Signs as a Theme in the Philosophy of Mathematical Practice [AAM, 13 April 2023]

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Introduction

That mathematical texts do not solely consist of arguments conducted in natural language is obvious: geometrical diagrams and symbolic formulas routinely serve as emblems for the discipline. Yet, in a sense, much of twentieth-century philosophy treated this fact as *inessential*, assuming that mathematics could be recast in some uniform mold—say first-order logic, itself often seen as little more than a regimentation of natural language—and that nothing philosophically crucial about the nature of mathematical knowledge would be lost in the process. In contrast, recent years have seen a surge of philosophical interest in the various 'signs' or 'representations' (more on terminology below) actually employed by mathematicians. My goal in this paper is to explain why: philosophically, what is there to gain by focusing on mathematical signs?

First, more needs to be said about the mathematical and philosophical context that made the variety of signs actually used in mathematics seem unimportant. One aspect of this context, namely the rejection of diagrams in 'rigorous' proofs, has been much discussed—and debated—over the past thirty years. However, the attitude toward diagrams is but one part of a broader story: as we shall see, there has been a concomitant tendency to also treat symbolic notations as a superficial matter. Section 1 offers a brief and tentative sketch of this background.

The paper then presents recent work that takes mathematical signs seriously by distinguishing three broad reasons, or philosophical agendas, for doing so. Some work looks to the signs (and in particular to diagrams) to recover norms of informal or historical mathematical practices (Section 2); attempts to draw philosophical conclusions from such efforts often look to the (broadly speaking Peircean) thesis that what makes mathematical knowledge special is not logical inference in the traditional sense, but the reliance on the construction and manipulation of signs.

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Some work studies signs as a way to highlight the heuristic, or more generally the 'non-rigorous' aspects of mathematical practice, emphasizing how the ambiguity and openness to reinterpretations of mathematical signs is crucial to the historical development of the discipline (Section 3). Finally, the work surveyed in Section 4 focuses on what may, at first, appear to be psychological or pragmatic features of mathematical signs: the way they leverage human cognitive abilities; the way they make certain tasks more or less efficient. One reason why such a study matters philosophically, as we shall see, is that questions of computational efficiency—and hence, the signs we use—shape our mathematics quite deeply, constraining which mathematical objects end up being salient for us.

With respect to the existing literature and to other papers in this volume, my focus here is distinctive in two ways. On the one hand, I restrict myself to external, *non-verbal* signs (like diagrams and symbolic notations), excluding mere written text. While this choice reflects recent trends, and makes sense as a reaction to the philosophical context alluded to above, one should keep in mind that there is no clear dividing line between verbal and non-verbal signs—between, say, introducing new terminology and introducing a new piece of symbolic notation. The language of mathematical texts, even when it does not rely on specific symbols and looks just like, say, English, ancient Greek, or Chinese, can in fact be quite specialized, constrained, and in a sense 'artificial', as emphasized by historians (see Netz, 1999; Chemla, 2006); indeed, the close parallel between questions of terminology or concepts on the one hand, and questions of notation or representation on the other, will come up repeatedly below. Accordingly, some authors choose a broader scope from the outset, discussing 'mathematical language' (Avigad, this section) or the 'semiotics of mathematics' (Wagner, this section) without separating the seemingly verbal from the seemingly non-verbal (for a short general introduction, see also Colyvan 2012, chap. 8). On the other hand, as mentioned already, I focus on non-verbal signs *in general*, including symbolic notations; many surveys are more restricted, covering only visualization (Mancosu, 2005; Giaquinto, 2020) or diagrams (Shin, Lemon, Mumma, 2013; Giardino, 2017).

Before getting started, a terminological remark is in order. How should one refer, generically, to diagrams, graphs, formulas, matrices, and so on—in short, to the many non-verbal inscriptions used in mathematical practice? An obvious choice is to call them *representations*, which is in line with well-established usage in philosophy of science.[1] It is not without problems, however; among other things, the term already has technical uses in mathematics. In keeping with the title of this section of the *Handbook*, on the *semiology* of mathematics, I decided to talk of *signs* instead, though no substantive position should be taken to hinge on this.

[1] See, e.g., Frigg, Nguyen (2020).

1 The ideal of signless mathematics

It would be hopeless to survey the entirety of twentieth-century thinking about mathematics here, and preposterous to claim that all of it was marked by a dismissal of the role of signs: there certainly have been major countervailing tendencies (some varieties of formalism, Wittgenstein's late philosophy of mathematics, and much of 'post-structuralist' continental philosophy come to mind). Nevertheless, it is a crucial background for the rest of this paper that some of the main currents of thought of the period *have* made the issue of mathematical signs decidedly marginal. To bring home this point, I highlight two suggestive elements of context, one philosophical and one mathematical.

In the philosophy of mathematics, it is the conjunction of two widely held views—inherited from the intense work on the foundations of mathematics in the first half of the twentieth century—that presents the most significant obstacle to the study of mathematical signs. The first view is that mathematical proofs, and hence mathematical knowledge, can be adequately captured as derivations in some formal system. The general notion of a formal system, of course, is very broad: it includes systems that encode the legal moves in chess, rewriting systems encoding algebraic computations, the lambda calculus and its derivatives, and indeed the systems encoding the construction and manipulation of diagrams that we'll meet in Section 2. But this first view is often conjoined with a second one: namely, that the formal systems appropriate for mathematics are *logical*, roughly in the sense that their derivation steps regiment, model, or capture logical inferences. The prototypical examples of such systems are the various formulations of first-order logic (in which set theory is standardly phrased), typical inference rules of which are *modus ponens* and universal generalization. Such rules are meant to be general rules of reasoning, which could be formulated in words just as well as in symbols.

In combination, these two theses yield a view of mathematics in which, *in principle*, any proof could be replaced by a formal derivation in some logical system, and this formal derivation in turn by a piece of reasoning conducted in natural language. It is often conceded that such a reduction is not practical, but the impression remains that, as far as the nature of mathematical knowledge goes, little is lost *philosophically* by focusing on natural language arguments and idealizing all the rest away. The result is not just to exclude diagrams and other ostensibly non-linguistic signs from serious consideration, but also to downplay the distinctiveness of symbolic calculi and computations by assimilating them to verbal reasoning.

While some philosophers of mathematical practice might be inclined to reject both theses at once, notice—anticipating the next section—that the first thesis, formalizability, does not on its own preclude a substantial investigation of the variety of mathematical signs. Nothing in the concept of a formal system, at least as understood in mathematical logic or computer science, immediately prevents applying it to ostensibly non-linguistic signs like diagrams, or more broadly to sign manipulations of any kind that do not obviously track verbal proof steps. In particular, we shall encounter a range of efforts at formalizing diagrammatic reasoning in Section 2; tellingly, many have come from researchers trained in mathematical logic or computer science rather than from philosophers.

The second element of context are attitudes, prevalent in parts of pure mathematics in the twentieth century, according to which the discipline should become 'conceptual' in some sense, working from intrinsic definitions of its objects rather than relying on notations and computations. As a signpost to broader developments, let me focus on a few suggestive remarks by members of the influential Bourbaki group in the middle of the century.

At the heart of Bourbaki's outlook (as presented in their manifesto 'The Architecture of Mathematics'[2]) is what they call the 'axiomatic method', which invites identifying and separating out, axiomatically, the elementary structures that are normally mixed together in any given mathematical theory or problem. Their ideal is that, once the right structures have been isolated and the crucial ideas properly encoded in the theory, the proofs should flow easily, in what may be called a purely 'conceptual' way—that is, without recourse to opaque calculations or geometric intuitions not adequately captured by the axioms. As they famously phrased it (taking up a slogan from Dirichlet that is typically associated with a purported nineteenth-century German school of 'conceptual mathematics'[3]), an overarching goal of theirs was to 'substitute *ideas* for *calculations*';[4] in the retrospective words of Chevalley, one of the founding members of the group, 'anything that was purely the result of a calculation was not considered by us to be a good proof.'[5]

This is where the connection with signs arises: from such a perspective, the real mathematical action ought to take place at the conceptual level, so that, once the appropriate structures have been worked out, the particular signs used ought to be inessential. An offhand remark by Weil, another founding member of Bourbaki, is particularly telling in this respect. In a famous (and famously nasty) polemical reply to Unguru—a historian of mathematics who had insisted that, to obtain a proper understanding of ancient Greek mathematics, it was crucial to refrain from transcribing ancient results into modern algebraic symbolism and, instead, to respect ancient phraseology and notations—Weil wrote, as though it were self-evident, that 'the content of a theorem does not change greatly, whether it is expressed in words or in formulas; the choice, as we all know, is mostly a matter of taste and of style.'[6] In Weil's view, what marks out the real mathematician is precisely the competence and ability to *see beyond the signs*.

Again, the two trends just discussed—in academic philosophy, a focus on logical formalization; among some mathematicians, a preference for 'conceptual' methods—should not be taken to represent the entirety of thinking about mathematics in the twentieth century. Nevertheless, they played a major role in making

[2] Bourbaki (1950) = Bourbaki (1948).

[3] For a first introduction, see Ferreirós (2007, 24–38); but see Haffner (2014, chap. 1) for a good overview of the difficulties involved in identifying such a school, or in tracing the origins and meaning of this slogan.

[4] Bourbaki (1950, 231) = Bourbaki (1948, 47); emphasis in the original French.

[5] Guedj (1985, 22).

[6] Weil (1978, 92).

the diversity of signs actually used in the practice of mathematics seem peripheral. Explaining how and why the tide has now turned, at least in some parts of the philosophy of mathematics, is the goal of what follows.

2 Proof beyond logic: the norms governing non-verbal signs

Mathematical practice is a peculiarly constrained activity: there seem to be rigid norms—more rigid, at least, than in most other human pursuits—governing which statements and justifications practitioners take to be correct. At first sight, formalizations in logical systems offer an explanation for this rigidity: a mathematical proof is correct, one might say, if and only if it can be routinely formalized in a suitable logical system. However, such formalizations are far removed from mathematics as it is actually found in the classroom or in research: proofs there might contain diagrams, say, or algebraic calculations; even when they look like a purely logical argument, they are not usually couched in a formal language like that of first-order logic, nor are they formally correct—and formalization can prove quite difficult. And yet it is clear that generations of pupils have found their mathematics teachers more exacting and rigid in their expectations than any others. *Prima facie*, it is not clear how formalizations that are neither used, nor even accessible in practice, can nevertheless account for the manifest rigidity of practitioners' judgments.

Accordingly, one motivation for looking at the particular signs used by mathematicians is to describe the *surface* norms, so to speak, that govern their practices. One possible goal can be to provide a *bridge* to underlying formalizations: if we take formalizability as the criterion for correctness of mathematical proofs, a full account of mathematical practice requires explaining how *informal* proofs are actually judged to be correct, and how such informal correctness judgments manage (at least in many cases) to track formalizability. We shall discuss efforts of this kind with respect both to a much-studied historical example, namely elementary plane geometry in the style of Euclid, and to more contemporary uses of diagrams.

A more radical goal, as we shall see, can be to account *directly* for the rigidity and distinctiveness of mathematics, bypassing formalization accounts altogether: work on diagrams (and on non-verbal signs in general) has the potential to broaden our vision of proof beyond logical inferences as standardly construed, or at least—if one prefers to keep the usual meaning of 'proof' fixed—to broaden our view of the distinctive normativity of mathematics beyond (logical) proof. One can end up arguing that what makes mathematics unique is not its link to logical deductions, but the fact that it is based on strictly normed sign manipulations.

2.1 A historical example: Euclid's geometry

First, consider a historical example—one much-discussed in the recent literature—namely elementary plane geometry, with its *diagram-based* proofs that the angles at the base of an isosceles triangle are equal, that the angles in any triangle sum to two right angles, and the like. Under various guises, such proofs have been a mainstay of mathematics in the West at least since Euclid's *Elements*, often serving until recently as *the* exemplar of mathematics. Paradoxically, though, their use of diagrams makes them difficult to accommodate for contemporary philosophical views, a discrepancy that has of late spawned a fairly large literature.[7]

Let us first clarify the problem. Roughly speaking, from a contemporary point of view, elementary geometry studies what can be deduced logically from fundamental, verbally formulated assumptions about space ('axioms')—where, as a rough approximation, one can think of logical deduction as corresponding to the use of a handful of simple rules like *modus ponens* and universal generalization. But, though Euclid's text does appear axiomatic—with 'postulates' and 'common notions' that, at first sight, play the role of axioms, and an apparent commitment to prove everything on the basis of these—his proofs come with diagrams and *crucially rely on them*, in the straightforward sense that if the diagrams are removed, some claims in the proofs no longer appear supported by preceding ones.[8] From the vantage point of contemporary mathematics, we can only see this as a flaw, since it is not clear whether or how those steps that rely on the diagram could be deduced logically from the hypotheses, postulates or common notions. Hence, insofar as we accept Euclid's theorems or proofs as correct at all, it can only be because *we* are able to *patch them up* according to our standards, eliminating any use of diagrams in the process—or so it seems to the modern observer. While one could try to argue that our patched-up proof is, in some sense, 'essentially' the same as the ancient one, it seems clear that such 'corrections' import later tools and standards into the earlier proofs instead of assessing them on their own terms.

This perspective on Euclid, based on modern views of proof, raises two crucial problems. First, the use of diagrams obviously does not mean that 'anything goes'. In ancient Greece—just as today—geometry was seen as a distinctively rigorous enterprise, with uniquely little room for disagreement among practitioners: it is clear that ancient geometers had their own norms as to what counted as a correct proof,

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[7] Of course, there is no unitary practice spanning the two millennia and counting from Euclid to today's classrooms; for the sake of precision, the literature has mostly focused on the first books of the *Elements* themselves, usually without attending too closely to the details of the text.

[8] See Netz (1999, esp. chap. 5, 175–182) for a careful investigation of the various ways diagrams are used and relied upon in Euclid's proofs. In some cases, the diagram only serves as a kind of shortcut, in the sense that unproblematic chains of logical inferences could easily be added to the text to 'fill in' the apparent justificatory blank left by the removal of the diagram; in others, such a 'filling in' would require adding postulates; in others still (proposition 35 of book I is a simple example), Euclid's propositions seem general, as does the beginning of their proofs, but at a certain point the proofs turn out to be about a particular case displayed by the diagram (i.e., about particular relative positions of the relevant points and lines), so that, absent the diagram, some statements in the proof no longer follow from preceding ones at all—the proof would have to be reorganized.

which we cannot hope to describe by invoking standards inaccessible to them. Second, even admitting that we need to patch up Euclid's proofs before we can accept his theorems as correct, how is it that we are able to do this for pretty much *all* of Euclid, without stumbling upon clear mistakes? We need to explain this fact, unless we are prepared to conclude that ancient geometers were incredibly lucky, or that later mathematicians were so committed to making the *Elements* come out as true that they found creative ways of reinterpreting and correcting them so as to *make* them correct (but see Vinciguerra, this section).

These two problems motivate much recent work, the goals of which are, accordingly, the following: first, to describe the norms that governed Euclidean geometric practice as the practitioners themselves understood them; second, to account for the fact that these norms, old-fashioned though they may be, largely produce conclusions that are *correct by contemporary standards*. The most philosophically sophisticated discussions along these lines are those by Manders (2008a,b) and by Panza (2012) (see also Netz 1999 for a philological perspective, and Ferreirós 2016, chap. 5 for a helpful introduction).

On the picture of Euclid that emerges, diagrams are not—as the modern perspective would have it—mere illustrations, sometimes used spuriously in the course of proofs, of a subject matter that could be specified axiomatically independently of them. Instead, they are a full-fledged part of the practice and are subject to precise norms, much like the pieces of reasoning conducted in Euclid's text.

The norms governing the diagrams are, in the first instance, codified in Euclid's postulates, which turn out not to be axioms in the modern sense: instead, they serve to codify how diagrams can be *constructed* and *enlarged*—if one insists on an analogy with modern logic, they are more akin to *formation* and (to a certain extent) *transformation* rules than to axioms. Once the diagrams are constructed, one can draw conclusions based on their appearance. While nowhere codified explicitly, this, too, turns out to be done only in very constrained and controlled ways. Equalities of lengths or angles, for example, are not (and indeed could not) be deduced from the appearance of diagrams. One *can*, however, rely on other features of diagrams, for instance on the fact that two circles that are partly inside one another have a point of intersection (like in Euclid's infamous first proposition)—features that turn out to be such that they can be reliably reproduced by different practitioners, working on different and imperfect diagrams. Other aspects of the practice also seem designed to secure agreement among practitioners: the division of the text into propositions whose proofs are kept relatively short by a systematic reliance on previously proved propositions, for instance, ensures that diagrams remain simple and uncluttered enough to be reliably reproduced, checked, and inspected.

In sum, a close inspection of Euclid's practice show that there are clear and substantial norms governing how diagrams can be constructed, transformed, and exploited, and that these norms seem designed to ensure—as far as possible—univocal agreement among experienced practitioners.

A question then remains: since Euclid's diagram manipulations are not justified by reference to other principles—in the sense that the diagrams are not illustrations of a subject matter that Euclid had means to define independently of them, and that, therefore, the norms governing their use could not be reduced to properties of this independent subject matter—what is Euclid's geometry *about*? Historians emphasize that it is a theory of figures (line segments, triangles, squares, circles, etc.) and *not*, as today's geometry, a theory of space.[9] But then, what exactly are these figures that cannot be defined independently of the diagrams yet are not reducible to them? Netz (1999, section 2.3.2) explains the problem well: On the one hand, Euclid's geometry—full of expressions like 'let a circle have been drawn', 'let the given bounded straight line be AB'—appears to be *about the diagrams themselves*, or at least to identify the objects it talks about with the concrete lettered diagrams that are referred to within the text. On the other hand, this cannot be quite right, as the diagrams clearly do not exactly have, and are not meant to exactly have, the properties ascribed by the text to its objects.

Panza (2012) articulates and defends the most straightforward answer: Euclid's geometry is about abstract objects that are *individuated*, and some properties of which are *determined*, by the concrete diagrams. (This makes these objects *quasi-concrete* in the sense of Parsons, like the 'expression types' familiar from the type-token distinction for languages: they 'have an intrinsic relation to the concrete', in the sense that they 'are determined by their concrete embodiments.'[10]) In other words, Euclid's constrained practice with concrete diagrams gives rise to a *sui generis* domain of abstract objects.

Instead, Manders (2008b) suggests focusing on the inferential norms governing the practice and doing away with any talk of geometrical objects, arguing in particular that Euclid's use of 'impossible' diagrams in proofs by contradiction—like the diagram with squished circles that is used to prove, in proposition 10 of book III, that two circles cannot cut in more than two points—puts too much pressure on accounts of diagrams as representations for them to be viable. (For an analysis of this argument, see Waszek 2022.)

2.2 Norms of non-verbal signs in contemporary mathematics

In contrast to Euclid, contemporary mathematics typically does possess resources to define its subject matter independently of diagrams (e.g., by defining its objects axiomatically or constructing them from set theory). Nevertheless, controlled uses of diagrams do still make their way into widely accepted proofs; as we shall see, this seemingly threatens common contemporary views about mathematical rigor.

Here are some examples from the recent literature. De Toffoli, partly in collaboration with Giardino (De Toffoli, Giardino, 2014, 2015a; De Toffoli, 2017, 2023), explores the use of various kinds of diagrams in knot theory and more broadly in low-dimensional topology, as well as in homological algebra: knot diagrams, polygons representing surfaces, arrow diagrams, etc. Giardino, Patras (n.d.) examine the

[9] On the transformation of geometry from a theory of figures to a theory of space, see for instance De Risi (2015).

[10] Parsons (2008, 33, and the entirety of §7).

role of Feynman diagrams in a proof from a recent research paper straddling algebra and mathematical physics.

In these cases, diagrams may be eliminable *in principle*, but in practice they are crucial to the way practitioners assess proofs as correct or incorrect. They may even, at least in the Feynman-diagrams case study, be in some sense *more fundamental* than any non-diagrammatic definition of the relevant subject matter: while there is no doubt that an appropriate non-diagrammatic definition of ‘Feynman diagrams’ could be provided, it would be cumbersome and, more importantly, it seems that the standard by which its *adequacy* would be judged would precisely be the diagrams themselves. For contemporary (informal) proofs too, then, attending to the signs actually used allows one to describe *surface* norms governing the practice.

Such diagram-based practices from contemporary mathematics put pressure on what is sometimes called the ‘standard view’ of mathematical rigor, most carefully spelled out by Hamami (2022): on this view, (1) an informal mathematical proof is *rigorous* if it can be *routinely translated* into a formal proof; (2) practitioners *judge* whether informal proofs are rigorous by recognizing what one might call ‘synactical shortcuts’, essentially higher-order inference rules; which (3) ensures that practitioners’ rigor judgments track actual rigor, that is, *track translatability into formal proofs*. If in fact practitioners—at least sometimes—judge proofs on the basis of sign manipulations that do not straightforwardly correspond to logical inferences or sequences thereof, one will have to offer another account of whether and how informal proofs that are judged correct by practitioners manage to track formal proofs.

One might hold that correct informal proofs do not, in fact, track formal proofs at all; see for instance Rav (1999, 2007). Alternatively, one might maintain that formalizability holds, but articulate other accounts of the relationship between informal proofs and formal proofs. Burgess, De Toffoli (2022) map out some possible options. One well-known proposal is Azzouni’s (2005; 2009). He introduces the notion of ‘inference packages’, of which the use of diagrams in elementary geometry is the prototypical example: according to him, these function as ‘black boxes’, allowing practitioners to extract consequences *reliably* from sets of assumptions while hiding from them the precise logical relations linking hypotheses and conclusions.[11]

2.3 Philosophical ramifications

Let us take stock: practices prototypically called ‘mathematical’ sometimes—perhaps even frequently—rely, not only on logical inferences in the usual sense, but also on controlled manipulations of non-verbal signs (prominently including diagrams). What should we make of this? On the one hand, historians or sociologists of mathematics would do well to adopt a broad notion of ‘proof’ that can accommodate such phenomena. This is precisely the move made by Chemla (2012), who suggests that a misplaced philosophical obsession with absolute certainty and hence logical proof

[11] For an attempt to apply this idea to a contemporary example, see again Giardino, Patras (n.d.) on Feynman diagrams.

has truncated and distorted our perception, not only of contemporary mathematics, but also of its ancient counterparts:

> By the term 'proof' [...] we simply mean texts in which the ambition of accounting for the truth of an assertion or the correctness of an algorithm can be identified as one of the actors' intentions.[12]

On the other hand, philosophically, doing this leaves us with an puzzle: if any justification appearing in a mathematical context can be called a proof, what—if anything—distinguishes 'mathematical' practices from other practices? If the peculiar rigidity of mathematics does not come from a specific method of demonstration, where does it come from?

One might answer that the question is misguided. Perhaps so-called mathematical practices are only special insofar as they rely on logical inference, which they normally do to a partial extent at best; as soon as one would leave behind the firm ground of logical proof, there would be nothing but a continuum of practices with no natural boundary for what should properly count as a mathematical justification. In particular, the various diagram-based practices discussed above would be nothing but more or less imperfect relatives of logical deduction.

The work just surveyed, however, suggests a different answer, one that has been most explicitly articulated by Peirce: perhaps the peculiar character of mathematics comes not from logical inference but from controlled sign manipulations. Peirce's account has, for this reason, undergone a revival in the philosophy of mathematical practice (see in particular Carter, 2014, 2020). For him, mathematics is about drawing 'necessary consequences' from hypotheses, but in contrast to more standard deductivist positions, this drawing of necessary consequences does not proceed by logical inference in the usual sense, but by the construction and manipulation of signs:

> [I]n drawing those [necessary] consequences, the mathematician uses what, in geometry, is called a "construction," or in general a diagram, or visual array of characters or lines. Such a construction is formed according to a precept furnished by the hypothesis. Being formed, the construction is submitted to the scrutiny of observation, and new relations are discovered among its parts, not stated in the precept by which it was formed, and are found, by a little mental experimentation, to be such that they will always be present in such a construction. Thus the necessary reasoning of mathematics is performed by means of observation and experiment, and its necessary character is due simply to the circumstance that the subject of this observation and experiment is a diagram of our own creation, the conditions of whose being we know all about. (Peirce, 1931–1958, §3.560)

In fact, for Peirce, logical inference is *itself* a species of the broader genus of reasoning through the construction and observation of signs—a broader genus which Peirce variously calls *deduction* (in a broad sense) or *diagrammatic reasoning*, it being understood that his use of 'diagram' includes such things as algebraic formulas and the schematic sentences of syllogistic logic.[13]

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[12] Chemla (2012, 18).

[13] He writes that 'all deductive reasoning, even simple syllogism, involves an element of observation' and that, for instance, the 'syllogistic formula, All M is P, S is M ∴ S is P' is 'a diagram of the relations of S, M, and P' (Peirce, 1885, 182).

Further discussion of Peirce's views go beyond the scope of this paper; for a helpful introduction written from the perspective of mathematical practice, see Carter (2020).

2.4 The problem of reliability and diagrammatic formal systems

Views of the normativity of mathematics that, like Peirce's, center on sign manipulations face an apparent roadblock in the oft-repeated (though distinctively modern) idea that diagrams are a source of fallacies.Indeed, it is often supposed that mathematics is made distinctive by its certainty and reliability—so is it not counterintuitive, from a modern point of view, to model one's *overall* account of mathematics, including logical inferences and simple algebraic computations, on something reputedly so unreliable as diagrammatic reasoning?

On the face of it, the work discussed above does show that even geometrical diagrams can be used in controlled and (at least to some extent) reliable ways. As for non-geometrical diagrams, many of those used in contemporary mathematics (like the arrow diagrams used in homological algebra or category theory) do not seem to threaten reliability at all. Might it nevertheless be the case that the use of diagrams, or at least of some of them, is inherently less safe than purely deductive reasoning?

In Euclid's geometry—to again use it as an example—one faces the so-called *generality problem*: one proves a *general* statement by constructing and inspecting a *particular* diagram; but is the particular diagram adequate to the general claim, or are there other, substantially different diagrams in which the claim might be wrong, or for which a further proof would be required? Applying Peirce's remarks, quoted above, to the case of Euclidean diagram-based proofs (which were, for him, a prototypical example), one needs to ensure 'by a little mental experimentation' that the conclusions drawn from the constructed diagram are 'such that they will always be present in such a construction'. This, however, cannot be done according to mechanical rules; it requires tact and care, and leaves one exposed to the risk of an oversight—a risk that, in the common image of mathematics, bulletproof logical deductions are meant to avert.

There are two complementary strategies to mitigate such worries. On the one hand, one can emphasize that run-of-the-mill, non-diagrammatic proofs *too* are fallible (though often very reliable); De Toffoli, for instance, was led—after her work on diagrams, though her motivations are broader—to a fallibilist account of mathematical knowledge (De Toffoli, 2021). On the other hand, one can bolster diagrams' claim to reliability by attempting to *formalize* (instances of) diagrammatic reasoning, which by now has been done in a number of cases, including for the simplest parts of a reputedly difficult case, namely Euclid's geometry (for surveys, see Mancosu, 2005; Shin, Lemon, Mumma, 2018; Giaquinto, 2020).

The first strategy ties in with broader discussions about the nature of informal proofs. Above, I wrote as if diagrammatic proofs raised a unique challenge to the 'standard view' of mathematical rigor (Hamami, 2022)—i.e., as if there were questionable proofs using diagrams on the one side and rigorous deductive proofs on the other, the latter basically being similar to formal proofs, just with *gaps* that correspond to the application higher-order inference rules and hence can routinely be filled. However, much recent work has argued that informal mathematical proofs are often *not* like that, especially in research contexts. This is not to say that informal proofs are unreliable; rather, the disagreement is on where the reliability of informal proofs comes from. On the 'standard view' as articulated by Hamami, informal proofs reliably track formal proofs because they are *sketches* of formal proofs and are *recognized as such* by practitioners; their reliability then comes directly from the unimpeachable solidity of the underlying formal proofs. In contrast, the argument made in papers such as Avigad (2021) and De Toffoli (2021) is that the reliability of informal proofs—the fact that they *do* track formal derivations—comes from a host of features of mathematical practice that allow for effective communal error-checking. On such views, informal proofs do not straightforwardly inherit their solidity from the formal derivations they track; the relationship between the two can be quite complex.[14]

From such perspectives, the fallibility or defeasibility that some diagrammatic proofs might display comes to be seen as typical, but it is compatible with high reliability: the remaining question is merely one of degree. In Manders's vocabulary, does a certain practice afford us enough *control*—by way of error-checking and dispute-resolution mechanisms—to avoid a fall into *disarray*, with recurrent errors and endless controversies? As Mancosu (2005) already concluded, this will have to be adjudicated on a case-by-case basis, whether there are diagrams or not. Euclid-like diagram use, for instance, certainly seems to offer sufficient control for elementary geometry, if not in more advanced settings; in many contemporary cases, diagrams do not seem to threaten reliability at all, and in fact might sometimes *increase* it by introducing helpful error-checking mechanisms (see Avigad 2021, 7392–7393 for such an argument).

The second strategy is to argue for the reliability of (some uses of) diagrams by developing *diagrammatic formal systems*. These come in two broad flavors, which face different challenges.

Systems of the first kind have a syntax that is truly diagrammatic, in the sense that it is *not* made up of strings of symbols; instead, the formation and transformation rules instruct one to draw, erase, or transform geometric objects like lines or curves. Systems of this kind were mostly developed around Barwise and Etchemendy's diagrammatic reasoning program (see Allwein, Barwise 1996 for an overview), and include the first attempt at a diagrammatic formal system for plane geometry (Luengo, 1996); the best-known of them is Sun-Joo Shin's (1994) system for Venn diagrams. By explicitly codifying how diagrams can be constructed and manipulated, such systems are meant to allow—as usual formal systems do—conducting consistency or soundness proofs, as well as performing 'move by move' checks of the correctness of purported proofs.

[14] See again Burgess, De Toffoli (2022) for a survey of the various ways of understanding this relationship.

In systems of this kind, however, issues like the generality problem are still lurking. Setting them up or reasoning about their properties requires being able to survey the totality of possible diagrams that could be constructed using certain rules. But, in contrast to standard formal systems—whose inductive formation rules give us a sufficient grasp on possible formulas to reason about them—diagrams may have non-obvious spatial properties: we may have difficulty picturing the full range of possible cases, and may make mistakes by only having simple and unrepresentative examples in mind.[15] It does seem that genuinely diagrammatic systems, despite their appearance of perfect codification, raise problems of control that do not arise with standard formal systems.

Diagrammatic formal systems of the second kind—for instance, those developed by Miller (2007), Mumma (2006; 2019), and Avigad, E Dean, Mumma (2009) for Euclid's geometry—have a more familiar syntax that is made up of strings of symbols, but is meant to *model* the diagrams and the reasoning conducted on them. Since their syntax is not itself diagrammatic, these systems are no less reliable than the standard ones found in logic textbooks. Instead, the challenge they face is one of *goodness of fit*: do they closely track the pre-existing diagrammatic practice, or are they more akin to *reconstructions* that eliminate the difficulties involved in diagram use? Consider again the case of Euclid. Rabouin (2015, note 33) points out that the extant systems for formalizing the *Elements* all end up rejecting at least a few of Euclid's proofs. The problem comes from those *reductio* proofs that require drawing apparently 'impossible' diagrams (like the impossible diagram for proposition 10 of book III, with two 'circles' that cut in four points): formalization, because it requires stipulating exactly what kind of diagrams can legally be drawn, is liable to block at least some of these proofs.

A thorough assessment of diagrammatic systems lies beyond the scope of this survey. However successful such formalizations may be, and whether or not one ends up accepting a Peircean view of mathematics, one conclusion is clear. Diagrammatic formal systems demonstrate that formalizing does not necessarily mean logicizing, at least not in the traditional sense: one can formalize norms of mathematical sign manipulations that do not correspond to logical inferences as usually construed. Note, though, that one can express the same conclusion in a different vocabulary: instead of talking of sign manipulations that are not logical, one can attempt to redefine logic so as to cover diagram manipulations, as suggested in particular by Barwise, Etchemendy and Shin on the basis of a semantic view of logical consequence (see for instance Barwise, Hammer 1994 and Shin 2004).

[15] Shin's original system turned out to contain a mistake of this kind: it had a transformation rule permitting the erasure of any curve in a Venn diagram, which overlooked the fact that in complex diagrams (having more than four curves), though not in simpler ones, such a transformation can, for topological reasons, lead to a diagram that is not well formed (see Scotto di Luzio, 2002; Howse, Molina, Shin, Taylor, 2002).

3 The open-endedness of signs and the historical development of mathematics

Let us now turn to work that, instead of looking to mathematical signs to recover *norms* of informal or historical mathematical practices, does—if anything—the very opposite, emphasizing the ambiguity and incidental features of signs, their openness to reinterpretation and change, and the way these drive the historical development of mathematics. The main thrust of such work, which often goes hand in hand with discussions of mathematical discovery and of the historical growth of mathematical knowledge, is to combat a vision of mathematics as an isolated and inward-looking realm governed by strict rules, and instead to emphasize its plasticity and open-endedness. My aim here is to give a quick feel for the relevant literature; the topic is covered in much more detail in Wagner's survey (this section).

The simplest point to be made will be familiar to everyone who has studied mathematics: it is not unusual for definitions, theorems, or proofs to be motivated by a visualization or a heuristic computation, only to be rigorously formulated or established in a different way. Such bifurcations between demonstration and heuristics—where the heuristics often rely on diagrams or other sign manipulations—go at least as far back as the work of the ancient Greek mathematician Archimedes (see, e.g., Detlefsen 2008, section 2, for a historical survey), and reflect the norms in force as to what counts as an acceptable proof. In the recent literature, Carter (2010) and Starikova (2012) nicely document examples of diagram-driven discovery in contemporary mathematical research (see also Giaquinto 2020, section 4 for a survey).

Perhaps most interesting, however, are cases in which the line between the demonstrative and the heuristic—between what is a legitimate sign manipulation, sanctioned by clear rules, and what is an unintended representational artifact—is not so clear-cut.

As a first example, consider the derivatives of a function f. In principle, it should not matter much whether they are written f', f'', etc. (Lagrange's notation) or $\frac{df}{dx}$, $\frac{d^2f}{dx^2}$, etc. (a descendant of Leibniz's notation); there may be 'pragmatic' differences, of a kind we shall return to in Section 4—the Lagrangian notation may be more economical, and the Leibnizian one better suited for remembering the chain rule, say—but the rules of the differential calculus themselves should remain the same whether they are couched in one notation or the other. From this point of view, the fact that differential operators $\frac{d}{dx}$, $\frac{d^2}{dx^2}$ appear separately from the function f in the Leibnizian case is merely an artifact of the notation. Heuristically, though, this apparent artifact matters: it invites officially illicit, but sometimes productive, algebraic manipulations of the differential operator itself; this seems to have given rise historically to the collection of methods called the 'operational calculus', in which differentials operators *are* used algebraically. While the manipulations in question do not follow any pre-established norms, they depend on particular features of the signs previously in use, by creatively exploiting analogies these signs open or opportunities they afford.

Note that caution is required here: the idea that a mere artifact of notation is what led to the operational calculus is a caricature. Rarely if ever does it happen that an incidental feature of a notation adopted for unrelated reasons is solely responsible for a major breakthrough, however tempting the idea may be. Nevertheless, this caricature gets something right: notations can matter in exploratory contexts. For a cautious attempt at seeing how much of a role notational changes did play in the earliest attempts, by Leibniz and Johann Bernoulli, at an algebraic treatment of differential operators, see Waszek (n.d.).

Importantly, there is already more at play in this example than a disjunction between discovery and justification; it is not merely a case where something was discovered at some point based on incidental features of previously used signs, but was then justified by a different route. Methods associated with the 'operational calculus', defined by their algebraic use of differential symbols, ended up being used in a *systematic fashion* over extended periods of time, often with no more than partial and piecemeal justifications available. (On the history of the operational calculus, see Koppelman 1971 and Lützen 1979; for an engaging and mathematically sophisticated survey of various instances in which sketchy algebraic manipulations turn out to be surprisingly and systematically helpful, see Cartier 2000. Feynman diagrams are a well-known analogous case, but with diagrams rather than symbolic formulas; see, e.g., Kaiser 2005.)

The operational calculus—especially methods developed in the late nineteenth century by the electrical engineer Oliver Heaviside[16]—is one of the favorite examples of Wilson (2006, in part. chap. 8; 2021), who emphasizes that scientific and mathematical progress often comes by way of exploratory sign manipulations that, at first, cannot be justified nor fully understood. His broader point, which is pragmatic in spirit, is that it often takes a lot of scientific developments before we are able to understand why and how our successful methods work, and that insisting on perfect rigor prematurely is counterproductive.

Crucially, though, Wilson sees the operational calculus merely as an extreme and obvious instance of a phenomenon that is pervasive throughout scientific practice. He argues through numerous examples that signs (and more broadly, concepts) tend to shift in meaning in subtle and often unrecognized ways depending on context—even in core scientific work that is not considered heuristic by practitioners—and that this allows for successful and efficient methods whose underpinnings we only *think* we understand.

Work by Grosholz (2007) and Wagner (2017) on the role of ambiguity in mathematics bolsters such claims. Wagner's study of generating functions (2017, chap. 4) provides us with a particularly clear example coming from contemporary mathematical practice. Generating functions are series of the form $\sum a_n x^n$ that are used to solve combinatorial problems. What Wagner shows is that the various techniques commonly used in this field require interpreting the symbol x in different ways: sometimes x has to be a real or complex variable, sometimes a mere placeholder defining a formal power series, sometimes a (transcendental) element of an extension of the

[16] On these, see Lützen (1979).

field to which the coefficients belong. While any particular technique—save perhaps for delicate cases at the frontier of research—can be justified satisfactorily, the interpretations and mappings between interpretations that would be required to do this are not usually spelled out, and often could not be except by sophisticated practitioners. In effect, under a veneer of rigor, the field thrives on ambiguity.

More radically, Wagner and collaborators emphasize that signs permit circulations, not just between different mathematical contexts, but also between mathematical and non-mathematical ones. (For ease of exposition, I take for granted that there is a clear boundary between the mathematical and the non-mathematical, but of course the point of such work is to call this into question.) Wagner (2010; 2017, chap. 2) focuses on Italian abbacist treatises of the sixteenth century; he argues that the way they conceptualized and handled (what we would now call) negative numbers, fractional numbers, and square roots, was closely related to *economic* practices of currency conversion. In other work (2009; 2017, chap. 4), he studies a specific combinatorial problem that, in the mathematical literature, is often phrased in terms of marriages; he argues that, through such choices of terminology and notations, gender stereotypes can end up shaping how the problem is framed and what kind of questions are asked about it.

In sum, and in contrast to the image of perfect rigor that mathematics often enjoys, the work we surveyed in this section argues that the ability of signs to function ambiguously, to be experimented upon beyond what is allowed in principle, to be reinterpreted, and to allow circulations from contexts apparently far removed from mathematics, plays a crucial role in the practice and historical development of the discipline.

4 Mathematics for finite agents: From psychology to understanding

Our last motivation for studying the signs actually used in mathematics is to explore how finite human beings, with their specific cognitive make-up and computational limitations, can practice mathematics and attain mathematical knowledge.[17] Indeed, while in principle, one could practice arithmetic using the symbols of set theory or do geometry without any diagrams, both are effectively impracticable: for humans, the particular signs used matter considerably.

It may seem that such differences between signs are merely ‘psychological’ or ‘pragmatic’, in the sense that they do not impact the *justification* or *content* of mathematics: they are irrelevant—one might think—to whether a theorem is true or to what kind of objects (if any) it is about. It is true that part of the literature rests content with fitting mathematical knowledge into a reasonable account of cognition and ignores ontological questions altogether. But as we shall see, the stakes are higher

[17] As such, it is an instance of what Carter (2019) has called the ‘agent-based’ strand of the philosophy of mathematical practice.

than this. Some recent work clearly suggests that human computational limitations, and the representational tools that allow us to make do with these limitations, deeply shape our mathematics, including in its justifications and its ontology.

Note that similar questions have long been discussed in relation to the philosophically central topic of *infinity*. Indeed, one way of formulating Hilbert's foundational program (at least in its mature formulations, from the second half of the 1920s) is as aiming to justify all talk of the actual infinite on the grounds that it ultimately provides an *efficient* (though in principle eliminable) way of talking about the finite (see Zach, 2023, section 3), thus seemingly grounding our mathematical ontology on concerns of pragmatic accessibility. Later on, questions about how different notations impact practical feasibility have been raised in the context of discussions of strict finitism (see W Dean, 2018). Early efforts at developing an explicitly semiotic account of mathematics were also largely motivated by the problem of infinity (Rotman 1993, 2000; on this, see also Van Bendegem 1996). Despite its obvious relevance, however, the question of infinity has not had a central role in recent work on mathematical signs; moreover, it would deserve an extended discussion. This topic is therefore best left for elsewhere. (For a starting point, see Easwaran, Hájek, Mancosu, Oppy 2021.)

4.1 Mathematical cognition

Let us start from a few examples of psychologically oriented research that explores how human cognitive abilities constrain, and take advantage of, the various signs used in mathematics. Landy, Goldstone, and collaborators have shown experimentally that perceptual cues, like spacing, impact accuracy and performance in symbol manipulation tasks—e.g., that $a \quad + \quad b * c \quad + \quad d$ is easier than $a + b \quad * \quad c + d$ to parse (properly) as $a + (b * c) + d$—and, more broadly, that fluency in parsing symbolic expressions relies on a kind of *perceptual training*.[18] Wege, Batchelor, Inglis, Mistry, Schlimm (2020) show that even the most superficial aspects of mathematical signs, like symmetry, can impact performance. Experiments by Hamami, Mumma, Amalric (2021) suggest that human reasoners typically assess the validity of elementary geometric claims through a diagram-based search for counterexamples. Finally, in a more phenomenological (rather than experimental) vein, De Toffoli, Giardino (2014) argue that the practice of topology often relies on the use of sophisticated visuospatial skills to imaginatively manipulate diagrams of three-dimensional objects.[19]

Such work reintegrates mathematics into cognition and, at its best, has the potential to reveal unexpected ways in which mathematics taps into the strengths of the human sensorimotor apparatus. Its core message is that the practice of mathematics involves the body—the eye and the hand—in many ways. As such, its target is less

[18] See Landy, Goldstone (2007); Landy, Allen, Zednik (2014); Marghetis, Landy, Goldstone (2016).

[19] See also De Toffoli, Giardino (2015a); De Toffoli, Giardino (2015b).

traditional philosophy of mathematics than, on the one hand, a certain cultural image of mathematics as abstract and disembodied, and, on the other hand, broad theories of cognition that conceive thinking as a computational process taking place 'inside the head'—showing, instead, how much mathematics relies on the body, with its specific sensory apparatus, as well as on external signs.[20] In this context, mathematics is discussed less for its own sake than as a strategic battleground in broader debates on the embodiment of cognition—a position mathematics owes to the fact that it is typically seen as *the* paradigmatic instance of abstract thought.

However, it is clear that differences between signs are not just a matter of psychology. Think of algebraic symbolism: details of the perceptual system may determine, in part, what kind of symbolic expressions (including spacing, the shapes of symbols, the spatial encoding of hierarchical structure, and so on) are easy for humans to process. Nevertheless, the fact that the calculus used in modular arithmetic, say, makes the solution of many divisibility problems much more accessible than run-of-the-mill algebra, which is symbolically quite similar, cannot have much to do with psychology at all.

Admittedly, the boundary between the psychological and the non-psychological is sometimes subtle. For instance, given three sets A, B, and C such that $B \supseteq A$ and $C \cap B = \emptyset$, noticing that $A \cap C = \emptyset$ may be easier on an Euler diagram (which represents the sets A, B, and C as circles, with circle A located inside circle B and circle C disjoint from the others) than on the symbolic relations just given. Presumably, this is partly due to our visual ease at perceiving containment and non-containment of shapes (though even this is tricky to make precise: it might be due to human ease at processing spatial relations rather than to anything specifically visual, as Euler diagrams can be helpful even if perceived through touch). But even there, our visual abilities can only come into play because of an *antecedent*, *non*-psychological fact, namely that Euler circles systematically make set inclusion and set disjointness accessible in the form of geometric containment and non-containment patterns, independently of the fact that we are good at visually picking up the latter—while conversely, a list of various relations such as $B \supseteq A$ and $C \cap B = \emptyset$, which could be phrased in a multitude of other ways, does not make inclusion and disjointness systematically accessible as single patterns.

4.2 The broader issue of computational limitations

Wherever the boundary lies between factors that do and factors that do not depend on the peculiarities of the human sensorimotor apparatus, any mathematics practiced by *computationally limited agents* will be sensitive to the particular signs used, because these impact what is easily accessible. A study of how sign-systems systematically reorganize accessibility—in theorem proving, problem solving as well as exploration—can (and should) also be conducted independently of psychology, al-

[20] For general discussions of mathematical signs (diagrams as well as symbolic notations) in this spirit, see for example Giardino (2016); Vold, Schlimm (2020).

though both psychological and non-psychological investigations will have to come together for a full account of mathematics as practiced by humans.

In this spirit, there are a number of recent case studies describing the advantages of particular mathematical signs in particular contexts, both historical and contemporary. To take just a few examples, De Toffoli (2017, 2023) discusses the use of commutative diagrams in homological algebra, and of arrow diagrams for representing closed connected surfaces; Feferman (2012) explores the use of some diagrams that contain ellipses (i.e., '. . . ') to indicate that they could be continued indefinitely, including a commutative diagram used in homological algebra and a diagram often used to prove the Cantor-Bernstein theorem in set theory; Carter (2010) discusses the role an ad-hoc diagram played in the discovery of particular concepts and later of a particular proof; and, in a more historical vein, Manders (1999) (an unpublished, but widely shared manuscript) compares traditional geometry in Euclid's style, based on diagrams and verbal arguments, with Descartes' addition of algebraic equations.

So far in this survey, logic has mostly appeared as a foil—a provider of uniform symbolic languages that risk erasing the diversity of mathematical signs. However, logical notations too are designed for specific purposes (e.g., the perspicuous decomposition of sentences into constituents or of deductions into elementary inferences; the effective checking of the correctness of a deduction; the effective discovery of proofs; etc.). They are subject to psychological and computational constraints and can be analyzed in such terms. For example, Schlimm (2018) offers such an analysis of Frege's *Begriffsschrift* notation (or concept-script); Waszek, Schlimm (2021) revisit, from the standpoint of their goals and (relatedly) of their respective pragmatic advantages, the contrast between the notations of Boole and of Frege. There has also been work in this spirit on Peirce's many notations for logic, including his diagrammatic 'existential graphs' (e.g., Schlimm, Waszek 2020; Bellucci, Pietarinen 2016).

The points made in the case studies just mentioned, be it in mathematics or in logic, usually boil down to the following: using certain signs rather than others can make it more *efficient* for agents to perform certain tasks. The tasks in question are most often related to the discovery (or reconstruction) of proofs (or of solutions to problems). For instance, to borrow an example discussed by Feferman (2012) and De Toffoli (2017, 2023), using commutative diagrams rather than long lists of (symbolically formulated) commutation relations can make it considerably easier to reconstruct the proofs of many lemmas in homological algebra.

However, it is frequent to find such advantages phrased in a different idiom, namely in terms of gains in 'understanding'. In broad terms, one can make sense of such terminology if one explicates understanding in terms of the possession of *abilities* to perform certain tasks (to construct proofs, etc.); such an account is often endorsed at least implicitly in discussions of signs or representations (see, e.g., Feferman 2012), and is most explicitly articulated in Avigad (2008). To tighten the connection between understanding and the efficiency advantage of the signs used, however, one needs to clarify *which* tasks one has in mind and what these are good for.

Upon closer inspection, the precise tasks that certain signs make easier can be quite specific and varied. For example, they can make it easier to formulate a high-level sketch of the proof; to locate a particular step in such a sketch; to identify which steps one could possibly take next in a proof, and guess which is likely to help; etc. Intuitively, all of these are related to 'understanding what one is doing' in a more precise sense than the mere ability to prove theorems efficiently. For attempts at identifying the most relevant tasks in particular examples, and at elaborating a suitable theoretical vocabulary, see Manders (1999, 2012); Keränen (2005).

Trying to generalize, as just discussed, about the *kinds* of efficiency advantages that are most relevant to mathematical practice is one way of making case studies like the above more systematic. Another avenue of progress for going beyond the descriptive character of case studies lies in the following question: What exactly does it mean to say that, for certain purposes (solving a certain kind of problem, say, or proving a theorem), using certain signs offers advantages in efficiency? To give a clear answer to this question, one needs a *systematic way of comparing* different approaches, relying on different signs, to one and the same task. Though I cannot enter into details here, two broad strategies for making such comparisons more precise have emerged in the literature.

The first strategy to compare different signs, following Larkin, Simon (1987), is based on a computer model: problem solving is modelled by computer programs, and the signs or representations used in the problem-solving process are modelled by the data-storage schemes used by these programs—in computer science terminology, by their 'data structures'. The idea is that different ways of storing the same data may lend themselves to different kinds of processing; in Larkin and Simon's terms, they may be 'informationally equivalent' but 'computationally different'. Two ways of solving the same mathematical problem using different signs would then be modelled by two programs using different data structures, and their efficiency would be compared by counting the number of elementary operations they require before reaching a solution. (For a general discussion of this strategy, see Waszek 2023; for examples of recent attempts to apply this idea to mathematical signs, see Schlimm, Neth 2008 and Carter 2018.)

The second strategy boils down to treating different signs as the *syntax* of different *logical systems*, in order to quantify their efficiency for particular purposes by counting proof steps. In the case of diagrams, such efforts rely on diagrammatic formal systems as discussed in Section 2 (see Stapleton, Jamnik, Shimojima 2017; Stapleton, Shimojima, Jamnik 2018 for examples).

Both strategies rely on quantifying the efficiency of using certain signs by *counting*—elementary operations in one case, proof steps in the other—and both face a similar challenge, in that getting results for *particular* problems may seem of limited interest (and certainly has not much interested logicians), while getting *general* results, about the comparative efficiency of certain signs in a broad range of cases, is tricky. For instance, an obvious candidate for producing such general results would be computational complexity theory, but this approach requires identifying a class of similar problems of *arbitrarily large* size and only yields asymptotic results, which is of dubious relevance to practical concerns (see W Dean 2019). Identifying classes of problems that are large enough to yield general results while still being relevant in practice is difficult.

4.3 The philosophical stakes

Why should we care *philosophically*? Aren't the kinds of differences between signs discussed above, if not merely psychological, then merely 'pragmatic'—in the sense that they are a matter of convenience or efficiency rather than substance? I believe that the very formulation of this question is wrong: for limited beings like us, considerations of efficiency end up shaping the substance. In particular, the signs we use for (at least partly) pragmatic reasons end up shaping what our mathematical objects and theorems are. Though research on this topic remains underdeveloped, two suggestive examples, one on notations and one on diagram use, will make this point clearer.

Before turning to examples, though, it may be helpful to notice that the parallel claim for *concepts* or *definitions* is easier to accept. Think of the central concepts in abstract algebra, say those of group, ring, or field. A main rationale for adopting them is surely efficiency—so much so, in fact, that the Bourbaki group famously referred to their axiomatic method, centered around isolating and studying such concepts, as the ' "Taylor system" for mathematics':[21] for instance, instead of studying related phenomena separately for numbers, permutations, geometric transformations, and so on, the theory of abstract groups allows focusing on them only once and then applying them to many cases (for a deeper analysis of this and related advantages, see Avigad 2020). But evidently, these concepts come with a change of subject matter: abstract algebra, even if applicable to number theory or geometry, is profoundly different from them—it is ostensibly about different objects, and its theorems are not reducible to their instances in other fields. Accordingly, the philosophical importance of conceptual or definitional choice is perhaps more evident than that of notational choices;[22] the Bourbaki group, one of whose main members (Weil) we have seen downplaying the role of notations, certainly staked its entire project on the importance of organizing mathematics around the right (structural) concepts. Nevertheless, the examples below suggest that choices of signs and choices of concepts or definitions lie on a continuum; both are perhaps best seen as species of the same genus. In practice, conceptual shifts often go hand in hand with changes in (non-verbal) signs, and both can end up reorganizing the subject matter. (For a further articulation of this parallel, see Waszek n.d..)

As a rough metaphor, think of people engaged in mathematical problem solving, theorem proving, or mere exploration, as travelers making their way through rough terrain. The signs or representations they use—and more broadly, their concepts and general organization of knowledge—trace paths and roads across the land-

[21] See Bourbaki (1950, 227) = Bourbaki (1948, 42).

[22] For discussion on this topic, see in particular Tappenden (2008a,b); Lange (2017, chap. 9).

scape, which privilege certain itineraries and make certain places *more accessible* to them than others. Nothing stops them from traveling cross-country once in a while (though some ravines may prove impassable without exceptional skill); it's just that taking the highway is more straightforward and efficient. But this 'pragmatic' efficiency advantage ends up structuring most travelers' understanding of the land: which features of it are most salient, which places everyone visits and which remain largely unknown. Of roughly this sort, I believe, is the impact of mathematical signs.

To make this more concrete, my first example comes from Avigad, Morris (2016), who discuss a famous theorem of number theory, first proved by Dirichlet: any arithmetic progression whose terms do not all share a common factor contains an infinity of primes.[23] The proofs found in modern textbooks rely on the notion of *character* of a group; they involve summing over characters, the relevant sums being evaluated term by term based on the properties of the indexing characters. Dirichlet's original proof, however, does not refer to characters at all: his sums are indexed by intricate data and, while they contain what we can retrospectively recognize as the explicit algebraic expressions of the relevant characters, these are not the explicit focus of attention. In fits and starts, presentations of the proof progressively extricated what we now call characters, introducing specific *notations* for them and isolating their properties, thus making the proof much more surveyable.[24]

Dirichlet's original proof is just as valid as its successive reformulations, up to modern, character-based versions; in fact, there is an intuitive sense in which all of them are essentially the same. From this perspective, as Avigad and Morris note, it is tempting to dismiss the differences as 'merely pragmatic'. Yet in the historical arc they survey, these 'pragmatic' differences had, as end result, a clear conceptual and even *ontological* change: the concept of character emerged, and characters became accepted as objects in their own right.[25] Notice the continuity, in this story, between apparently simple *notational* abbreviations for characters and full-fledged conceptual developments.

My second example comes from Manders (1996), who uses the history of geometry to argue against the idea that 'individuation of content, and hence *content*, is representation-neutral': in other words, he argues that the very content of our mathematical statements ultimately depends upon the representations (or signs) we use. His strategy is to contrast traditional geometry in the style of Euclid with *projective* geometry—more precisely, the form of it practised by the nineteenth-century mathematician Poncelet.

[23] For more historical detail, see also their companion paper, Avigad, Morris (2014).

[24] Among other things, introducing characters allows one to hide the complexity of the underlying explicit formulas, making the indexation of the sum easier to grasp, and making the proof more focused by spinning off the general properties of characters to separate lemmas.

[25] I should emphasize that the story is more complex than this sketch: treating (what we now call) characters as full-fledged objects, rather than just shorthands for parts of complex calculations, relies on a shift—associated with broader evolutions in nineteenth-century mathematics—from seeing them as algebraic expressions to seeing them as functions defined by their input–output behavior. Avigad, Morris (2016) deserves a full reading.

Euclid, Manders explains, proved two theorems that amount to the following: 'when two lines through a point O meet a circle, say, one in A and B and the other in C and D, the rectangles (products) OA by OB and OC by OD are equal.'[26] But Euclid did not state, and in fact did not have the means to handle, the result in this general form. Instead, he proved it separately for O inside and for O outside the circle.[27] This is because Euclid-style proofs always come with, and rely on, a diagram, and Eulidean diagrams *have to* settle such questions as whether a point is inside or outside a circle. Poncelet's projective geometry, on the other hand, does have means to state and prove all of these cases uniformly. So is there a single theorem here, or two? In Euclid-style geometry, the best we can say is that there are two somewhat analogous theorems with different proofs, one about 'point inside circle' and one about 'point outside circle'. If instead we follow Poncelet, we can state and prove a single theorem about the undifferentiated content 'point and circle', which Euclid cannot handle.

Before further analysis, two remarks will help forestall misunderstandings. First, there obviously *are* cases in which Poncelet's geometry goes beyond Euclid's. Manders's choice to focus on a case in which Poncelet and Euclid's theorems are intertranslatable is strategic: it is intended to bracket other phenomena so as to highlight the reorganization effected by the representational differences between them. Second, while Manders chose an example in which the focus is on the *diagrammatic* means of representation, similar points have also been made about the *conceptual* (or *definitional*) differences between Euclidean and projective geometry, highlighting again the continuity between representational and definitional differences. For instance, many of the unifications permitted by projective geometry turn upon accepting, on top of normal points, *points at infinity* at which parallel lines intersect—in other words, they turn upon redefining the concept of point. (The value of this redefinition is that any two lines, including parallel ones, will then intersect somewhere, which allows many theorems mentioning intersecting lines to also cover the case where the lines are parallel.) In practice, the concepts and the signs (or means of representation) are intertwined: the change in the concept of point is tied to a new way of seeing geometrical diagrams as invariant under projective transformations, some of which can send a run-of-the-mill point to infinity.

Returning to Manders's point, why does it matter if there is a single theorem (as in Poncelet) rather than two (as in Euclid)? Is it not the same knowledge, just packaged differently? Above all, the reorganization Manders analyzes matters because it alters what is *salient* for mathematicians in contexts of research and problem solving. If using diagrams like Poncelet, one will, for instance, be attuned to any

[26] Manders (1996, 396). In modern terms, these theorems are about what is sometimes called the 'power of a point to a circle'.

[27] The case with O inside is proposition 35 of book III of Euclid's *Elements*. The case with O outside is proposition 36, but with a further complication: the theorem still holds if one or both of the lines is *tangent* to the circle (e.g., if the first line is tangent, it will have a single intersection point A with the circle, and the theorem holds with the square on OA instead of the rectangle OA by OB), which means that there should be three cases to consider, depending on whether two, one or none of the lines are tangents. Euclid only proves the case with one tangency, presumably because the others immediately follow.

situation involving a point and a circle, irrespective of their relative positions, and may expect further theorems to also hold generally for points and circles. In other words, when faced with a problem, the signs one is in the habit of using—those in which one's theorems are phrased—determine what one pays attention to, what expectations one has, what potential moves one will think of first.

The metaphor of the traveler introduced above should be clearer by now: much like concepts, the signs used trace privileged paths across the mathematical landscape, in a way that impact what kind of theorems, and even what kind of objects, are seen as structuring the land.

As a last point, note that these considerations have interesting connections to a different literature—one that did not originate in philosophy of mathematics, but in the *historiography* of mathematics. The question at issue is whether historians can legitimately use 'anachronistic' mathematical notations and concepts to make sense of historical mathematics. Our discussion above—about the wide-ranging consequences that 'pragmatic' differences between signs can have—should make the potential dangers of such a procedure clear: summarily translating an ancient source into modern algebraic symbolism is like trying to retrace the winding itinerary of an ancient traveler by following the contemporary highway system; while convenient and doubtlessly useful for orientation, it is often inadequate in the details.

Conclusion

In recent years, a large literature has developed around 'representations' or (as I have put it here) 'signs' in mathematics, the aims of which—beyond that of offering precise descriptions of aspects of mathematical practice—are diverse and sometimes hard to discern. This survey attempted to clarify what is at stake.

Part of the literature discussed above is not much concerned with philosophy of mathematics in the traditional sense. Some authors investigate mathematical cognition; others mainly aim at altering our perception of mathematics, say by shifting the emphasis away from certainty, rigor or abstraction and towards open-endedness, creativity or embodiment.

For the rest, we encountered two main ways in which work on signs bears on more strictly philosophical questions. First, work on the norms governing nonverbal signs in various mathematical practices has the potential to revive views of the nature of mathematics that center on sign manipulations rather than on logical inference, perhaps along Peircean lines (Section 2.3). It is fair to say that such views remain marginal today, and are insufficiently discussed head-on. However, they will likely keep proving attractive to researchers wishing to accommodate the variety of mathematical practices without giving up on the specificity of mathematics. From an ontological perspective, discussions of how reference to abstract objects can emerge from sign manipulations—like those of Parsons for symbols and of Panza for Euclidean geometricals—will be relevant here (see Section 2.1).

Second, a focus on signs (including concepts, though my emphasis here has been on non-verbal signs like symbols and diagrams, where the issues can often be formulated more sharply) allows approaching the way human computational limitations shape the mathematics that we do, and in particular determine what our salient mathematical objects and theorems are (Section 4, esp. 4.3). Moreover, studying signs is a promising avenue for tackling in a precise way the topic of mathematical understanding, to which—under various names and guises—a large portion of the philosophy of mathematical practice literature is devoted.

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