Logique & Analyse **221** (2013), 25–44

WRITING REASON

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Both in the practice of mathematics and in the practice of logic one uses a variety of specially devised written signs. Why? What is the nature and role of writing in reasoning in mathematics, and in reasoning about reasoning in logic? My discussion of these questions aims to be at once a substantive contribution to a practice-based philosophy of logic and a catalyst for further developments.

I begin by rehearsing three very different views of the role of writing in mathematical practice. These views are not merely abstract possibilities; all have been recently defended in print. I will suggest that two of the three, though apparently incompatible, are each in their own way deeply insightful about the role of writing in mathematical practice. But if they are then the third cannot be. Because this third view is very common both among philosophers and among mathematicians, I turn in the second section to the historical antecedents of the view, in particular to developments in mathematics in Germany in the nineteenth century and to developments in logic in the United Kingdom in that same century. These developments, we will see, suggest a further possibility, and in the third section I turn to Frege in whose work, it will be suggested, this possibility is realized. The essay ends with some questions for further research.

I. *Writing and Reasoning in Mathematical Practice*

Kant famously held that writing broadly conceived as the inscribing of marks is a constitutive feature of mathematical practice. By contrast with the practice of philosophy, which makes a "discursive use of reason in accordance with concepts", the practice of mathematics makes "an intuitive use [of reason] through the construction of concepts".[1] And constructions, as Kant conceives of them, essentially involve some sort of writing, actual or imagined: "I construct a triangle by exhibiting an object corresponding to this concept, either through mere imagination, in pure intuition, or on paper, in empirical

[1] Immanuel Kant, *Critique of Pure Reason*, trans. and ed. Paul Guyer and Allen Wood (Cambridge: Cambridge University Press, 1998), A719/B747.

intuition, but in both cases completely *a priori*, without having to borrow the pattern for it from any experience" (A713/B741). Similarly, in (elementary) algebra, "mathematics ... chooses a certain notation for all construction of magnitudes in general (numbers), as well as addition, subtraction, extraction of roots, etc., and ... thereby achieves by a symbolic construction equally well what geometry does by an ostensive or geometrical construction (of the objects themselves)" (A717/B745). Reason in its discursive use through concepts does not in the same way rely on a system of written marks. Indeed, Kant seems to think, reason in its discursive use does not rely (at least in a certain sense) on any sort of language. As he explains in the pre-critical essay "Inquiry concerning the distinctness of the principles of natural theology and morality" (1763), "the signs employed in philosophical reflection are never anything other than words. And words can neither show in their composition the constituent concepts of which the whole idea, indicated by the word, consists; nor are they capable of indicating in their combinations the relations of the philosophical thoughts to each other. Hence, in reflection in this kind of cognition, one has to focus one's attention on the thing itself".[2] Unlike the written marks that mathematicians use, the words of natural language, whether spoken or written, are of no use in philosophical reasoning Kant thinks; the philosopher must consider the universal *in abstracto*.

Kant's idea that writing, inscribing marks, is somehow constitutive of mathematical practice has its defenders still today. Rotman, for example, argues in *Mathematics as Sign* that mathematics "is a form of graphism, an inscriptional practice based on a system of writing".[3] According to him,

> mathematics is essentially a symbolic practice resting on a vast and never-finished language ... One doesn't speak mathematics but writes it. Equally important, one doesn't write it as one writes or notates speech; rather one 'writes' in some other, more originating and constitutive sense.[4]
>
> Mathematical signs do not record or code or transcribe any language prior to themselves. They certainly do not arise as abbreviations or symbolic transcriptions of words in some natural language ... the symbol '$\surd$' is not a mathematical sign for 'is the square root of,'

[2] In *Theoretical Philosophy*, 1755–1770, trans. and ed. David Walford, with Ralf Meerbote (Cambridge: Cambridge University Press, 1992), p. 251. At this point, Kant clearly thinks that there can be no system of written signs for discursive reasoning from concepts. (See also the "New Elucidation", Scholium to Section I, Proposition 2, and §2 of the First Reflection in the "Inquiry".) Later, as we will see, Kant seems to change his mind.

[3] Brian Rotman, *Mathematics as Sign: Writing, Imagining, Counting* (Stanford: Stanford University Press, 2000), p. 44.

[4] *Mathematics as Sign*, p. ix.

> neither is '=' the sign for 'is equal to'; rather, these English locutions are renderings of mathematical notions prior to them.[5]

Others, we will see, maintain just the opposite view; they hold that mathematical symbols are merely convenient shorthand.

A second view of the role of writing in mathematical reasoning is that Rotman calls the documentist view — which he ascribes, for instance, to Husserl.[6] This is the view that language, whether written or spoken, is invariably after the fact in mathematics, serving only to document or report results obtained independently. (The documentist view of the role of language in mathematical reasoning would seem, then, to be essentially Kant's view of the role of language in reason's discursive use.) The gifted mathematician and Field's medalist William Thurston defends just this view in "On Proof and Progress in Mathematics".[7] It is, he suggests, one thing to do mathematics, quite another to communicate, or at least to try to communicate, one's mathematical ideas, whether verbally or in writing; the way mathematicians write is not the way mathematicians think.[8] Indeed, Thurston thinks, it is just this that explains the fact that mathematical ideas can be communicated much more effectively in face-to-face conversation than they can in writing:

> one-on-one people use wide channels of communication that go far beyond formal [written] mathematical language. They use gestures, they draw pictures, they make sound effects and use body language . . . With these channels of communication, they are in a much better position to convey what is going on, not just in their linguistic faculties, but in their other mental faculties as well.[9]

The practice of mathematics as Thurston understands it is clearly not essentially written.

A third view, common among philosophers and assumed also by some mathematicians, is that mathematics is not merely reported, after the fact, in some written (or spoken) language as Thurston suggests, but actually *done in*

[5] *Mathematics as Sign*, p. 44–45.

[6] *Mathematics as Sign*, pp. 47–48.

[7] This essay originally appeared in the *Bulletin of the American Mathematical Society* (1994): 161–177. It is reprinted in Reuben Hersh (ed.) *18 Unconventional Essays on the Nature of Mathematics* (Springer Science+Business Media, Inc., 2006). All page references are to this reprinting.

[8] One main aim of the essay is to establish that there are "big differences between how we think about mathematics and how we write it" (p. 44).

[9] "On Proof and Progress", p. 43.

natural language, and hence is not essentially written at all. According to Benacerraf, for example, mathematics is done in a “sublanguage” of English.[10] For Wittgenstein similarly, the languages of mathematics are a “suburb” of the town that is language as a whole, one distinguished only by its “straight regular streets and uniform houses”.[11] Suppes takes this view in his classic *Introduction to Logic*.[12] And it is echoed in college-level mathematics textbooks. Mathematics, we are told in one such text, “is done using a specialized dialect of English”, that is, in natural language. “The symbols are simply a convenience: It is easier to write ‘x^2’ than ‘the square of x’, and ‘$x \in A$’ is more compact that ‘x is an element of the set A’. In each case the meaning is the same”.[13] On this view, which manifestly contradicts not only Rotman’s view but (along a different dimension) Thurston’s as well, all the signs that are used in mathematics are merely shorthand. Though it would be more tedious, one could do all of mathematics in natural language.

We are left with two questions. Is writing *constitutive* of mathematics or *irrelevant* to mathematics? Is mathematics *done* in natural language or is mathematics not done but only *reported* in natural language?

If one thinks of a problem in arithmetic, say, that of dividing thirty-three trillion sixty-seven million eight hundred and forty-seven thousand nine hundred and thirty-seven by sixty-seven thousand two hundred and eight, the writing seems essential insofar as, although practically anyone can solve this problem (tedious though it would be to do), most (all?) of us can solve it *only* in the positional system of Arabic numeration. One simply cannot calculate in English, or any other natural language, as one can in Arabic numeration; and, again, for most (all?) of us, there is just no other way to solve an arithmetical problem involving such large numbers. The Arabic numeration system is an extraordinarily powerful means of reasoning in arithmetic, and a paradigm of a system of written signs within which to work in mathematics.

One does not *say* a calculation in Arabic numeration but can only show it in writing, or describe what it would look like if one performed it. But one *can say*, for example, the ancient proof that there is no largest prime number. Here it is, in written English, though of course it could as easily be spoken.

[10] Paul Benacerraf, “Mathematical Truth”, *Journal of Philosophy* 70 (1973): 661–80; reprinted in *Philosophy of Mathematics: Selected Readings*, second edition, ed. Paul Benacerraf and Hilary Putnam (Cambridge: Cambridge University Press, 1983), p. 410.

[11] Ludwig Wittgenstein, *Philosophical Investigations*, trans. G.E.M. Anscombe (Oxford: Basil Blackwell, 1976), §18.

[12] Patrick Suppes, *Introduction to Logic* (New York: Van Nostrand Reinhold, 1957; Dover edition, 1999). See especially the introduction.

[13] Carol Schumacher, *Chapter Zero: Fundamental Notions of Abstract Mathematics*, second edition (Addison Wesley, 2001). The first quotation is from p. 1, the second from p. 5.

> Suppose, for *reductio*, that some finite, ordered list of primes comprises all the primes that there are, and consider the number that is the product of all these primes plus one. Either this new number is prime or it is not. If it is prime then we have a prime number that is larger than all those originally listed; and if it is not prime then, because none of the numbers on our list divide this new number without remainder (because it is the product of those primes plus one), this new number must have a prime divisor larger than any of the primes on our list. Either way there is a prime number larger than any with which we began. Q.E.D.

Though written, this proof clearly in no way relies on our system of written English, or any other form of written natural language. The proof could have been discovered, perhaps was discovered, prior to the development of any means of inscribing utterances of natural language. Nor, obviously, does it depend on any other system of written signs. What it depends on is the capacity to think, to reason or infer.

In the case of the proof that there is no largest prime, the words, whether spoken or written, clearly communicate the argument; they do convey the reasoning. It does not follow that one is reasoning *in* natural language in this case. Certainly it is not the words, written or spoken, that one attends to, not in the way one attends to a drawn diagram in working through a demonstration in Euclid, or to an equation in Descartes' or Euler's mathematics, or to Arabic numerals in the course of an arithmetical calculation. What one attends to in the case of the proof that there is no largest prime, much as in the case of reasoning discursively in philosophy according to Kant, is not the *words* but the relevant ideas, central among them the idea of a number that is the product of a collection of primes plus one. The task of the proof is to think through what follows in the case of such a number. Here there is simply nothing to look at comparable to a Euclidean diagram, an algebraic formula, or an Arabic numeral. The proof can be recorded and communicated in natural language, conveyed by means of it, but the proof is not *in* the words; the words that are used to convey it do not put the proof itself before one's eyes (or ears).

The ancient proof that there is no largest prime shows that mathematics is not essentially written. This is in any case something we might have expected. The systems of written signs that have been devised for mathematics were devised for mathematics that already existed; it would be impossible to design a notation for mathematics without knowing at least some of the mathematics that the notation was designed to capture. But it also seems clear that some at least of the systems of signs that have been devised in mathematics are enormously powerful vehicles of mathematical reasoning.

Although it is clearly possible to discover significant mathematical results, that is, solutions to mathematical problems and proofs of theorems, independent of any system of written signs within which to work, it is also manifest that such systems of signs are developed in mathematics and can enable results that could not (or could not so easily) be discovered without them. Writing seems, then, neither essential nor irrelevant to mathematical practice, at least if it is taken overall.

Throughout history there have been so-called "natural" calculators able to identify even very large primes or the solutions to quite difficult arithmetical problems without appeal to any sort of written language. Most of us, however, need the Arabic numeration system to perform such tasks. Although various algebraic results were discovered by gifted mathematicians before (in some cases, long before) the development of an adequate symbolism of elementary algebra, most of us can understand those results only when they are expressed in the symbolic language of arithmetic and algebra, only when we can *see* the reasoning in the formula language. In these cases, mathematics that at first can only be reported, conveyed in natural language, comes later to be displayed in a specially devised system of written signs. It is furthermore clear that what the systems of signs capture in these cases is not the way the mathematicians themselves were thinking. "Natural" calculators do not calculate as one calculates in Arabic numeration (albeit somehow "naturally"); that system was not devised to mimic the reasoning of such people.[14] Instead the system of Arabic numeration seems to provide a kind of rational reconstruction of the mathematics. It shows not how natural calculators reason, how they discover their results, but how the mathematical reasoning *itself* works, at least in this particular corner of mathematics. It lays out *the* reasoning as contrasted with some person's (processes of) reasoning.[15]

Mathematical languages, paradigmatically, the symbolic language of arithmetic and algebra, enable non-mathematicians, and in some cases even mathematicians themselves, to understand mathematics that would otherwise be wholly inaccessible. The point is not that the mathematics is *harder* without

[14] We do not currently know how they do it. But even if we did, this would be of no interest to mathematics, or to philosophy. The question how natural calculators do it is a question for psychology and cognitive science, not for philosophy. For a discussion of the phenomenon of "natural" calculation and some of the literature, see the section "Savant Skills and Other Phenomena" in Chapter One of *The Mind of the Mathematician*, Michael Fitzgerald and Ioan James (Baltimore: The Johns Hokpins University Press, 2007).

[15] Depending on the system, the given collection of marks may have to be supplemented with a commentary. We find this, for instance, in a demonstration in Euclid's *Elements*. The diagram is not in general fully intelligible (as a calculation in Arabic numeration is) without the accompanying text, even though, as I have argued elsewhere, one reasons *in* the diagram in Euclid. See my "Diagrammatic Reasoning in Euclid's *Elements*", in *Philosophical Perspectives on Mathematical Practice, Texts in Philosophy*, vol. 12, ed. Bart Van Kerkhove, Jonas De Vuyst, and Jean Paul Van Bendegem (London: College Publications, 2010).

a system of signs within which to work but nonetheless perfectly possible. Such systems of signs in mathematics do not merely make things easier; they make things easier *because* (as I have argued elsewhere) they *embody* processes of mathematical reasoning — again, not processes of mathematicians' thinking but of mathematical thinking itself. They display *how the mathematics works*.[16] And they do so, I have suggested, by providing a rational reconstruction of that mathematical reasoning. But if that is right, then we can reconcile Rotman's and Thurston's views. Rotman is right: one can *do* mathematics, at least as a public, observable act, only in a specially devised system of written signs. But so is Thurston: in areas of mathematics for which an adequate expressive notation has not been devised, language, that is, natural language, whether written or spoken, supplemented with whatever signs from mathematics seem helpful, is invariably after the fact, serving only to document results obtained independently. In such cases, though gifted mathematicians will still be doing mathematics, the act will not be public and observable. They will not be able to show us how it goes but can only tell us, describe in words, what they have discovered and why it seems to be true.

II. *Whence the Idea that Mathematics is Done in Natural Language?*

I have suggested that although mathematics is not constitutively written, as Kant thought and Rotman has more recently argued, nonetheless writing can play a crucially important role in mathematics insofar as a good notation of mathematics (such as the formula language of arithmetic and algebra) enables the display of an actual course of mathematical reasoning from starting point to end. It follows directly that mathematics is not done in natural language, because if it were then one *could* display the course of reasoning already in natural language. Why is it, then, that so many philosophers and mathematicians currently think that one does mathematics in natural language, that the systems of signs that have been developed in mathematics are only a convenient shorthand. The explanation, I will suggest, lies in two important developments, one in mathematics and one in logic, beginning in the nineteenth century.

In the nineteenth century, the mathematical practice of algebraic problem solving that had been inaugurated by Descartes in the seventeenth and came to dominate the practice of mathematics in the eighteenth, began to be supplanted in the work of Riemann and others by a new practice of analyzing

[16] I defend this claim, through an examination of some actual cases, in my "Seeing How It Goes: Paper-and-Pencil Reasoning in Mathematical Practice", *Philosophia Mathematica* 20(1) (2011): 58–85. Available online at `http://philmat.oxfordjournals.org/cgi/content/full/nkr006?ijkey=9ceiChH0LrmQoPu&keytype=ref`.

and explicitly defining concepts, and proving theorems on the basis of those definitions. For example, rather than thinking of a function as an analytical expression — as, for instance, Euler had — Riemann "would catalogue the singularities of a function . . . note certain properties, then prove that *there must exist* a function with those properties without producing an explicit expression".[17] For Riemann, "the objects of mathematics were no longer formulas but not yet sets. They were concepts".[18] Dedekind too championed this new practice focused on the development of and reasoning from concepts: "it is preferable . . . to seek to draw the demonstrations, no longer from calculations, but directly from the characteristic fundamental concepts, and to construct the theory in such a way that it will, on the contrary, be in a position to predict the results of the calculation".[19] Dedekind describes his theory of ideals similarly, as "based exclusively on concepts . . . that can be defined without any particular representation of number".[20]

Because, on Kant's view, the analysis of concepts and discursive reasoning from concepts belong to the practice of philosophy rather than to the practice of mathematics, this new form of mathematical practice seemed to blur the line between philosophy and mathematics, to make mathematics more like philosophy insofar as this new mathematical practice seemed to employ reason in its discursive rather than in its intuitive use. Riemann in his Habilitation lecture, for example, describes his work as "of a philosophical nature" because "the difficulties lie more in the concepts than in the construction".[21] And there were those who regarded this "philosophical" turn that the new mathematical practice was taking with deep suspicion. Kronecker, for instance, writes to Cantor in 1884: "I acknowledge true scientific value — in

[17] Jamie Tappenden, "The Riemannian Background to Frege's Philosophy", in *The Architecture of Modern Mathematics: Essays in History and Philosophy*, ed. José Ferreirós and Jeremy J. Gray (Oxford: Oxford University Press, 2006), p. 121.

[18] Detlef Laugwitz, *Bernhard Riemann 1826–1866: Turning Points in the Conception of Mathematics*, trans. Abe Shenitzer (Basel, Berlin, and Boston: Birhäuser, 1999), p. 337.

[19] Quoted in Howard Stein, "*Logos*, Logic and *Logistiké*: Some Philosophical Remarks on Nineteenth Century Transformation of Mathematics", in *History and Philosophy of Modern Mathematics*, ed. William Aspray and Philip Kitcher, *Minnesota Studies in the Philosophy of Science*, vol. XI (Minneapolis: Minnesota University Press, 1988), p. 245. See also Dedekind's letter to Lipschitz quoted in Jeremy Avigad's "Methodology and Metaphysics in the Development of Dedekind's Theory of Ideals", in *The Architecture of Modern Mathematics*, pp. 166–67.

[20] Quoted in Avigad, "Methodology and Metaphysics", p. 170; see also p. 183. In "*Logos*, Logic and *Logistiké*", Stein suggests that Dedekind's rejection of the need for a representation of number has "Dirichletian resonance, since Dirichlet in his work on trigonometric series played a significant role in legitimating the notion of an 'absolutely arbitrary function', unrestricted by any reference to a formula or 'rule"' (p. 249).

[21] Quoted in Gregory Nowak, "Riemann's *Habilitationsvortrag* and the Synthetic *A Priori* Status of Geometry", in *The History of Modern Mathematics*, vol. I: Ideas and their reception, ed. David E. Rowe and John McCleary (Boston: Harcourt Brace Jovanovich, 1989), p. 26.

the field of mathematics — only in concrete mathematical truth, or to state it more pointedly, 'only in mathematical formulas'. The history of mathematics teaches us that these alone are imperishable".[22] Weierstrass similarly suggests in a lecture delivered in 1886 that "even though it may be interesting and useful to find properties of the function without paying attention to its representation ... the ultimate aim is always the representation of a function".[23] The problem, of course, was that there was no system of written signs within which to do this work. The new mathematics could only be reported, after the fact, in natural language in essentially the way we reported the proof that there is no largest prime above.

The second big development was in logic, beginning with Boole. It was Boole who first had the idea — announced in the title of his first (1847) logic book, *The Mathematical Analysis of Logic* — of studying patterns of valid reasoning not as a philosopher but as a mathematician. That is, he would study not the actual practice or activity of inferring (as, for instance, Ryle does in "'If,' 'So,' and 'Because"'[24]), but instead the patterns among sentences that valid acts of inferring display. He would investigate not reasoning but instead the relation of logical consequence that holds, for instance, between, on the one hand, the major and minor premises of an Aristotelian syllogism, and on the other, its conclusion. Because Boole, along with such Cambridge logicians as Peacock, Gregory, and de Morgan, thought of algebra as a science of uninterpreted symbols together with rules for their manipulation, his goal in his new mathematical logic was similarly to set out the rules governing uninterpreted signs in logic.[25] It was to devise an uninterpreted algebra of logic. This mathematical, and formalist, treatment of the relation of logical consequence would later be expanded to include quantifiers and the logic of relations, and would eventually became what we think of today as logic.[26] Frege's work was of course instrumental to those further developments, but not, I will suggest, in quite the way we tend to think.

Developments in mathematics in nineteenth-century Germany suggested that mathematics, at least as it was coming to be practiced, involved not what Kant thought of as an intuitive use of reason but instead reason in what Kant called its discursive use. And as we have already seen, courses of

[22] Quoted in Laugwitz, p. 327.

[23] Quoted in Laugwitz, p. 329.

[24] Gilbert Ryle, "'If,' 'So,' and 'Because"', in *Philosophical Analysis*, ed. Max Black (Ithaca: Cornell University Press, 1950).

[25] For some of this history, see Ernest Nagel "'Impossible Numbers': A Chapter in the History of Modern Logic", originally published in 1935 in *Studies in the History of Ideas*, vol. III, and reprinted in *Teleology Revisited and Other Essays in the Philosophy and History of Science* (New York: Columbia University Press, 1979).

[26] See, for some of this history, Warren Goldfarb, "Logic in the Twenties: The Nature of the Quantifier", *The Journal of Symbolic Logic* 44 (1979): 351–368.

reasoning in its discursive use can only be reported after the fact in some natural language or other, or so Kant thought. But perhaps the developments in mathematics and logic showed that this was wrong. Perhaps reasoning is everywhere the same; perhaps there are not, as Kant thought, two uses of reason, but only one. But if so then surely it is the case that, if one reasons in any language at all, then one reasons in natural language, or at least in a slightly regimented version of natural language. The development of an adequate logic of relations, that is, the full polyadic predicate calculus for natural language, seemed to many to *show* (*pace* Kant) that mathematicians have in fact been reasoning in natural language all along. Nevertheless, as already indicated, this view is profoundly mistaken.

III. *An Expressive Notation for Reasoning from Defined Concepts*

I have suggested that although mathematics can be done independent of any form of notation within which to work, it is also clearly possible to develop notations within which to do various sorts of mathematics. Indeed, for most of its long history in the West, mathematics *has* been done in specially developed systems of written marks, first in Euclidean diagrams and then, beginning in the seventeenth century, in the symbolic language of arithmetic and algebra, in formulae. It is only since the nineteenth century that the form of mathematical practice dominating the discipline, the practice of reasoning deductively from defined concepts, has lacked any system of written signs within which to do its work. One reason for this anomalous state of affairs may be traced to Kant insofar as his distinction between an intuitive and a discursive use of reason seemed to make the very idea of an "intuitive", or paper-and-pencil, use of reasoning directly from concepts completely unintelligible. But even Kant seems eventually to have come to think that one might develop a system of written signs for discursive reasoning from concepts. He writes in a letter to his trusted expositor Johann Schultz (26 August 1783): " …properties of the table of categories …seem to me to contain the material for a possibly significant invention …the construction of an *ars characteristica combinatoria*". "Perhaps", he suggests a little further along, "your penetrating mind, supported by mathematics, will find a clearer prospect here where I have only been able to make out something hovering vaguely before me, obscured by a fog, as it were."[27] Frege, I will suggest, invented just what Kant envisages here, an *ars characteristica combinatoria*, one that would do for the new form of mathematics that emerged over the

[27] *Correspondence*, trans. and ed. Arnulf Zweig (Cambridge: Cambridge University Press, 1999), p. 208; AK 10:351.

course of the nineteenth century precisely what earlier mathematical notations had done: it would enable mathematical reasoning *within* the system of signs. But if that is right, then as we will see, Frege's system of signs must function in a way that is radically different from the way it is generally taken to function.

Although he was a mathematician, Frege was not, as Boole was, interested in pursuing a mathematical investigation into patterns of valid inference. What he was after was a language *within which* to reason in mathematics, that is, a language within which to formulate definitions of mathematical concepts and on that basis to prove theorems about those concepts. His system of written signs was, as he explains in the preface to his 1879 *Begriffsschrift*, to be at least a partial realization of the Leibnizian idea of a universal characteristic, a *calculus philosophicus* or *ratiocinator*, "a method of notation which fits things themselves".[28]

This idea of a universal characteristic, which Leibniz first explores in his "Dissertation on the Art of Combinations" (1666), goes back at least to Descartes. As Descartes explains in a letter to Mersenne, 20 November 1629, "in a single day one can learn to name every one of the infinite series of numbers, and thus to write infinitely many different words in an unknown language. The same could be done for all other words necessary to express all the other things which fall under the purview of the human mind."[29] To learn the system of Arabic numeration is to learn to write any natural number and although such a written numeral will be read, that is, pronounced, differently in different languages — for instance, the numeral '84' is read in English as 'eighty-four' but in French as 'quatre-vingt-quatre' — it will be *written* the same way for all. So, Descartes suggests, the same could be done for all the words of natural language. But, he cautions,

> the discovery of such a language depends upon the true philosophy. For without that philosophy it is impossible to number and order all the thoughts of men or even to separate them out into clear and simple thoughts ... If someone were to explain correctly what are the simple ideas in the human imagination out of which all human thoughts are compounded ... I would dare hope for a universal language.[30]

[28] Gottlob Frege, *Conceptual Notation and Related Articles*, trans. and ed. T.W. Bynum (Oxford: Clarendon Press, 1972), p. 105.

[29] *The Philosophical Writings of Descartes*, vol. III: The Correspondence, trans. John Cottingham, Robert Stoothoff, Dugald Murdoch, and Anthony Kenny (Cambridge: Cambridge University Pres, 1991), p. 12.

[30] Correspondence, p. 13.

The realization of a universal language not only of our ideas of numbers but of all our ideas depends on an adequate analysis of those ideas into their primitive elements, the "simples" of thought, and hence, Descartes thinks, on "the true philosophy".

Frege appears to have come to the idea of such a language from Trendelenberg's 1867 essay "On Leibniz's Project of a Universal Characteristic", which Frege refers to in the Preface of the 1879 logic. (It is also from Trendelenberg that Frege takes the word "*Begriffsschrift*" as the name for his symbolic notation.) As Trendelenberg observes in that essay, in ordinary language there is usually only an accidental connection between the way words are formed from sounds and other words and the way the things signified are or are not related: "only to a small extent is there an internal relation between the sign and the signified idea".[31] A properly Leibnizian *lingua characterica* would be, as Trendelenberg explains, a language in which signs for complex concepts are constructed out of signs for simple concepts (as signs for larger numbers are constructed out of signs for smaller), and constructed in such a way that reasoning on the basis of such concepts could be conducted according to determinate rules (as arithmetical calculations are). Such a language, Trendelenberg thinks, would be properly described as a *Begriffsschrift*, concept-script, because in such a language "the shape of the sign [would be brought] in direct contact with the content of the concept".[32]

Frege makes just these points in the long essay contrasting Boole's logic with his own that he wrote in response to Schröder's review of *Begriffsschrift*. He begins the essay by recalling Leibniz's

> idea of a *lingua characterica*, an idea which in his [Leibniz's] mind had the closest possible links with that of a *calculus ratiocinator*. That it made it possible to perform a type of computation, it was precisely this fact that Leibniz saw as a principal advantage of a script which compounded a concept out of its constituents rather

[31] Quoted in Hans D. Sluga, *Gottlob Frege* (London, Boston, and Henley: Routledge and Kegan Paul, 1980), p. 50. See also Volker Peckhaus, "Language and Logic in German Post-Hegelian Philosophy" in The Baltic International Yearbook of Cognition, Logic and Communication, volume 4: *200 Years of Analytic Philosophy*, DOI: 10.4148/biyclc.v4i0.135, for some of the larger context. Peckhaus also usefully emphasizes two features of Trendelenberg's conception of logic that would be central for Frege as well, a focus on the dynamic aspects of reasoning, that is, on reasoning as a process (rather than on static logical relations), and the wholesale rejection of any form/content distinction in logic.

[32] Sluga, p. 50.

> than a word out of sounds, and of all hopes he cherished in this matter, we can even today share this one with complete confidence.[33]

Frege's own *Begriffsschrift*, we are told, is "a fresh approach to the Leibnizian idea of a *lingua characterica*".[34]

As Trendelenberg had, Frege motivates the idea of such a language by reminding us of the lack of any systematic connection between ordinary words and that which they signify, which a *lingua characterica* requires.

> There is [in verbal language] only an imperfect correspondence between the way words are concatenated and the structure of the concepts. The words 'lifeboat' and 'deathbed' are similarly constructed though the logical relations of the constituents are different. So the latter isn't expressed at all, but is left to guesswork. Speech often only indicates by inessential marks or by imagery what a concept-script should spell out in full ... A lingua characterica ought, as Leibniz says, *peindre non pas les paroles, mais les pensées*.[35]

And again the language of mathematics provides the model for what is wanted in such a concept-script: "The formula languages of mathematics come much closer to this goal, indeed in part they arrive at it". But, as Frege goes on,

> at precisely the most important points, when new concepts are to be introduced, new foundations laid, it has to abandon the field to verbal language, since it only forms numbers out of numbers and can only express those judgments which treat of the equality of numbers which have been generated in different ways. But arithmetic in the broadest sense also forms concepts — and concepts of such richness and fineness in their internal structure that in perhaps no other science are they to be found combined with the same logical perfection. And there are other judgments which arithmetic makes, besides equalities and inequalities.[36]

The task of a *Begriffsschrift*, then, is to form expressions for complex mathematical concepts out of signs for primitive concepts in a way that displays

[33] Gottlob Frege, "Boole's logical Calculus and the Concept-script", in *Posthumous Writings*, trans. Peter Long and Roger White (Chicago: University of Chicago Press, 1979), p. 9.

[34] "Boole's logical Calculus", p. 12.

[35] "Boole's logical Calculus", pp. 12–13.

[36] "Boole's logical Calculus", p. 13.

their significance for reasoning, and for this the formula language of arithmetic must be supplemented with signs for the logical element that will serve as the "logical cement" binding together the primitive signs available already in arithmetic. It is just this that Frege's own concept-script attempts: "to supplement the formula language of arithmetic with symbols for the logical relations in order to produce — at first just for arithmetic — a conceptual notation of the kind I have presented as desirable"[37], a notation that has "simple modes of expression for the logical relations" that are "suitable for combining most intimately with a content".[38] "I wish to blend together the few symbols which I introduce and the symbols already available in mathematics to form a single formula language".[39] As one builds numbers out of numbers in the formula language of arithmetic so Frege would build concepts out of concepts in his formula language of pure thought. In *Begriffsschrift* "we use old concepts to construct new ones . . . by means of the signs for generality, negation and the conditional".[40]

Isolating the primitive logical notions that are needed to serve as the "logical cement" binding old concepts into new, and devising written signs for them, was the easy part. The hard part was to figure out how it is possible to exhibit the contents of concepts at all. Frege needed not merely to *say* what a particular content amounts to — as we do when we say, for instance, that a prime number is a number that is not divisible without remainder by any other number except one — but to *exhibit* the inferentially articulated contents of concepts themselves. Much as an Arabic numeral displays the arithmetical content of a number in a way enabling calculations in that system of notation, so Frege needed a way of writing that would display the contents of concepts such as that of being prime in a way that would support inferences in the system. The task was not merely to *record* necessary and sufficient conditions for the application of a concept, what is the case if the concept applies; it was to *show*, to set out in written marks, the contents of concepts as these contents matter to inference. And to do that Frege needed to invent a radically new sort of written language, not merely a new system of written marks but a fundamentally new *kind* of system of written marks.[41]

[37] Gottlob Frege, "On the Scientific Justification of a Conceptual Notation", in *Conceptual Notation*, p. 89.

[38] "On the Scientific Justification", p. 88.

[39] Gottlob Frege, "On the Aim of the 'Conceptual Notation"', in *Conceptual Notation*, p. 93.

[40] "Boole's Logical Calculus", p. 34.

[41] It can help to think here of Arabic numeration as contrasted with Roman numeration. Relative to that older system, Arabic numeration is an essentially different kind of system of written marks; it operates according to radically different, and more sophisticated, principles from those of Roman numeration.

We begin with a mathematical language, that is, a system of written marks within which to do mathematics, specifically, the formula language of arithmetic. In this system the various signs — the numerals, the signs for arithmetical operations, and so on — all have their usual meanings. In virtue of those meanings, equations in the language serve to display various arithmetical relations that obtain among numbers (or magnitudes more generally). The equation '$2^4 = 16$', for instance, displays an arithmetical relation that obtains among the numbers two, four, and sixteen.[42] In this equation, the Arabic numeral '2' stands for the number two, the numeral '4' stands for four, the numeral '16' stands for sixteen, and the manner of their combination shows the arithmetical relation they stand in. Now we learn to read the language differently, as a fundamentally different kind of language from that it was developed to be.[43] Instead of taking the primitive signs of the language to designate prior to and independent of any context of use, as we needed to do to devise the language in the first place, now we take those same signs only to express a sense prior to and independent of any context of use. Only in the context of a whole judgment and relative to some one function/argument analysis will we arrive at sub-sentential expressions, whether simple or complex, that designate something. If, for instance, we take the numeral '2' to mark the argument place, the remaining expression designates the concept *fourth root of sixteen* — where a concept is to be understood on the model of a function as it is understood by nineteenth century mathematicians such as Riemann. A Fregean concept is a mapping, or as Frege sometimes puts it, a law of correlation, objects to truth-values in the case of first-level concepts and lower-level concepts to truth-values in the case of higher-level concepts.

If now we instead regard the numeral '4' in '$2^4 = 16$' as marking the argument place, the remainder designates the concept *logarithm of sixteen to the base two*. Other analyses are possible as well, and none are in any way privileged. A primitive sign in the language of arithmetic as Frege learns to read it can be seen now as, say, a name for the number two and now as a part of a concept word for the concept *logarithm of sixteen to the base two*; independent of a way of seeing it is neither the one nor the other. In the language as Frege conceives it, the primitive signs only express a sense independent

[42] The example is Frege's in "Boole's logical Calculus", pp. 16–17.

[43] That a language devised for one use can come to be read in a new way is illustrated already in the case of written languages such as written English. Its sentences were designed to be a record of utterances, the sounds speakers make; but its words come to be read as themselves the bearers of meaning. Frege makes this point in the fragment "Logical Generality", in *Posthumous Writings*, p. 260.

of a context of use.[44] Only in the context of a proposition and relative to an analysis into function and argument do the sub-sentential expressions of the language, whether simple or complex, serve to designate anything.[45]

As Frege reads it, the equation '$2^4 = 16$' does not exhibit an arithmetical relation but instead expresses a thought, a Fregean sense. It is "a mapping of a thought";[46] "the structure of the sentence can serve as a picture of the structure of the thought".[47] Of course we could not learn to read the language this way if we did not first understand it differently, as a radically different, and conceptual prior, sort of language, as a language within which to exhibit arithmetical relations. But given that we do understand it in that more primitive way, we can learn also to read it Frege's way. All that is needed now in order to deal with *logically* articulated concepts and thoughts is to add Frege's signs for the logical relations, "in this way forming — at least for a certain domain — a complete concept-script".[48]

Frege's *Begriffsschrift*, like earlier mathematical languages, is essentially written. Unlike those earlier languages, it was devised specifically as a language within which to reason deductively from defined concepts; it was designed, in other words, to embody just the sort of deductive mathematical reasoning from explicitly defined concepts that is reported in natural language in the works of Riemann, Dedekind, and other nineteenth century mathematicians. It is for just this reason that, as Frege himself notes, it is not the derivations from axioms in Part II of the 1879 logic but Frege's derivations of various theorems in the theory of sequences from four definitions in Part III that "are meant to give a general idea of how to handle this 'conceptual notation"' (*Begriffsschrift* §23). Frege will furthermore claim in *Grundlagen* (§91) that such reasoning, that is, the derivation of theorems from definitions of concepts and in particular the derivation of theorem 133 from his definitions of the concepts *following in a sequence* and *single-valued function*, is ampliative, a real extension of our knowledge, despite being strictly deductive. I have addressed this idea of ampliative deductive

[44] Of course Frege could put the point this way, that is, in terms of the distinction of sense (*Sinn*) and meaning (*Bedeutung*), only after 1890. Nevertheless, in his practice, the distinction is there from the beginning; although made explicit only in 1892, it is implicit already in the 1879 logic that formulae of Frege's language only express Fregean thoughts (and designate truth-values) independent of an analysis into function and argument, that sub-sentential expressions, whether simple or complex, only designate relative to a function/argument analysis of the whole.

[45] This feature of Frege's notation is explored at length in my *Frege's Logic* (Cambridge, Mass.: Harvard University Press, 2005).

[46] "Notes for Ludwig Darmstaedter", in *Posthumous Writings*, p. 255.

[47] "Compound Thoughts", in *Collected Papers on Mathematics, Logic, and Philosophy*, ed. Brian McGuinness and trans. Max Black et al. (Oxford: Basil Blackwell, 1984), p. 390.

[48] "Boole's logical Calculus", p. 14.

proofs in Frege's concept-script elsewhere.[49] Here I want to focus on Frege's notion of sense in order further to clarify the nature of the mathematical language within which to reason that Frege devised.

Frege's notion of sense is generally taken not to be a properly logical notion at all; it is taken to concern instead the cognitive value of some content to a thinker, where two sentences that have the same truth-conditions can differ in cognitive value for a thinker if that thinker can, without irrationality, assent to one but not the other. Sameness and difference in semantic value, a difference in truth-conditions, in the meaning expressed by a sentence, is a properly logical notion. Frege's notion of sense, it is thought, is not. And the reason it is thought not to be a logical, I will argue, is that meaning in standard logic is understood in terms of truth (and perhaps satisfaction), *and* it is assumed that a specification of truth-conditions gives one everything that is needed for a correct inference, that is, that two sentences that have the same truth conditions cannot differ in their inferential consequences. Once we see that sameness in truth conditions does not entail sameness in inferential consequences, we will be able to see that Fregean sense concerns inferential consequences, and is obviously a logical notion.

Consider, first, the very familiar debate between Millians and descriptivists over proper names. We assume for the purposes of the argument that if two sentences have the same truth-conditions then they have the same inferential significance, the same consequences, and will use as our example the familiar pair of sentences, 'Hesperus is a planet' and 'Phosphorus is a planet'. According to the Millian, this pair of sentences shows that proper names are merely labels for objects because, after all, both sentences refer to one and the same thing (Venus) and say the same thing about it (that it is a planet). One might not know that they have precisely the same truth-conditions, but they do. The differences between them, for instance, the fact that one does not infer one from the other, is to be explained by appeal to the notion of cognitive value. On the Millian view, the two sentences have the same meaning, the same semantic value, but can have different cognitive values for a thinker.

Where the Millian argues modus ponens from our assumption, that is, that the two sentences have the same truth-conditions and hence the same inferential consequences, the descriptivist argues modus tollens: the two sentences do not (*pace* the Millian) have the same inferential consequences, as is shown by the fact that there is no valid inference from one to the other without the additional premise that Hesperus is identical to Phosphorus, and hence the two sentences also do not have the same truth-conditions. The difference between the two sentences, in other words, must be not merely cognitive but semantic, a difference in meaning, in what is the case if the

[49] See my "Diagrammatic Reasoning in Frege's *Begriffsschrift*", in *Synthese* 186 (2012): 289–314.

sentences are true. And yet the Millian seems to be *right* about the truth-conditions of the two sentences: *they are identical*. The descriptivist nonetheless seems to be right about the *inferential consequences* of the two sentences: they are *different*. But if so, then it is the conditional that both agree on that is wrong: a specification of truth-conditions is not thereby a specification of inference potential.

The second debate has just the same structure but concerns the nature of quantifiers, in particular, whether a universally quantified claim is logically equivalent to a conjunction of instances, and an existentially quantified claim logically equivalent to a disjunction of instances (as the substitutionalist claims), or whether they are logically different (as the objectual interpretation has it).[50] The substitutionalist, seeing that the truth-conditions are the same in the two cases, argues that there is only a cognitive difference between a quantified sentence and the relevant truth-function of instances. The objectualist, seeing that the inferential consequences of the two are different, argues that there must then be a semantic difference between the two, a difference in meaning, in the truth-conditions in the two cases. Only there does not seem to be, and so neither side is able to convince the other. Again, one wants to conclude that both are half right: the truth-conditions of the quantified sentence and the truth-function are identical though the inferential consequences are not. It is the assumption that sameness in truth-conditions entails sameness in inferential consequences that is mistaken.

Let us, then, distinguish as two logically different notions, on the one hand, the notion of what is the case if a sentence is true, that is, its truth conditions, and on the other, the notion of what follows if it is true, its inference potential. Now we can ask, which of the two do we want our language to map or trace? Frege's answer is clear: "everything necessary for a correct inference" (*Begriffsschrift* §3), that is, inference potential. A Fregean thought, which is the sense expressed by a sentence of *Begriffsschrift*, is the expression of the inference potential of a sentence. One can recover truth-conditions from such a sentence, but only by giving it an analysis into function and argument. What the sentence directly maps is inference potential. And because it does, Frege's *Begriffsschrift*, his concept-script, serves as a language within which to reason in mathematics.

[50] This debate is less well known. Important papers include: Ruth Barcan Marcus, "Modalities and Intensional Languages", *Synthese* 27 (1962): 303–322; Willard van Ormand Quine, "Reply to Professor Marcus", *Synthese* 27 (1962): 323–330; Joseph Camp, "Truth and Substitutional Quantifiers", *Nous* 9 (1975): 165–185; Saul Kripke, "Is There a Problem about Substitutional Quantification?", in *Truth and Meaning: Essays in Semantics*, ed. Gareth Evans and John McDowell (Oxford: Clarendon Press, 1976); Peter van Inwagen "Why I Don't Understand Substitutional Quantification", *Philosophical Studies* 39 (1981): 281–285; and Daniel Bonevac, "Quantity and Quantification", *Nous* 19 (1985): 229–247.

We have seen that although mathematics is not invariably done in a system of written signs, when it is, the system of signs is distinctive insofar as it enables one to reason *in* the language, something that is not possible in natural language. Frege's *Begriffsschrift*, I have suggested, is such a system of signs. It was designed to enable one to reason deductively from defined concepts in the new form of mathematical practice that had emerged in the work of Riemann and others. The language, read as Frege intended, embodies this form of reasoning; it shows how it goes in a way that is impossible otherwise. It enables even the less gifted of us to reproduce the results. And it can do, I have indicated, because what a formula of *Begriffsschrift* (directly) maps is not truth-conditions but inference potential.

Frege's language was not understood, and it was not understood because no one, including Frege, realized that reasoning from defined concepts in mathematics is quite unlike reasoning in other contexts (say, in philosophy or in everyday life) insofar as it can be embodied in a specially devised notation such as *Begriffsschrift*.[51] Frege's notation was read as if it functions as a notation of logic in just the way other notations of logic were thought to function, for instance, the Peano notation that Russell used. It came to seem that the only important difference between Frege's language and the other logical languages then under development, beginning with Boole's, was that Frege's was always already interpreted whereas the others were not.[52] It came to seem that Frege was the founder of modern quantificational logic, that the signs of logic are merely a convenient shorthand for natural language expressions, and that mathematics itself is done in natural language and so can be reproduced in our formal languages. Only very recently have practice-based philosophers of mathematics come to see that mathematical logic, both as a practice and in its results, is largely irrelevant to the practice of mathematics and to our prospects for understanding that practice.

IV. *Conclusion*

I have suggested that systems of written signs function in mathematics to embody reasoning, that one reasons *in* the language in such cases. But if that is right, if such languages put the reasoning itself before our eyes, then it is those languages and the reasoning they embody that we need to study if we are to understand how the reasoning works as mathematical reasoning. The same is true, it would seem, for a practice-based philosophy of logic.

[51] Though I cannot explore the point here, a crucial aspect of this difference is the nature and role of definitions in mathematical practice, and the correlative difference between mathematical concepts and other concepts.

[52] See Jean van Heijenoort, "Logical as Calculus and Logic as Language", *Synthese* 17 (1967): 324–30.

We need to determine how the signs of the logicians' languages work. Do they function merely as abbreviations or do they function in some other way? And if they *are* mere abbreviations, then what is the status of the logician's proof? Does it embody reasoning, or merely report it? What, in sum, *is* the role of specially devised written signs in the practice of logic? Only a practice-based philosophy of logic can tell us.

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