Reasoning In Mathematics and Machines: The Place of Mathematical Logic in Mathematical Understanding

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Abstract. Mathematical logic and mechanical reasoning have turned out to be largely irrelevant to the practice of mathematics, and to our philosophical understanding of the nature of that practice. My aim is to understand how this can be. We will see that the problem is not merely that the logician formalizes. Nor even is it, as Poincaré argues, that logicians replace all distinctively mathematical steps of reasoning with strictly logical ones. Instead, as will be shown by way of a variety of examples, the problem lies in the way the symbolic language of mathematical logic has been read.

What has mathematical logic to do with mathematical understanding?[1] One would have thought quite a lot. Mathematics is a paradigm of rational activity, of rigorous reasoning; and rigorous reasoning is a central concern of mathematical logic. So, one would think, any adequate understanding of mathematical practice would essentially involve appeal to mathematical logic. One would think. And yet it is by now *clear* that mathematical logic, together with its formalized, mechanistic proofs in which every step conforms to a recognized rule of that logic, is of *no* mathematical interest. Such proofs do not advance mathematical understanding; they are not more rigorous than the informal proofs that mathematicians actually produce; and very often they are simply unintelligible.[2] Mathematical logic, it has turned out, is irrelevant to the practice of mathematics—and to our philosophical investigations into the nature of that practice. [3] Where mathematical logic *has* proved exceptionally fruitful is, of course, in computing. Indeed, according to Kriesel ([2], 143-4), "the clear recognition of just how much reasoning—that is, as far as results are concerned, never mind the processes—can be mechanized is surely the most outstanding contribution of 20th century logic *sub specie aeternitatis*." I think that we should be *very* puzzled by this. Mathematical logic—which, as Burgess ([9], 9) points out, "was developed . . . as an extension of traditional logic mainly, if not solely, about proof procedures in mathematics"—provides the foundations for computer science, mechanical reasoning, but seems to be altogether irrelevant to mathematical reasoning. *How can this be?*

For much of the twentieth century the received view was that mathematical logic and rigorous, mechanical reasoning are less relevant to mathematical practice than one might initially have expected because fully rigorous, formalized proofs are simply too long and tedious to be bothered with in mathematical practice. On this view, mathematicians in their practice take for granted myriad little steps of logic, focusing instead on the mathematically significant steps of a proof. Because in a formalization of a mathematician's proof there are no jumps or gaps in the chain of reasoning, because every step conforms to a small number of antecedently specified rules of logic, what is mathematically interesting about a proof tends, so it is claimed, to get buried in the logical detail of a fully formalized proof.[4] But this is not right. The relationship between a mathematician's proof and a fully formalized proof is not in general that between a gappy and a gap-free proof. In fact, "the translation from informal to formal is by no means a mere matter of routine [as it would be were one only filling in missing steps of logic]. In most cases it requires considerable ingenuity, and has the feel of a fresh and separate mathematical problem in itself. In some cases the formalization is so elusive as to seem impossible" (Robinson [5], 54). Formalizing a mathematician's proof is not so much a matter of formalizing *that* proof (by filling in all the steps) as it is giving a completely different proof, indeed, a different kind of proof. A mathematician's proof is, for example, often explanatory; a formalized proof is not.[5] Mathematicians' proofs are not sketches of formal proofs, essentially like them save for omitting some steps, but instead something quite different.

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[1] It perhaps needs to be emphasized that my concern here is with mathematical *understanding*, not with mathematics as such. That mathematical logic, for example, model theory, has made useful contributions to the discipline of mathematics seems clear—though even here mathematical logic has contributed less to mathematics as a discipline than one might have anticipated it would.

[2] All these points are well documented in the literature. See, for example, [1, 2, 3, 4, 5], and [6].

[3] That "mathematical logic cannot provide the tools for an adequate analysis of mathematics and its development" is, according to Mancosu [7], 5, one of the three main tenets of the "maverick" tradition in the philosophy of mathematics. It is also a main theme in Grosholz [8].

[4] For a logician's account see, for example, Suppes [10], 128. Mac Lane [11], 377, gives a mathematician's slant on the claim.

[5] As Robinson [5], 56, notes, "formalizing a proof has nothing whatsoever to do with its cognitive role as an *explanation*—indeed, it typically destroys all traces of the explanatory power of the informal proof".

Mathematical reasoning, the reasoning that mathematicians actually engage in, and logical reasoning as understood in mathematical logic, as essentially mechanical, are very different.[6] Most obviously, mathematical reasoning is focused on mathematical ideas while logical reasoning takes account only of logical form. Whereas a fully rigorous proof, in the logician's sense of rigor, is one each step of which conforms to some antecedently specified rule of pure logic and is thoroughly machine checkable, a rigorous proof in the mathematician's sense of rigor is instead one that a mathematician can see to be compelling by focusing on the relevant mathematical ideas and their implications. The two notions of rigor are different and often they are incompatible insofar as the logician's formalizations can undermine the rigor—in the mathematician's sense of rigor—of a chain of reasoning. As Detlefsen explains: "we're most certain to avoid gaps in reasoning when premises *explain* conclusions . . . 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" ([13], 19).

And there are other differences between the two sorts of proof as well. For example, although the mathematical logician focuses on the logical consequences of given axioms or other starting points, actual mathematical practice is more correctly described as problem solving: one starts not with axioms but instead with a conjecture and working backwards one seeks the starting points that would enable one to prove that conjecture.[7] Finished proofs are, furthermore, of interest to mathematicians not primarily because they establish the truth of their conclusions, which is and must be the primary focus of the mathematical logician, but because they are explanatory, or because they introduce proof techniques that can be brought to bear on other problems.[8] Similarly, what is for the mathematical logician merely a means of introducing an abbreviation can, for the mathematician, constitute a very significant mathematical advance. Although in logic definitions merely abbreviate, in mathematics good definitions, definitions that are fruitful, interesting, and natural, can be exceptionally important, both in themselves, for the understanding they enable, and for what they equip one to prove. For example, it is, as Tappenden [15], 264, argues, "a mathematical question whether the Legendre symbol carves mathematical reality at the joints". Given that the answer to this mathematical question has proved to be an unequivocal "yes", the Legendre symbol cannot be merely an abbreviation. It signifies something mathematically substantive, something of real and enduring mathematical interest.

It is unquestionable that mathematical practice is very different from what the logician and computer scientist would lead one to expect. But to know this is not yet to know *why* it is. Interestingly, the problem is *not* merely that the logician formalizes. "A formal proof," we will say following Harrison (2008, 1395), "is a proof written in a precise artificial language that admits only a fixed repertoire of stylized steps." The logician's formalized proofs clearly fit this characterization. But so do myriad proofs that *anyone* would deem properly mathematical. Consider, for example, this little proof of the theorem that the product of two sums of integer squares is itself a sum of integer squares. We begin by formulating the idea of a product of two sums of integer squares in the familiar symbolic language of arithmetic and algebra:

$$(a^2 + b^2)(c^2 + d^2).$$

Now we rewrite as licensed by the familiar axioms of elementary algebra, omitting obvious steps that could easily be included:

$$a^2c^2 + a^2d^2 + b^2c^2 + b^2d^2$$
$$a^2c^2 + b^2d^2 + a^2d^2 + b^2c^2$$
$$a^2c^2 + 2acbd + b^2d^2 + a^2d^2 - 2adbc + b^2c^2$$
$$(ac + bd)^2 + (ad - bc)^2.$$

This last expression is a sum of two integer squares, which is what we were to show, and so we are done. Our proof is, or could be made to be, fully formal in Harrison's sense: it is "written in a precise artificial language that admits only a fixed repertoire of stylized steps". And yet it is clearly mathematical. It follows directly that being formal is compatible with being of mathematical significance.

The symbolic language of arithmetic and algebra together with the familiar rules governing the use of its symbols is a paradigm of a formal language in Harrison's sense; it is "a precise artificial language that admits only a fixed repertoire of stylized steps". And proofs in this language are, or can easily be made to be, completely gap-free, fully rigorous. But even so the symbolic language of elementary algebra with its rules of use is not destructive of mathematical understanding but instead an enormous *boon* to mathematical understanding. As Grabiner once remarked [16], 357, *that* language has been "the greatest instrument of discovery in the history of mathematics"—of *discovery*. Why is it, then, that in the case of the symbolic language of elementary algebra, the formalization is *transformative* of mathematical practice, whereas in our case, the case of mathematical logic and machine reasoning, the formalization is utterly irrelevant to mathematical practice? What is the difference that is

[6] Again, this is a point that is often made in the literature. See, for example, Devlin [12], Rav [4], and Detlefsen [13].
[7] Cellucci has long emphasized this point. See, for instance, [14]. See also Rav [4], 6: "the essence of mathematics resides in inventing methods, tools, strategies and concepts for *solving problems*".
[8] That is why mathematicians so often reprove theorems. If all they cared about were the truth of theorems this would be inexplicable.

making the difference in the two cases if it is not the mere fact of formalization?

The problem of mathematical logic is not merely that one formalizes in it. Perhaps, then, the problem is that, as Poincaré argues, the logician *replaces* distinctively mathematical reasoning with purely logical, that is, mechanical, reasoning. After all, in our example of products and sums of integer squares we were still working with mathematical ideas, with sums, products, and so on. So, perhaps the real problem with the logician's formalization is not that it is a formalization, but that it is a strictly *logical* one. Perhaps, again as Poincaré argues, to reduce a step of reasoning that mathematicians can see to be valid to a series of little logical steps that anyone, or even a machine, can see to be valid is to destroy the mathematics; perhaps it is to *replace* mathematical knowledge—which constitutively involves one's grasping mathematical ideas and having the ability to see what follows on the basis of those ideas—with merely logical knowledge. Certainly it is true that having the ability to manipulate symbols according to rules, which is what machines can do and what is needed to do mathematical logic, is *not* to be able to do mathematics. So maybe Poincaré is right: to formalize a proof, replace all its distinctively mathematical steps with strictly logical ones is to destroy it, at least as a piece of mathematics.[9]

Poincaré's thought is that mathematical reasoning and understanding are grounded in grasp of mathematical ideas. Because they are, to reduce those ideas, and reasoning and understanding to logic, which is not about anything in particular, is irretrievably to lose the mathematics. This is not clearly right. Consider, first, the case in which what the mathematician takes to be a distinctively mathematical mode of reasoning is shown by the logician to consist in fact in a series of little steps all of which are purely logical. To show that seems clearly to show that what the mathematician had taken to be a distinctively mathematical step of reasoning is at bottom purely logical, strictly deductive. This would seem, furthermore, to be an interesting *mathematical* result: what the mathematician had taken to be a non-logical and presumably ampliative step of reasoning has been revealed to be strictly logical and hence merely explicative. In sum, to discover that some step of reasoning that we had assumed was distinctively mathematical is after all strictly logical is to discover something important *about mathematics*. But if that is right then, in at least some cases, the reduction is not destructive of mathematics but instead a contribution to it.

On the other hand, it does seem right to say, with Poincaré, that there is a crucial difference between the person who can only follow all the little logical steps and the person who can *also* discern the mathematical ideas at work in a proof. As Detlefsen explains: "even perfect *logical* mastery of a body of axioms would not, in his [Poincaré's] view, represent genuine mathematical mastery of the mathematics thus axiomatized. Indeed it would not in itself be indicative of any appreciable degree of mathematical knowledge at all: knowledge of a body of mathematical propositions, plus mastery over their logical manipulation, does not amount to mathematical knowledge either of those propositions or of the propositions logically derived from them" ([18], 210). According to Poincaré, replacing all mathematical modes of inference with a series of purely logical little steps destroys the mathematical unity of the proof that is essential to any adequate understanding of it. But why, and how, does it do that? Again, if what we had thought was a distinctively mathematical mode of reasoning turns out to be reducible to a series of strictly logical steps then that is an important, and importantly mathematical, discovery. So the cases of concern must be ones in which, paradigmatically, steps that are mathematically motivated are made explicit in conditionals, so that the conclusion can now follow as a matter of pure logic.[10] And now someone not in the know might well understand the step merely as a matter of logic: if A then B (which here formulates a mathematical rule), but A, therefore B. But is there any reason to think that the *mathematician* could not still see that what is crucial mathematically is that if A then B, that it is this mathematical rule that is licensing the move from A to B? If there remains a discernable difference between cases in which some *mathematical* rule is being followed and cases that merely involve some truth-function, either not-A or B, then there will remain a difference between what the mathematician can discern in the proof and what the non-mathematician will discern.

Suppose, for example, that we took our little proof that the product of two sums of integer squares and made it strictly logical, that is, every step in conformity with a rule of logic. Where before we had drawn a mathematical inference, we now write down the relevant conditional and justify the step by modus ponens. Once we have done this for all the steps of the proof, it might well be much harder to discern the important steps of the proof, as well as its key ideas—to order the summands in a certain way and then add and subtract the same thing so as to be able to factor—but those steps and ideas would still be there to be discerned. The formalized proof would not in that case destroy the mathematics—though it also would no longer highlight it. But if that is right then Poincaré's claim that replacing distinctively mathematical forms of reasoning with strictly logical ones destroys the mathematics cannot be quite right. The complete and utter lack of interest mathematicians show for formalized proofs strongly

[9] This, the mathematical logician is likely to respond, is merely a matter of psychology, and irrelevant to our philosophical understanding of what is going on in a piece of mathematical reasoning. See Goldfarb [17].

[10] Detlefsen [19] considers this sort of case.

suggests that, just as Poincaré charges, the mathematics *is* being lost in the formalization. But given that this loss is not a necessary result of formalizing in the language of logic, we have yet to understand what is really going on here, *why* the mathematical logician's formalized, mechanical proofs are so completely irrelevant to mathematical practice.

Mathematicians do not need to study logic and they do not use the signs of logic except here and there as abbreviations for everyday words: "the everyday use of logical symbols we see [in mathematical practice] today closely resembles an intermediate 'syncopation' stage in the development of existing mathematical notation, where the symbols were essentially used for their abbreviatory role alone" (Harrison [1], 1398). And so, it is sometimes claimed, the signs even of a mathematical language such as the symbolic language of elementary algebra similarly do nothing more than to provide abbreviations of words of natural language. But this is simply (and really rather obviously) not true: mathematical languages such as the symbolic language of algebra, as they are actually used, function in a fundamentally different way from the way natural languages function. In particular, one can reason *in* a mathematical language in a way that is simply impossible in natural language. Although one cannot, for instance divide the words 'six hundred and seventy-three' by the word 'seventeen', one *can* divide the Arabic numeral '673' by the numeral '17'. In the latter case one works out the answer on paper, through a chain of paper-and-pencil reasoning (or else one imagines oneself doing this). Even more obviously, although one cannot bisect the word 'line' one *can* bisect a Euclidean (drawn) line.

But not all mathematical reasoning is a matter of scribbling in a specially devised system of written marks. Is the reasoning in other cases instead done in natural language? It is not, at least not in the way that it *is* done in a specially devised written mathematical language. Where there is no system of written marks within which to work, the reasoning is instead performed *mentally*, by reflecting on ideas in ways that can then be *reported* in natural language.[11] The ancient proof that there is no largest prime is a familiar example of such a report of mental mathematics. Lacking any means of displaying what it is to be a prime number, or even what it is to be a product of numbers, ancient Greek mathematicians could nonetheless work mentally with the idea of a prime number, and with the idea of a product of a finite list of primes plus one, and could recognize that such a product of primes plus one must either be prime or have a prime divisor larger than any hitherto considered. And having determined this, they could report their reasoning in just the way Euclid in fact does in the *Elements*. Al-Khwarizmi, a ninth century Islamic algebraist, similarly can tell us in natural language how to find a particular root. What he cannot do is *show* us how to determine that root by performing a calculation.[12]

Sometimes we can work out the solution to a mathematical problem by paper-and-pencil reasoning. In other cases, we instead must reflect on the relevant ideas in order to solve the problem by a chain of mental reasoning. It can also happen that a piece of mathematical reasoning that at first can only be reported in natural language can later have a counterpart displayed in a mathematical language. A very simple example is this from Euclid's *Elements*, Proposition IX.21: if as many even numbers as you like are added together, the whole will be even. The crucial step in the reasoning, as reported by Euclid, is that since each of the numbers added together is even, each has, by the definition of *even*, a half part; thus it follows that the whole has a half part, and hence (by definition) is even. That is, we are simply to *see*, as it were with the mind's eye, that if each summand has a half part then the sum does as well. And this is, admittedly, very easy to see; but it is not by logic alone, or any antecedently specified step of mathematical reasoning, that we see this. It is an intuitively obvious step of reasoning but nevertheless one that is *not* justified by any rule. The inference is only reported, and either one gets it or one does not. But a comparable step *can* be shown in the symbolic language of algebra, and in that case, the conclusion *does* follow by an antecedently specified rule. First, we display in the language what it is to be even, that is, the *form* that even numbers take in the language, namely, $2n$, for natural number n. Now we display an arbitrarily long finite sum of such numbers: $2a + 2b + 2c + \ldots + 2n$.[13] Because there are explicitly formulated rules governing the use of such signs, we can apply a rule to transform the expression thus: $2(a + b + c$

[11] There are also a wide variety of intermediate cases, cases involving systems of written marks together with some mental mathematics. Leaving these out of consideration does not affect the points at issue here; what matters for our purposes is the two extremes, the case in which one has a system of written marks within which to reason and the case in which one instead engages in purely mental reasoning, the results of which can be reported in natural language.

[12] al-Khwarizmi writes: "*Roots and squares are equal to numbers*: for instance, 'one square, and ten roots of the same, amount to thirty-nine dirhems'; that is to say, what must be the square which, when increased by ten of its own roots, amounts to thirty-nine? The solution is this: you halve the number of roots, which in the present instance yields five. This you multiply by itself; the product is twenty-five. Add this to thirty-nine; the sum is sixty-four. Now take the root of this, which is eight, and subtract from it have the number of the roots, which is five; the remainder is three. This is the root of the square which you sought for; the square itself is nine" ([20], 229). The correctness of the implicit rule would have been demonstrated geometrically.

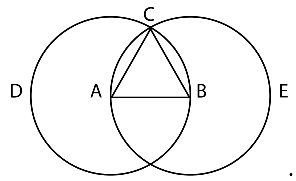
[13] It is worth noting in this context that our symbolic expression is arbitrary along two different dimensions. First, each of the letters '*a*', '*b*', '*c*', and so on stand in for some natural number not further specified. The letter '*n*' is different insofar as it is *also* arbitrarily large. My thanks to Jean Paul Van Bendegem for making this explicit.

+ . . . + *n*). This is manifestly an even number; we have our proof.

As this little example of the sum of even numbers shows, and Whitehead [21], 34, explicitly says, "by the aid of symbolism, we can make transitions in reasoning almost mechanically by the eye, which otherwise would call into play the higher faculties of the brain". Once we have symbolized our problem we do not have to *think* about what follows from the fact that each number in the sum has a half part. We simply have to apply a rule that enables us to *show* that the sum is even. Of course, we do need to be able to see the mathematical ideas in the symbolism, for example, that the expression '$2(a + b + c + . . . + n)$' designates an even number; but it is the symbolism, not the ideas, that enables us to operate as we do. "In mathematics, granted that we are giving any serious attention to mathematical ideas, the symbolism is invariably an immense simplification. It is not only of practical use, but is of great interest. For it represents an analysis of the ideas of the subject and an almost pictorial representation of their relations to each other" (Whitehead [21], 33). Again, when one is working in a written mathematical language such as the symbolic language of arithmetic and algebra one does not have to *think* about the relevant mathematical ideas in the way one *does* have to think about them in the absence of such a language. And *that* is just our problem: we have in mathematical logic as in, say, the symbolic language of elementary algebra, a "precise artificial language that admits only a fixed repertoire of stylized steps," a formal language "designed so that there is a purely mechanical process by which the correctness of a proof in the language can be verified" (Harrison [1], 1395). But unlike the symbolic language of elementary algebra, the language of mathematical logic is of no mathematical interest or utility. *Why*?

Although it might have been expected to, the language of mathematical logic and mechanical reasoning has not proved to be a mathematically tractable language, a language within which to reason in mathematics. Mathematicians working today do not display their reasoning in the formal language of mathematical logic but only report it.[14] We need, then, to think about what is required of a language within which to display mathematical reasoning. The short answer, explicit already in Leibniz, is that the language must exhibit mathematical content in a mathematically tractable way, that is, in a form that enables reasoning in the guise of a series of rule-governed manipulations of signs. It must be, as Frege also saw, at once a *lingua characteristica* and a *calculus ratiocinator*. There is, however, a hitch: it is possible to read one and the same notation *either* as formulating content in a mathematically tractable way *or* as merely recording information in a way enabling mechanical reasoning. And because one and the same notation can be read in either of these two very different ways, it is impossible to show what is needed in a mathematical language by appeal only to a system of signs. One must also take into account *how expressions in the system are understood*. Some examples will help to clarify this essential point.

Consider, first, the familiar distinction between a mathematical and a mechanical proof, which we here apply to the first proposition of Euclid's *Elements*: to draw an equilateral triangle on a given straight line. The diagram for both the mechanical and the mathematical proof is this:



But it is drawn with very different intentions in the two cases. Because, in a mechanical proof, the aim is to construct an actual, *empirical* triangle, one with, as far as possible, sides that are actually equal in length, one is well advised, in that case, to use a compass to draw the required circles and a straight-edge to draw the lines that are radii of the circles and form the sides of the triangle. One could then measure the lines to determine how closely they approximate lines equal in length. In a *mathematical* demonstration no such precautions are necessary because the drawn circles are not intended in this case to be *instances* approximating as far as possible the ideal of a mathematical circle. Instead they are drawn to formulate or display the *content* of the concept of a circle, *what it is* to be a circle, namely, a plane figure all points on the circumference of which are equidistant from a center.[15] As formulating such content, the drawn circles license inferences: if one has two radii of one circle then one can infer that they are equal in length—whether or not they *look* equal in length in one's drawing. What in the mechanical proof is treated as a means of constructing some *particular* triangle (with its particular spatial location, and particular size and orientation) is in the mathematical proof a way of solving a strictly mathematical and hence constitutively general problem, the problem of the construction of *an* equilateral triangle—not any equilateral triangle in particular—on a given straight line. As Shabel [25], 101, puts the point in a

[14] Avigad [22] makes this point. It is also the basis for Azzouni's [23] derivation-indicator account of mathematical proofs. Rav [4], 13, makes the point in an especially graphic way: "The argument-style of a paper in mathematics usually takes the following form: ' . . . from so and so it follows that . . . . , hence . . . .: as is well known, one sees that . . . ; consequently, on the basis of Fehlermeister's Principal Theorem, taking into consideration α, β, γ, . . . , ω, one concludes . . . , as claimed'."

[15] See my [24], Chapter 2.

discussion of Kant on pure and empirical intuition in mathematical practice, "the mechanical demonstration is not distinguished from the mathematical demonstration by virtue of a distinction between an actually constructed figure and an imagined figure, but rather by the way in which we operate on and draw inferences from that actually constructed figure". One and the same drawing is regarded in two systematically different ways in a mechanical and a mathematical proof.

A second example is this. Suppose that, having not yet learned various simple sums (but knowing how to count), one wished to determine how many seven things and five things make when taken together. One might proceed by marking out seven strokes and then five more and counting how many that is. This is a mechanical reading of the display of seven and five strokes. One thinks of it as presenting two collections of things, namely, strokes that taken together make a collection of twelve things—as one discovers by counting the whole collection. The proof is mechanical insofar as one is actually putting things together in order to see empirically, by counting, what totality they make. That one is working with a system of written marks is irrelevant; one could have worked as easily with pebbles, or peaches, or puppies. (Well, maybe not *as* easily.)

Now we regard the strokes differently, not merely mechanically but as signs of a Leibnizian language within which to formulate content and to reason. In this case we do not regard each stroke as standing in for a thing to be counted, or indeed as itself a thing to be counted. Instead we regard each stroke as expressing something like a Fregean sense, as contributing to the sense of a whole collection of signs that together, as *one* complex sign, designates a number, say, the number seven, or the number five. So regarded, the collection of seven strokes exhibits *what it is* to be the number seven, namely, a certain multiplicity. The collection of seven strokes is not in this case a collection of seven things; it is a *single* complex sign for *one* number, a sign that, by contrast with a simple sign such as the Arabic numeral '7', displays what it is to be seven in a mathematically tractable way. Given the display of five and seven using the Leibnizian stroke language, one can progressively reconfigure the whole display, adding strokes from the sign for the number five to the strokes making up the sign for seven in such a way that one eventually achieves a complex sign for the number twelve. Much as in Euclid's system one shows (mathematically) that an equilateral triangle can be constructed on a given straight line, so here one shows that (a sign for) the number twelve can be constructed from (signs for) the numbers seven and five. And the result in both cases is synthetic a priori insofar as what one has to begin with provides everything one needs in order to perform the required construction through a course of mathematical reasoning. In the mathematical demonstrations, the triangle, and the number twelve, are *not* contained already implicitly in one's starting points, but the *potential* for achieving them is there in the starting points. They *can be* produced, which means that the result is synthetic rather than analytic. But they are not produced mechanically, that is, empirically, as in a mechanical proof. They are produced mathematically. The result is a priori.

Notice further that in both the Euclidean diagram and the Leibnizian stroke language, the signs are taken to function in a very distinctive way. In the case of the Euclidean diagram, what are at first seen as two radii of a circle (required in order to determine that they are equal in length) are later seen as sides of a triangle. One and the same sign, namely, a drawn line, is in the context of one collection of signs a radius of a circle and in the context of another collection of signs a side of a triangle. We can take it either way. What we cannot do, of course, is take that same line as, say, an angle or the circumference of a circle. The drawn line expresses a sense that completely and perfectly delimits its possibilities for designating in this or that use in a diagram. Similarly, and even more simply, for the strokes: a stroke that I first see as a part of the sign for five, as contributing a sense to the complex sign designating five, I later see as part of the sign for, say, the number eight constructed out of the original seven strokes plus one more. There is nothing like this in the mechanical proofs. In mechanical proofs, the marks are simply material things that are constrained by the physics of the case. The expressive intentions of a thinker are irrelevant when one is proving mechanically.

We have seen that in a mechanical proof one pictures or records something, for instance, a particular circle or how many in a collection. In a mathematical proof one instead *formulates content*, what it is to be, say, a circle or the number seven; and one does so in a way that enables reasoning in the system of signs. We can similarly read a complex sign of Arabic numeration in either way, *either* as recording how many (how many units, tens, hundreds, and so on), that is, mechanically, *or* as formulating the arithmetical or computational content of numbers. If one sees the numeral the former way then one will take it that a calculation in Arabic numeration is merely a mechanical expedient for arriving at a desired result, not in any essential way different from the sort of mechanical manipulations that can be made on Roman numerals.[16] If one instead sees the Arabic numeral as expressing arithmetical content, one will think of the calculation as a bit of *mathematical reasoning*, as an episode of mathematical thought rather than as something mechanical, and hence as something quite different from the manipulations that can be made on Roman numerals.[17]

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[16] See Schlimm and Neth [26] for such a view.

[17] I am of course assuming that the signs of Roman numeration are being read mechanically, and this is certainly the most natural way to read

In the examples we have so far considered one has a system of written marks that can be conceived in either of two fundamentally different ways, either mechanically, as providing an instance or record of something that can then be operated on in some way to yield the desired result, or mathematically. And in the mathematical case, we have seen, one formulates the content of some mathematical notion—the content of the concept of a circle, say, or that of some particular number—and one does so in a way that enables reasoning *in* the system of signs. Now we need to consider how things stand with systems of signs of logic.

Consider, first, Peirce's system of alpha graphs. Shin [27] has shown that although we can take the primitives of the system directly to picture or record, we can also take them only to express senses independent of their involvement in a proposition, to contribute a sense to the whole thought expressed, which thought can then be variously analyzed.[18] In Peirce's system considered the first way, that is, mechanically, to enclose a propositional sign in parentheses just is to negate it; the concatenation of signs serves similarly as conjunction.[19] The complex sign '((A)(B))', then, is to be read as recording the fact that it is not the case that not-A and not-B. But we can also read this same complex sign as an expression of a *Leibnizian* language, as exhibiting a thought that can be variously regarded, for instance, as the disjunction of A and B, or as the conditional 'if not-A then B', or as the conditional 'if not-B then A'. Much as a line in a Euclidean diagram is a radius or side of a triangle *only* relative to a way of regarding that diagram, so here on the Leibnizian reading, the collections of signs is a disjunction or conditional only relative to a way of regarding it. And of course just this same point can be applied to the standard notation of mathematical logic and as well to Frege's *Begriffsschrift* notation. Expressions in all these various systems of notation can be read *either* as picturing some state of affairs, say, that if not-A then B, *or* as displaying logical content in a way that can be regarded in turn *either* as, say, a conditional with a negated antecedent *or* as a disjunction, depending on whether one takes the tilde (negation stroke) to attach to the content A or to function together with the horseshoe (conditional stroke) to designate disjunction.

Read mechanically a notation such as that of mathematical logic or Frege's *Begriffsschrift* records information, and the rules governing the manipulation of the signs enable one to show that other information is also contained therein. Manipulating the signs according to the rules can thus make explicit what is contained already implicitly. The deduction is merely explicative. Much as making seven strokes and then making five more is already to have twelve strokes, so that counting up the resultant number of strokes is a mechanical means of determining how many, so manipulating the signs of some premises expressed in the language of mathematical logic, as it is generally conceived, is a mechanical means of showing that certain information is contained already in one's starting points. But, we now know, we can also read the notation differently, as a notation of what I have been calling a Leibnizian language. Furthermore, we know that in general, because the signs of a Leibnizian language only express senses independent of a context of use, those signs can be used to formulate the contents of concepts. Can the signs of a logical language, read as a Leibnizian language, similarly be used to formulate the contents of concepts and to do so in a way that enables reasoning in the system of signs? They can.

In Euclidean diagrammatic reasoning, the content of the concept of, say, a circle is conceived diagrammatically, that is, as something that can be exhibited in a drawn circle. In Descartes's analytic geometry, the content of that same concept is conceived instead arithmetically. It is given in the equation '$x^2 + y^2 = r^2$'. We have further seen that although the content of the concept of an even number, or of an odd number, cannot be displayed in a Euclidean diagram, those contents *can* be displayed in a mathematically tractable way in the language of arithmetic and algebra, the notion of an even number as '$2n$' and that of an odd number as '$2n + 1$'.

Different mathematical languages can thus involve very different conceptions of what are in fact the same mathematical concepts, very different analyses of those concepts. What sort of analysis is needed, then, for the sort of reasoning from concepts that is characteristic of contemporary mathematical practice? Given that the mathematical practice we are concerned with is that of *deductive* reasoning from concepts, the answer is clear: a logical analysis. We need to be able to display the contents of concepts as they matter to inference.

What we are after is a way to formulate the contents of mathematical concepts that enables deductive reasoning in the system of signs. And we know by now that to achieve this it is not enough to introduce various signs together with rules governing their use because any such system of signs can be read either as a Leibnizian language or merely mechanically. To exhibit the contents of concepts in a mathematically tractable way, we need to read the system of signs as a *Leibnizian* language, its primitive signs as only expressing senses independent of any context of use, because only so can a whole complex of signs serve to designate a *single* concept, only so can we display content at all.

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them. But our Leibnizian stroke language suggests that it may be possible, if difficult, to read signs of that language likewise as the signs of a Leibnizian language.

[18] Shin does not put the point this Fregean way, but could have done.

[19] In Peirce's system one encircles propositional signs rather than enclosing them in parentheses. The latter is, however, more convenient here and works in essentially the same way.

Think again of our simple stroke language or of the system of Arabic numeration. In both cases we can treat the primitive signs either as having their meaning or designation independent of any context of use or as having only a sense independent of a context of use. Taken in the former way, as having meaning (designation) independent of any context of use, the signs are signs of a mechanical language: a collection of five strokes is just that, a collection of five things, and an Arabic numeral such as '376' similarly denotes a collection, a collection of three hundreds and seven tens and six ones. A numeral such as '3' in the language so conceived invariably denotes some particular number, here the number three; its position serves only to indicate what is being so counted, whether ones or tens or hundreds or something larger. But we know that we can also read the language differently, the primitive signs of the language as only expressing senses independent of a context of use. In that case, the collection of five strokes is a complex sign that designates *one* thing (not five things), namely, the number five. And the Arabic numeral '376' similarly is a complex name of one number. The numeral '3' does not in this case designate three (of something) no matter what the context; instead it contributes a sense to a whole that only as a whole functions as a name for something, namely, in our example, for the number three hundred and seventy-six. In just the same way, we can regard a definition of a mathematical concept in a written system of logical and mathematical signs *either* as recording necessary and sufficient conditions, the state of affairs that obtains if the concept applies, *or* as exhibiting the content of the concept as it matters to inference.

In mathematical logic and computing, the definiens of a definition is understood to provide necessary and sufficient conditions for the application of the concept, and the definition as a whole is taken merely to introduce an abbreviation for those conditions. The definition has no philosophical or mathematical significance; it is a convenience. The defined concept is, in that case, reduced to, or replaced by, a set of conditions much as a number is reduced to, replaced by, a collection of things when it is represented mechanically by a series of strokes. But again, in actual mathematical practice, definitions—both those that stipulate a simple sign for some complex notion and those that provide a new and deeper analysis for some concept already in use—can constitute a significant mathematical advance, one that is just as important mathematically as a new proof. And the definition *is* mathematically important precisely because and insofar as it formulates mathematical content in a tractable way, in a way enabling new and better, more explanatory proofs. But in order to do that in a specially devised system of signs, the system of signs must be read as a Leibnizian language the primitive signs of which only express senses.

In a definition in a Leibnizian language the defined concept is not *reduced* to something else but instead designated. Indeed, it is designated twice, once by a simple sign, the definiendum, and again by a complex sign, the definiens. The two signs have the same designation or meaning. But although they designate one and the same concept, the two signs express two very different Fregean senses. And one can just see that they do insofar as the one sign is simple while the other is complex. Because the definiens is a complex sign that is made up of a variety of primitive signs of the language, the transformation rules of the language can be applied to it in a way that is manifestly impossible in the case of the simple sign that is the definiendum. The simple sign, the definiendum, is unequivocally a name for the relevant concept. The complex sign, the definiens, is also a name for that concept but because it is complex it can enable one to reason in light of the content it displays and discover thereby new truths about the concept in question. But, of course, one can see all this to be going on only if one understands the system of signs as we have done here, not merely mechanically but as a Leibnizian system the primitive signs of which only express senses independent of any context of use. In a fully formalized proof in a Leibnizian language the mathematics is not destroyed but instead displayed, and although superficially each step is the same as any other, one and all steps of logic, the knowledgeable reader can nonetheless distinguish those steps that are mathematically important from those that are trivial, and can discern as well the key mathematical ideas of the proof. The language functions, in other words, in much the way the symbolic language of arithmetic and algebra does, to extend our mathematical knowledge.

It has long been known that the reasoning mathematicians engage in is quite unlike reasoning as it is understood in mathematical logic and computer science. What has proved much harder to determine is why that is. The problem is not merely that the logician formalizes, either in the sense of producing proofs that are completely gap-free or in the sense of working in an artificial symbolic language the licensed moves of which are all specified in advance. Nor even is it, as Poincaré suggests, that logicians replace all distinctively mathematical steps of reasoning with strictly logical ones. We know that all these explanations fail because it is possible to find or develop examples of mathematical proofs in the formula language of arithmetic and algebra that exhibit some or all of the features that have been focused on and nevertheless *retain* their mathematical interest. The explanation for the irrelevance of mathematical logic to mathematics must, then, be something distinctive of that logic in particular. And so it is: the reason mathematical logic is irrelevant to mathematical practice is that its language is read mechanically. Because reasoning in mathematics is *not* merely mechanical, to formalize a mathematician's proof in mathematical logic really does destroy it as a piece of mathematical reasoning—just as Poincaré thought. Because the language is read mechanically, all differences between mathematically significant steps of reasoning and merely trivial steps of logic are completely effaced. No one, not even the mathematician, can now discern what is mathematically important in the proof.[20]

I began with a question: what has mathematical logic to do with mathematical understanding? In particular, why is it that a fully formalized, mechanical proof in mathematical logic destroys the mathematical interest of the proof given that in other cases of formalizations, paradigmatically in the symbolic language of arithmetic and elementary algebra, the result is of clear and significant mathematical interest? The problem, we have found, does not lie in the language of mathematical logic conceived simply as a system of signs. The problem lies in the way that system of signs is conceived, in the fact that it is conceived mechanistically. Were it to be conceived instead as a Leibnizian language—that is, as a language within which to display the contents of concepts in a way enabling one to reason on the basis of those contents in the system of signs—then it could be used in formalizations in much the way the language of arithmetic and algebra is. It could be used, that is, to clarify and enrich both mathematical practice and our understanding of that practice. And *that* is to say that it could be used in just the way Frege envisaged the use of his *Begriffsschrift*, his concept-script—if only we had understood him.[21]

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[20] Mathematical logic is so named because and insofar as it is (as Boole explicitly urged it should be) a branch of mathematics; it is a mathematical investigation into (mathematically investigable) patterns of reasoning. The logic that one would need for the purpose of actually reasoning in the system of signs in mathematics would be a mathematical logic in a very different sense.

[21] Frege explicitly notes that his aim was different from Boole's, and different in just the way I have tried to bring out here. He writes in "On the Aim of the 'Conceptual Notation'": "I did not wish to present an abstract logic in formulas [as Boole did], but to express a content through written symbols in a more precise and perspicuous way than is possible with words. In fact, I wished to produce, not merely a *calculus ratiocinator*, but a *lingua characteristica* in the Leibnizian sense" ([28], 90-91). Or again in "Boole's logical Calculus and the Concept-script": "In contrast [to what Boole aimed for] we may now set out the aim of my concept-script. Right from the start I had in mind the *expression of a content*. What I am striving after is a *lingua characterica* in the first instance for mathematics, not a *calculus* restricted to pure logic" ([29], 12). See my [30] for an extended defence of this way of reading Frege's distinctive two-dimensional notation.

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