This is evident already in the correspondence between Christian Goldbach and Leonhard Euler in which the conjecture first occurred. Goldbach had presented his conjecture in a letter of 7 June 1742. In his reply on 30 June, Euler states that he considers it a completely certain theorem that every number is the sum of two prime numbers ('eine summa duorum primorum'), although he cannot prove it.[17] In a case like this the conviction can be expressed in a willingness to search for a proof.

The fact that the mathematical community is not satisfied with the extensive computer testing that indicate the correctness of the conjecture shows that a proof is expected to add something more.[18] This is often expressed thus: the proof eliminates every possible doubt (whereas some doubt may linger as to whether there is some very large even number for which the conjecture does not hold).[19] There is truth to this claim, but there is unclarity too. In the case of Goldbach's conjecture, it is unclear whether there is, in practice, any possible doubt. More importantly, however, the exclusion of doubt is a consequence of the understanding of the theorem that proofs often add. It is this want to understand *why* a theorem holds and how one should understand it that often necessitates finding or taking part of a proof.

[17] P. H. Fuss, ed. *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIéme siècle*. Vol. 1. St.-Pétersbourg: L'Académie impériale des sciences de St.-Pétersbourg, 1843, p. 135.

[18] In the 1930's, the Åbo Akademi University mathematician Nils Pipping verified this conjecture for numbers $n \leq 10^5$, without the aid of digital computers. Nils Pipping. 'Die Goldbachsche Vermutung und der Goldbach-Vinogradovsche Satz'. In: *Acta Academiae Aboensis. Ser. B, Mathematica et physica* 11 (1938), pp. 4–25. By 2013, the conjecture was verified for numbers $n \leq 4 \cdot 10^{18}$. Tomás Oliveira e Silva, Siegfried Herzog, and Silvio Pardi. 'Empirical Verification of the Even Goldbach Conjecture and Computation of Prime Gaps up to $4 \cdot 10^{18}$'. In: *Mathematics of Computation* 83 (2014), pp. 2033–60.

[19] Cf. the quote from Shapiro on p. 20.

The proof of the *four colour map theorem* by Kenneth Appel and Wolfgang Haken in 1976 instigated much debate because it relied on computer calculations that were so extensive that no human being could check if they were in order. If the only thing that mattered was conviction that the theorem is true, this could have been a minor problem. However, the very understanding of what a proof is seemed to be threatened if the Appel–Haken solution was accepted. An essential feature of proofs was felt to be lacking. Thurston remarks: 'I interpret the controversy as having little to do with doubt people had as to the veracity of the theorem or the correctness of the proof. Rather, it reflected a continuing desire for *human understanding* of a proof, in addition to knowledge that the theorem is true.'[20] The proof allows one to understand *why* the theorem holds. As this is taken to be something over and above knowledge that the theorem is true, one can see that Thurston is requesting the *illumination* that a proof can provide (see Bell's list).

A perhaps even more important reason to ask for a proof is that the proof allows for an understanding of what the theorem *says*. Rav notes: 'Proofs are the mathematician's way to *display the mathematical machinery* for solving problems and to *justify* that a proposed solution to a problem is indeed a solution.'[21] Understanding which mathematical machinery that is involved in a theorem is vital to the understanding of the theorem. We may know that it is possible to write every even number greater than two as the sum of two primes but this in itself is perhaps not that interesting. A proof might, by contrast, connect it to certain techniques and concepts and thereby open up new possibilities that the formulation of the conjecture as such does not. It can be difficult to know what to do with a theorem if it is not given a meaningful context. This brings me to the most important theme of this chapter, the connection between proofs and the meaning of theorems.

The discussion in the present chapter has so far been an attempt to show that the importance of proof derives, not from a supposed uniform convincing power, but from its diverse positions in mathematics: as providing conviction and understanding, as a means of communication, etc. If one realises that a proof may play many different parts in the dealings with propositions and calculations in mathematics, a question as 'How does a proof prove?' will have to be countered with another: 'Do you mean "How does a proof convince?" or perhaps "How does a proof further my understanding?"?' I am, furthermore, not convinced that these questions have any general answers that encompass everything we call a proof in mathematics. I also think one should be careful not to see proofs as having a power, or somehow having an effect. That makes proofs appear as self-standing objects, whereas it is important that proving things is

[20] Thurston, 'On Proof and Progress in Mathematics', p. 162.
[21] Rav, 'Why Do We Prove Theorems?', p. 13.

something we do. This activity is not simply manufacturing a proof that has a certain effect on us, but a work on our understanding of the concepts involved in a particular mathematical problem. This applies to someone who is taking part of an already known proof too.

Many of the writers who have emphasised the different roles of proofs do so out of a quasi-empirical perspective. They stress that there is no sharp boundary between mathematics and empirical sciences and that this is because proofs do not carry any absolute verifying power. They draw on Lakatos's idea that proofs are fallible and open to refutation, as are investigations in the empirical sciences. Instead, they often stress that proofs can have the role of furthering understanding. As I, too, want to emphasise this role of proofs, I will indicate where the differences between my view and the quasi-empiricist view lie. I do not think the quasi-empiricist thesis that there is no line to be drawn between mathematical and empirical knowledge is correct. As I see it, there are differences between mathematics and empirical sciences that are important for the philosophical problems about mathematics. These will be illuminated by the discussion on proofs and experiments. As discussed in chapter 2, it is easy to view the different areas of knowledge as a spectrum ranging from less to more certain – with mathematics at the far end achieving full certainty. At least traditionally, mathematics is considered to achieve the highest degree of certainty – where it is the same concept (the same measure) *certainty* that the disciplines are weighed on. It appears to me that quasi-empiricism retains this image of a spectrum, merely placing mathematics a few notches from the top.

5.3 Proof and Concept-formation

As was mentioned at the outset of the present chapter, the possibility of proving propositions in mathematics seems to grant to them a peculiar certainty and to set mathematics apart from other disciplines. Now, although the importance of proof was seen not to be connected to a uniform convincing power, but rather to several different roles that proofs can play, one may still feel a philosophical need to understand what it means to be convinced after having proved something or having read a proof. I will, therefore, continue to a discussion of the connection between the understanding and the conviction that a mathematical proof may give. This undertaking will be aided by a trio of ideas from Wittgenstein's philosophy of mathematics. He emphasises three aspects of proofs: (1) that proofs contribute to the meaning of the concepts involved in theorems, (2) that there is a fundamental difference between proofs and experiments, and (3) that proofs must be surveyable. The first aspect was mentioned already, but Wittgenstein sees it as inseparable from the other two.

The following will involve some exegetical remarks, but that is not the main purpose of discussing the three aspects. What is important is rather that the perspective provided by these aspects allows for an outlook on proofs that differs from the one criticised above – an outlook that, furthermore, is more in line with the emphasis on skill and ability to use that has been a main theme of the previous chapters.

I shall now proceed to a discussion of Wittgenstein's idea that proofs contribute to the meaning of theorems and that proofs form concepts.

He often remarks that accepting a proof means accepting a new criterion for something or a new paradigm for the evaluation of something. He also mentions that it involves adopting a new rule of expression, a new rule of grammar, or a new *concept* of something. Wittgenstein's writings on the philosophy of mathematics are replete with this kind of remark; here are two typical examples:

> 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.[22]

> The proof is now our model of correctly counting 200 apples and 200 apples together: that is to say, it defines a new concept: 'the counting of 200 and 200 objects together'. Or as we could also say: "a new criterion for nothing's having been lost or added".
>
> The proof defines 'correctly counting together'.[23]

A similar example is the discussion in one of his lectures of correlating the fingers of a hand and the points of a pentagram. 'We *accept* this figure as a proof that the hand and the pentagram have the same number', and he comments on the concept-forming role that this acceptance plays: 'I have now changed the meaning of the phrase "having the same number" – because I now accept an entirely new criterion for it.'[24]

The claim that one upon inspection of a pentagram and a hand accepts a new criterion for 'having the same number', and in particular the claim 'entirely new criterion' sounds unwarrantedly strong. This particular formulation comes from published lecture notes, but elsewhere in written manuscripts these formulations are nuanced (a common difference between lecture notes and Wittgenstein's written manuscripts):

> In what sense can a proposition of arithmetic be said to give us a concept? Well let us interpret it … as a … connexion of concepts. … "To give a new concept" can only mean to introduce a new employment of a concept, a new practice.[25]

[22] RFM, I § 32.
[23] RFM, III § 24.
[24] LFM, p. 73.
[25] RFM, VII § 70.

> Mathematics teaches us to operate with concepts in a new way. And hence it can be said to change the way we work with concepts.[26]

When these remarks are taken into account, the pentagram discussion can be said to illustrate how a proof may bring about a new connection between two hitherto unrelated concepts – in this case a geometrical figure and the technique of determining the cardinality of a set. It also shows how connecting these concepts could involve the establishment of a new practice.

I shall now examine three example proofs that illuminate this issue further. The first is taken from a problem that a waitress working in a bar encountered. Her problem was to calculate the correct change to give customers, who often paid with ten-euro or twenty-euro notes. The job was often busy and tiring, and this increased the risk of giving too much or too little change. A method to simplify and speed up this process was therefore valuable. If the order was for €13.20 and the customer gave a twenty-euro note, she had to give €6.80 in return. She formulated a rule for herself: 'The one-euro digit counts up to 9 and the ten-cent digit to 10.' If the order was for €13.20 and she was given €20, she calculated: '3 is 6 short to 9 and 2 is 8 short to 10' and gave the customer €6.80. At first, she had merely noticed that the sum of the order and the change displayed this kind of regularity. Upon formulating the rule, she wondered if the rule actually held true. By devising the following proof, she concluded that it did: The one-euro digit adds up to 9 because the ten-cent digit that adds up to 10 provides the missing euro in the one-euro place. Visually this idea can be demonstrated:

$$\begin{array}{c} 1€ \\ \mathrm{XX.}\boxed{\mathrm{XX}} \\ \downarrow \\ \mathrm{X0.00} \end{array}$$

This is not a proof in the sense that one becomes accustomed to in university level studies in mathematics, but its status as proof of the method is clear. That she accepted this as a proof of her method meant that she was now prepared to use the rule in her work in the bar where it was important that the correct change was given.

Finding the proof to be adequate can be described as adopting a new rule for the calculation of the change or a new criterion for the correctness of the change. In this example the phrase 'adopting a new concept' is perhaps far-fetched, but the proof is essential to a proper understanding of the meaning of this rule. That the one-euro digit should add up to 9 may at first sound strange, but it makes sense when one hears the proof. The proof allows one to understand what the

[26] RFM, VII § 45.

rule says. The understanding of this proof is also facilitated by a familiarity with the algorithm for adding numbers. The proof's wording about the missing euro that is supplied by the cents' adding up to a whole euro is easily understood by someone who has learnt the addition algorithm and the technique of writing a *carry number* if the addition in a particular column exceeds nine. The proof thus merges the new rule with a familiar technique and in this way extends the familiar. In a lecture, Wittgenstein remarks concerning a proof's ability to extend the familiar: 'A mathematical proof persuades us by making certain connexions. It puts [a proposition] in the middle of a huge system – it gives it a place.'[27]

I will now consider the Bolzano-Weierstrass theorem from analysis – a more typical example of a proof – and it will be seen that Wittgenstein's perspective is illuminating in cases taken from higher mathematics as well. There are different formulations of the theorem, some involving the concept of an *accumulation* (or *cluster* or *limit*) point, some involving sequences and subsequences. The following version is from Colin W. Clark's textbook *Elementary Mathematical Analysis*. An accumulation point can be defined thus:

Definition. *Let S be a subset of $\mathbb{R}$. A point $x_0 \in \mathbb{R}$ is an accumulation point of S if in every deleted neighbourhood of x_0, there exists a point $x \in S$, i.e. that*

$$\forall \varepsilon > 0 \; \exists x \in S : \; x \in (x_0 - \varepsilon, x_0 + \varepsilon).$$

Utilising this definition, and borrowing the elegant formulation of Clark, one has:

Theorem (Bolzano-Weierstrass). *A bounded, infinite set of real numbers has at least one accumulation point.*[28]

When the theorem is formulated using the notion of accumulation points the proof is clear and uncomplicated.

Proof. Let $S \subset \mathbb{R}$ be bounded and infinite. Since S is bounded there is an interval $I_1 = [a_1, b_1]$, such that $S \subset I_1$. If the interval I_1 is halved there are two intervals

$$[a_1, c_1] \text{ and } [c_1, b_1], \text{ where } c_1 = \frac{1}{2}(a_1 + b_1).$$

Since S is infinite, (at least) one of these intervals contains infinitely many points of S. Choose the one that contains infinitely many points of S and call it $I_2 = [a_2, b_2]$. The interval I_2 can, in turn, be halved, and the process of halving the intervals can be continued indefinitely because the interval that contains infinitely many points is chosen in each halving. Two sequences $\{a_n\}$ and $\{b_n\}$ are

[27] LFM, p. 134.
[28] Clark, *Elementary Mathematical Analysis*, p. 115.

thereby formed, the sequence $\{a_n\}$ being increasing and $\{b_n\}$ decreasing. This is due to the construction of the intervals where

$$a_n \geq a_{n-1} \text{ and } b_n \leq b_{n-1} \text{ for every } n.$$

These two sequences converge because they are bounded and increasing (decreasing).[29] Furthermore, the length of each of the intervals will be half of the previous one:

$$b_n - a_n = (\frac{1}{2})^{n-1}(b_1 - a_1).$$

Therefore, the sequences a_n and b_n both converge to the same number, call it c, since

$$\lim_{n\to\infty} (\frac{1}{2})^{n-1}(b_1 - a_1) = 0.$$

That the sequence $\{a_n\}$ converges to c means that there is an N such that if $n > N$ then $a_n \in (c - \varepsilon, c + \varepsilon)$ for any $\varepsilon > 0$. Moreover, every interval $[a_n, b_n]$ contains infinitely many points of S, and, in consequence, every deleted neighbourhood of c contains at least one point of S (indeed, infinitely many points of S). The convergence point c is therefore the required accumulation point of S. □

As a corollary to this theorem one can prove that *every bounded sequence has a convergent subsequence*. This corollary is in many presentations of analysis taken to be the Bolzano-Weierstrass theorem. There are many different proofs of the corollary (or theorem), but the main idea is to show a way of constructing a *monotonous subsequence* from a *bounded sequence* and then use the property that bounded, monotonous sequences converge (or show that the subsequence is a Cauchy sequence) in order to show that the subsequence converges.

This theorem is a good example of when a proof shapes the meaning of the concepts involved. What it means for a bounded, infinite set to have an accumulation point is not clear from the proposition as such. One may of course form a visual image of a line with a dense cluster of points somewhere, but this kind of image is not, as such, a mathematical understanding of the theorem. It needs support from mathematical work, otherwise it risks being misleading.

The notion of accumulation point, as it is defined above, is not obviously connected to the convergence of sequences. If one compares the definition of accumulation point with the definition of convergence, the connection is clear. However, this clarity depends on a familiarity with the technique of showing convergence for sequences using the ε-notation. The proof shows the meaning of the concept 'accumulation point' by showing that, and how, such a point is a

[29] That a bounded and monotonous (increasing/decreasing) sequence converges is either taken as an axiom of analysis or proved as a theorem if another axiom such as the least upper bound property replaces it.

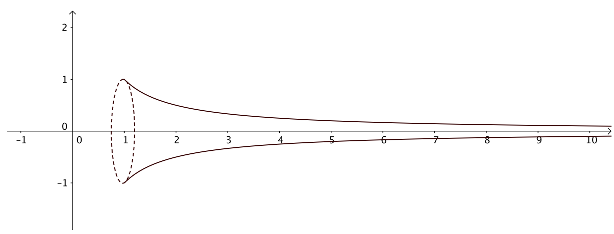
convergence point of a sequence. The proof is, arguably, more important for the concept of accumulation point than the definition.

Taking the corollary into account, one can see that the concept of sequence is also illuminated. A sequence is not necessarily convergent merely by the fact that it is bounded, but boundedness is enough for a subsequence to converge. Additionally, a consequence of having read the proof is that collections of infinitely many points located on some particular limited portion of the continuum can be handled by viewing the points as elements of a sequence.

On a larger scale, this theorem, together with other theorems and definitions concerning convergence and continuity, establishes a connection between converging sequences and the notion of continuity. In short, it illuminates the continuum, and it does this by establishing techniques that enable problem solving and the proving of further theorems.

The third example proof also illustrates the issue of concept-change. It is sometimes referred to as the 'horn of Gabriel' and sometimes as 'Torricelli's trumpet'. It is often presented as a surprising and counter-intuitive result, and it gave rise to considerable debate among mathematicians, as well as philosophers, when Evangelista Torricelli showed the features of the horn in 1641. Among other things it spurred questions about the infinite but also about the ontological nature of mathematical objects. The following is a modern variant of Torricelli's theorem. Torricelli calculated the area and volume of a similar solid using the technique of indivisibles that his teacher Bonaventura Cavalieri had developed.[30]

Theorem. *If the hyperbola $y = \frac{1}{x}$ rotates around one of its asymptotes, e.g. the x-axis, and the values of the abscissa are limited to $x \geq 1$, an infinitely long trumpet-shaped solid is formed. The area of this solid is infinite, whereas its volume is a finite number.*



Proof. The area can be determined by calculating the following improper Riemann integral:

$$\lim_{a\to\infty} \int_1^a 2\pi \frac{1}{x} dx = \lim_{a\to\infty} 2\pi \Big[\ln x\Big]_1^a = \infty.$$

[30]For details, see Paolo Mancosu. *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*. Oxford: Oxford University Press, 1996, ch. 5.

The area is thus infinite. The volume is calculated analogously but is finite and equal to π:

$$\lim_{a\to\infty}\int_1^a \pi(\frac{1}{x})^2dx = \lim_{a\to\infty}\pi\Big[-\frac{1}{x}\Big]_1^a = \pi.$$

□

The reaction when hearing that the length and the area are infinite but the volume, nevertheless, finite is perhaps that one wants to see a proof. It is a natural reaction of disbelief that one wants to hear the arguments in favour of what one questions, but it also signals that the proper understanding of the proposition will be derived from understanding the proof. This example also shows how the acceptance of the proof involves a change in concepts. Furthermore, in this case the change takes place on two levels. In a discussion of Torricelli's trumpet, Kajsa Bråting and Anders Öberg draw attention to a shift in the concept of volume that occurs and that this shift 'was codified through the methods that were used for the calculations.'[31] They remark that the concept of volume underwent a shift due to the new possibility of calculating volumes, i.e. the method of indivisibles. On this level, the shift concerned the concept of volume in general: it extended what one was prepared to count as a volume. However, not all of Torricelli's contemporaries were prepared to accept the method of indivisibles because of the consequences of its application on the infinitely long solid. For those who did, volume was now something one could ascribe to infinitely long objects, and this is clearly an addition to the ordinary concept of volume.

The strangeness of this proof can at least partly be explained by this shift in concepts. What one expects of the concept of volume as it is used in everyday applications is not applicable to the trumpet. If one thinks of a plugged funnel, infinitely long and made out of an infinitely large tinplate, with an opening diameter of 20 centimetres, the idea that one could fill it up to the brim by pouring about 3.14 litres of water into it sounds paradoxical. A reminder that the concept 'volume' is altered here and that what we call volume with regard to the trumpet is a different although related concept of volume can temper the feeling of paradox.

On another level, there is a more local change of concepts. When reading and accepting the proof, one accepts the values of the integrals as the values of the area and volume of the solid. That one understands the proof means that one grasps how the values of these integrals can be the area and the volume, respectively, of this solid. Again, grasping this depends on a prior grasp of the techniques utilised in the proof.

[31]Kajsa Bråting and Anders Öberg. 'Om matematiska begrepp – en filosofisk undersökning med tillämpningar'. In: *Filosofisk tidskrift* 26.4 (2005), pp. 11–17, my translation from Swedish.

Another historical example where a concept is changed as a consequence of a proof is Cardano's proof that an equation can have an *imaginary* solution (see p. 52). This case shows that it is even misleading to say that concepts are changed *due to* an understanding of the proof, because *seeing* it as a proof of a proposition cannot be separated from experiencing a change in one's conception of, in the Cardano example, *root*.

Thus, the issue of concept-formation cannot be described thus: we have a problem which we understand, we obtain a proof, and now our concepts change because of the proof. Wittgenstein's formulations sometimes suggest such an understanding. This understanding of the concept-forming role of proofs bears similarities to the view of proofs having a convincing effect on the reader, only, now the proof has a concept-forming effect. I would rather say that understanding *manifests* itself in a shift in the concepts.

That proofs contribute to the meaning of concepts can also be connected to the comparison between mathematical propositions and rules. That our understanding of the concepts involved is changed as we grasp a proof will influence what we consider to be the correct use of these concepts. Consequently, the theorem will play a normative role. The other two aspects of proof discussed below will elucidate this remark.

5.4 Proof and Experiment

The second of the ideas of Wittgenstein under scrutiny here is present throughout his philosophy. He repeatedly emphasises the difference between a mathematical proof and an experiment by calling the former a picture of an experiment. 'I might say: the proof does not serve as an experiment; but it does serve as the picture of an experiment.'[32] This comparison is also found with respect to *calculations* and experiments: 'It is enlightening to look on a calculation as a picture of an experiment.'[33]

If a mathematical proposition is seen as a *true description* about something, as it would from the body of truths perspective, it may seem as if one could convince oneself of its truth either by making experiments or by proving it – and the difference in certainty would only be one of degree. If one thinks of an example

[32]RFM, I § 36. In a lecture, a similar remark is found: 'One might say that this figure is not an experiment but the picture of an experiment. A picture or film of an ordinary experiment is not the same as an experiment.' *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, p. 72. Throughout *Remarks on the Foundations of Mathematics*, proofs are also often viewed as models, paradigms, and patterns. In *Philosophical Grammar*, Wittgenstein gives this difference a strong emphasis: 'Nothing is more fatal to philosophical understanding than the notion of proof and experience as two different but comparable methods of verification.' PG, II, V, 22, p. 361.

[33]LFM, p. 98. In this form, the remark is found already in *Tractatus Logico-Philosophicus*: 'Calculation is not an experiment.' TLP, § 6.2331.

such as Goldbach's conjecture, the difference might lie only in the generality of the proposition: whereas a proof will show exactly for which cases the proposition holds (perhaps for all cases), experiments will not tell one where the range of applicability goes. The point of both procedures would be to *convince one that something is the case*.[34]

There are features of proofs and calculations that indicate an essential difference between experiments and proofs. Wittgenstein draws attention to the fact that while a picture of a proof is still a proof, a picture of an experiment would not be an experiment.[35] This remark, in all its seeming simplicity, points to a fundamental difference. A proof is not tied to any particular physical circumstances, whereas an experiment is something located in time and space. Hertzberg has suggested an example which also serves to illuminate this. Suppose that I find a drawing consisting of a series of pictures of somebody performing certain actions; if I can see a proof in these pictures, it does not matter who has drawn the pictures, whether this person is reliable or not, it will still be a proof. On the other hand, if I in the pictures see a report of an experiment, the value of this report is affected by the reliability of the author.[36]

Another noteworthy difference is that if we conduct an experiment we await the outcome, take note of it, and, in a sense, we have to accept the result whatever it is. When performing calculations and especially when proving something, one does not await the result. In calculation, we might make sense of 'taking note of the result', but the acceptance of the result is always dependent on having calculated *correctly*, and this introduces another dimension into the activity of proving and calculating.

It may be objected that there are also wrong and right outcomes of an ex-

[34] Arthur Jaffe and Frank Quinn claim that 'the role of rigorous proof in mathematics is functionally analogous to experiments in the natural sciences.' Their claim seems, at first glance, to run counter to the difference emphasised here. However, a look at the similarities they point to reveals that the disagreement is only on the surface. The subject of their discussion is the status of speculative or conjectural mathematics. They note that proofs, as well as experiments, function as a rein on the more speculative parts of the respective sciences. That proofs and experiments both function as the final arbiters of truth does not mean that they play this role in the same way. Frank and Quinn make no claims about the logical similarities or differences between proofs and experiments. Arthur Jaffe and Frank Quinn. '"Theoretical Mathematics": Towards a Cultural Synthesis of Mathematics and Theoretical Physics'. In: *Bulletin of The American Mathematical Society* 29 (1993), pp. 1–13, quote on p. 2.

[35] Ludwig Wittgenstein. *Wittgenstein's Nachlass: The Bergen Electronic Edition*. Oxford; Bergen: Oxford University Press & Wittgenstein Archives at the University of Bergen, 2000, MS 127, p. 169. MS 127, together with MS 126, are the sources for part V of *Remarks on the Foundations of Mathematics*. This remark has been left out by the editors. In a lecture, he makes a similar remark: 'The description of the proof is the proof itself, whereas to find the thing at the North Pole [it is not enough to describe it]. You must make the expedition.' Ludwig Wittgenstein. *Wittgenstein's Lectures, Cambridge 1932–1935*. Ed. by Alice Ambrose. Oxford: Blackwell, 1979, p. 7.

[36] Personal discussion. Cf. also Wolgast, *Paradoxes of Knowledge*, pp. 107–08.

periment. This is true thus far: if there is an unexpected outcome one will probably conclude that something is interfering with the experiment. For example, the scales were not calibrated or the containers were contaminated. However, if the circumstances of the experiment are controlled, the outcome is as good as any. A deviation in the outcome of an experiment is as interesting as the expected one (arguably more interesting). In the case of a surprising outcome, it is surprising because one cannot fully overview the causal processes of an experiment. The task is then to give an explanation as to why this outcome is possible – to incorporate it into the scientific theory. The point of a scientific theory can be described as a way of coming to terms with this inability of ours to see the processes of nature, and experiments are a means for gaining insight into these processes. Von Wright identifies two attitudes to causal relations: an active and a passive one, and he observes that experiments exploit both. 'The active component is the putting in motion of systems through producing their initial states. The passive component consists in observing what happens inside the systems – as far as possible without disturbing them.'[37]

A related remark is made by Mühlhölzer, who notes that the identity of experiments and proofs, respectively, is determined in different ways. In order to repeat the same experiment, one has to set it up in the same way, under the same conditions as the experiment one wishes to repeat. The identity of a proof, by contrast, requires that the result is repeated.[38] Calculation and proof are not examples of passive observation. If one did not overview the exact path to the result of a calculation, it would not be a calculation at all. There cannot be any interesting deviations in the results of a calculation – a deviation is a mistake. Likewise, there cannot be any interference that causes a calculation to give out another result. If something interferes with the person who is calculating or drawing a conclusion, it is not the calculation that gives a different result, it is the calculator who was brought out of concentration and made a mistake.[39]

Yet another objection could be that one has to accept the result of a calculation just as much as that of an experiment. One cannot *decide* that it is right or wrong. This is kind of criticism is commonly brought up against ideas that put realism into question (cf. p. 55). It is true that one will have to accept the result of a calculation too, and calculating is often described as an activity where

[37] Georg Henrik von Wright. *Explanation and Understanding*. London: Routledge & Kegan Paul, 1971, p. 82.

[38] Felix Mühlhölzer. '"A Mathematical Proof Must Be Surveyable": What Wittgenstein Meant by This and What It Implies'. In: *Grazer Philosophische Studien* 71 (2005), pp. 57–86, p. 60.

[39] This is also the reason for Wittgenstein's controversial claim that surprise with regard to a result in mathematics is a sign that something is not understood. See RFM, App. II. For an interesting discussion of surprises in mathematics, see Felix Mühlhölzer. 'Wittgenstein and Surprises in Mathematics'. In: *Wittgenstein and the Future of Philosophy. A Reassessment after 50 years. Proceedings of the 24th International Wittgenstein-Symposium, 12th to 18th August 2001 Kirchberg am Wechsel*. Ed. by R. Haller and K. Puhl. Wien: öbv & hpt, 2002.

one proceeds according to the rules and sees where they take one. There are two important things to notice in regard to this observation, however.

Firstly, just as one has to judge whether the outcome of an experiment is plausible or not in order to know if one should accept it or look for possible disturbances, one has to judge whether the result of a calculation is correct or not too. Since it is possible to be mistaken in calculation or inference, there is a need for checking the outcome, but this is a different kind of checking. To evaluate an experiment is to judge whether or not the set-up is correct in order for the causal process to take place unhindered. To check a calculation is to judge whether the rules have been followed correctly in each step. One is not checking the surroundings of the calculation but the process itself. The result that one has to accept is completely determined by the correct application of the rules. As will be discussed below, what the correct result is must be completely perspicuous in the calculation. In an experiment, one has to work out a theory to explain the outcome.

Secondly, the objection invites the comparison between oneself and a computing machine, which, when given a certain input, gives out a definite output. Is it not possible to give oneself a task to calculate something, perform the calculation under favourable circumstances according to the rules, and then observe the result? Could this not be called an experiment? Furthermore, would the possibility of calling it an experiment imply that the difference between calculation and experiment is not so fundamental after all? If one did this but entered the task into a calculating machine or a computer instead of doing it by hand, this could be described as an experiment. The result of the experiment would then be the answer the machine gives out. One could also perform a similar experiment with many machines; the experiment could be to test whether all of the machines give out the same answer or not. The result of such an experiment would not be the numbers that appear on the screens, but either 'yes (they give out the same number)' or 'no'. One could make a similar experiment with humans, testing if a group of people come to the same result when given a certain calculating task. Again, the result of the experiment would not be the result of their calculations, and the value of the result of the experiment would not suffer if the results in the calculations are wrong. Could I then make an experiment by giving *myself* a task and test what comes out? Wittgenstein writes: 'It is the use that is made of something that turns it into an experiment.'[40] Could one view the situation of self-testing as an experiment? This may be a genuine possibility, but my attitude to the result of the calculation and thus to the experiment would be different from the case where I simply do the calculations to find out the correct result. The following comment by Wittgenstein sheds light on the difference: 'If a proof

[40] RFM, I § 161.

is conceived as an experiment, at any rate the result of the experiment is not what is called the result of the proof. The result of the calculation is the proposition with which it concludes; the result of the experiment is that from these propositions, by means of these rules, I was led to this proposition.'[41] It is important to notice that, from the experiment-perspective sketched in the quote, there is room for a further evaluation: 'Is it correct?' When I make this evaluation and the answer is 'yes' – this turns it into a proof. The realisation that it is correct is a realisation that this *had to be* the result. This observation relates to the comparison between mathematical propositions and rules, and the connection will become clearer still when the surveyability of proofs is taken into consideration.

A recurring theme in Wittgenstein's writings on rule-following is the picture of a mechanism and his remarks that following a rule is not like the working of a rigid mechanism. This distinction is parallel to the one between proof or calculation and experiment.[42] The idea in critical focus is the following: rules – if we follow them correctly – compel us to behave exactly as they prescribe, as rigidly as the causal laws of nature determine how a physical mechanism behaves. Wittgenstein writes that one easily pictures a kind of mechanism behind our calculation that determines, as it were, the movements of our calculations and inferences. In *Philosophical Investigations*, he mentions that this picture is a false analogy. One forgets that machines break and malfunction. It would therefore be necessary to picture calculation as a super rigid mechanism.[43] When one makes a mistake in calculation – is that a result of the machinery malfunctioning? How does one in this case know when it does its job as it should and when it does not – i.e. malfunctions? The distinction between malfunction and mistake cannot be drawn when one is calculating. The picture one has of oneself as a calculating machine that follows simple mechanical rules is not an innocent one. That it still seems to capture something central to our calculating practices is a consequence of what a rule is. As Wittgenstein writes: that one feels completely compelled to do what the rule demands is simply a feature of having understood the rule.[44] This does not require a causal determination. '"Mechanical" – that means without thinking. But *entirely* without thinking? Without *reflecting*.'[45] The discussion of mechanisms and rule-following can thus be seen to show yet another aspect of the difference between proof and experiment as methods of showing the truth of something.

The risk of conflating experiments and proofs may seem somewhat remote, and one may wonder why so much attention is devoted to the distinction. One

[41] RFM, I § 162.
[42] In part VII of *Remarks on the Foundations of Mathematics*, they even merge. See § 73.
[43] PI, § 193.
[44] PI, § 231.
[45] RFM, VII § 60.

reason is that it sheds light on the nature of mathematical propositions as discussed above. Another reason is found in Wittgenstein's treatment of the question of how rules determine one to follow them in a particular way. I shall not go into this problem here, but I mention it due to its connection with the main theme of this thesis. The certainty of mathematics is sometimes thought of as arising from the force of logical inferences, from the idea that the laws of logic force us to infer in the only possible way. This idea is analogous to the view of proof and calculation as being on a par with experiments. The laws of logic appear compelling in the same way as the laws of nature – only even more compelling. Since Frege appears to have held such a conception of the laws of logic, dealing with this view is important to Wittgenstein.[46] From this perspective, there is an inclination to view calculation and inference as something that happens beyond one's control. The one who infers is, as it were, compelled much like one is compelled by causal processes.[47] Thus Wittgenstein writes: 'Do not look on the proof as a procedure that *compels* you, but as one that *guides* you. – And what it guides is your *conception* of a (particular) situation.'[48] As discussed above, the proof is involved in determining the meaning of concepts, but not in the sense that the proof has an effect on the reader. For something to guide my conception, in contrast to compel me, it must be possible for me to follow the guidance, it must be possible to overview it. This brings me to the third ingredient in Wittgenstein's discussion of proofs.

5.5 Proof and Surveyability

The third aspect that Wittgenstein emphasises is that proofs have to be surveyable.[49] This aspect is not separable from the two previously discussed: the

[46] Gottlob Frege. 'The Thought: A Logical Inquiry'. In: *Mind* 65 (1956), pp. 289–311, pp. 289–90. Cf. also LFM, p. 214. Frege's view on the laws of logic (laws of thought) has attracted much discussion, but I will not go into the issue here. It is unclear whether he viewed these laws as constitutive of thought in the manner of Kant, or whether he saw them as merely prescribing how thought should look if it is to be proper thought. It seems that he either wavered between the two views or simply changed views at some point. Cf. James Conant's introduction to Hilary Putnam. *Words and Life*. Ed. by James Conant. Cambridge MA: Harvard University Press, 1994.

[47] It is also in this context that the remarks on rigid mechanisms appear.

[48] RFM, III § 30.

[49] Regarding terminology, it is worth mentioning that Wittgenstein uses the words 'übersichtlich', 'übersehbar', and 'überblickbar'. In the English translation, these are translated as 'perspicuous', 'surveyable', and 'possible to take in'. Mühlhölzer comments that Elizabeth Anscombe's choice of English counterparts is problematic because 'perspicuous' and 'take in' imply that an understanding of the proof is involved. Moreover, the requirement that a proof be perspicuous carries connotations to 'ease of understanding'. The German words do not, according to Mühlhölzer, presuppose understanding, and as Wittgenstein uses them interchangeably he suggests using 'surveyable' as the translation for all of them. Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', pp. 58–59. I will, therefore, use 'surveyable' when discussing this aspect, but the

concept-forming nature of proofs and the difference between proof and experimental verification. It must be possible to see from the proof how the concepts involved are connected. The affinity is explicitly mentioned: '"Proof must be capable of being taken in [surveyable]" really means nothing but: a proof is not an experiment.'[50]

In the above discussion, it was emphasised that in a proof or calculation, one *overviews* the path to the result. Experiments, by contrast, are conducted in order to study a causal process that one does not overview. 'When I wrote "proof must be perspicuous" that meant: *causality* plays no part in the proof.'[51]

These are still negative characterisations of surveyability, but there are positive ones especially in part III of *Remarks on the Foundations of Mathematics*. Mühlhölzer mentions several different ways that Wittgenstein describes surveyability, but these can, in essence, be summarised in two. Firstly, a proof should be *reproducible*: '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. … [W]e must be sure we can exactly reproduce what is essential to the proof.'[52] Secondly, a proof should be *plain to view* or *intuitive*. In this second characterisation, the visual element in surveyability is conveyed: 'Proof must be a procedure plain to view.'[53]

There are two perplexing issues in these two characterisations. The first concerns that which is *essential* to proofs and that one should be able to reproduce. The second concerns the role of the visual element in 'plain to view' or 'intuitive'.

Beginning with the first of these issues, it is clear that the reproduction of proof does not hang on, for instance, typographical features. Some variations in different presentations of a proof can be tolerated. However, as Mühlhölzer asks, how far does this toleration extend? What is essential to a particular proof? Is the essential of a particular proof reproduced if the purported reproduction relies on another *proof idea* than the first? Mühlhölzer claims that, according to Wittgenstein, that would not be a reproduction. Drawing on material from part III of *Remarks on the Foundations of Mathematics*, Mühlhölzer compares Wittgenstein's notion of surveyability to the one found in Hilbert's descriptions

quotes from *Remarks on the Foundations of Mathematics* are not modified.

[50] RFM, III § 39.

[51] RFM, IV § 41.

[52] RFM, III § 1.

[53] RFM, III § 42. Mühlhölzer mentions that 'plain to view' is Anscombe's translation of 'anschaulich', and he draws attention to several other passages in the manuscript MS 122 (which forms the source of two thirds of part III of *Remarks on the Foundations of Mathematics*) where proofs are said to be *anschaulich* and that *Anschauung* is required. He remarks that 'intuitive' and 'intuition' would be a more neutral choice of translation, but stresses that it is the everyday meaning 'plain to view' that Wittgenstein had in mind – Kantian connotations notwithstanding. Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', pp. 69–70.

of his proof theory.[54] The surveyability that is associated with the reproduction of proofs should thus allow one to see that it is exactly the same proof. This includes that no step is left out and that no signs have switched places accidentally. This kind of surveyability is not present in the causal processes studied in an experiment. Moreover, it is not the kind of surveyability that could come into question if the proof and the supposed reproduction utilised different proof ideas. In the proof of the Bolzano-Weierstrass theorem, two different proof ideas could be seen in the different formulations of the theorem and the differences in the proofs that these formulations necessitate (i.e. making use of accumulation points or the convergence of subsequences). Another example of diverging proof ideas could be a different kind of construction of the monotonous sequence. Yet, even if proof ideas may differ, there is still a sense in which two such proofs can be essentially the same. This is, arguably, an important, albeit different, sense of 'essentially the same' that is not covered by the surveyability that is related to the reproduction of proofs. I will shortly return to the kind of surveyability that allows one to see that two slightly different proofs are essentially the same. Most importantly, however, the kind of surveyability that is in focus here is one that is tied to the use of a notation that allows for the kind of diagrammatic perceiving mentioned on p. 94. In his discussions of the foundational programmes, Wittgenstein is critical of (among other things) the employment of a signs that 'cannot be recognized by its shape.'[55]

The second issue, i.e. what it means for a proof to be intuitive and plain to view, can be understood along the lines of the above remark about the essential features of a proof. What is essential to the proof should be possible to see, it should not be hidden in some way. The contrast where things are hidden is, again, causal processes that one is trying to understand with the help of an experiment. Mühlhölzer summarises this: 'All our reasons to accept the correctness and conclusiveness of a proof only refer to things which we can *see* in the proof.'[56] The two positive characterisations of 'surveyable' are, thus, intimately linked.

As Muhlhölzer, but also Mathieu Marion, mention, much of Wittgenstein's writings on the surveyability of proofs is directed at Russell's and Whitehead's logicist project although his remarks are relevant for a broader discussion of proofs

[54] Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', p. 62. See the Hilbert quote on p. 83.

[55] RFM, III § 10.

[56] Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', p. 70. In this passage, Mühlhölzer echoes Hilbert's assertion (which he also quotes): 'If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation, their difference, their succession (like the objects themselves) must exist for us immediately, intuitively, as something that cannot be reduced to something else.' Hilbert, 'The New Grounding of Mathematics: First Report', p. 202.

too.[57] The first thing to notice in this criticism is that proofs written in Russell's and Whitehead's system (or in any formal system) will not, in general, be surveyable, simply because they are too long for anybody to overview or reproduce. In conclusion, they can be called proofs only in a figurative sense. Marion mentions that the surveyability remarks have been taken to imply a strict finitism on Wittgenstein's part. The reason for this interpretation is that surveyability is interpreted as setting a limit to the length of proofs.[58] As both Marion and Mühlhölzer remark, it is not primarily the length of the proofs that make the proofs of *Principia Mathematica* unsurveyable. It is the notational systems which focus on a minimum of symbols that quickly lose their surveyability as the formulas grow longer. The criticism of the deduction of arithmetic from logic, thus, takes the following form: the signs employed to represent numbers make even small numbers impossible to distinguish from each other, if one does not count the number of brackets.[59] This makes the reproduction of such a proof problematic, since the notation makes it impossible to see if a string of signs representing, for instance, a number has been reproduced or not. The proofs of *Principia* thus lack surveyability in the sense of *reproducibility*.

Since the point of reducing arithmetic to logic was to benefit from the (supposed) greater certainty of logic, this dependence on counting is problematic. The proofs that should rely on nothing but logic need the techniques of arithmetic to work as proofs, and this vindicates the philosophical part of the project. As Marion remarks, due to this lack of surveyability there arises a circularity in the logicist programme. Mühlhölzer also draws attention to this circularity and summarises it clearly: '[I]n a foundational system, the concepts, sentences and proofs on the higher levels should be *constituted* by the concepts, sentences and proofs on the foundational level. But if the *identity* of the foundational sentences and proofs are dependent on what happens on the higher levels, this idea of a "constitution" is compromised.'[60]

One may object to this particular vindication of logicism that what is important to logicism is to show that such a deduction is *possible* – never mind that the signs on the foundational level are impossible to work with in practice.[61]

[57]Mathieu Marion. 'Wittgenstein on Surveyability of Proofs'. In: *The Oxford Handbook of Wittgenstein*. Ed. by Oskari Kuusela and Marie McGinn. Oxford: Oxford University Press, 2011.

[58]There has been some discussion about whether or not Wittgenstein intends to limit what is acceptable as a proof or if he simply intends to describe what we take to be a proof. E.g., Shanker argues for the latter option. Shanker, *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*, p. 129. I will briefly discuss the issue of the length of proofs below.

[59]This phenomenon can be seen already in a very simple formula such as the set theoretic example 4.8 on p. 91.

[60]Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', p. 80.

[61]Mühlhölzer calls this the 'theoreticity rejoinder' since the formal proofs are thought to exist only as theoretically postulated entities. Ibid., p. 75.

That this deduction is possible is indisputable, but it does not imply anything more for the certainty of mathematics than any other mathematical result. D. S. Shwayder's remark is to the point when he writes that 'what is proven rather, and proven perspicuously, is a general correspondence between two systems'.[62]

It is important, however, that Wittgenstein's dismissal of logicism does not amount to a wholesale dismissal of formal proofs, but merely of the philosophical thought behind the use of formal proofs in logicism: 'We incline to the belief that *logical* proof has a peculiar, absolute cogency, deriving from the unconditional certainty in logic of the fundamental laws and the laws of inference. Whereas propositions proved in this way can after all not be more certain than is the correctness of the way those laws of inference are *applied*.'[63] Wittgenstein introduces the concept *geometrical cogency* and writes that 'the cogency of logical proof stands and falls with its geometrical cogency'. He then brings this to bear on Russell's system: '[L]ogical proof, e.g. of the Russellian kind, is cogent only so long as it also possesses geometrical cogency. And an abbreviation of such a logical proof may have this cogency and so be a proof when the Russellian construction, completely carried out is not.'[64]

Formal proofs may, with the help of suitable abbreviations of the syntax, be perfectly surveyable proofs, but one has, at the same time, lost contact with the level of simplicity which was supposed to guarantee a greater certainty. Wittgenstein remarks that '[a] shortened procedure tells me what *ought* to come out with the unshortened one. (Instead of the other way round.)'[65] He also writes that abbreviating a formal proof by means of suitable definitions will introduce new concepts and a new system. Any philosophical particularities of the unabbreviated system are not necessarily passed on to the new system.[66] As I understand it, the conclusion of this should not be that no mathematics will live up to the certainty that we find in logic, but, on the contrary, that this certainty can be found in all parts of mathematics, in mathematical logic and in other sub-disciplines.

I shall now return to the question concerning the kind of surveyability that allows one to see that two slightly different proofs are essentially the same. A related problem is what it means for two proofs to be essentially the same although they differ with regard to proof idea or method of proof (e.g. indirect proof, induction), but I will not go into it here.[67] For the present discussion, however, it

[62]D. S. Shwayder. 'Wittgenstein on Mathematics'. In: *Studies in the Philosophy of Wittgestein*. Ed. by Peter Winch. London: Routledge & Kegan Paul, 1969, p. 87.

[63]RFM, III § 43.

[64]RFM, III § 43.

[65]RFM, III § 18.

[66]RFM, III §§ 45–46.

[67]In a blog entry on October 4, 2007, Tim Gowers discusses where to draw the line between 'essentially the same' and 'genuinely different'. That we do identify some proofs as being essentially the same is clear, but it is not settled whether it is possible to make a sharp distinction or not. Tim

is interesting to note that we need to be able to *survey* the line of argument of the proof in order to compare it with another. Importantly, we also need to survey the argument already in order to *understand* the proof. This sense of surveyable is crucial for the philosophical understanding of the concept of proof, but it is a different sense than the one discussed above, which concerned the forms of notation that allow for a clear and distinct view of the proof.

This other sense is connected to *my understanding* of a proof. Within this sense of 'surveyability', one can, following O. Bradley Bassler, distinguish *local* from *global* surveyability: 'local surveyability requires the surveying of each of the individual steps in a proof in some order, while global surveyability requires the surveying of the entire proof as a comprehensible whole'.[68]

Local surveyability, naturally, places some conditions on proof in that the proof has to be such that it is possible to verify each step. What comes easily to mind is a proof that is built up of a series of simpler deductions which taken together form a longer argument. However, even a diagram in a proof in geometry may possess this local surveyability if it is evident what the essential features of the construction are.

Global surveyability does not admit of any precise description, but it involves that it must be possible to form an overarching understanding of the working of the proof. This, however, seems to be no more specific a description than the minimal requirement that the proof be logical. Even so, to form a thorough overarching understanding of a proof demands much of the reader. This relates to the fact that one often has to work on the proof in order to achieve an overview in this sense. This kind of surveyability is even more than the local kind dependent on a prior understanding of the reader.

Tying this to the problem of how it is possible to recognise the similarity between two slightly different proofs, one can see that this depends on global surveyability. This can be illustrated by the Bolzano-Weierstrass theorem and the alternative formulations mentioned above. It does not matter which of the many possible proofs one settles on; as long as the proof centres around the construction of a monotonous (sub)sequence the essential is reproduced.

Importantly, it is the achievement of the understanding related both to the local surveyability and, in particular, to the global surveyability that allows one to see that a proof actually establishes a theorem. As Bassler remarks, this is an understanding that goes beyond the recognition that each of the steps is valid: 'the collective force of the proof steps requires a further conceptual acknowledg-

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Gowers. 'When are two proofs essentially the same?' In: *Gower's Weblog: Mathematics Related Discussions* (4/10/2007). URL: https://gowers.wordpress.com/2007/10/04/when-are-two-proofs-essentially-the-same/ (Accessed 09/05/2016).

[68] O. Bradley Bassler. 'The Surveyability of Mathematical Proof: A Historical Perspective'. In: *Synthese* 148 (2006), pp. 99–133, p. 100.

ment. … Such a conceptual acknowledgement, that the proof steps fit together in such a way that they *establish* the claim, is a minimal requirement for global surveyability.'[69] The quote from Bourbaki in chapter 4 (p. 96) can be interpreted as stressing the importance of the understanding facilitated by global surveyability.

At this point, it is easy to jump to the unwarranted conclusion that mathematical certainty is *explained* by the presence of surveyability in proofs, especially if one is sympathetic to the above line of thought. That 'surveyability' is not a concept that can play such a role is seen from the fact that it cannot be given a precise definition, and it is not possible to say, independently of a reader, whether a proof has this quality or not.

Instead, finding a proof to be surveyable depends on having achieved a practical familiarity, a skill in using the symbols and concepts involved. Importantly, this holds for all three senses of surveyable. This, in turn, shows why taking matters of 'skill' and 'ability to use' into account is vital for the project at hand. This is evident in the case of global surveyability. Thurston comments that in the case of some of his proofs, he had to put considerable effort into conveying the 'mathematical infrastructure' that allowed people to understand them.[70] Thurston's example is, of course, one that is accessible to comparatively few persons, but the same phenomenon occurs early on when learning mathematics. It is difficult to appreciate the certainty of proofs in elementary analysis – e.g. of the proof of the Bolzano-Weierstrass theorem – if one has not previously achieved some (working) familiarity with concepts such as convergence and continuity.

It may be tempting to draw a line between local and global surveyability and claim, with Edwin Coleman, that local surveyability 'is an objective property of the written proof itself.'[71] This is a mistake, however. That the individual steps of a given proof are such that they are possible to verify is not separable from the ability of the person going through them. What is surveyable in the local sense is dependent on one's prior knowledge. Reviewing, surveying, the steps where the volume or area is calculated for the infinitely long solid above is possible only to somebody who knows how to compute the integrals. This is the case even in formal deductions as will be seen below.

Even the visual or formal surveyability emphasised by Mühlhölzer cannot be distinguished from the ability to use the signs that are visually surveyed. As was argued in chapter 4, the idea of something purely formal or purely visual arises from a conceptual confusion.

As a summary of the discussion of these three different kinds of surveyability,

[69] Bassler, 'The Surveyability of Mathematical Proof', p. 102.

[70] Thurston, 'On Proof and Progress in Mathematics', p. 175.

[71] Edwin Coleman. 'The Surveyability of Long Proofs'. In: *Foundations of Science* 14 (2009), pp. 27–43, p. 40.

one can say that they are all aspects of our practice of working with proofs. Now, what is essential is only obvious to someone who has a thorough understanding of the techniques involved. Surveyability is something that characterises the things we call proofs. It will not serve to explain *why* proofs establish results, but it will arguably be part of a picture of mathematics where there is room for certainty. The following portrayal of the concept by Coleman is sympathetic: 'Surveyability is the requirement that the proof be capable of supporting the construction of a perspicuous representation of the proof-idea.'[72] If one searches for a clear definition, this does, naturally, not suffice. Here one approaches an important point concerning the surveyability of proofs, namely that to identify a proof as surveyable is not to say much more than that it is a proof.

Lately, the question of how the existence of very long proofs affect the future of proof has received much attention. I will comment briefly on this since it poses a problem for the surveyability of proofs. It is important to see clearly in what sense surveyability is threatened.

The fact that the length of formal proofs quickly grows out of hand has been discussed since the introduction of formal proofs. Since ordinary mathematics does not depend on proving things formally, the length of such proofs has not been alarming. However, there have been proofs that are long for other and perhaps more serious reasons. The proof of the four colour map theorem by Appel and Haken was already mentioned, and another is the *classification of finite simple groups*, the proof of which is said to be several thousand pages in length. The length of the four colour theorem is due to its dependence on extensive computer calculations that no human being could possibly verify. The classification of finite simple groups is, by contrast, a result of a collective effort stretching over more than a hundred years, and it encompasses about 15,000 pages. The individual publications may, taken in isolation, be fully satisfactory and surveyable, but it has been argued that probably no one can overview all of this mass of research.[73] If a mathematician makes use of the theorem, there is thus an element of trusting other mathematicians. Interestingly, Coleman argues that the four colour theorem *is* surveyable in the *global* sense. It is possible to overview the working of the proof even though it is lacking in local surveyability, due to the sheer amount of steps performed by the computer.[74] In this case, one could say that the mathematician who makes use of the four colour theorem puts her trust in the accuracy of the computers that were employed in the proof.

These examples are often brought up because they raise questions concerning the status of proof. Coleman argues that length is not, in general, a problem

[72] Ibid., pp. 40–41.

[73] Daniel Gorenstein. 'The Enormous Theorem'. In: *Scientific American* 253.6 (1985), pp. 104–15.

[74] Coleman, 'The Surveyability of Long Proofs', p. 40.

for the concept of proof since long proofs are abundant in mathematics. The examples under discussion are, of course, extreme cases, but achieving an overview is in general no easy task. Coleman interprets the passage where Wittgenstein states that 'surveyability' means that the reproduction of a proof should be an 'easy task' as putting too strict a requirement on the concept of proof. According to Mühlhölzer, the remark does not mean that it must be easy to *understand* or *memorise* the proof.[75] With the three different senses of surveyable at hand it is possible to see that this remark of Wittgenstein's refers to the minimum requirement that the symbols used in a proof must be such that they can be surveyed in the formal or visual sense mentioned in connection with the criticism of *Principia Mathematica*. He sets no limits on the difficulty of achieving an overview in the global sense (which Coleman clearly thinks he does) – even if this, of course, must be possible. Perhaps one cannot even set any limit to the surveyability that a proof must possess in this sense, because what will be surveyable will vary from person to person.

These three aspects on proofs – concept-formation, the difference between proofs and experiments, and surveyability – can now be seen to contribute to a sketch of the concept of proof that is different from the idea that a proof convinces me of the truth of a proposition that can be understood in isolation from the proof. Understanding and accepting a proof has more in common with understanding and accepting that certain concepts can be used in a certain way – indeed must be used in a certain way – rather than becoming convinced that something is the case (which might be the case when performing experiments). The conviction that a proof may bring about cannot therefore be separated from a conviction that the concepts can be used in a certain way. As Wittgenstein writes: 'In producing a new concept [the proof] convinces me of something.'[76] The issue of surveyability is thus seen to be indistinguishable from the issue of concept-formation. If the understanding of a proof is to be such that it changes one's comprehension of the concepts involved, it has to provide an overview that allows one to see how the concepts must be used. In the example with the infinitely long horn, it is possible to form an overarching understanding of how the integrals relate to the geometrical object in that the values of the integrand function coincide with the radii of the horn. The computation of the, perhaps startling, values is also surveyable. It is thus possible to survey in what way one may ascribe an area and a volume to the horn despite its infinite length and, furthermore, that they have the values they have.

With regard to the discussion of the role played by conviction in the first part of this chapter, it is important to notice that we distance ourselves from the problem 'How does a proof convince me?' once we see that a proof is compelling

[75] Mühlhölzer, "'A Mathematical Proof Must Be Surveyable'", p. 61.
[76] RFM, VII § 72.

only to someone who knows the requisite techniques and takes the time to work through it. A proof must be such that it shows the connections, but taking part of a proof does not by necessity compel me to accept it. Working through the proof can be easy if one is familiar with the kind of reasoning and with the concepts involved. It may, however, sometimes require that the reader learns the use of the concepts at the same time as she understands the proof. In this sense the work of the reader bears similarities to the work that is undertaken by the one who devised the proof in the first place. Thus, taking part of a proof should not be viewed as receiving a message (that convinces one), but as a process of working on one's understanding of the concepts involved.

Furthermore, the request for an explanation of how a proof proves could be answered only in relation to a specific proof. For example, the proof of the Bolzano-Weierstrass theorem proves because it shows that there will be infinitely many points in a neighbourhood of at least one point of a bounded interval containing infinitely many points since there is at least one point on a bounded interval which is a convergence point of a sequence. Of course, this explanation does not satisfy someone asking the philosophical question 'How can a proof manage to establish a theorem with certainty?' It will not produce a general account of what happens. It may be tempting to search for such a general account by searching for some kind of logical structure on a deeper level that will explain how proofs in general prove. Logicism can, arguably, be seen as such a project. Azzouni's 'derivation-indicator' view is also an example. It is, nevertheless, unclear what status such an explanation would have, or even what value it would have. Would it further my understanding of mathematics more than attending to the mathematics that we do?

The idea of viewing mathematical propositions on a par with rules or norms is closely related to the above discussion of the three aspects on proof that Wittgenstein emphasises. One point of contact lies in his remark that an empirical proposition can be hardened into a rule and after that serve as a ground on which to evaluate other empirical propositions. To be specific, this remark is echoed when it comes to experiments and proofs: 'So up to now the testing was, so to speak, experimental. Now it is taken as a proof. And the proof is the *picture* of a test.'[77] In the example of the pentagram discussed above, the placing of the fingers on the points of a pentagram can be an experiment. Are there enough fingers to cover the points? When we see that they even out, we may accept this sight (picture) as proof that they are equal in number, and this correlation may then become a new criterion for, say, judging whether a figure is a pentagram or not. Furthermore, this picture allows one to overview the possible outcomes of a future experiment where one tries to correlate one's fingers with the points of

[77] RFM, VI § 2.

a pentagram. Another point of contact lies in the observation that proofs form concepts. In forming concepts, proofs shape one's understanding of how one ought to use them within mathematics, but also in applications to matters external to mathematics.

In this sense, my understanding of a proof changes my conception of, for instance, giving change to customers, so that I am *certain* that I give the correct change to the customers if I follow the rule for calculating the change. I may be in doubt about whether or not I calculated correctly and whether or not I received a twenty-euro or a ten-euro note, but the rule itself is removed from the possible sources of error once I have grasped the proof. The normativity of the rule is thus established by the proof. In this sense, the certainty of the method for calculating change is given a different role than, for example, the certainty that one actually received a twenty-euro note and not ten-euro note.

In conclusion, it can be said that the role of proofs in the certainty of mathematics lies in that we form ourselves norms for how we think about other things by proving theorems. The certainty of mathematics is not, however, explained by saying that proofs form concepts or that they are surveyable. This would imply that 'concept-formation' and 'surveyability' could be understood as such and then be brought in to explain how proofs establish theorems and give them a normative character. Rather, these are features of our practice of proving, and what it means for a proof to form concepts or to be surveyable can only be understood by looking at examples of proofs. By drawing attention to these features of our practice, we can see in what sense it can be said that our mathematics is certain.

5.6 Simple Deductions

At this point, I shall briefly discuss the role of formal deductions with regard to the certainty of mathematics. The attraction that formal systems have enjoyed can be seen in the special status assigned to formal deductions. As was mentioned in chapter 2, it is often pointed out that mathematics is a deductive science and that this distinguishes it from other sciences, in particular with regard to the reliability of its results. For the present investigation, it becomes interesting to consider the question 'In what way does mathematics' status as a deductive science contribute to its certainty (if it does)?' It seems to me that part of what makes it attractive to account for the certainty of mathematics by alluding to its deductive character is that one pictures mathematical reasoning to be a chain of simple and completely surveyable steps of deductive inferences. Moreover, if we are dealing with *formal* deductions, the conclusions are said to be reached solely on the basis of the form of the premisses. Furthermore, the conclusions follow *necessarily*.

In order to facilitate the discussion, it may be helpful to have an example at hand. The following is a proof in natural deduction of the *law of contraposition*: '$(A \to B) \to (\neg B \to \neg A)$'.

1. $A \to B$	
2. $\neg B$	
3. A	
4. B	$\to$ **Elim:** 1, 3
5. $\bot$	$\bot$ **Intro:** 2, 4
6. $\neg A$	$\neg$ **Intro:** 3–5
7. $\neg B \to \neg A$	$\to$ **Intro:** 2–6
8. $(A \to B) \to (\neg B \to \neg A)$	$\to$ **Intro:** 1–7

In chapter 4, it was argued that the form one sees in a proposition is tied to one's ability to make meaningful use of it in the practice where it belongs. The idea that the conclusion in a deduction is drawn solely on the basis of the form of the propositions must, therefore, not be thought of as something isolated from the ability to use them. On the view that was questioned in chapter 4, the form of the expression – being an objective part of it – determines what can be inferred. Since logical form, according to this view, is a feature of the expression, what follows is, *in principle*, determined even though no one actually infers anything. Still, even as this idea of form is rejected, one may wonder how the rules of inference license, for example, the step where '$\neg B \to \neg A$' is inferred from lines 2–6 in the deduction above.

If recognising logical form is connected to using the expression in a certain way – what does using a formal expression involve? Is it not, among other things, *inferring*? This means that the logical form of a proposition is internally connected to that which one infers from the proposition. The idea that logical form determines what can be inferred – i.e. the idea that one could first discern the logical form of a proposition and once that is established, work out what follows – is therefore being questioned. More specifically, a view that is affected by this criticism is *representationalism*, i.e. the view that the meaning of a formal expression is determined by the meaning of the constitutive signs.

A contrast to representationalism can be seen in the following remark of Wittgenstein's: 'We can conceive the rules of inference – I want to say – as giving the signs their meaning, because they are rules for the use of these signs. So that the rules of inference are involved in the determination of the meaning of the signs.'[78] This passage has provided reasons for labelling (the later) Wittgenstein an *inferentialist* with regard to the meaning of signs. I will argue that passages like this should be read in a different way, a way that illuminates the philosophical problems pertaining to proof and deduction.

In one of the first passages of *Remarks on the Foundations of Mathematics*, another remark that could be read as an expression of *inferentialism* is found:

> 'But doesn't e.g. '*fa*' have to follow from '$(x).fx$' *if* '$(x).fx$' is meant in the way we mean it?' – And how does *the way* we mean it come out? Doesn't it come out in the constant practice of its use? – But it is as if there were also something attached to the word "all", when *we* say it; something with which a different use could not be combined; namely, the *meaning*. …
>
> One learns the meaning of "all" by learning that '*fa*' follows from '$(x).fx$'. – The exercises which drill us in the use of this word, which teach its meaning, always make it natural to rule out any exception.[79]

This theme can be seen as a continuation of the idea that proofs contribute to the meaning of concepts. Only, in the case of inference rules, they contribute to the meaning of the signs involved although nothing is (yet) proved. Here the common feature lies in the use one is prepared to make of the symbols or concepts involved. A proof can be said to shape concepts since it shows, in a surveyable manner, a possible use of them. Inference rules will perhaps not as such determine the meaning of the signs, but in entering into the practice of deducing according to the rules and in becoming proficient in the use of them, their meanings open up. For example, one would not say that someone has grasped the meaning of the *modus ponens* rule if the person in question does not also realise that *B* on line 4 follows from lines 1 and 3 in the above proof. A similarity to this entering into the practice of deducing can be seen in how children learn to count and perform basic arithmetical operations, i.e. how they learn the meaning of the symbols '+', '−', etc. The usual procedure is to count objects of manageable size, rather than to study the symbols of arithmetic in isolation from the applications. Especially the first operations, adding and subtracting, are learnt by adding objects to or removing them from a group of objects.

The point of this discussion will become clearer if the perspective described is contrasted with inferentialism. The idea that rules of inference determine the meaning of the signs that are used in propositions is susceptible to such criticism as was voiced by A. N. Prior.[80] If inferentialism is correct, he suggests that one could introduce a connective 'tonk' which has the following inference rules:

$$\frac{A}{A \text{ tonk } B} \qquad \frac{A \text{ tonk } B}{B}$$

[78] RFM, VII § 30.

[79] RFM, I § 10.

[80] A. N. Prior. 'The Runaway Inference-Ticket'. In: *Analysis* 21 (1960), pp. 38–39.

If, as inferentialism claims, there is no meaning that the connective has prior to these inference rules, it seems that it could be satisfactorily introduced this way. With such introduction and elimination rules, however, it becomes possible to prove anything, and the whole point of deduction collapses. Prior's criticism can be seen to affect such forms of inferentialism that sees the inference rules as giving a meaning to the signs which are as such without meaning. This is a form of inferentialism which tries to explain the meaning in terms of their inference rules.

A solution to this problem (besides abandoning inferentialism) is, according to Nuel D. Belnap, to demand that any new connectives introduced must form a *conservative* extension of the notion of deducibility. This rules out the introduction of 'tonk'. It requires, however, a previously settled notion of deducibility which such a connective as 'tonk' will contradict.[81] This seems unsatisfactory, however, since having a previously settled notion of deducibility requires that an established practice is already in place. If so, the connectives already have an established use, and this means that the meaning of the signs is not given exclusively by the inference rules.

However, as was suggested above, Wittgenstein's remarks are not to be read as advancing a view like inferentialism. Therefore, the perspective on proof presented here – which draws on such remarks of Wittgenstein and in which the practice of proving and of deducing come to the fore – is not affected by this criticism. Martin Gustafsson discusses Prior's challenge to inferentialism and he argues that Wittgenstein's view is not open to Prior's 'tonk'-argument.

> The introduction rule and elimination rule for 'tonk' can seem to determine a unified pattern of use only if it is taken for granted that 'p' and 'q' can be identified extra-logically, in merely orthographic terms, as sign-designs, concatenations of letters, or whatever – and, hence, that the so-called use determined by such rules is externally imposed on an already given raw material of logically inarticulate sounds and shapes.[82]

Inferentialism is a semantic theory which attempts to explain how meaningless signs acquire meaning. Robert Brandom's inferentialism is a good example. He describes his project: 'The major explanatory challenge for inferentialists is rather to explain the representational dimension of semantic content – to construe *referential* relations in terms of inferential ones.'[83] As I argued in chapter 4, however, the idea of meaningless signs that can be identified as mathematical units prior to a practice of using them is a mistake. When Wittgenstein writes

[81] Nuel D. Belnap. 'Tonk, Plonk, and Plink'. In: *Analysis* 22 (1962), pp. 130–134.

[82] Martin Gustafsson. 'Wittgenstein and "Tonk": Inference and Representation in the *Tractatus* (and Beyond)'. In: *Philosophical Topics* 42.2 (2014), pp. 75–99, p. 82.

[83] Robert B. Brandom. *Making it Explicit*. Cambridge MA: Harvard University Press, 1994, p. xvi. Brandom counters Prior's 'tonk'-argument by invoking the limitation on new connectives suggested by Belnap. Ibid., p. 125.

that the rules of inference are 'involved in the determination of the meaning of the signs', the idea is not to present a theory about how meaningless signs acquire a meaning (although the formulation taken out of context may seem to imply that). As is seen in the second quote above, the emphasis is on the practice of using, for example, the universal quantifier and on the process of learning it. The meaning of the universal quantifier as a mathematical symbol is not given only through the rules of inference, but learning its rules of inference is a part of the successful entering into the practice of its use. One could, employing the distinction between sign and symbol, say that Wittgenstein is describing how a symbol is established as a symbol.[84]

Interestingly, Brandom also notes this dependence of the rules of inference on an established practice: 'Norms that are *explicit* in the form of rules presuppose norms *implicit* in practices.'[85] The kind of inferentialism that wants to explain the meaning of a connective by pointing to its introduction and elimination rules is treating the connective as a sign which does not yet have a meaning but becomes a fully fledged mathematical symbol with a proper use when the rules of inference are given. This is evident since it distances itself from the representationalist idea that a connective has a meaning that is established before it is put to use in inferences. This idea, too, is according to the perspective I am proposing misguided.

The focus on the ability to use the symbols of mathematical expressions and proofs may seem to introduce a certain arbitrariness into the practice of working with such expressions. Is there, for example, not a circularity in the idea that we identify the symbols as the symbols they are through the rules of inference – yet the rules are formulated using employing those symbols? Still worse, it seems that we cannot identify the symbols as the symbols they are without being part of a practice where they are used. How can one become part of such a practice of using the symbols which that practice is said to allow us to identify?

The answer to this question lies in attending to the way we learn and become members of these practices. We learn them gradually, first by concentrating on the simplest examples and through them learning elementary uses of the symbols. In the beginning, it may be difficult even to write the proper signs on a paper. As one becomes more secure in the use of the symbols, the writing of the signs comes more and more automatically. When learning connectives, they are often introduced through truth-tables. Becoming skilled at using them in truth-tables opens up the understanding of them – at least to some extent. When learning the inference rules this understanding is widened, complemented. Regardless of which of these techniques one learns first, the learning of the second

[84] See also the quote from Stenlund on p. 85.
[85] Brandom, *Making it Explicit*, p. 20.

one will add to the meaning that they have for the learner. There is thus a certain circularity to this learning process.

What I have said so far about logical form being connected with use within a practice may at first sight seem to put the objectivity of mathematical truths into question. If the logical form of a proposition is dependent on its use, this may give the impression that a certain arbitrariness is introduced into the notion of inferring, while the whole point of inferring is to establish what follows logically, that is, regardless of such things as how it is used in a particular situation. What becomes of inferring then?

This objection, however, presupposes that the physical signs are there and that what logical form one sees in them is somehow open and that, therefore, what follows from this particular string of signs would be arbitrary. This seems to introduce arbitrariness into every step of a chain of inferences, since how one should interpret the outcome of the previous step would seem equally open.[86] One may, on the other hand, dispute the claim that something is being interpreted. When I am faced with a proposition, it is not the case that I also realise that it happens to have a certain logical form – but what logical form I see is internally connected to what proposition I see. Understanding that by '$a \vee b$', I mean a disjunction cannot be separated from my seeing that logical form. That is, I could not understand the expression as a disjunction and still vacillate about what form it has. Which symbol (e.g. the symbol for addition) one sees on the paper is internally connected to what form one sees (e.g. addition). If a word is scribbled on a paper and it is not clear which word it is – or even if it is to be seen as a word at all and not mere doodles – one is, naturally, interpreting it, but this is an exception when reading. Once one sees a word it is almost impossible to change back to the attitude where one did not see a word. This is especially striking when one is starting to discern the spoken words of a language that one is learning or when learning to decipher old handwriting.

Thus, the message of chapter 4 naturally applies to the case of simple deductions. The need for a skill, an ability to use the symbols involved in accordance with an established practice is vital for inferring. When it is said that the conclusion follows solely on the basis of the form of the premisses, the notion of form that is involved is the one discussed as 'logical form' in chapter 4 and that was tied to a possible use of the symbols. Simple deductions are thereby not distinct from ordinary proofs in any absolute sense. Their surveyability is just as much dependent on the skill of the one reading them. If one searches among simple formal deductions, it is possible to find particularly clear examples of proofs,

[86] This is, in essence, Michael Dummett's interpretation of Wittgenstein's *Remarks on the Foundations of Mathematics* in his review of the book. Michael Dummett. 'Wittgenstein's Philosophy of Mathematics'. In: *The Philosophical Review* 68 (1959), pp. 324–48.

where the surveyability is evident and it is easy to achieve an overview of the proof in both the local and the global sense. However, such examples can be found also outside of the domain of formal deductions. One can thus conclude that for the understanding of mathematical certainty, simple formal deductions are not set apart from other kinds of mathematics.

5.7 Concluding Remarks

In this chapter, I have contrasted two perspectives on proof. The first perspective is one where proofs convince me of the truth of a proposition the meaning of which can be known beforehand even without a proof. This perspective was seen to go well together with the body of truths conception of mathematics. It, furthermore, makes the convincing or establishing power of a proof into something puzzling. How can a proof accomplish such a thing? It has been emphasised throughout this thesis that the body of truths conception of mathematics is likely to be misleading if it is allowed to guide one's philosophical thinking on mathematics, and this is shown in the case of proofs too.

The other perspective departs from the observation that proofs are often needed in order to show us the meaning of what is being proved. This accords with the emphasis on the view of mathematical knowledge as an ability. The three aspects of proofs introduced by Wittgenstein – that proofs form concepts, that proofs and experiments are fundamentally different, and that proofs must be surveyably – served to highlight in what sense a proof can be said to contribute to the meaning of a theorem. That a proof shapes our understanding of the concepts involved in the proof, and that it does so by showing in a surveyable manner the use of the concepts, means that the proof (and thus the theorem) has a normative role in our understanding of mathematics, but also with regard to matters that the theorem may be applied to outside of mathematics. This normative role of the results of mathematics sets it apart from other activities in terms of certainty. This is a way of portraying the compelling quality of proofs and their ability to extend the 'measures of language' – and, thus, of proofs' role in the certainty of mathematics. Mühlhölzer summarises this: 'To Wittgenstein, [the inexorability of mathematics] does not lie in the mathematical entities and facts, to which the mathematical signs and sentences refer, but in the way we use the mathematical signs and sentences, and particularly in the way we use mathematical proofs.'[87]

[87] Mühlhölzer, '"A Mathematical Proof Must Be Surveyable"', p. 60.

6. Conclusion

In this thesis, the aim has been to sketch a view of mathematics where there is room for certainty. This is a concept that was discussed frequently in the beginning of the twentieth century in relation to the foundational crisis. The possibility of speaking of certainty was tied to the success of the programmes and as the programmes were halted due to Gödel's incompleteness proofs, the concept attracted less attention. It has been my aim to bring back this concept into the contemporary discussion by loosening its ties to foundations and showing instead how certainty is a part of our practice of mathematics. That is, the certainty of mathematics does not require a philosophical justification, it does not need a foundation. Rather, I have pointed to features of our practice that indicate in which way certainty is part of it.

My sketch started with the role that simple arithmetical rules have in everyday situations. It continued with examples of how we learn about and study mathematical objects. The role of symbolism and formality was discussed and, finally, our practice of proof was put to scrutiny.

In order to delineate this view, I have contrasted it with another view which I have called the body of truths conception. This conception is not portrayed as a position in the contemporary discussion; rather, it functions as a tacit assumption that tends to guide our thinking. As such, it influences our thinking in ways that are not obvious, and in the philosophy of mathematics, it is potentially misleading.

A picture of mathematics emerges where knowledge is to be understood primarily as skill in using the techniques of mathematics. I do not wish to rule out the possibility of describing this as a knowledge of the mathematical objects, but if the connection to the ability to use them is forgotten, it will invite questions such as 'How can we know anything about such objects?' or 'What is the ontological status of such objects?' This picture is also one where proofs can be thought of as extending this ability to use the concepts of mathematics, and where mathematical propositions function like norms that guide our use of these concepts. Thus, the peculiar certainty of mathematics is not dependent on the connection to special abstract objects but part and parcel of their status as norms.

I shall conclude by briefly considering a feeling of misgiving that has sometimes been voiced against the perspective proposed here. Does it not lead to relativism? Resnik, following his presentation of the philosophical problems that pertain to proofs (see p. 105), expresses his qualms:

> For many of us a glib answer to both questions may apply. This is the response that in our mathematics courses we are trained to accept proofs as giving good reasons, and we have been conditioned to believe things which we think we have good reasons to believe. Some, influenced perhaps by Wittgenstein, would interject that the glib answer applies to all of us – practicing mathematicians and uninspired mathematics students alike. Mathematics, on their view, is not a science, and there are no mathematical facts. There is nothing but a certain social practice in which proving plays a major role. Thus, they would continue, the question we should ask is: how did our *practice* of proving mathematical statements evolve?[1]

There is much in this quote that is problematic. The suggestion that the perspective advanced in this thesis involves the claim that mathematics is not a science and that there are no mathematical facts seems absurd. To counter such misgivings, it is important to notice the outlook on mathematics and philosophy that can be sensed in this accusation and especially in the formulation: 'nothing *but* a social practice'. This formulation seems to be a consequence of the thought that mathematical certainty – or rather objectivity – stands in need of a philosophical justification. Accordingly, if no justification is given, if, say, no mathematical objects that could stand as a guarantee for the objectivity are identified, it follows that the objectivity (and hence certainty) is threatened. This line of thought is exemplified by Azzouni who argues that there must be something underlying our mathematical practices that explain the agreement among mathematicians (see pp. 55 and 96).

Even the conventionalist idea – that some kind of communal decision lies behind the agreement that mathematicians display in their work – is only a variant of this picture. Conventionalism merely replaces the idea of an external foundation with one arising from the community of people involved in mathematics.

The perspective advanced in this thesis is not that there is nothing but a social practice, although I have frequently emphasised the role of mathematical practice for the philosophical understanding of mathematics. Instead, attending to practice is a way of maintaining a 'realistic spirit', to use Diamond's phrase.

As was mentioned in section 2.6, doubts about the fruitfulness of foundationalism, paired with an attentiveness to practice, have led some philosophers to question the certainty of mathematics. Quasi-empiricism provides a good example. I shall briefly indicate why I do not draw this conclusion. The quasi-empiricism of Lakatos is a valuable approach in its focus on mathematical practice and in its attentiveness to the historical development of mathematics. I do not, however, agree with the claim advanced by quasi-empiricists to the effect that mathematical truths are revisable, or that the certainty of mathematics is an illusion. It may be granted that proofs are refined and changed due to criti-

[1] Resnik, 'Proof as a Source of Truth', pp. 10–11.

cism as Lakatos describes in *Proofs and Refutations*. It may also be granted that the standards of proof are changing throughout the history of mathematics, as E. T. Bell remarks. However, the idea that there is no metaphysical foundation underlying our mathematical practice does not necessarily lead to uncertainty. The phenomena pointed out in section 2.6 may indicate that the idea of a foundation that mathematics rests on is a prejudice. They may also go some distance in showing that there is uncertainty concerning some results in mathematics. There may be details that need revision in a theorem that has recently been proved and not yet been digested by the mathematical community. Moreover, it is not improbable that some published results contain errors. Still, these phenomena seem to me to move on the fringes of mathematics and cannot be taken to show that mathematics is uncertain, wholesale. That a proof is refined or changed due to criticism may be part of the research process in mathematics, but that need not put well established theorems on the same footing as newly discovered proofs, let alone elementary arithmetic. It may simply be an indication of the difficulty involved in finding a proof.

The conclusion drawn by the quasi-empiricists, i.e. that there is no certainty in mathematics, seems to be the result of retaining the idea that mathematical certainty or objectivity would need a justification. When they successfully criticise the existence of such a justification, they conclude that uncertainty follows. This applies to the criticism of Resnik too.

The charges of relativism, that the perspective proposed here makes the certainty and truth of mathematics relative to our practice, are genuinely worrying. It seems that certainty is a concept that demands that what is properly so called must not be relative *to anything*. This worry would require a more thorough discussion, and I shall only indicate how I think it could be answered.

In 'Rethinking Mathematical Necessity', Putnam sketches a view that bears a similarity to the one presented here. He poses the question whether something that is found to be true is genuinely necessary or merely 'quasi-necessary relative to our present conceptual scheme'. He remarks that answering this question necessitates an ability to judge our thinking, as it were, *sub specie aeternitatis*. That would indeed be a substantial assumption. Putnam remarks: 'The illusion that there is in all cases a fact of the matter as to whether a statement is "necessary or only quasi-necessary" is the illusion that there is a God's-Eye View from which all possible epistemic situations can be surveyed and judged; and that is indeed an illusion.'[2] A philosophical project that wishes to pass judgement on mathematics – whether this involves the verdict that mathematics gives us certain knowledge or that it does not – is seen to involve such assumptions. Thus, Putnam comments: 'To insist that these statements *must* be falsifiable, or that *all*

[2] Putnam, 'Rethinking Mathematical Necessity', p. 258.

statements must be falsifiable – is to make falsifiability a third (or is it fourth by now?) dogma of empiricism.'[3]

What we in philosophy want to say about mathematics – that it is absolutely certain – requires taking a God's-Eye View on our mathematics. However, mathematics and logic are part of how we make judgments, and to pass judgment on mathematics would seem to require that we transgress our own understanding of it. The charges of relativity thus demand of philosophy something that it could not possibly fulfil. Mühlhölzer gives an interesting turn to this: '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.'[4]

In this thesis, I have wanted to understand *our* mathematics. A major idea has been that we should not look for a justification for mathematics outside of what can be seen in mathematical practice. Thus, the themes discussed should not be taken as a justification of the certainty of mathematics. This was pointed out in the case of the surveyability of proofs. It is tempting to take this idea as an explanation of why proofs confer certainty on theorems. Likewise, when the normative status of mathematics is pointed out it is easy to interpret this as a presumptive explanation of the certainty of mathematics. Taken as explanations, they seem rather weak. However, the features of our dealings with mathematics that I have discussed should not be thought of as providing an alternative foundation for mathematics. They are features of doing mathematics that anybody who has learnt mathematics should be able to recognise. I have tried to resist the impulse to look for something underlying this practice that would explain its certainty. My proposal is then that these features show what it means for mathematics to be certain.

[3]Putnam, 'Rethinking Mathematical Necessity', p. 258.

[4]Sebastien Grève and Felix Mühlhölzer. 'Wittgenstein's Philosophy of Mathematics: Felix Mühlhölzer in Conversation with Sebastian Grève'. In: *Nordic Wittgenstein Review* 3.2 (2014), pp. 151–78, p. 168.

Svensk sammanfattning – Swedish summary

Denna avhandling är en undersökning av matematikens speciella status bland våra kunskapsformer. Matematisk kunskap har ofta givits en särställning eftersom den förefaller vara sann med en visshet som annan kunskap inte kan uppnå. Avhandlingens syfte är att förstå vad som avses med matematikens säkerhet. Avsikten är inte att, som hos grundvalsprogrammen i början av 1900-talet, försöka bevisa att matematiken ger oss visshet, utan att förutsättningslöst fråga sig hur vi ska förstå detta begrepp. Säkerheten hos matematiken antas alltså inte vara knuten till, till exempel, möjligheten att bevisa dess motsägelsefrihet. Den begreppsanalys som företas i avhandlingen utgår från hur vår praktiska användning av matematiken gestaltar sig. En grundidé är att den säkerhet matematiken ger oss framträder i vår matematiska praxis. Tanken är alltså inte att försöka berättiga den säkerhet matematiken uppvisar (och ifall detta misslyckas att frånkänna matematiken säkerhet). Frågan är alltså inte huruvida matematiken är säker eller inte, utan att utgå ifrån den säkerhet matematiken uppvisar och förstå denna bättre.

Den moderna matematikfilosofiska diskussionen domineras av positioner såsom platonism, strukturalism och nominalism. Begreppet säkerhet har fallit ur denna diskussion som istället fokuserar på matematikens objektiva giltighet och de matematiska objektens ontologiska natur. Inlagorna i denna debatt utgår i stor utsträckning från en särskild bild av matematiken. Matematiken ses som en samling av sanningar om abstrakta objekt. Att denna bild utgör en utgångspunkt förblir dock outtalat och den tillåts därför styra diskussionen utan närmare granskning. I avhandlingen befinns bilden vara missvisande som utgångspunkt för filosofiskt tänkande. Mot den ställs en annan bild där den praktiska skickligheten i användningen av de matematiska begreppen får en framträdande roll. Matematikens satser får, genom att de styr denna verksamhet, en normativ status; det visar sig att de hellre ska jämföras med regler för hur denna verksamhet ska bedrivas än med satser som beskriver matematiska objekt.

Två för den matematiska verksamheten centrala företeelser – arbetet med formella uttryck och att bevisa satser – granskas utgående från den bild där den praktiska skickligheten betonas. Möjligheten i matematiken att arbeta med rent formella uttryck framställs ofta som en garant för säkerhet. Då uppfattas den rena formen som något som kommer före förståelsen av uttrycken och därmed som något som är befriat från de problem som tolkning och förståelse är behäftade med, t.ex. missförstånd och tvetydighet. En närmare granskning visar däremot att även de uttryck som ur ett matematiskt perspektiv kan kallas rent formella ändå kräver att den som använder dem har tillägnat sig en typ av för-

ståelse, nämligen hur de ska användas enligt de regler som kalkylen föreskriver. Säkerheten som förknippas med det formella angreppssättet måste därför förstås på ett annat sätt än som att den har sitt ursprung i något rent formellt.

Bevisbegreppet belyses ur tre för Wittgenstein typiska idéer: att bevis formar våra begrepp, att bevis och experiment är väsensskilda och att bevis måste ha en överskådlighet för att kunna vara bevis. I många fall uppfattas bevisets roll vara att övertyga läsaren om att en sats är sann och dessutom uppfattas satsen som något man kan begripa även utan ett bevis. Denna föreställning om bevis harmonierar med uppfattningen om matematiken som en samling sanna satser (som kan förstås som sådana även utan bevis). Hur beviset då lyckas övertyga med säkerhet förefaller kräva en förklaring. Om man däremot beaktar att beviset ofta är det som överhuvudtaget möjliggör en förståelse av satsen och att beviset gör detta genom att på ett överskådligt sätt framställa användningen av de ingående begreppen så kommer bevisets förmåga inte att framstå som något mystiskt. Hur bevisandet sker måste däremot granskas från fall till fall, en allmängiltig förklaring av hur det sker är inte möjlig.

Ifall man utgår från bilden av matematiken som en samling sanna satser om matematiska objekt, så kommer matematikens säkerhet att kopplas till dessa satsers pålitlighet och våra möjligheter att avgöra om de beskriver de matematiska objekten korrekt. Den bild av matematikens säkerhet som framträder i ljuset av de olika övervägandena i avhandlingen är en annan. Ifall vi jämför matematiska satser med regler för användningen av matematiska begrepp så kommer den som lärt sig matematik inte att tvivla på de matematiska satserna. De matematiska satserna är en del av de strukturer med vars hjälp vi avgör om andra satser är sanna eller falska, och kommer därför inte att vara föremål för samma typ av värdering som vi utsätter andra satser för. De kommer att ha rollen av självklarheter som inte kan ifrågasättas. Den säkerhet som matematiken uppvisar hänger ihop med att vi i arbetet med bevis inser hur begreppen måste användas. De bevisade satserna får därmed karaktären av regler för hur vi ska handskas med de begrepp som ingår i beviset – både inom matematiken men också i tillämpningar på fenomen utanför matematiken.

Bibliography

Aspray, William and Philip Kitcher, eds. *History and Philosophy of Modern Mathematics*. Minneapolis: University of Minnesota Press, 1988.

Auslander, Joseph. 'On the Roles of Proofs in Mathematics'. In: *Proof and Other Dilemmas: Mathematics and Philosophy*. Ed. by Bonnie Gold and Roger A. Simons. Spectrum. Washington DC: Mathematical Association of America, 2008.

Avigad, Jeremy. 'Understanding Proofs'. In: *The Philosophy of Mathematical Practice*. Ed. by Paolo Mancosu. Oxford: Oxford University Press, 2008.

Ayer, Alfred Jules. *Language, Truth and Logic*. Harmondsworth: Penguin, 1971.

Azzouni, Jody. 'That We *See* That Some Diagrammatic Proofs Are Perfectly Rigorous'. In: *Philosophia Mathematica* 21 (2013), pp. 323–38.

— 'The Derivation-Indicator View of Mathematical Practice'. In: *Philosophia Mathematica* 12 (2004), pp. 81–105.

— *Tracking Reason: Proof, Consequence, and Truth*. New York: Oxford University Press, 2006.

Balacheff, Nicolas. 'Bridging Knowing and Proving in Mathematics: A Didactical Perspective'. In: *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives*. Ed. by Gila Hanna, Hans Niels Jahnke, and Helmut Pulte. New York: Springer, 2010.

Balaguer, Mark. *Platonism and Anti-Platonism in Mathematics*. New York: Oxford University Press, 1998.

Bassler, O. Bradley. 'The Surveyability of Mathematical Proof: A Historical Perspective'. In: *Synthese* 148 (2006), pp. 99–133.

Bell, A. W. 'A Study of Pupils' Proof-explanations in Mathematical Situations'. In: *Educational Studies in Mathematics* 7 (1976), pp. 23–40.

Bell, E. T. 'Mathematics and Credulity'. In: *The Journal of Philosophy* 22 (1925), pp. 449–58.

— *The Development of Mathematics*. 2nd ed. New York: McGraw-Hill, 1945.

Belnap, Nuel D. 'Tonk, Plonk, and Plink'. In: *Analysis* 22 (1962), pp. 130–134.

Benacerraf, Paul. 'Mathematical Truth'. In: *The Journal of Philosophy* 70 (1973), pp. 661–80.

— 'What Numbers Could Not Be'. In: *Philosophical Review* 74 (1965), pp. 47–73.

Benacerraf, Paul and Hilary Putnam, eds. *Philosophy of mathematics: Selected readings*. 2nd ed. Cambridge: Cambridge University Press, 1983.

Bernays, Paul. 'Reply to the Note by Mr. Aloys Müller, "On Numbers as Signs"'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. by Paolo Mancosu. New York: Oxford University Press, 1998.

— 'The Philosophy of Mathematics and Hilbert's Proof Theory'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998.

Black, Max. *The Nature of Mathematics: A Critical Survey*. London: Kegan Paul, Trench, Trubner, 1933.

Blumberg, Albert E. 'Logic, Modern'. In: *Encyclopedia of Philosophy*. Ed. by Paul Edwards. Vol. 5. New York: Macmillan, 1967.

Bourbaki, Nicholas. 'Architecture of Mathematics'. In: *The American Mathematical Monthly* 57 (1950), pp. 221–32.

Brandom, Robert B. *Making it Explicit*. Cambridge MA: Harvard University Press, 1994.

Bråting, Kajsa and Anders Öberg. 'Om matematiska begrepp – en filosofisk undersökning med tillämpningar'. In: *Filosofisk tidskrift* 26.4 (2005), pp. 11–17.

Breger, Herbert. 'Tacit Knowledge and Mathematical Progress'. In: *The Growth of Mathematical Knowledge*. Ed. by Emily Grosholz and Herbert Breger. Dordrecht: Kluwer, 2000.

Bueno, Otávio and Øystein Linnebo, eds. *New Waves in Philosophy of Mathematics*. Houndmills, Basingstoke: Palgrave Macmillan, 2009.

Burgess, John P. 'Review of Stewart Shapiro. *Philosophy of Mathematics: Structure and Ontology*'. In: *Notre Dame Journal of Formal Logic* 40 (1999), pp. 283–91.

Cardano, Girolamo. 'Cardan's Treatment of Imaginary Roots'. In: *A Source Book in Mathematics*. Ed. by David Eugene Smith. New York: McGraw-Hill, 1929.

Carnap, Rudolf. *Logical Syntax of Language*. New York: Humanities Press, 1951.

Carroll, Lewis. 'What the Tortoise Said to Achilles'. In: *Mind* 4 (1895), pp. 278–80.

Cellucci, Carlo. 'Why Proof? What is a Proof?' In: *Deduction, Computation, Experiment: Exploring the Effectiveness of Proof*. Ed. by G. Corsi and R. Lupacchini. Berlin: Springer, 2008.

Clark, Colin W. *Elementary Mathematical Analysis*. 2nd ed. Pacific Grove CA: Brooks/Cole, 1982.

Coleman, Edwin. 'The Surveyability of Long Proofs'. In: *Foundations of Science* 14 (2009), pp. 27–43.

Curry, Haskell B. 'Remarks on the Definition and Nature of Mathematics'. In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983.

Davis, Philip J. and Reuben Hersh. *The Mathematical Experience*. Boston: Birkhäuser, 1981.

Dawson, Jr., John W. 'Why Do Mathematicians Re-prove Theorems?' In: *Philosophia Mathematica* 14 (2006), pp. 269–86.

Descartes, René. 'Discourse on the Method'. In: *The Philosophical Writings of Descartes*. Ed. by John Cottingham, Robert Stoothoff, and Dugald Murdoch. Vol. 1. Cambridge: Cambridge University Press, 1985.

Detlefsen, Michael. 'Proof: Its Nature and Significance'. In: *Proof and Other Dilemmas: Mathematics and Philosophy*. Ed. by Bonnie Gold and Roger A. Simons. Spectrum. Washington DC: Mathematical Association of America, 2008.

— 'The Kantian Character of Hilbert's Formalism'. In: *Proceedings of the 15th International Wittgenstein-Symposium*. Vol. 1: *Philosophy of Mathematics*. Ed. by Johannes Czermak. Wien: Hölder-Pichler-Tempsky, 1993.

Devlin, Keith. *Mathematics: The Science of Patterns: The Search for Order in Life, Mind, and the Universe*. New York: Scientific American Library, 1994.

Diamond, Cora. 'How Long Is the Standard Meter in Paris?' In: *Wittgenstein in America*. Ed. by Timothy McCarthy and Sean C. Stidd. Oxford: Clarendon Press, 2001.

— *The Realistic Spirit: Wittgenstein, Philosophy and the Mind*. Cambridge MA: The MIT Press, 1991.

— 'Wittgenstein, Mathematics and Ethics: Resisting the Attractions of Realism'. In: *Cambridge Companion to Wittgenstein*. Ed. by Hans Sluga and David G. Stern. Cambridge: Cambridge University Press, 1996.

Dummett, Michael. 'Wittgenstein's Philosophy of Mathematics'. In: *The Philosophical Review* 68 (1959), pp. 324–48.

Epple, Moritz. 'The End of the Science of Quantity: Foundations of Analysis, 1860–1910'. In: *A History of Analysis*. Ed. by Hans Niels Jahnke. Providence RI: American

Mathematical Society and London Mathematical Society, 2003.

Etchemendy, John. *The Concept of Logical Consequence*. Cambridge MA: Harvard University Press, 1990.

— 'The Doctrine of Logic as Form'. In: *Linguistics and Philosophy* 6 (1983), pp. 319–34.

Feferman, Solomon. 'Mathematical Intuition vs. Mathematical Monsters'. In: *Synthese* 125 (2000), pp. 317–32.

Floyd, Juliet. 'Wittgenstein on $2, 2, 2 \ldots$: The Opening of *Remarks on the Foundations of Mathematics*'. In: *Synthese* 87 (1991), pp. 143–80.

Frege, Gottlob. 'Begriffsschrift'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967.

— *Nachgelassene Schriften und Wissenschaftlicher Briefwechsel*. Ed. by Hans Hermer, Friedrich Kambartel, and Friedrich Kaulbach. Vol. 2. Hamburg: Felix Meiner, 1976.

— *The Foundations of Arithmetic: A Logico-Mathematical Enquiry into the Concept of Number*. 2nd ed. Oxford: Blackwell, 1953.

— 'The Thought: A Logical Inquiry'. In: *Mind* 65 (1956), pp. 289–311.

Friederich, Simon. 'Motivating Wittgenstein's Perspective on Mathematical Sentences as Norms'. In: *Philosophia Mathematica* 19 (2011), pp. 1–19.

Fuss, P. H., ed. *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIéme siècle*. Vol. 1. St.-Pétersbourg: L'Académie impériale des sciences de St.-Pétersbourg, 1843.

Giaquinto, Marcus. *The Search for Certainty: A Philosophical Account of Foundations of Mathematics*. Oxford: Oxford University Press, 2002.

Gödel, Kurt. 'What is Cantor's Continuum Problem?' In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983.

Gold, Bonnie and Roger A. Simons, eds. *Proof and Other Dilemmas: Mathematics and Philosophy*. Spectrum. Washington DC: Mathematical Association of America, 2008.

Gorenstein, Daniel. 'The Enormous Theorem'. In: *Scientific American* 253.6 (1985), pp. 104–15.

Gowers, Tim. 'When are two proofs essentially the same?' In: *Gower's Weblog: Mathematics Related Discussions* (4/10/2007). URL: https://gowers.wordpress.com/2007/10/04/when-are-two-proofs-essentially-the-same/ (Accessed 09/05/2016).

Grattan-Guinness, Ivor, ed. *From Calculus to Set Theory 1630–1910: An Introductory History*. Princeton: Princeton University Press, 1980.

Grève, Sebastien and Felix Mühlhölzer. 'Wittgenstein's Philosophy of Mathematics: Felix Mühlhölzer in Conversation with Sebastian Grève'. In: *Nordic Wittgenstein Review* 3.2 (2014), pp. 151–78.

Gustafsson, Martin. 'Wittgenstein and "Tonk": Inference and Representation in the *Tractatus* (and Beyond)'. In: *Philosophical Topics* 42.2 (2014), pp. 75–99.

Häggström, Olle. 'Objective Truth versus Human Understanding in Mathematics and in Chess'. In: *The Montana Mathematics Enthusiast* 4 (2007), pp. 140–53.

Hanna, Gila. 'Proof, Explanation and Exploration: An Overview'. In: *Educational Studies in Mathematics* 44 (2000), pp. 5–23.

Hardy, G. H. *A Mathematician's Apology*. Cambridge: Cambridge University Press, 1967.

Heijenoort, Jean van, ed. *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*. Cambridge MA: Harvard University Press, 1967.

Hellman, Geoffrey. 'Structuralism'. In: *The Oxford Handbook of Philosophy of Mathematics and Logic*. Ed. by Stewart Shapiro. Oxford Handbooks in Philosophy. New York: Oxford University Press, 2005.

Hempel, Carl G. 'On the Nature of Mathematical Truth'. In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983.

'Interview with Alan Hajek'. In: *Masses of Formal Philosophy*. Ed. by Vincent F. Hendricks and John Symons. Automatic Press / VIP, 2006.

Henle, James. 'The Happy Formalist'. In: *The Mathematics Intelligencer* 13 (1991), pp. 12–18.

Hersh, Reuben. 'Proving Is Convincing and Explaining'. In: *Educational Studies in Mathematics* 24 (1993), pp. 389–99.

— 'Some Proposals for Reviving the Philosophy of Mathematics'. In: *New Directions in the Philosophy of Mathematics*. Ed. by Thomas Tymoczko. 2nd ed. Princeton: Princeton University Press, 1998.

Hertzberg, Lars. '"The Kind of Certainty is the Kind of Language Game"'. In: *Wittgenstein: Attention to Particulars. Essays in Honour of Rush Rhees (1905–89)*. Ed. by Dewi Z. Phillips and Peter Winch. London: Macmillan, 1989.

Hilbert, David. 'On the Infinite'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967.

— 'Problems of the Grounding of Mathematics'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998.

— *The Foundations of Geometry*. La Salle IL: Open Court, 1950.

— 'The New Grounding of Mathematics: First Report'. In: *From Brouwer To Hilbert. The Debate on the Foundations of Mathematics in the 1920s*. Ed. and trans. by Paolo Mancosu. New York: Oxford University Press, 1998.

Horsten, Leon. 'Philosophy of Mathematics'. In: *The Stanford Encyclopedia of Philosophy*. Ed. by Edward N. Zalta. Spring 2015. URL: http://plato.stanford.edu/archives/spr2015/entries/philosophy-mathematics/.

Hume, David. *A Treatise of Human Nature*. Ed. by David Fate Norton and Mary J. Norton. Oxford: Oxford University Press, 2000.

Jaffe, Arthur and Frank Quinn. '"Theoretical Mathematics": Towards a Cultural Synthesis of Mathematics and Theoretical Physics'. In: *Bulletin of The American Mathematical Society* 29 (1993), pp. 1–13.

Jahnke, Hans Niels, ed. *A History of Analysis*. Providence RI: American Mathematical Society and London Mathematical Society, 2003.

Keränen, Jukka. 'The Identity Problem for Realist Structuralism'. In: *Philosophia Mathematica* 9 (2001), pp. 308–30.

Kitcher, Philip. 'Mathematical Rigor – Who needs it?' In: *Noûs* 15 (1981), pp. 469–93.

— *The Nature of Mathematical Knowledge*. New York: Oxford University Press, 1984.

Kleene, Stephen Cole. *Introduction to Metamathematics*. New York: D. van Nostrand, 1952.

Kline, Morris. *Mathematics in Western Culture*. Oxford: Oxford University Press, 1953.

Kreisel, Georg. 'Hilbert's Programme'. In: *Philosophy of mathematics. Selected readings*. Ed. by Paul Benacerraf and Hilary Putnam. 2nd ed. Cambridge: Cambridge University Press, 1983.

— 'The Formalist-Positivist Doctrine of Mathematical Precision in the Light of Experience'. In: *L'Âge de la Science* 3 (1969), pp. 17–46.

Kripke, Saul A. *Wittgenstein on Rules and Private Language: An Elementary Exposition*. Oxford: Blackwell, 1982.

Lakatos, Imre. 'Infinite Regress and Foundations of Mathematics'. In: *Philosophical Papers*. Vol. 2: *Mathematics, Science and Epistemology*. Ed. by John Worrall and Gregory Currie. Cambridge: Cambridge University Press, 1978.

— *Proofs and Refutations: The Logic of Mathematical Discovery*. Ed. by John Worrall and Elie Zahar. Cambridge: Cambridge University Press, 1976.

Leibniz, Gottfried Wilhelm. *Philosophische Schriften*. Vol. 4: *Schriften zur Logik und zur philosophischen Grundlegung von Mathematik und Naturwissenschaft*. Ed. by Herbert Herring. Darmstadt: Wissenschaftliche Buchgesellschaft, 1992.

Leitgeb, Hannes. 'On Formal and Informal Provability'. In: *New Waves in Philosophy of Mathematics*. Ed. by Otávio Bueno and Øystein Linnebo. Houndmills, Basingstoke: Palgrave Macmillan, 2009.

Leng, Mary. '"Algebraic" Approaches to Mathematics'. In: *New Waves in Philosophy of Mathematics*. Ed. by Otávio Bueno and Øystein Linnebo. Houndmills, Basingstoke: Palgrave Macmillan, 2009.

Lipschütz-Yevick, Miriam. 'The Happy (Nonformalist) Mathematician'. In: *The Mathematics Intelligencer* 14 (1992), pp. 4–6.

Lützen, Jesper. 'The Foundation of Analysis in the 19th Century'. In: *A History of Analysis*. Ed. by Hans Niels Jahnke. Providence RI: American Mathematical Society and London Mathematical Society, 2003.

Mac Lane, Saunders. 'Structure in Mathematics'. In: *Philosophia Mathematica* 4 (1996), pp. 174–83.

Maddy, Penelope. *Defending the Axioms: On the Philosophical Foundations of Set Theory*. Oxford: Oxford University Press, 2011.

— 'Set Theoretic Naturalism'. In: *The Journal of Symbolic Logic* 61 (1996), pp. 490–514.

Malcolm, Norman. *Nothing Is Hidden: Wittgenstein's Criticism of his Early Thought*. Oxford: Blackwell, 1986.

Mancosu, Paolo, ed. *From Brouwer To Hilbert: The Debate on the Foundations of Mathematics in the 1920s*. New York: Oxford University Press, 1998.

— *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*. Oxford: Oxford University Press, 1996.

Marion, Mathieu. 'Wittgenstein on Surveyability of Proofs'. In: *The Oxford Handbook of Wittgenstein*. Ed. by Oskari Kuusela and Marie McGinn. Oxford: Oxford University Press, 2011.

Merzbach, Uta C. and Carl B. Boyer. *A History of Mathematics*. 3rd ed. Hoboken NJ: Wiley, 2011.

Mill, John Stuart. *Collected Works of John Stuart Mill*. Vol. 7–8: *A System of Logic: Ratiocinative and Inductive. Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation*. Ed. by John M. Robson. Toronto; London: University of Toronto Press; Routledge & Kegan Paul, 1974.

Moore, George Edward. 'A Defence of Common Sense'. In: *Contemporary British Philosophy: Personal Statements. (2nd series)*. Ed. by J. H. Muirhead. London: George Allen & Unwin, 1925.

— 'Proof of an External World'. In: *Proceedings of the British Academy* 25 (1939), pp. 273–300.

Moyal-Sharrock, Danièle. *Understanding Wittgenstein's* On certainty. New York: Palgrave Macmillan, 2004.

Mühlhölzer, Felix. '"A Mathematical Proof Must Be Surveyable": What Wittgenstein Meant by This and What It Implies'. In: *Grazer Philosophische Studien* 71 (2005), pp. 57–86.

— 'Mathematical Intuition and Natural Numbers: A Critical Discussion. Review of Charles Parsons' *Mathematical Thought and Its Objects*'. In: *Erkenntnis* 73 (2010), pp. 265–92.

— 'Wittgenstein and Surprises in Mathematics'. In: *Wittgenstein and the Future of Philosophy. A Reassessment after 50 years. Proceedings of the 24th International Wittgenstein-Symposium, 12th to 18th August 2001 Kirchberg am Wechsel.* Ed. by R. Haller and K. Puhl. Wien: öbv & hpt, 2002.

Müller, Aloys. 'Über Zahlen als Zeichen'. In: *Mathematische Annalen* 90 (1923), pp. 153–58.

Oliveira e Silva, Tomás, Siegfried Herzog, and Silvio Pardi. 'Empirical Verification of the Even Goldbach Conjecture and Computation of Prime Gaps up to $4 \cdot 10^{18}$'. In: *Mathematics of Computation* 83 (2014), pp. 2033–60.

Parsons, Charles. 'The Structuralist View of Mathematical Objects'. In: *Synthese* 84 (1990), pp. 303–46.

Pelc, Andrzej. 'Why Do We Believe Theorems?' In: *Philosophia Mathematica* 17 (2009), pp. 84–94.

Pipping, Nils. 'Die Goldbachsche Vermutung und der Goldbach-Vinogradovsche Satz'. In: *Acta Academiae Aboensis. Ser. B, Mathematica et physica* 11 (1938), pp. 4–25.

Plato. *The Republic*. New York: P. F. Collier, 1901.

Prior, A. N. 'The Runaway Inference-Ticket'. In: *Analysis* 21 (1960), pp. 38–39.

Putnam, Hilary. *Philosophical Papers*. Vol. 1: *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975.

— 'Mathematics Without Foundations'. In: *Philosophical Papers*. Vol. 1: *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975.

— 'Philosophy of Mathematics: Why Nothing Works'. In: *Words and Life*. Ed. by James Conant. Cambridge MA: Harvard University Press, 1994.

— 'Rethinking Mathematical Necessity'. In: *Words and Life*. Ed. by James Conant. Cambridge MA: Harvard University Press, 1994.

— 'What is Mathematical Truth?' In: *Philosophical Papers*. Vol. 1: *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975.

— *Words and Life*. Ed. by James Conant. Cambridge MA: Harvard University Press, 1994.

Quine, Willard Van Orman. 'Two Dogmas of Empiricism'. In: *Philosophical Review* 60 (1951), pp. 20–43.

— *Word and Object*. Cambridge MA: The MIT Press, 1960.

Rav, Yehuda. 'Why Do We Prove Theorems?' In: *Philosophia Mathematica* 7 (1999), pp. 5–41.

Räz, Tim. 'Say My Name: An Objection to *Ante Rem* Structuralism'. In: *Philosophia Mathematica* 23 (2014), pp. 116–25.

Resnik, Michael D. 'How Nominalist Is Hartry Field's Nominalism?' In: *Philosophical Studies* 47 (1985), pp. 163–81.

— *Mathematics as a Science of Patterns*. Oxford: Clarendon Press, 1997.

— 'Mathematics as a Science of Patterns: Ontology and Reference'. In: *Noûs* 15 (1981), pp. 529–50.

— 'Proof as a Source of Truth'. In: *Proof and Knowledge in Mathematics*. Ed. by Michael Detlefsen. London: Routledge, 1992.

Russell, Bertrand. *Introduction to Mathematical Philosophy*. London: George Allen & Unwin, 1919.

— *Portraits from Memory: and Other Essays*. London: George Allen & Unwin, 1956.

Schoenfield, Joseph R. *Mathematical Logic*. Reading MA: Addison-Wesley, 1967.

Shanker, Stuart G. *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*. New York: State Universtity of New York Press, 1987.

Shapiro, Stewart. *Foundations without Foundationalism: A Case for Second-order Logic*. Oxford: Oxford University Press, 2000.

— 'Identity, Indiscernibility, and *ante rem* Structuralism: The Tale of i and $-i$'. In: *Philosophia Mathematica* 16 (2008), pp. 285–309.

— *Philosophy of Mathematics: Structure and Ontology*. New York: Oxford University Press, 1997.

— 'Second-Order Languages and Mathematical Practice'. In: *The Journal of Symbolic Logic* 50 (1985), pp. 714–42.

— *Thinking about mathematics: The philosophy of mathematics*. Oxford: Oxford University Press, 2000.

Shwayder, D. S. 'Wittgenstein on Mathematics'. In: *Studies in the Philosophy of Wittgestein*. Ed. by Peter Winch. London: Routledge & Kegan Paul, 1969.

Sjögren, Jörgen. 'A Note on the Relation Between Formal and Informal Proof'. In: *Acta Analytica* 25 (2010), pp. 447–58.

Smart, Harold R. 'Is Mathematics a "Deductive" Science?' In: *The Philosophical Review* 38 (1929), pp. 232–45.

Stenius, Erik. '*Anschauung* and Formal Proof: A Comment on *Tractatus* 6.233'. In: *Critical Essays*. Ed. by Ingmar Pörn. Vol. 2. Helsinki: Societas Philosophica Fennica, 1989.

Stenlund, Sören. 'Different Senses of Finitude: An Inquiry into Hilbert's Finitism'. In: *Synthese* 185 (2012), pp. 335–63.

— 'Hilbert and the Problem of Clarifying the Infinite'. In: *Logicism, Intuitionism and Formalism: What Has Become of Them?* Ed. by Sten Lindström et al. Dordrecht: Springer, 2009.

— *Language and Philosophical Problems*. London: Routledge, 1990.

— 'The Limits of Formalization'. In: *Logic and Philosophy of Science in Uppsala: Papers from the 9th International Congress of Logic, Methodology and Philosophy of Science*. Ed. by Dag Prawitz and Dag Westerståhl. Dordrecht: Kluwer, 1994.

— *The Origin of Symbolic Mathematics and the End of the Science of Quantity*. Uppsala: Department of Philosophy, Uppsala University, 2014.

Stillwell, John. 'Logic and the Philosophy of Mathematics in the Nineteenth Century'. In: *Routledge History of Philosophy*. Vol. 7: *The Nineteenth Centuty*. Ed. by C. L. Ten. London: Routledge, 1994.

Tait, W. W. 'Truth and Proof: The Platonism of Mathematics'. In: *Synthese* 69 (1986), pp. 341–70.

Tanswell, Fenner. 'A Problem with the Dependence of Informal Proofs on Formal Proofs'. In: *Philosophia Mathematica* 23 (2015), pp. 295–310.

Tarski, Alfred. *Logic, Semantics, Metamathematics: Papers from 1923 to 1938*. Trans. by J. H. Woodger. Oxford: Clarendon Press, 1956.

— 'On the Concept of Logical Consequence'. In: *Logic, Semantics, Metamathematics. Papers from 1923 to 1938*. Trans. by J. H. Woodger. Oxford: Clarendon Press, 1956.

— 'The Concept of Truth in Formalized Languages'. In: *Logic, Semantics, Metamathematics. Papers from 1923 to 1938*. Trans. by J. H. Woodger. Oxford: Clarendon Press, 1956.

— 'The Semantic Conception of Truth: and the Foundations of Semantics'. In: *Philosophy and Phenomenological Research* 4 (1944), pp. 341–76.

Thurston, William P. 'On Proof and Progress in Mathematics'. In: *Bulletin of The American Mathematical Society* 30 (1994), pp. 161–77.

Villiers, Michael de. 'The Role and Function of Proof in Mathematics'. In: *Pythagoras* 24 (1990), pp. 17–24.

Weyl, Hermann. 'Comments on Hilbert's Second lecture on the Foundations of Mathematics'. In: *From Frege to Gödel. A Source Book in Mathematical Logic, 1879-1931*. Ed. by Jean van Heijenoort. Cambridge MA: Harvard University Press, 1967.

Wiener, Norbert. 'Is Mathematical Certainty Absolute?' In: *The Journal of Philosophy, Psychology and Scientific Methods* 12 (1915), pp. 568–74.

Wittgenstein, Ludwig. *On Certainty*. Ed. by G. E. M. Anscombe and G. H. von Wright. Oxford: Blackwell, 1969.

— *Philosophical Grammar*. Ed. by Rush Rhees. Oxford: Blackwell, 1974.

— *Philosophical Investigations*. Ed. by G. E. M. Anscombe and Rush Rhees. 3rd ed. Oxford: Blackwell, 2001.

— *Remarks on the Foundations of Mathematics*. Ed. by G. E. M. Ansombe, Rush Rhees, and G. H. von Wright. 3rd ed. Oxford: Blackwell, 1978.

Wittgenstein, Ludwig. *Tractatus Logico-Philosophicus*. London: Routledge & Kegan Paul, 1922.

— *Wittgenstein's Lectures, Cambridge 1932–1935*. Ed. by Alice Ambrose. Oxford: Blackwell, 1979.

— *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*. Ed. by Cora Diamond. Ithaca NY: Cornell University Press, 1976.

— *Wittgenstein's Nachlass: The Bergen Electronic Edition*. Oxford; Bergen: Oxford University Press & Wittgenstein Archives at the University of Bergen, 2000.

Wójtowicz, Krzysztof. 'Object Realism versus Mathematical Structuralism'. In: *Semiotica* 188 (2012), pp. 157–69.

Woleński, Jan. 'What is Formal in Formal Semantics?' In: *Essays on Logic and its Applications in Philosophy*. Frankfurt am Main: Peter Lang, 2011.

Wolgast, Elizabeth. *Paradoxes of Knowledge*. Ithaca NY: Cornell University Press, 1977.

— 'Whether Certainty Is a Form of Life'. In: *The Philosphical Quarterly* 37 (1987), pp. 151–65.

Wright, Georg Henrik von. *Explanation and Understanding*. London: Routledge & Kegan Paul, 1971.

— *Form and Content in Logic: An Inaugural Lecture*. Cambridge: Cambridge University Press, 1949.

— *Logik, filosofi och språk*. 2nd ed. Lund: Doxa, 1965.

Kim-Erik Berts

The Certainty of Mathematics

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Mathematics is often said to give us knowledge that it more reliable than that of other sciences. What do we mean by 'mathematical certainty'? Do we have a clear understanding of this concept?

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