The following lecture, delivered at Utrecht University on 3.11.2009, is a slightly modified excerpt from a very much longer essay entitled 'Grammar and Necessity' in the 2nd edition of G. P. Baker and P. M. S. Hacker, *Wittgenstein: Rules, Grammar and Necessity* (Wiley-Blackwell, Oxford, 2009), pp. 241-370.

Proof in mathematics

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To say that mathematics is normative (RFM 425) is to characterize the *use* of propositions of mathematics (MS 123, 49v). There is no doubt, Wittgenstein wrote, that mathematical propositions in certain language-games play the role of rules of representation, are schemata of description rather than descriptions – even though this sharp differentiation fades away in all directions (MS 124, 34). Mathematics forms networks of norms (RFM 431). This insight lies at the heart of Wittgenstein's reflections on the nature of mathematics. Much of the compilation known as *Remarks on the Foundations of Mathematics* is the elaborate working out of its radical consequences. It is failure to see this that explains some of the most common and widespread misinterpretations of Wittgenstein's philosophy of mathematics.

He wrote 'One could also express my problem thus: is it correct to conceive of mathematics as a class of true propositions' (MS 122, 115r). The answer has to be 'Yes, and No'. Yes – this is indeed how we represent propositions of mathematics to ourselves: we call '25 × 25 = 625' a true proposition of arithmetic, and call '25 × 25 = 624' a false one. And there is nothing amiss with that. No – in as much as true propositions of mathematics are not true descriptions but rules, and mathematics itself is a network of rules woven together by proofs and calculations. For an arithmetical proposition to be true is not for it to *correspond* to anything, and it is not for *things in reality to be as the proposition says they are*.

Precisely because mathematics is a system of acknowledged rules, rather than a system of beliefs, it is essential to the system that it has an application. 'It is essential to mathematics', Wittgenstein wrote, 'that its signs are also employed in *mufti*. It is the use outside mathematics, and so the *meaning* of the signs [die *Bedeutung* der Zeichen], that makes the sign-game into mathematics' (RFM 257). The translation of 'also die *Bedeutung* der Zeichen' as 'and so the *meaning* of the signs' is, I think, misleading. One could equally well translate this phrase as ‘that is to say, the significance of the signs’ – the significance of the signs lying precisely in *their use outside mathematics*, in their civic function, as it were. This different translation has the merit of eliminating the apparent conflict between Wittgenstein’s asserting, on the one hand, that the sense of a mathematical equation is determined by its proof, and, on the other, holding that its meaning is determined by its extra-mathematical use. It also eliminates any suggestion that Wittgenstein is here invoking a distinction between ‘Sinn’ and ‘Bedeutung’. Rather, what he is pointing out is that the system of mathematics, this complex system of signs and patterns of derivation, gains its significance from its extra-mathematical application.

What we call *knowing propositions of mathematics* is not independent of knowing the use we make of them. If there were people who used numerals and mathematical proofs – impeccable and sophisticated proofs to boot – *exclusively to decorate wallpaper* (LFM 36), could we say that they knew mathematics and had mastered mathematical concepts? Would they not be like someone who was acquainted with the standard metre bar in Paris, but knew nothing about the institution of measuring (RFM 167)? Knowing that *this* measure is 1 metre is a preparation for measuring. Similarly mathematics is part of the *apparatus* of language – a preparation for the use of mathematical concepts in extra-mathematical contexts (LFM 250). Of course, the use of mathematics *within* mathematics (e.g. ‘The number of reals is greater than the number of rationals’ or ‘quadratic equations have two roots’) makes it appear as if this is a descriptive application of mathematics to mathematics itself, and none other is needed to give the rules of mathematics a use (LFM 48). Indeed, this is *one* root of the idea of a mathematical domain of mathematical objects that can be studied by the mathematician (LFM 150) and described by metamathematical propositions. But this, Wittgenstein argued, is mistaken – for such reflexive employments of mathematics merely generate *further rules* within the body mathematical – the results are deposited in the archives, just like any other mathematical propositions. Metamathematics is just more mathematics – a calculus like any other (WWK 121, 136; PG 296).

The mathematician is not a discoverer of anything, but an inventor or creator of new forms of description. His inventions enable us to describe and transform descriptions of magnitudes and quantities, rates of change, wave forms and oscillations, and so on and so forth. A form of description has a use in giving and transforming descriptions – *otherwise a network of forms is just a game with signs*. Mathematical propositions can indeed be compared to a fishing net – each strand of which is a proof connecting mathematical propositions with other mathematical propositions by means of proofs that knot each new strand into the web. The resultant net enables us to catch fish – the fish being truths about reality arrived at by empirical sciences such as physics and chemistry, economics and psychology that employ mathematical techniques in describing, explaining and predicting empirical phenomena. And now reflect that if 'fishing nets' were used exclusively for decorating the walls of taverns and never for catching fish, they would not be fishing nets at all; something is a fishing net only if its point and purpose is to catch fish.) It is from this vantage point that we shall view Wittgenstein's widely misunderstood remarks on mathematical proof.

We must start from an obvious objection: surely *a proof* is not just *constructing a proposition* – doesn't it also show *that the proposition is true*? Of course – Wittgenstein does not deny that mathematical propositions are true. But insisting that a what a proof proves is that its result is true gets us nowhere. For to say that the proposition that *p* is true is equivalent to saying that *p* (LFM 68). The moot questions are:

i. How is the proof related to the proposition proved?

ii. What exactly is it that has been proved when a mathematical proposition has been proved?

iii. What does one non-trivially agree to when one accepts that the result, the proven theorem, follows from the premises of the proof?

It is tempting to think that the relationship between a mathematical proposition and its proof is analogous to that between an empirical proposition and its verification. For we are inclined to think that the meanings (senses) of the constituent symbols and their mode of combination give the unproven mathematical proposition its meaning (sense), while its proof determines its truth-value (which is, roughly speaking, how Frege thought of the matter). But that, Wittgenstein wrote, 'is exactly what I *don't* want to say. Or: it is exactly this that seems to me to be the misleading aspect'

(MS 123, 64v).

'Nothing is more disastrous to philosophical understanding', he wrote, 'than the conception of proof and experience as two different – yet still comparable – methods of verification' (BT 419). When we try to verify a scientific proposition, the sense of the proposition is determined and the only question is whether it is true. Hence we can describe a *possible* way of verifying it prior to *actually* verifying it. So, for example, prior to the eclipse of the sun in 1921, Eddington could describe exactly what changes in the photographic plates that he exposed would verify Einstein's theory and what would falsify it. But when we prove a mathematical proposition we are determining its sense – *for the proof is part of the grammar of the proposition proved* (BT 630, 636). But, Wittgenstein argued, as we have seen, that the mathematician is not like an explorer who can describe the North Pole and what it is like to arrive there in advance of the expedition. In mathematics one cannot describe a possible proof without giving the proof. Here, one might say, whatever is possible is actual. If one can describe the route to the mathematical objective, then one has arrived there. In constructing a proof we are forming a new conceptual link and hence *a new concept*. What a proof does is not to show that a certain proposition correctly describes how things are, but rather it integrates a new rule into a body of rules – weaving a new strand into the web of mathematics. It effects *an extension of the grammar of number* – not a new addition to a body of well-confirmed truths about numbers, on the model of the addition of a newly verified scientific hypothesis to the body of empirical knowledge.

It is precisely because of this that Wittgenstein emphasized that the result of a proof is the end-face of a proof-body. The result of a proof should be seen as the terminus of a mathematical process; the proof-process does not *lead to* this result – the result *is itself part of the process* (the end part). Hence if one wants to know what is proved, one must look at the proof, not simply at the theorem proved. For what the proof effects is a road from existing mathematical rules (propositions) to a new mathematical rule (proposition). So one finds, not the result, but *that one reaches it* (RFM 248). It is not, for example, the property of a certain number that such-and-such a process leads to it, but rather the number *is* the end of this process. And it is not getting such-and-such a result that makes us accept it, but rather *its being the end of this route* (RFM 171). For it is the chain of linked propositions that warrant the novel concept-formation. It is an exaggeration (hence partly true and partly false), but an illuminating one, to say that one cannot know what has been proved until one knows what is called a proof of it (LFM 39). Hence Wittgenstein's radical remarks on mathematical conjectures.

A proof is not like a dark tunnel connecting premises and theorem – it must be *surveyable*. This thought is central to Wittgenstein's reflections. What Wittgenstein meant when he spoke of the requirement of the surveyability of mathematical proof, although perhaps related to his insistence that the concept of a surveyable representation is an essential aspect of his new methodology and conception of philosophy (PI §122), it is nevertheless not the same point that is being made. After all, he notes again and again that it is a distinctive and important feature of the grammars of natural languages that they are *deficient* in surveyability. And what is distinctive about his new philosophical methods is that they give a surveyable representation of some aspect or other of grammar that causes conceptual problems and unclarities. But proof in mathematics, unlike the grammar of natural language, *must be surveyable*. The surveyability of proof is multifaceted. I shall summarize the salient features that Wittgenstein sought to capture with this often invoked requirement.

First of all, this dictum is invoked as one aspect of Wittgenstein's extensive efforts to hammer home the differences between a proof and an experiment (RFM 170). Unlike the result of an experiment, we don't accept the result of a proof because we get it once or because we usually get it when we try. Rather, we see in the proof the reason for saying that this *must* be the result (ibid.) – and only if the proof is surveyable does it make sense to say that we see this.[1] The identity of an experiment (which is not the same as an experimental demonstration to pupils at school or university) is determined by the experimental conditions and the experimental set-up. The result of the experiment remains to be seen – it is in the hands of nature and causal laws. The identity of a proof, by contrast, turns not merely on the premises, but also on the result – the result is a part of the proof. If there were a different result, it would be a different proof. Hence too, to repeat an experiment (as

[1] Hence unless a computer-generated proof (e.g. the Apel-Haken proof for the four-colour problem) is merely an enormous amount of 'homework', it is debatable what is meant by calling it a proof at all.

opposed to doing a demonstration) means to repeat the appropriate conditions under which a certain result was previously obtained – and then *to see what happens*. But causality plays no part in a proof (RFM 246). To repeat a proof means to repeat every step *and the result*. Each proof reproduction must contain the force of the proof acknowledged in acceptance of its result (RFM 187). Finally, in a proof, unlike in an experiment, there cannot be a hidden process that produces the result without our knowing how it does so (MS 117 (Vol. XIII), 199).

Secondly, it is an important fact that mathematicians do not in general quarrel over the result of a calculation (PPF §341), that disputes do not break out among them over whether a rule has been followed or not (PI §240). That would not be so were proofs not *readily and exactly reproducible* – and in that further sense, *surveyable.* Were they not, then our concept of mathematical certainty would not exist either (PPF §341). Ready reproducibility is essential for the peaceful agreement that we find regarding correctness of calculations and ratifications of proofs (RFM 365), since a condition for that agreement is that we can be *certain* that we have the same proof a second time. So the criteria of identity of a proof must be perspicuous, must involve precisely those features that are essential for the proof and no others (e.g. in a geometrical proof the lines or angles of the drawn figure need not be absolutely exact in order for the reproduction to be exact). All this is constitutive of what we deem a proof. These conditions are corollaries of the requirement that we should be able to work over the same proof again and again, to check it, and to show it to others. If there were an *unsurveyable* proof pattern (as one would find in a Russellian proof that 25 × 25 = 625) and if by a change in notation (e.g. by the use of decimal notation) we produced a surveyable one – then we would have proof where there was none before (RFM 143). When, in a Hilbertian calculus using strings of ‘1’-s or ‘1 + 1 + 1 + . . .’ to represent natural numbers or in a stroke calculus in which natural numbers are represented by sequences of strokes (such as | | | | | | | | | | | | | | | | | | | | | | | | | | | | |), the signs are no longer surveyable, then the unsurveyable pattern to which they belong no longer counts as a proof.

Thirdly, Wittgenstein associated the surveyability of a proof with the requirement that the identity of the transformations of the proof must be established immediately or directly (*unmittelbar*) – it must be evident that *this* follows from *that*. In this sense, the steps of the proof pattern must be intuitive (*anschaulich*): 'if we are no longer convinced by what we *see*, then the proof has lost its force' (MS 122, 80v). Everything relevant to the proof must be in plain view. To repeat, we *see* in the proof the reason for saying that this *must* be the result (RFM 170), i.e. that this *follows* from that.

Fourthly, the dictum that a proof must be surveyable means that a proof must be capable of being taken as a picture, model or paradigm – a standard of what we shall count as a correct calculation. We are persuaded to take the proof-picture as a model for what it is like if . . . (RFM 170). It must not be *imaginable* for *this* substitution in *this* expression to yield anything else (RFM 173).

Finally, Wittgenstein invokes the same dictum to emphasize that the *way* in which the proof produces its result is incorporated into what he called the proof-picture (MS 123, 58r). What a proof shows is not just *that* this proposition follows from that one, but *why* it follows (RFM 159). Hence too, a proof 'does not merely show *that* it is like this, but: *how* it is like this. It shows [for example] *how* 13 + 14 yield 27' (RFM 159). For the proof pattern is a paradigm for *the kind of way* in which the proof result can be produced. But

> . . . suppose that in *that way* you got one time this and another time another result; would you accept this? Would you not say: "I must have made a mistake; the *same* kind of way would always have to produce the same result"? This shows that you are incorporating the result of the transformation into the kind of way the transforming is done.' (RFM 70)

That is why, as we have seen, Wittgenstein invoked the metaphor that understanding a proof is grasping a route, not reaching a destination – a proof introduces a rule as the endpoint of a new road. (One might say that the proof is the road, not a building that lies at the end of the road. To construct a proof is to build a road – the buildings one may *then* erect lie in the application of what was proved, in what one then does with it.) That is also why he argued that there are no surprises in mathematics – no dark underground passages leading one from premises to conclusion – for nothing is hidden.

Does mathematics, then, 'just twist and turn about within these rules? – It forms ever new rules: is always building new roads for traffic; by extending the network of the old ones' ((RFM 99). 'A mathematical proposition determines a path, lays down a path for us' (RFM 228). Of course, a proven mathematical proposition seems to point to a reality outside itself. Nothing is more likely than that the propositional form of the result of a mathematical proof will delude us with a myth of a mathematical reality (RFM 162). But in fact the proven mathematical proposition, viewed as the end-face of the construction of a rule, 'is only the expression of acceptance of a new measure of reality'. For 'we take the constructability (provability) of this symbol (that is, of the mathematical proposition) as a sign that we are to transform symbols in such-and-such a way' (RFM 163).

An arithmetical proof changes the grammar of number by introducing a new rule, and in so doing, of course, it changes our concepts (MS 122,44v). For it makes new grammatical connections where there were none before (RFM 166). For each new proof weaves fresh strands into the web of mathematics. It does not discover external properties of numbers or functions, but grafts further *internal* properties onto the existing pattern of internal relations.

It makes no sense to suppose that *rules of grammar* pre-exist our linguistic practices. So if Wittgenstein is right in suggesting that mathematical propositions are rules, that they constitute the grammar of number, then the very idea of mathematicians *discovering* mathematical truths, the idea that the truths of mathematics are Eternal Truths that mathematicians have to *find out*, is incoherent. For not only does this confuse sempiternality with atemporality, it also confuses true descriptions with rules. Mathematical propositions cannot be said to be 'true for all eternity' nor can one say of them that 'they were true before human beings existed and will be true after there are no longer any human beings' – rather their truth is non-temporal, time does not come into them at all. That is a mark of their being concept-forming. In this respect they are akin to other grammatical propositions that give the appearance of sempiternality and are commonly thought of as 'metaphysical', for example that red is more like orange than like yellow, that it is darker than pink, that nothing can be red and green all over at the same time', and so forth. For here too time does not enter these propositions – not because they are sempiternal, but because they are atemporal propositions that constitute the grammar of colour. So too, in as much as grammatical propositions are rules, and in as much as arithmetical propositions constitute the grammar of number, they cannot intelligibly be said to *pre-exist* our linguistic practices or to be something that we *discover* as we discover the laws of nature.

This is one of the lessons that we should have learned from Wittgenstein's detailed examination of the concept of following a rule, of the relationship between a rule and what counts as an act in accordance with it, and of the pivotal role of human practices in determining that internal relation. Let me very briefly remind you of the salient points of that formidably difficult argument. That a rule formulation expresses a rule that is complied with by such-and-such behaviour depends upon the regular behaviour that is viewed normatively by participants in a practice of following the rule in question. That is: the regularity must be *apprehended* as a uniformity, and that uniformity must be regarded as *standard setting* and exhibited in a *practice* of behaviour that counts as compliance. For it is the practice of employing the rule *R* as a guide to conduct and as a standard of correct conduct in appropriate circumstances that fixes the concepts of *complying with R* and *transgressing R*. It is only in the context of the appropriate normative behaviour of justifying what one does by reference to the rule (and its formulation) and explaining what one does by citing the rule, as well as criticizing deviations from the rule by explaining that *this* does not count as *accord* but *that* does – it is only in such a context that it even makes sense to speak of there being a rule at all. For the internal relation that obtains between a rule and acts of following the rule reflects the grammatical relation between the expression of the rule and the description of what counts as acting in accord with it. And that relation obtains insofar as there is a practice of acting thus in the circumstances, and of the complex normative behaviour that surrounds the normative practice.

We should now apply those reflections to the case of a proof – where we now conceive of the result of a proof as a new rule. What would it mean to say that a mathematical rule, which allows one to transform a linguistic description, existed antecedently to its proof. Does it make sense to speak of *discovering* new rules? Can there *be* a rule before anyone knows the rule – and indeed before anyone would even have been able to understand the expression of the rule? There is no such thing as there being a rule for the use of a sign which no one understands, and no such thing as a rule for the transformation of signs which no one ever uses as a standard for the correct use of the signs. Rules can be said to exist only in the context of human practices – and they owe their identity to what *counts*, in the practice of their use, as *following them*. A rule for the use of a sign establishes an internal relation between the rule and the acts that accord with it – but only insofar as there is a regular practice of acting thus-and-so, of judging that such-and-such accords with it and that such-and-such conflicts with it. For only then *is* there a rule.

Mathematical proof effects connections allowing the replacement of one expression by another. In making such connections, it also creates the concepts of these connections – concepts that were not there prior to the proof (RFM 298). A proven equation, for example, links two concepts, so that now one can pass from the one to the other (RFM 267). Even the mundane multiplication of cardinal numbers creates new concepts, e.g. the concept of *being the product of 25 × 25* – a concept under which 625 falls. So, in the course of a proof our way of seeing is changed, remodelled (RFM 239). We can now redescribe a collection of 25 boxes, each of 25 widgets, as consisting of 625 widgets in all, *without counting up afresh*. Moreover, we now have a new criterion of counting correctly, for if one now counts them up and finds that there are 625 widgets, then one's count is correct, since 25 × 25 *is* 625. Further, we have a new criterion for nothing's having been added or taken away – for assuming that one has counted them all up correctly, then if there are not 625 widgets in all, then either some were added or some were taken away. So, a proof teaches us to operate with concepts in a new way, and so changes the way we work with concepts (RFM 413). We can now make such-and-such calculations instead of counting and measuring *de novo*.

Opmerking: Explain this in more detail

Opmerking: Insert here a remark on the use of multiplication of negative integers

A proof demonstrates an internal relation of a structure (a proposition, a number, etc.) (MS 123, 59v-60r). Does the proposition proved *assert* the obtaining of an internal relation? That would be misleading. It asserts what its proof proves (namely: the content of a rule), and the proof demonstrates an internal relation; but the mathematical proposition itself cannot be said to *assert* an internal relation. It proves that we now have such-and-such an instrument for such-and-such a use (MS 123, 65v-66r). One might, however, say that what the mathematical proof proves is *set up* as an internal relation and withdrawn from doubt (RFM 363). And that is the mark of the determination of a rule for description: that henceforth *this* is to count as *that*, that *this* transformation is to be *called* a correct inference. ‘What the proof proves is that the proposition is true: that here we *have* an instrument for *such-and-such* a use’ (MS 123, 66r).

Of course, this is not done by a kind of magic or by stipulation. In producing a concept, a proof convinces one of something (RFM 435). On the one hand, it must be a procedure of which one could say as one goes through it: ‘This is how it has to be; this must come out if I proceed according to this rule’ (RFM 160). On the other hand, it has to persuade one to *do* something. For its purpose, after all, is not to convince one of how things are – that is the function of empirical evidence that confirms a hypothesis. What the proof has done is to forge a link from a rule or rules to a new rule – not to convince one of a new truth about an extra-symbolic reality. This conception goes against the grain of a long tradition of philosophical and mathematical reflection. But it is an inevitable corollary of Wittgenstein’s normative conception of mathematics – as he noted ‘Everything lies in looking at the familiar from a new side’ (MS 122, 68r).

What misleads us most in our endeavour to grasp the nature and role of proof in mathematics is the introduction of psychological concepts – as when we say that one must *agree* with the steps of the proof, that a proof must *convince* one, that one is brought to *believe* a mathematical proposition, and so forth. The epistemological vocabulary induces us to think that the acceptance of a proof is akin to the acceptance of the verification of a hypothesis. But the employment of epistemic terms in relation to necessary propositions involves subtle but crucial deviations from their more common use in relation to empirical propositions. Acknowledging a mathematical proof is not a matter of having a feeling of conviction, of nodding one’s head, or of uttering the result of the proof with confidence. What is important is what one *does* with the proposition one thus acknowledges – what *use* the acknowledgement expresses (MS 123, 59r). To be sure, a proof convinces us of something – but what is the object of this conviction? The assertion that a proof convinces one of the truth of the proposition proved ‘leaves us cold’, Wittgenstein wrote, ‘since this expression is capable of the most various constructions’ (RFM 161). In particular, as long as one continues to look at mathematical propositions solely under the guise of truths verified by proofs, the concept of the truth of a mathematical proposition will mislead us (just as the concept of the truth of a moral principle, such as Democritus’s principle, is liable to mislead one). The mathematical proposition of which one is convinced by a proof is an *instrument*. The function of the proof is not to convince one to believe or not to believe, to nod or shake one's head, but to *use* the instrument. The question is: *how is the instrument now to be applied*. We are convinced by a simple multiplication, say of 25 by 25. And now what? What do we do with the 'conviction' that 25 × 25 = 625 (MS 122, 52r.)? We must always bear in mind that in mathematics we are convinced of *grammatical propositions*. The result of our being convinced, the *expression* of our being convinced, is that we *accept a rule* (RFM 162). Hence it is not enough to say 'I'm willing to recognize this construction as the proof of this proposition', for one must add: 'this proposition, which I use thus-and-so' (MS 122, 61r). The proof does not persuade one *that* things are thus-and-so – but rather, it persuades one *that one should extend one's concept*, change it thus-and-so, apply it in these and these new ways (MS 122,59r).

What we call a proof is in effect the integration of a new rule of grammar into the network of rules of the grammar of number. It persuades us by making connections – it puts the proven proposition in the midst of a huge system of rules, and we are taught to adopt any rule that can be so produced (LFM 134f.). It has a sense *in the system of proofs*. Outside that network it would be a perfectly useless substitution-rule, which merely says that instead of these signs one may write those signs (LFM 132). It would be a strand that has not been interwoven into the net. But within the net, the new strand creates a new concept – and the consequence of accepting the proof is that one *applies* the concept determined by the rule, according to the rule. To adopt a rule is to *decide to act* in certain ways. Hence Wittgenstein queried 'Why should I not say: in the proof I have won through to a decision?' he immediately added that one might also say 'The proof convinces me that this rule serves my purposes', but adds cautiously 'But to say this might easily be misunderstood' (RFM 163) – for he is not defending a form of pragmatism regarding mathematical truths.

It is important to realize that Wittgenstein's preoccupation with *decision* in relation to proof has nothing to do with 'mathematical existentialism' or 'full-blooded conventionalism', but everything to do with what is involved in the acceptance of a new rule of representation. Dummett, in his review of the *Remarks on the Foundations of Mathematics*, contended that according to

Wittgenstein we are free to accept or reject any proof as we please. He expressed bafflement that according to Wittgenstein a proof does not *compel* one to accept the truth of its result, but 'is supposed to have the effect of persuading us, inducing us, to count such-and-such a form of words as unassailably true . . . [but] it seems quite unclear how the proof accomplishes this remarkable feat' (ibid. p. 332). This mislocates the point at which decision comes in, as well as what the decision is. The misinterpretation stems from supposing that according to Wittgenstein, we make a decision *on whether the result of the proof is true*. To be sure, we are all used to thinking of a proof as *compelling* one – that if one were to refuse to accept the conclusion of a proof, then 'logic would take you by the throat, and *force* you to' (Lewis Carroll). It seems to us, once we have followed the proof, that we *cannot* accept the premises without going on to accept the result. If these and these propositions are true, then *this* proposition *must* be true. – But now transform this picture into normative terms. If these and these rules have been adopted, and if these rules of logical inference are employed – then *this* rule, consonant with the existing system of such rules, follows. So what? What follows from the fact that the new rule follows? Well, the proof guides one's thought into new conceptual channels – it *persuades* one *to use a new instrument in one's reasoning*. Hence Wittgenstein queries:

> Can I say: "A proof induces us to make a certain decision; namely that of accepting a particular concept-formation"?
>
> Do not look at 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. (RFM 238f.)

One may still object. After all, *a concept* does not convince one of anything! A concept does not show one any new fact the obtaining of which is shown by the proof – but *a proof* convinces one! – That is indeed how we think of proofs. But it is misleading – as should be evident as soon as one starts to think of proofs as laying down new paths for thought. After all, the new paths are useless unless one decides to go down them! And the proof cannot *force* one to do anything – how, to use Dummett's phrase, could it perform such a remarkable feat? Should one not rather say that the proof generates a new concept and convinces one *to use that concept*? The proof, one might say, guides one to adopt a new concept, and the new concept immediately licenses transitions to judgments (RFM 298).

Let us dismantle the notion of compulsion further, while acknowledging that we do naturally think of a proof as compelling us. What is it supposed to compel us to do? It shows that such-and-such follows from these premises according to acknowledged rules of inference. We are inclined to say: it follows *necessarily.* This may mean one of two different things. It may simply mean that it 'follows within this system (within this network of norms of representation)'. In that case, the 'necessarily' adds nothing to the 'follows'. Alternatively, 'it follows necessarily' merely draws a contrast between proving that an equation has only one solution, and proving that an equation has a disjunction of solutions. But this latter contrast is immaterial here.

Still, one wants to exclaim: "*This* follows inexorably from *that.*" – True, Wittgenstein replies, 'in this demonstration this issues from that. This is a demonstration for whoever acknowledges it as a demonstration. If anyone *doesn't* acknowledge it, doesn't go by it as a demonstration, then he has parted company with us before anything is said' (RFM 60). He hammers this crucial point home in a powerful passage:

> "Then according to you everybody could continue the series as he likes; and so infer *any*how!" In that case we shan't call it "continuing the series" and presumably not "inference". And thinking and inferring (like counting) is of course bounded for us, not by an arbitrary definition, but by the natural limits corresponding to the body of what can be called the role of thinking and inferring in our life.
>
> For we are at one over this, that the laws of inference do not compel him to say or to write such and such like rails compelling a locomotive. And if you say that while he may indeed *say* it, still he can't *think* it, then I am only saying that this means, not: try as he may he can't think it, but: it is for us an essential part of 'thinking' that – in talking, writing, etc. – he makes *this sort* of transition. (RFM 80)

So compulsion is not the issue. One can do as one pleases, but to do anything other than THIS *is to refuse to infer correctly*.

> When I say "This proposition follows from that one", that is to accept a rule. This acceptance is *based* on the proof. That is to say, I find this chain (this figure) acceptable as a *proof*. – "But could I do otherwise? Don't I *have* to find it acceptable?" – Why do you say you have to? Because at the end of the proof you say, e.g.: "Yes, I have to accept this conclusion." But that is after all, only the expression of your unconditional acceptance. (RFM 50)

One says 'I have to admit this' with reference to each step of the proof, and with reference to the proof as a whole, i.e. as one traces the route from premises to the proposition proved. For what one admits is that *this* step follows from the previous one, and that this proposition, the theorem, follows from the proof, is the end-face of a proof-body. So why the talk of compulsion? We are inclined to say of the proof pattern that it is inexorable. But, Wittgenstein retorts, 'it can be "inexorable" only *in its consequences*! For otherwise it is nothing but a picture' (RFM 61). It is a picture, i.e. a pattern that we have created according to rules. When we follow through the proof, and having reached the end – the result of the proof – we say with conviction 'Yes, that's right' (RFM 97). We may indeed say 'Yes, it *must* be like that' – but what does this mean? The 'must', Wittgenstein remarks, shows that one has gone in a circle, in the sense that now *this outcome is defined as essential to this* (*mathematical*) *process*. We now take arriving at this result as a criterion for having followed this process. In short, we adopt a new concept (RFM 309). The 'action at a distance' of the proof pattern consists in the fact that henceforth one applies it in one's reasoning and in one's descriptions (RFM 61f.).

That is why Wittgenstein suggested that one may say that in the proof *one wins through to a decision*. But this is not a decision on the *truth* of the proposition proved. It is a decision to form and apply a new concept in judgements (RFM 238). The proof shows what it makes *sense* to say (RFM 164) and one decides to fix one's use of language accordingly (RFM 309). *That* is what a proof *persuades* one to do – one lets it guide one's thought into new channels. Having made that decision for the reasons provided by the proof, having adopted the new concept and the transformation licence the proof gives, one *cleaves to the rule*. It is not that the new rule compels one to act thus-and-so, but rather 'that it makes it possible for me to hold by it and let it compel me' (RFM 429) – *as one lets the rules of a game compel one*.

Of course, there is a quite different way in which decision comes into mathematics. But again, the decision is not on the *truth* of anything. We must distinguish between acceptance of a new proof and the decision which that implies, and the introduction of a new system, for example the system of signed integers. Here acceptance of the new system involved a decision of a quite different kind, and it is a striking feature in the history of European mathematics how contentious that decision was.[2]

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[2] In the fifteenth century Nicholas Chuquet, and in the sixteenth Michael Stifel, spoke of negative numbers as absurdities. Girolamo Cardano gave negative numbers as roots of equations, but thought that these were impossible solutions, mere symbols. Descartes accepted them reluctantly and equivocally. Pascal remarked mockingly 'I have known those who could not understand that to take four from zero there remains zero.' Arnauld questioned whether – 1/+1 = +1/ – 1; since – 1 is less than + 1, how can the smaller stand to the greater as the greater to the smaller? John Wallis accepted negative numbers, but thought that division by a negative number must yield a ratio greater than infinity since the denominator is less than 0. Difficulties plagued mathematicians well into the nineteenth century (William Frend, Lazare Carnot, De Morgan). William Rowan Hamilton summed up the 'absurdities' involved in accepting negative (and complex) numbers in 1837:

> But it requires no peculiar scepticism to doubt or even to disbelieve, the doctrine of Negatives and Imaginaries, when set forth (as it has commonly been) with principles like these: that *a greater magnitude may be subtracted from a less*, and that the remainder is *less than nothing*; that *two negative numbers*, or numbers denoting magnitudes each less than nothing, may be multiplied the one by the other, and that the product will be a positive number, or a number denoting a magnitude greater than nothing; and that although the *square* of a number, or the product obtained by multiplying that number by itself, is therefore *always positive*, whether the number be positive or negative, yet that numbers, called *imaginary*, can be found or conceived or determined, and operated on by all the rules of positive and negative numbers, as if they were subject to those rules, *although they have negative squares*, and must therefore be supposed to be themselves neither positives nor negative, nor yet null numbers, so that the magnitudes which they are supposed to denote can be neither greater than nothing, nor less than nothing, nor even equal to nothing. It must be hard to found a SCIENCE on such grounds as these. . . .

The issue is amusingly discussed in M. Kline, *Mathematics, the Loss of Certainty* (Oxford University Press, New York, 1980). In fact, the difficulties are not only amusing, but also philosophically illuminating. On the one hand, calculations with negative integers were often castigated as mere manipulation of symbols, whereas computations with positive integers were held to have genuine content. Controversy focused on whether any reality corresponded to negative integers, whether negative numbers could be clearly conceived, and what was revealed by the fact that successful predictions in mechanics could be derived from calculations involving negative numbers. What should have been scrutinized instead was what it means to claim that computations with positive integers are *not* mere manipulations of symbols or that *some reality corresponds to positive integers*. On the other hand, mathematical qualms about negative integers reflected misconceptions about the applications of arithmetic coupled with a dim apprehension of the conceptual connection between mathematical and non-mathematical statements. We are inclined to take the primary application of elementary arithmetic of the integers to be to *counting* things. Hence we explain subtraction of integers in terms of taking away a subset from a set of objects or of excluding a subset and counting the remainder. Thus '12 – 7 = 5' seems to express a principle governing counting in the same way that '12 + 7 = 19' does. But from this point of view, '5 – 7 = – 2' is unintelligible. Of course, it makes no sense to construe '0 – 4 = – 4' as saying how many things are left if four are taken away from none. But it would betray even greater confusion to conclude that our acceptance of the use of negative integers reveals that, contrary to Pascal's blinkered view, this question can be answered by saying 'minus four'. Rather, what should be scrutinized is what the *full* range of applications of arithmetical

Nothing forced mathematicians to adopt the new system incorporating negative numbers, and nothing in the pre-existing system of natural numbers and arithmetical operations *compelled* them to countenance the prima facie outrageous ideas that, for example, for any negative numbers *a* and *b*, '$a \times b > 0$', or that $-1/1 = 1/-1$. Rather, they accepted an analogy between cardinal numbers and signed integers, and between operations on them. To accept the general propositions that for any integers *a* and *b*, $(-a) \times (-b) = a \times b$, that $(-a) \times b = -(a \times b)$ and that $a \times (-b) = -(a \times b)$ is the *only* extension of the operation of multiplication that preserves the distributive law for multiplication of cardinal numbers. That was a weighty consideration. But nothing *forced* mathematicians to extend arithmetic thus. The distributive law, after all, is not akin to the law of gravity. It does not stand to numbers as the law of gravity stands to objects with a mass. It is not a description of natural regularities, but a *rule* for a system of symbols. If it is to be extended, *that extension is not a discovery*. It is a free, although of course not an arbitrary, *decision*.

In general, however, mathematical creativity involving new extensions of mathematics is neither 'homework' (such as doing a novel multiplication) nor the introduction of a new system, but the devising of new proofs that extend mathematics and persuade us to adopt new rules and new concepts.

What I have tried to do in this lecture is not to persuade you that Wittgenstein's philosophy of mathematics is correct. I have hardly told you anything about Wittgenstein's philosophy of mathematics, let alone of its ramifying consequences. All I have tried to do is to get you to look in a direction different from that indicated by most writers on this subject. I have tried to get you into a position from which you can profitably start to read Wittgenstein's later reflections on mathematics, and to think seriously about what he says, unblinkered by the ready-mades of 'full-blooded

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equations is and whether equations between *negative* integers do not share other applications with equations between positive quantities. What now seems blinkered in controversies about negative integers in fact was derived from important but limited insights. And modern discussion of the history of mathematics would itself be blinkered if it failed to do justice to these insights or if it derided earlier mathematicians for their philosophical qualms in the belief that further mathematical developments have resolved these (e.g. the definition of negative integers as ordered pairs of integers). Philosophical qualms can only be resolved by philosophical elucidation, not by technical sophistication, which serves merely to conceal them.

conventionalism', 'strict-finitism', 'de-psychologized intuitionism', 'assertability-conditional semantics' and so forth. I hope you will be able to do this.

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