Chapter 4

The Concept of Proof

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The fact that there is in mathematics, unlike in other sciences, a practice of proving propositions conclusively is often cited as evidence of the superior certainty of mathematics. This feature of mathematics is indeed interesting, not only because it is limited to mathematics and related disciplines such as formal logic and mathematical physics, but also because proofs are philosophically intriguing in their own right. What is the mark of a genuine proof? 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 (alongside calculation) are what grants to mathematics "the peculiar certainty" that puzzled, among others, Mill.[1] The role of proof in the mathematical enterprise is also of great importance for the understanding of mathematical knowledge as was discussed in chapter 2.

The present chapter will, first, review some answers to the question about the nature of mathematical proofs, and in particular where they draw the line between proofs proper and half-measures. Second, I will discuss the issue of our need for proof. Why do we prove theorems? And, thirdly, the question about whether proofs can serve these needs, and in what way an increased understanding of the functions that a proof can serve affects our conception of proof.

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[1] Cf. the quote on page 16.

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, e.g. 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 proof *of* its last formula, and this formula is said to be *(formally) provable* or to be a *(formal) theorem*.[2]

The development of formal axiomatic systems and with them the notion of formal proof was in part motivated by an awareness that it is sometimes difficult to overview all the presuppositions of proofs. This was the case for Hilbert in his axiomatisation of Euclidean geometry in *Grundlagen der Geometrie*, and arguably for Frege too in *Grundgesetze der Arithmetik*. However, these systems were not formal systems in the sense that this term acquired in the metamathematical tradition, where everything from the formation of formulas, to axioms and inference rules, to proofs and theorems is strictly regulated. If there was an awareness of difference between more and less strictly regulated proofs before the modern notion of formal systems, there is now a sharp divide between formal and what has come to be referred to as "informal" proofs. Since proofs of these two kinds often differ to a considerable degree from each other there arises the question of the relation between the two. We can distinguish a range of possible answers to this question.

1. Informal proofs are the genuine kind of proofs, 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 proofs, whereas informal ones only approximate, 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 but none is given priority over the other. Furthermore, informal proofs often, but not necessarily, translate into formal counterparts.

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[2]Kleene, *Introduction to Metamathematics*, p. 83.

It is interesting that explicit proponents of the second attitude are not too easy to find in the contemporary literature. This is a tad odd since (2) is often alluded to as the traditional conception, suggesting that it is a common view.[3] Perhaps it is fair to say that it is often taken as a "goes-without-saying" for many philosophers. The concept of formal proof was first put forth by the formalist school in the debate about the foundations of mathematics in the early twentieth century. However, 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".[4] It seems, however, that the notion of formal proof gradually came to be seen as a correct analysis of proof. One finds, e.*g.*, in Tarski's 1944 article "The Semantic Conception of Truth" the following remark: "Due to the development of modern logic, the notion of mathematical proof has undergone a far-reaching simplification."[5] While there is in this passage no explicit claim as to the general nature of proof – indeed, in the following sentence Tarski is speaking of formal systems – one can sense that the conception of proof is shifting towards a view where formal proofs are taken to be the standard. 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.[6] Another view close to (2) is Jody Azzouni's. He takes a proof to "*indicate* an 'underlying' derivation."[7].

During the last thirty years or so the attitude (1) has seen gained much popularity among philosophers who stress the importance of paying attention to the practice of mathematics. This movement partly coincides with the quasi-empiricist philosophers.

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[3]Michael Resnik calls it "a popular view". Resnik, "Proof as a Source of Truth", p. 12. Hannes Leitgeb, on the other hand, comments that this is the way the "prevailing tradition" treats of 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.

[4]This view (2) referred to as the "Hilbert thesis" by Yehuda Rav. "Why Do We Prove Theorems?" In: *Philosophia Mathematica* 7 (1999), pp. 5–41; and as the "Hilbert-Gentzen thesis" by 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-Verlag, 2008. It is not clear, however, that (2) above was Hilbert's view, although he is one of the persons, perhaps the main one, 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. Michael Detlefsen. "The Kantian Character of Hilbert's Formalism". In: *Proceedings of the 15th International Wittgenstein-Symposium*. Ed. by Johannes Czermak. Vol. 1. Wien: Verlag Hölder-Pichler-Tempsky, 1993.

[5]Alfred Tarski. "The Semantic Conception of Truth: and the Foundations of Semantics". In: *Philosophy and Phenomenological Research* 4 (1944), pp. 341–76, p. 372, n. 17.

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

[7]Jody Azzouni. "The Derivation-Indicator View of Mathematical Practice". In: *Philosophia Mathematica* 12 (2004), pp. 81–105, p. 84.

In both cases an important source of inspiration is Lakatos' work on proof. However before the 70's not many spoke explicitly in favour of (1) – Wittgenstein being a note-worthy exception. This is odd because informal proofs have always been the kind found in mathematics literature – also after the concept of formal proof became generally known. This is also true of the metamathematical literature.[8] 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 a theorem but to show that a theorem that have been established informally is in fact derivable in the system in question. (One could say that the existence of a formal proof can be seen as an informal proof about the formal system.) Now, this would seemingly indicate that (1) has always been the common view of proofs. One can say that the kind of 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 un-derstanding of mathematics. This attitude ranges from moderate acknowledging as in Shapiro's *Philosophy of Mahtematics: Structure and Ontology*:

> 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. Mathemati-cians at work however, are not usually concerned with ultimate justifica-tion but with understanding and explanation [. . .] I suggest, therefore, that philosophers who are attempting to understand mathematics concentrate less on the little used standards of ultimate justification and more on the actual work of mathematicians [. . .] Indeed, how a given subject matter is grasped should have something to do with what it ultimately is.[9]

To Carlo Celluci's dismissing of, not only formal proofs, but axiomatic proofs in general for what he calls "analytic proofs": "Axiomatic proof is no viable alternative to analytic proof since it is inadequate."[10]

In order to say something useful about the notion of proof and its connection to certainty, what is needed is a better understanding of informal, or ordinary, proofs. I would not give priority to any one of these kinds of proof in terms of "being a genuine proof", or "providing certainty". (Whether a proof shows its conclusion with certainty

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[8]Kleene, e.g., stresses this difference and mentions the he will use "suggestive terminology" and refer to formal derivations as proofs, indicating that priority should be given to "informal" proofs. Kleene, *Introduction to Metamathematics*, p. 65

[9]Shapiro, *Philosophy of Mathematics. Structure and Ontology*, s. 186.

[10]Cellucci, "Why Proof? What is a Proof?", p. 12.

is not a matter of its being of a particular kind, but something that must be judged from case to case, from proof to proof.) I would however say, that informal proofs are logically 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. Furthermore, as became clear in chapter 3 one should not put too much weight on the difference between formal and informal mathematics. 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 left out, and where this sometimes leads to mistakes in the proofs. However, that steps are left out does not entail that mistakes arise, not even that mistakes are more probable. As we saw in the previous chapter, 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 now 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.[11] I think that a greater understanding of proofs (starting with informal proofs) can be achieved by investigating our need for proof, and futhermore, which roles a proof can play in mathematics. Therefore, the rest of this chapter will be devoted to the question that also Yehuda Rav discusses in an article with the same title: "Why do we prove theorems?"[12]

Resten av detta kapitel ska vara en översättning av en text jag presenterat tidigare och som börjar vara någorlunda i skick (Den finns också i decembernumret av Norsk filosofisk tidskrift). Det viktiga är att luckra upp den starka spontana uppfattningen att ett bevis övertygar om att en sats är sann. Att detta är bevisets enda uppgift och att bara ett bevis kan åstadkomma sådan övertygelse.

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[11]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.

[12]Rav, "Why Do We Prove Theorems?"

Chapter 5

Proof and Certainty

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.[1]

5.1 Introduction

The previous chapter was an attempt to show that the importance of the concept of proof might be due, not to a supposed uniform convincing power, but to its diverse position in mathematics: as convincing, as providing understanding, as a means of communication, as challenge, etc. If one realises that a proof may play many different parts in the dealings with propositions and calculations in mathematics, a question such 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?'", or the like. 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". This makes proofs appear as object-like things, whereas I believe it is important to keep in mind that proving things is something we do. And, 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.

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

As was mentioned in the previous chapter, the possibility of proving propositions in mathematics seems to grant to its propositions a peculiar certainty and set it 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 it can play, one may still feel a philosophical need to sort out what lies in being convinced after having proved something or having read a proof. I wrote (p. 104.) that one may ask whether being convinced is not a criterion for having understood a proof that belongs to the mathematical canon. This is connected to an experience many mathematics students have – that learning to prove things involves learning which proofs are convincing. What is convincing is not (at least not for everybody) evident without training. This may sound like sidestepping the problem, because (1) it leaves the finding of proofs for new theorems out of consideration, but more importantly (2) it makes it seem as if being convinced is an ability gained through indoctrination. I will therefore discuss the issue of conviction and its connection to mathematical understanding in this chapter. As I hope to show, conviction and understanding are related in a non-contingent way. [för vagt?]

Another reason for discussing this question in a separate chapter is that many of the writers who have emphasised the different roles of proofs, discussed in the previous chapter, 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 convincing power. Their inspiration is found, e.g. in Lakatos' idea that proofs are fallible and open to refutation, as are investigations in empirical sciences. Instead, they often stress that proofs can have the role of furthering understanding. As is evident from the previous chapter, I too want to emphasise this role of proofs. However, I do not agree with the quasi-empiricist thesis. As I see it, there are differences between mathematics and empirical sciences that are important for the philosophical problems about mathematics. As discussed in chapter 1 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 100% 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, merely placing mathematics a few notches from the top.

An important difference can be seen in a difference between proofs and experiments that Wittgenstein devotes much attention to in his writings on the philosophy of mathematics. This brings out a characteristic of proof that separates it from other areas of discourse. I believe, furthermore, that this difference and other themes in Wittgenstein's philosophy which are closely related to this distinction illuminate the above mentioned connection between conviction and understanding as well. It also does something to indicate that the concept of certainty must be understood on its own terms in mathematics.

5.2   Proof and Experiment

On more than one occasion Wittgenstein draws attention to 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."[2] This comparison is also found with respect to calculation and experiments: "It is enlightening to look on a calculation as a picture of an experiment."[3] What does he mean by calling them "pictures of experiments" and how can this increase our understanding of mathematical proofs?

I shall try to show the relevance of these remarks for the present investigation by contrasting them with two other important themes in Wittgenstein's philosophy of mathematics: the idea of proofs as "concept-forming", and the thought that a proof must be perspicuous. These themes combined provide an ample argument against the idea that the same concepts *certainty* and *knowledge* are in play in mathematics as well as in other areas of discourse.

The first of these themes, the difference between a proof or calculation on the one hand, and an experiment on the other, is present throughout Wittgenstein's philosophy. As early as in the *Tractatus Logico-Philosophicus* Wittgenstein drew attention to this difference: "Calculation is not an experiment."[4] In *Philosophical Grammar* Wittgenstein

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[2]Wittgenstein, *Remarks on the Foundations of Mathematics*, 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.

[3]Wittgenstein, *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, p. 98.

[4]Ludwig Wittgenstein. *Tractatus Logico-Philosophicus*. London: Routledge & Kegan Paul, 1922, § 6.2331.

gives it 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."[5]

These and other remarks on the philosophy of mathematics are sometimes seen as a critique of realism (or Platonism) in mathematics.[6] While this may be a correct interpretation of Wittgenstein, I would rather stress their value as reminders that mathematics need not be understood as a science about something, e.g., quantity and magnitude, and that mathematics need not be seen as a body-of-truths as was seen in chapter 2. On this view (the body-of-truths picture) mathematical propositions are easily seen in analogy to empirical propositions.

If a mathematical proposition is seen as a truth about something, 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. Or, 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 (it might not hold for all cases although one has made experiments showing that it holds for many cases). The point of both procedures would be to *convince one that something is the case*.

Now, as discussed in chapter 4, the point of proving is not only to convince oneself of the truth of a proposition. If this was the only point, then it would seem that running sufficiently many numerical tests would do just as well as working out a proof. In the case of Goldbach's conjecture,[7] this has indeed been done, e.g. by the Åbo Akademi University mathematician Nils Pipping who in the 1930's verified this conjecture for numbers $n \leq 10^5$, without the aid of digital computers.[8] Today, this testing is performed

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[5]Wittgenstein, *Philosophical Grammar*, II, V, 22, p. 361.

[6]Cf. Robert J. Fogelin. "Wittgenstein and Intuitionism". In: *American Philosophical Quarterly* 5 (1968), pp. 267–74, p. 267; and Barry Stroud. "Wittgenstein and Logical Necessity". In: *The Philosophical Review* 74 (1965), pp. 504–18, p. 505.

[7]Christian Goldbach proposed two conjectures in correspondence with Leonhard Euler in 1742, and in a reply of June, 30 the latter writes that these follow from another conjecture which Goldbach had already mentioned earlier. "Dass aber ein jeder numerus par eine summa duorum primorum sey, halte ich für ein ganz gewisses theorema, ungeachtet ich dasselbe nicht demonstrieren kann." P. H. Fuss, ed. *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIeme siècle*. Vol. 1. St.-Pétersbourg: L'académie Impériale des Sciences de Saint-Pétersbourg, 1843, p. 135. This is what is now usually referred to as "Goldbach's conjecture", and it can be phrased thus: *Every even number which is greater than two is the sum of two primes*. There is still no proof for it.

[8]Nils Pipping. "Die Goldbachsche Vermutung und der Goldbach-Vinogradovsche Satz". In: *Acta Academiae Aboensis. Ser. B, Mathematica et physica.* 11 (1938), 4–25.

with the help of computers. The search for a proof is, however, not abandoned although there is hardly any doubt as to the validity of the conjecture.

The proof of the four colour map theorem by Kenneth Appel and Wolfgand Haken in 1976 stirred up 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 would presumably not have been a problem. William 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.[9]

Wittgenstein's remark begins to draw attention to features of proofs that indicate the essential difference between experiments and proofs. He points out that while a picture of a proof is still a proof, a picture of an experiment would not be an experiment.[10] If we conduct an experiment we simply have to accept the outcome and take note of it. This is not the case in calculation or proof, since there is always the question of calculating *correctly* and drawing *correct* conclusions.

Two objections come to mind. A short examination of these will hopefully make the need for pointing out the difference more obvious. (1) "There are also 'wrong' and 'right' outcomes of an experiment." This is true thus far: if there is an unexpected outcome one will probably conclude that something is interfering with the experiment. The scales may not be calibrated, the containers were contaminated, etc. If the circumstances of the experiment are controlled, however, the outcome is as good as any. Deviations in outcomes of experiments are as interesting as the expected ones (arguably more interesting). In the case of a surprising outcome, it is surprising because one cannot 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 theories. The point of a scientific theory, I would say, is to come to terms with this inability of ours to see the processes in nature, and experiments are a means for getting some insight into these processes.

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[9]Thurston, "On Proof and Progress in Mathematics", p. 162.

[10]Ludwig Wittgenstein. *Wittgenstein's Nachlass. The Bergen Electronic Edition*. Oxford & New York; 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 have been left out by the editors.

If one, on the other hand, did not overview the exact path to the result of a calculation, it would not be a calculation at all. And 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. 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.[11]

Another objection is (2) "One has to accept the result of a calculation just as much as an experiment, one cannot *decide* that it is right or wrong." It is true that one will have to accept the result of a calculation too. But, 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 the correct one or not too. Since it is possible to be mistaken in calculation or inference, there is a need for checking the outcomes of calculations, but this is a different kind of judgment. In mathematics there is exactly one correct result and it is completely determined by the correct application of the rules. Why the correct result should be obtained is completely perspicuous in the calculation – in an experiment one has to work out a theory to explain the outcome.

If one has a mathematical model of a phenomenon of the world and uses it to predict the outcome of a future experiment, there is of course the question whether the outcome corresponds to the result of the calculation. If it does not, and one concludes that the experiment was *not* affected by some unwanted factors, one will have to revise the model instead. If someone arrives at a different result in calculation there is not a question of revising mathematics, because there is no such thing as a result of a calculation standing a part from the calculation that leads up to it. (Cf. above page 22) As Wittgenstein puts it:

> A mathematical proposition is related to its proof as the outer surface of a body is to the body itself. We might talk of the body of proof belonging to the proposition. Only on the assumption that there's a body behind the surface

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[11]Wittgenstein, *Remarks on the Foundations of Mathematics*, 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.

has the proposition any significance for us.[12]

There is nothing but the proof or chain of inferences or calculation that determines what is the result. An outcome of an experiment is a completely different matter.

Hertzberg has suggested an example which illuminates the difference between proofs and experiments. Suppose that I find a drawing consisting of a serier 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.[13]

A recurring theme in Wittgenstein's writings on rule-following is the picture of a mechanism and the remark that when one follows a rule it is not like working as a rigid mechanism. The idea in critical focus could be captured: rules somehow compel us to behave exactly as the rule prescribes (with the addition – if we are following it correctly), as rigidly as the causal laws of nature determine how a physical mechanism behaves. It therefore seems as if one could make an experiment and see what will come out if one performs a certain calculation according to the rules. This would be to regard oneself as a computing machine, which when given a certain input gives back a definite output. Testing different calculations with an electronic computer may, I think, rightfully be called experimenting. The difference between checking what result I will arrive at and what result a computer produces is, of course, that whereas the answer that an electronic calculator gives out is the product of a causal process, the result that the calculating human being comes up with is not. [detta borde kanske sägas något om men utan att gå destomera in på mind-body problemet] However, as Wittgenstein remarks, a calculation could be an experiment carried out with human beings too.[14] One could, e.g., give one or more persons the task to complete a calculation and take note of the result they produce. What would make this an experiment is the nature of it. "It is the use that is made of something that turns it into an experiment."[15] It could, e.g., be an experiment to find out on average how many percent of human beings that fail to produce the correct result. In this case it is not vital to the success of the experiment that the test persons calculate correctly, the outcome is interesting as such, as data for

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[12]Ludwig Wittgenstein. *Philosophical Remarks*. Ed. by Rush Rhees. Oxford: Basil Blackwell, 1975, XIII § 162, p. 192.

[13]Personal discussion.

[14]Cf. Wittgenstein, *Remarks on the Foundations of Mathematics*, I §§ 157–61.

[15]Ibid., I § 161.

further investigations. In this case it is obvious that a picture of the calculation that these persons have performed would still be a calculation, but it would no longer be the experiment that was conducted.

Wittgenstein's remarks on machines are similar to the ones on proofs.[16] He 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 points out that this picture does not do the job it is supposed to do. One has to picture some kind of super rigid mechanism, since ordinary machines do break, or malfunction.[17] 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 picture one has of oneself as a calculating machine that follows simple mechanical rules may not be an entirely innocent one. As Wittgenstein writes: that one feels completely compelled to do what the rule demands is simply a feature of having understood the rule.[18] This does in no way require a causal determination. "'Mechanical' – that means without thinking. But *entirely* without thinking? Without *reflecting*."[19]

On an other level we could study the relationship between a physical machine and a picture of it. This might show why it is misleading to regard mathematics as a kind of rigid mechanism that is completely determined as soon as axioms and rules of inference are laid down. From the picture of the machine, the blueprint, we could judge how the machine works and what kind of job it can perform. We can use the picture to draw all sorts of conclusions about the working of the machine. But does the machine have to behave according to what we have concluded about it from its blueprint? No, it could break for instance. Does this falsify the conclusions we drew? Not necessarily. We might just say that it would have worked according to our conclusions if it had not broken. The breaking of the machine was not, so to speak, part of the machine's proper behaviour, and we need not be troubled by the fact that it broke. A picture of the blueprint is still a blueprint, but a picture of how the machine works is not the working of the machine.

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[16]In *Remarks on the Foundations of Mathematics*, part VII they even merge. Cf § 73.

[17]Wittgenstein, *Philosophical Investigations*, § 193.

[18]Ibid., § 231.

[19]Wittgenstein, *Remarks on the Foundations of Mathematics*, VII § 60.

5.3   Concept-formation and perspicuity

In his writings on the philosophy of mathematics, Wittgenstein often remarks that accepting a proof means accepting a new criterion for something, a new paradigm for the evaluation of something, adopting a new rule of expression (or grammar) or a new *concept* of something. A typical 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 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."[20]

The claim that one upon inspection of, e.g., a pentagram and a hand accepts a new criterion for "having the same number", taken as a general description of what happens when a mathematical proposition is proven, sounds rather naïve. It does however illustrate how a proof may bring about a new connection between two hitherto unrelated concepts. It also allows one to see why Wittgenstein talks of "pictures of experiments". There placing of the fingers on the points of a pentagram might 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, e.g., judging whether a figure is a pentagram or not. Furhtermore, this "picture" allows one to overview the possible outcomes of an experiment where one tries to correlate one's fingers with the points of a pentagram.

In the case of the decimal expansion of π it is, says Wittgenstein, not clear what it means to say that there, e.g., occurs the sequence 777 somewhere in it. If it was proven that it this sequence does occur in the decimal expansion of π we would also know what the proposition means. A proof, i.e. the proof-techniques used, would show what it means for this particular sequence occurring means. It is not clear beforehand what it means in the same way as it is clear what it means that the figure "09" recurs in the decimal expansion of $\frac{1}{11}$ or what it means to find coffee in my cupboard – I know beforehand what it means that a certain figure recurs in the decimal expansion of a fraction and I know what I will count as a criterion for there being coffee in the cupboard, but I do not know beforehand what I will count as a criterion for there being

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[20]Wittgenstein, *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge 1939*, p. 73.

777 in the decimal expansion. A would-be proof would show me what it means, that is, *how* 777 can occur in the expansion, and thus give me a new concept, a new criterion for there occurring 777 in the expansion.

Perhaps our conundrum with the decimal expansion of π or with Goldbach's conjecture can be compared to the problems that the ancient Greeks faced with regard to, what we now call, irrational numbers. E.g., for the Pythagorean school, the ratio between the diagonal and the side of a square, which cannot be expressed as a ratio between natural numbers, caused much alarm. A mathematically satisfying solution to this problem could only be given when the notion of square root had been introduced. Problems such as the one with the decimal expansion of π and Goldbach's theorem can be formulated in one brief question or conjecture respectively and does not require any mathematical expertise to understand. It it therefore appears odd that the solutions should require that new concepts that we do not yet understand be introduced or that we extend concepts we have. How can this be when we understand the problem?

Another historical example where a concept is changed as a consequence of a proof is Cardano's proof that the equation $(10 - x)x = 40$ has a solution, only an *imaginary* solution. (See p. 67.) This case shows that it is even misleading to say that the concept is changed *due to* the proof, because *seeing* it as a proof cannot be separated from experiencing a change in one's conception of *solution*. Thus, the issue of concept-formation cannot be described thus: we have a problem which we understand, we get a proof and now our concepts change because of the proof. I do not think the following is a correct description either: we have a problem that we understand, and then a concept is changed and a solution to the problem becomes possible. In the first case the proof and the change of concepts are inseparable, and perhaps a change in the understanding of the problem is necessary too. In the second case a the new understanding of the concept enables a new understanding of the problem, and thus a solution that was not within reach before.

With regard to problems such as Goldbach's conjecture Wittgenstein writes: "There is no meaning to saying you can describe beforehand what a solution will be like in mathematics except in the cases where there is a known method of solution."[21]

> [W]hat kind of verification do I count as valid for my hypothesis? Or can I
> *faute de mieux* allow an empirical one to hold for the time being until I have

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[21]Ludwig Wittgenstein. *Wittgenstein's Lectures, Cambridge 1932–1935*. Ed. by Alice Ambrose. Oxford: Basil Blackwell, 1979, p. 7.

a "strict proof"? No. Until there is such a proof, there is no connection at all between my hypothesis and the "concept" of a prime number.[22]

Now, for a proof to have this role the proof must have a certain perspicuity. It must be possible to see from the proof how the concepts are connected, and also why it is that they can be connected this way. Wittgenstein writes:

> 'Proof must be capable of being taken in' really means nothing but: a proof is not an experiment. We do not accept the result of a proof because it results once, or because it often results. But we see in the proof the reason for saying that this *must* be the result.[23]

In this perspicuity lies also the key to understanding the certainty of proofs. It should allow one to form a thorough conception of how the concepts involved can be used and not used. But this also requires a background knowledge and a certain proficiency. Perspicuity is not an absolute notion but connected to what one is used to. Thurston comments on some of his proofs that he had to spend a great deal of time conveying the "mathematical infrastructure" in order for people to understand his proofs.[24] Thurston's example is of course of in the extreme end of not being generally accessible, but the same phenomenon occurs early in mathematics education. It is difficult to appreciate the certainty of proof in elementary analysis if one has not previously achieved some familiarity with concepts such as continuity, convergence, derivative etc. It is as if familiarity allows one to see the connections that the proof makes. Concerning this certainty Wittgenstein remarks:

> In producing a new concept [the proof] convinces me of something. For it is essential to this conviction that the procedure according to these rules must always produce the same configuration. ("Same", that is, by our ordinary rules of comparison and copying.)[25]

Understanding and accepting a proof has more in common with understanding and accepting that certain concepts can be used in a certain way, rather than becoming convinced that something is the case (which might be the case when performing experiments). Understanding a proof cannot therefore be separated from a conviction that the concepts can be used in a certain way.

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[22]Wittgenstein, *Philosophical Grammar*, II, V § 22, p. 359.

[23]Wittgenstein, *Remarks on the Foundations of Mathematics*, III § 39. Cf. also: "When I wrote 'proof must be perspicuous' that meant: *causality* plays no part in the proof." ibid., IV § 41.

[24]Thurston, "On Proof and Progress in Mathematics", p. 175.

[25]Wittgenstein, *Remarks on the Foundations of Mathematics*, VII § 72.

> And how does it come out that the proof *compels* me? Well, in the fact that once I have got it I go ahead in such-and-such a way, and refuse any other path. All I should further say as a final argument against someone who did not want to go that way, would be: "Why, don't you see . . . !" – and that is no *argument*.[26]

Imagine that I have read a proof and am satisfied that it proves a proposition, but that a friend does not see it as conclusive. What can we do to reach a consensus concerning the proof? That we are not prepared to accept the disagreement as we might if one of us prefers coffee while the other prefers tea is an important feature of our dealings with mathematics, as was pointed out in chapter 1. We would try to make each other understand the matter our way, but we do not try to convince each other to accept the other's "point of view" – that is we do not treat the disagreement as one of different points of view. What we say will be things that we say to a person who has not yet understood. Understanding cannot be separated from conviction.

This is not however a general description of what happens when one is convinced by a proof. How this happens will rather have to be seen separately in each proof.

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[26]Ibid., I § 34.

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