On the epistemic gain of dispensable applications of mathematics

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Abstract

If (or when) mathematics is *dispensable* for solving empirical problems, why can the detour through mathematics be so useful? Typical discussions of applicability only approach this problem indirectly, saying just enough about it to argue that whatever gain mathematics offers (e.g., a gain in inferential power) can be enjoyed without ontological costs. In this paper, I first clarify this problem and distinguish it from other, related ones; in particular, I make it clear that the problem has nothing specifically to do with the application of mathematics to empirical sciences, and arises in the same way for many so-called applications of mathematics to mathematics (which I argue are best defined as deployments of new mathematical resources to make progress on a mathematical problem). I then survey possible solutions, including a particularly promising one, inspired by Ken Manders and in line with recent work on reformulations: mathematics offers tools for systematic expressive restriction, allowing for attentional management in problem-solving.

Keywords: Applications of mathematics,representations,notations,scientific discovery

Introduction

It is an old idea that the fundamental role of mathematics in empirical science is "inferential." In a well-known metaphor, Hempel (1945, 554) wrote that mathematics has "the function of a theoretical juice extractor: the techniques of mathematical and logical theory can produce no more juice of factual information than is contained in the assumptions to which they are applied; but they may produce a great deal more juice of this kind than might have been anticipated [. . .]." Field (1980) famously defended a similar view. More recently, Bueno and Colyvan (2011, 352) argue that the main point of using mathematics in science is to make it possible "to obtain inferences that would otherwise be extremely hard (if not impossible) to obtain."

Such views aim at reducing mathematics to an auxiliary role, devoid of *ontological* import. Comparatively little is said about a problem they immediately suggest, but generally do not address (see also Ardourel et al. 2018): What is it about mathematics that allows it to afford such inferential advantages? Why do these advantages arise? I shall call this the problem of the *epistemic gain* of applying mathematics. It is my target in the present paper. More precisely, my eventual focus will be on the particularly pressing case in which the piece of mathematics being applied is *dispensable*, at least in principle, yet seems to offer substantial benefits in scientific problem solving by making solutions easier to *discover*.

The dearth of discussions of the problem of epistemic gain goes hand in hand with a tendency to treat mathematics as a black box. When the focus is on the delicate interplay between mathematics and the empirical world, there is little need to "look inside" the strictly mathematical parts of scientific reasoning—it is enough to invoke the inferential gains mathematics affords (while perhaps arguing that they can be enjoyed at no ontological cost). A main claim of the present paper, however, is that much of the inferential advantages afforded by mathematics have little to do specifically with the relationship between the mathematical and the empirical. The problem of epistemic gain arises just as much for what might be called applications of mathematics *to mathematics itself*,[1] and I shall argue that situating the problem in this broader context is of crucial help for seeing it clearly and addressing it.

My first task is thus to clarify the problem of (dispensable) epistemic gain, and to show, along the way, that it cuts across the boundary between applications of mathematics to empirical science and uses of new mathematical resources *within* mathematics. This requires some groundwork.

To start with, while the idea that there are applications of mathematics *to mathematics* is commonly encountered in the literature, what exactly should count as such is never spelled out. Section 1 examines what one might take intra-mathematical applications to be. This will force us to reconsider the idea of applications of mathematics *to empirical science*. If one's philosophical target is the presumed ontological gap between mathematics and the world, then the phenomena of interest are *narrower* than applications as usually construed: some ostensible applications of mathematics will be of little concern and should be left aside. For instance, the use of new mathematical means within an already thoroughly mathematized theory (e.g., the use of the Hamiltonian formalism in classical mechanics) plausibly does not raise ontological worries over and above those already raised by the pre-existing theory. On the other hand, if—as in the present paper—one's goal is to understand why using some mathematical resources can afford epistemic gains, then the range of relevant phenomena will be *broader* than *prima facie* applications, and will include changes of representation. The difficulty of defining applications of mathematics to mathematics clearly brings out the point.

In this spirit, Section 2 reexamines the philosophical problems that can be raised about applications of mathematics, so as to situate the problem of epistemic gain with respect to the existing literature. Section 3 then sharpens the specific problem of *dispensable* epistemic gain, distinguishing it from problems raised by indispensable uses of mathematics.

The rest of the paper tackles the problem of epistemic gain as it arises for dispensable applications of mathematics, engaging with and complementing recent work on reformulations by Hunt (2021b, 2025). First, I examine *what* the gains of interest may consist in,

[1] A similar move is made by Ken Manders in a well circulated, but unfortunately still unpublished draft, "Why Apply Math?" (1999), later revised as "Applying Mathematical Concepts" (2001). On these, see also Hacking (2014, 9–11).

exactly; the best way to explicate the "inferential" gains that classic discussions ascribe to applications of mathematics, I argue, is in terms of gains in how easy it is to solve problems (Section 4). Second, I turn to the question of *why* applications can give rise to such gains. Perhaps applications of mathematics can make it easier to find solutions to problems simply because, by unifying previously disparate phenomena, they allow transferring pre-existing knowledge between domains. Section 5 examines this idea and argues that it can, at best, only offer a partial solution. Section 6 then sketches another answer, inspired by the work of Ken Manders: an application of mathematics, in the broad sense intended here, can make it easier to solve a range of problems by offering tools for systematic expressive restriction, and hence attention management.

1 Applications of mathematics to mathematics

Applications of mathematics to empirical science have apparent analogs *within mathematics itself*: such examples as the use of complex analysis in number theory, the use of algebra to solve geometrical problems, or the use of groups in the theory of equations are regularly dubbed "applications of mathematics to mathematics."[2] There is a disanalogy, however. The very idea of applications of mathematics to science seems to presuppose a clear dividing line between the two sides—one that is often understood as ontological. By contrast, it is not clear how to draw a similar dividing line between different *kinds* of mathematics, so that the idea of applications of mathematics to mathematics remains uncomfortably vague. The goal of this section is to clarify how it could be rigorously defined, laying the groundwork for the rest of this paper.

Before turning to applications of mathematics within mathematics, we need to be clear about what we mean by applications of mathematics to empirical science. Mathematics is pervasive in contemporary science, and—depending on how exactly one defines and demarcates the mathematical—one might argue that at least some of it plays a *constitutive* role in our knowledge of the world. Standard discussions of the "applicability" of mathematics, however, only get off the ground for uses of mathematics that are *not* constitutive in this sense. Their focus is on *individual uses* of mathematics in empirical science that can be reconstructed in such a way as to *separate* the mathematics being applied from an empirical subject-matter that could be studied independently of it. (The question of whether *all* occurrences of mathematics in science can be so reconstructed is best left aside here; the discussion below, like most of the recent literature on applicability, only applies to those that can.) In this spirit, I shall assume, minimally, that an "application" of mathematics to science is a use of mathematics that can be reconstructed as involving three successive steps: one in which empirical data are "translated" into mathematics; one in which reasoning is conducted using mathematics; and one in which the conclusions reached mathematically are "translated back" or "interpreted" into empirical conclusions (Fig. 1). This three-step schema is inspired by Bueno and Colyvan (2011) (and by Hughes's 1997 model of scientific representation), but does not assume any of Bueno and Colyvan's more specific views, for instance that the horizontal arrows are partial structure-preserving mappings.

[2]E.g., Steiner (2005, 636); Hacking (2014, 9); Bueno and French (2018, 12).

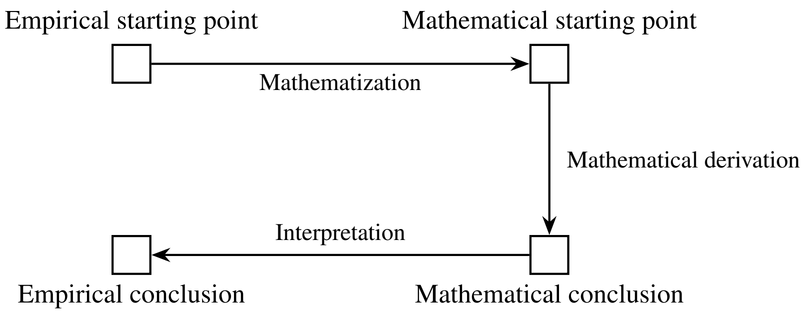

Fig. 1 The three-step process required in the present paper for some use of mathematics to count as an application

Crucially, in order to have an application in the preceding sense, it is *not* required that the empirical starting points and conclusions can be formulated in a purely nominalistic language—one that would be free from any mathematics whatsoever. Rather, what matters is merely that they can be stated without using the specific mathematics being applied. For instance, consider Hartry Field's discussion of the use of real numbers in physics (Field, 1980). Roughly speaking, Field starts from a theory of physical space-time that makes no reference to real numbers; he then shows that the "space-time points" making up any model of this theory can be equipped with a real-valued distance function, which, he argues, allows for more efficient derivations of real-number-free conclusions about space-time. One may worry that "space-time points" are themselves nominalistically suspicious, or even already mathematical. For our purposes, however, this question is irrelevant: as long as one can theorize about Field's physical space-time without any reference to real numbers, the use of real numbers will count as an application. Likewise, we need not assume that the reasoning conducted on the right-hand side of Fig. 1 is *purely* mathematical, i.e., never refers back to features of the empirical set-up, as long as it uses some mathematics not available on the left-hand side.

The mere fact that the uses of mathematics typically discussed as "applications" can be recast in the three-step scheme just presented is meant to be uncontroversial. Where views on applicability diverge is on further questions, in particular on *what more* needs to be said and on the nature of the horizontal arrows, i.e., the "translations" to and from mathematics.

Now consider the intra-mathematical case. To understand the appeal of talking, by analogy, of "applications" of mathematics to mathematics, start with a canonical example: the use of algebra in geometrical problem-solving. For distinctness and because of the advantages of hindsight, let us focus specifically on the way algebra was used in the early 17th century, following the work of Vieta, by such authors as Albert Girard and René Descartes. Schematically, they proceeded as follows. The starting point was some geometric problem: given a geometric configuration, represented diagrammatically, one was to "find" or construct an object satisfying certain conditions. The algebraic strategy consisted in deriving an equation that the object sought would, if given, have to satisfy. One then solved the equation algebraically. Finally, one went back to the original problem, locating the solution on the original diagram and perhaps indicating, based on its algebraic form, how it could be constructed using traditional tools (straightedge and compass, perhaps complemented with more complex tools such as conic sections). This three-step process lends itself to a diagrammatic representation (Fig. 2) that closely parallels the one for applications to science (Fig. 1).

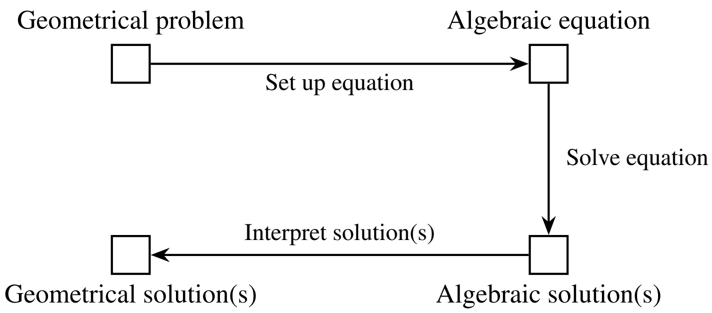

Fig. 2 Early uses of algebra in geometry

The analogy with scientific applications goes further than is already apparent. Despite the image of mathematics as uniquely rigorous, the geometric interpretation of algebraic results faced the same kind of messy difficulties as the empirical interpretation of mathematical models often does. For instance, the algebraic equation sometimes admitted several solutions, despite the fact that the initial geometric problem admitted only one. Which one should be selected as the right one and why? Sometimes the algebra yielded imaginary solutions—could one conclude that the problem was impossible? Sometimes it yielded negative solutions (even though, when setting up the equation, segments were treated as pure lengths, with no idea of direction involved)—how should they be interpreted? And so on.

The preceding example is typical in the following way: when intra-mathematical "applications" are brought up in the literature, they usually involve the use of pieces of mathematics that seem, in some sense, "distant" or "foreign" to the target area—in other words, they involve what is sometimes called "impure" proofs (e.g., algebraic proofs of geometric theorems; see Arana 2024). This, I believe, is the central intuition driving the parallel between the two cases: just as mathematics is taken to be about a different subject-matter than empirical science, so one piece of mathematics can be foreign to another area of mathematics. This foreignness is both what makes application-talk meaningful, and what makes applications philosophically puzzling. Without such an assumption, one would quickly end up calling all mathematical proofs "applications"; after all, they all involve mathematics!

Making the "foreignness" of one piece of mathematics to another into the definition of intra-mathematical applications is well and good, but how should this foreignness be explicated? Discussions in the literature suggest two lines of thought: perhaps a piece of mathematics is foreign to another if they belong to different "branches" or "areas" of mathematics; or perhaps if the first was developed independently of the second. While useful for some purposes, both of these have serious shortcomings; after examining them, I shall introduce a third one, which relies on Detlefsen and Arana's notion of "topical purity."

The first notion of foreignness could—borrowing terminology from Arana (2024)—be called "geographical": it relies on the idea, familiar from curricular organization or from the sociological structure of mathematical research, that mathematics is divided into well-defined branches. However, the divisions of mathematics that actually exist are neither consensual nor stable, and risk leading to a concept of internal applications that is heavily contextual.

Consider again the example of 17th-century uses of algebra in geometry. It may seem obvious to a contemporary reader that such uses involve the application of one branch of mathematics to a different one, but this can only feel obvious from a retrospective vantage point. At the turn of the 17th century, mathematics was seen as fundamentally divided into arithmetic and geometry. Algebra was not a branch in its own right, one that would have its own peculiar objects, distinct from numbers or geometric figures; instead, it mostly existed as a set of problem-solving techniques—some of which could, and increasingly were, used for *both* arithmetical and geometrical problems—and its foundational status was contested. Because of its link with practical computation on numbers, it was certainly possible to see algebra as growing out of, and essentially belonging to, arithmetic, in which case its use in geometry did create concerns of purity (a view later held by Newton; Arana 2024, 13). But it was equally possible to hold that an algebraic piece of reasoning belonged to arithmetic when used to solve a problem on numbers, and to geometry when used to solve one about figures; or that algebra pointed the way to a universal kind of mathematics that was more general than either traditional branch. From a historian's perspective, one possible reading is to see the rise of algebra in geometry as the development of systematic means to express, and reason about, quantitative relations between geometric objects considered independently of their positions—in continuity with practices that emerged in Arabic mathematics, before the kind of reasoning on letters later associated with them (Panza, 2007, 2008, 2010).

In other words, the early 17th-century use of algebra in geometry might, but *need not* be seen as the importing of tools from a different branch of mathematics; one can even describe it as the mere development of a new language to handle some geometric relations. Thus, defining applications of mathematics to mathematics in a geographical sense—by reference to the division of mathematics into branches—makes the status as an "application" of even such a canonical example as the use of algebra in geometry dependent on substantial further commitments. If one holds, *independently* of such commitments, that the algebraic methods are obviously not how Euclid was reasoning and that they offer clear gains in some contexts, and if the question one wants to raise by engaging in application-talk is *why* these gains obtain, then a geographical notion of application is not fit for purpose. We shall return to this point in the next section.

On the second reading, which we can call *genetic*, a piece of mathematics is "foreign," and hence can be "applied," to another if it was not developed with the latter in mind—if its usefulness in the context of application appears, in some sense, incidental. (It is certainly plausible that a number of algebraic techniques emerged out of arithmetic, and were only later used in geometry, though historical claims like these quickly become hard to adjudicate.) This line of thought is familiar from the literature on applications of mathematics to empirical science: it echoes Wigner's "unreasonable effectiveness" puzzle as articulated by Steiner (1998). The Wigner-Steiner puzzle can be raised for any piece of mathematics that was developed for reasons internal to pure mathematics, but was later fruitfully applied in empirical science (e.g., differential geometry or group theory). Within mathematics itself, applications *in the genetic sense* give rise to parallel puzzles (see Lange 2026; Bangu 2012, ch. 7). For other kinds of questions, however, the genetic concept of application is not so helpful. In the empirical case, Steiner distinguishes "canonical" applications from "non-canonical" ones. The latter are those for which the Wigner-Steiner puzzle arises—i.e., applications in the genetic sense—but he still calls the former "applications." There are mathematical analogs to Steiner's canonical applications, and unless we wish to deny them the name of "applications," they require an approach different from the genetic one.

It is a third approach that will prove most helpful in what follows. This third view is more liberal, in that it allows mathematical resources as simple as a single new notation, or a single new concept, to be called "foreign" (and hence "applicable") to some pre-existing piece of mathematics. The criterion parallels our definition above of an application of mathematics to empirical science. Intuitively, one piece of mathematics will be foreign to a second one in this sense if one can understand the second and reason about it without knowing or endorsing anything about the first. For instance, algebra can be called foreign, in this sense, to Euclidean geometrical problems—wholly independently of one's views on the foundational status or historical origins of algebra—because one can understand these problems and reason about them without knowing or accepting any algebra.

Such a notion of foreignness can be made precise by relying on Detlefsen and Arana's concept of "topical" purity (Detlefsen and Arana, 2011; Arana, 2024). The *topic* of a problem, according to them, is "the family of commitments that together determine what its content is for a given investigator," i.e., "the definitions, axioms, and inferences such that if the investigator were to stop accepting any one of them, then the content of the problem would not be what it is for her" (Arana, 2024, 30). Note that the "investigator" mentioned here is a placeholder that could stand for a research community just as well as for an individual; the point is that the "topic" needs to be defined by reference to the way the *meaning* of the problem is determined within the context of investigation.

Adapting this definition to our purposes, let us say that one piece of mathematics is *foreign*, in the *topical* sense, to some other piece of mathematics if the first is not part of the topic of the second. Whenever one solves a mathematical problem by using some mathematics foreign to the problem, one performs an "application" in the topical sense. Notice that any such solution to a mathematical problem can be represented using a three-step diagram similar to Figs. 1–2, where the right-hand side involves mathematical means that are not needed to understand the starting point and conclusion on the left.

The topical concept of application may seem so wide as to stretch the word "application" beyond plausibility. For our purposes, what justifies so broad a concept of application is the parallel with applications of mathematics to science: as the rest of this paper should make clear, the topical concept of application is the most natural context in which to raise some philosophical problems, in particular that of (dispensable) epistemic gain. Ultimately, however, the fact that our discussion leads us to a concept of intra-mathematical applications that seems impossibly broad reflects an underlying difficulty with the concept of application *also as it is used for scientific cases*. At issue is a matter of disciplinary boundaries and disciplinary culture. When discussing physics or other empirical sciences, it is tempting to call *any* use of new mathematical resources an application of mathematics and to treat it as a black box. But this will include cases that are in clear continuity with the more local instances of intra-mathematical applications in the topical sense.

Consider the use of "mature mathematical formalisms" as analyzed by Gelfert, which he defines as "a system of rules of conventions that deploys (and often adds to) the symbolic language of mathematics" (Gelfert 2011, 272; see also Ardourel et al. 2018). Take, for instance, the Hamiltonian formalism as used in classical mechanics. If we focus solely on the mathematical structure of the theory, leaving aside the work, essential in practice, of building models for particular phenomena and connecting them to experience (as in, e.g., Arnold

1989), then the use of the Hamiltonian formalism will appear as an application of mathematics to mathematics—but only in the *topical* sense. Indeed, as Gelfert emphasizes, mature mathematical formalisms "will typically be more local than any branch of mathematics such as topology, number theory, algebra, etc." (280): there is no application in the geographical sense here (nor, for that matter, in the genetic sense, as the history of Hamiltonian mechanics makes clear).

The definition of applications we should adopt will ultimately depend on our philosophical concerns. Accordingly, let us turn to the philosophical questions raised by applications, with an eye to the intra-mathematical case.

2 Puzzles of applicability

There is not one problem of the applicability of mathematics, but several (Steiner, 2005). Some concern applications in general, while others, such as the Wigner-Steiner puzzle, only arise for specific applications of mathematics. Some arise in the same way for applications to empirical science and for applications to mathematics itself, while others are more pressing in one case than in the other. My goal here is to bring out the problem of *epistemic gain*—a general problem of applicability, rarely addressed directly in the literature, that arises for both intra-mathematical and extra-mathematical applications. In order to situate this problem with respect to the existing literature, this section rehearses current debates about applicability through the lens of applications of mathematics to mathematics.

At the beginning of their recent book, Bueno and French raise the general puzzle of applicability as follows: "How can something apparently so different from science be so useful in scientific practice?" (2018, 1) Upon closer inspection, this type of phrasing condenses multiple questions into one. For our purposes, it is helpful to distinguish three. Why is it at all possible to bring mathematics to bear on empirical science, despite the apparent gap between them in ontological status or subject matter? Why does it work (when it does)? And why is it advantageous to do so (when it is)?

The problems that have loomed largest in the literature are the first two—call them the *heterogeneity* problem and the *success* problem:

(H) Given the heterogeneity of mathematics and the empirical world, why is it *at all possible* to systematically[3] bring mathematics to bear on empirical questions?

(S) Why is it possible to use mathematics to systematically obtain *empirically successful*[4] conclusions about the world?

To understand how existing discussions relate to intra-mathematical cases, it is important to realize that problems (H) and (S) have increasingly come apart in the literature. According to a large family of views—now often grouped under the heading of "mapping

[3]Strictly speaking, given any individual empirical statement (or finite set of such), one could artificially derive it via an "application of mathematics" in the sense of section 1 by stipulating arbitrary translations to and from some random piece of mathematics. A puzzle only really arises for cases in which there is a systematic, open-ended way of deriving empirical conclusions via the mathematics.

[4]In practice, what it means for a conclusion obtained mathematically to be "true" empirically is not straightforward: it could be approximately true, true only at a certain scale, and so on; the recent literature on scientific modeling has emphasized that models (including mathematical ones) are better assessed with respect to their fitness for purpose than with respect to some ideal benchmark of true representation of the world (e.g., Winsberg and Harvard 2024, section 2). Whatever our criterion of empirical success, however, the problem of why a given application of mathematics succeeds in the relevant sense will arise.

accounts"—applications of mathematics rely on some *identity of structure* between the empirical domain and some domain of mathematical objects.[5] On such views, answering (H) goes hand in hand with answering (S): the rough idea is that anything that is true of mathematical objects *just by virtue of their having a certain structure* shared with a domain of empirical objects will *ipso facto* be true of that empirical domain as well. Criticisms of mapping accounts, however, have motivated views that solely answer (H) while leaving (S) aside, most prominently the "inferential account" of Bueno and Colyvan (2011).[6] The only requirement for something to count as an application of mathematics, Bueno and Colyvan argue, is that the horizontal arrows of the three-step decomposition shown in Fig. 1 correspond to partial structure-preserving mappings. These mappings bridge the ontological gap between the empirical and the mathematical in a way that allows for systematic translations of empirical premises into mathematical premises, and of mathematical conclusions into empirical conclusions. Traditional "mapping accounts" would require these mappings to be straightforwardly inverse to one another (e.g., corresponding to a single isomorphism). Bueno and Colyvan do not; they thus offer a more flexible solution to (H), but at the cost of losing the means to answer (S).

Analogs of both (H) and (S) can be raised for intra-mathematical applications as well. However, within mathematics, it is (S) that is the more salient of the two, making the intra-mathematical case sit uneasily with the trends just discussed. Much of the urgency of (H) for applications of mathematics to science stems from the apparent ontological gap between the two sides: while a similar puzzle can in principle be raised for applications of mathematics to mathematics in the *geographical* sense—at least if we take ontological differences to underpin the division of mathematics into branches, as was traditionally the case for geometry and arithmetic—it is rarely felt to be as pressing (though see Hacking 2014, 9). By contrast, because of the centrality of justification in mathematics, problem (S) is more glaring: if a method consistently spits out solutions that turn out to be provably correct, we will want to know why it is systematically successful.

As an example, consider (once again) early 17th-century algebraic reasoning in geometry. It relied on an apparent mapping between the geometry and the algebra that involved reasoning on the *positive* lengths of segments. However, both Girard and Descartes noticed that *negative* solutions to the resulting algebraic equations could be interpreted geometrically in a systematic manner. This pattern instantiates Bueno and Colyvan's schema perfectly. Starting from a geometrical problem, there is a systematic way of setting up an equation (this corresponds to one mapping). Starting from positive *and negative* solutions to these equations, there is a systematic way of interpreting them geometrically (another mapping). But, on the face of it, there is no single shared structure that accounts for both of the mappings at once. Yet this leaves it a mystery that the procedure should work at all—why should negative solutions systematically lead to successful solutions in our geometrical diagrams? What we most

[5] For instance, they claim that both domains are instances of the same axiomatic theory, or that there is a structure-preserving mapping between them that allows transfers back and forth across the apparent ontological gap; see for instance Leng (2021) and Pincock (2012).

[6] Let us mention three salient criticisms of mapping accounts. First is the possibility of *mis*applying mathematics: one can systematically use mathematics to derive empirical conclusions that turn out to be wrong, and—it is argued—a solution to (H) should account for such uses. Second, many applications of mathematics rely on "idealizing" assumptions that we know to be false about the target empirical domain, apparently precluding any strong identity of structure. Third, the mathematics being applied is often richer than the empirical set-up (for instance by including negative numbers while distances between points in space are initially thought of as necessarily positive), and it is often not clear which parts of this extra mathematics may eventually find an interpretation, thus calling for a solution to (H) that is agnostic on this count.

want in this case is a solution to the relevant instance of (S). With hindsight, this could be done by offering a more comprehensive mapping between *directed* segments and (positive and negative) numbers.

Most interesting about the parallel between intra- and extra-mathematical applications, however, is that it highlights a further question, different from both (H) and (S). Admitting that the use of algebra in geometry leads to correct results, one is tempted to ask why it is *advantageous*: why would we want to use algebra in the first place, rather than stick to pure geometry? Notice the difference with the success problem. Presumably, an application of mathematics can only be advantageous if it also meets some measure of success. But the advantage it offers in the context of a given problem goes *beyond* the mere fact that it yields satisfactory conclusions. It is perfectly possible for an application of mathematics to allow deriving, in a long-winded and inconvenient way, correct empirical conclusions that could be reached easily and straightforwardly *without* this application. In such a case, problem (S) would still arise, but there may be no *advantage* to explain.

More precisely, two sub-questions can be distinguished:

(G-What) *What* can the gain be of using a piece of mathematics to reason from premises to a conclusion that can all be stated and understood without that piece of mathematics?

(G-Why) *Why* does the piece of mathematics in question give rise to such a gain (when there is one)?

Together, these two questions form the problem of *epistemic gain* (G): call them the *what* and the *why* questions of epistemic gain, respectively. It is clear that they can be raised, not just for any application of mathematics to empirical science as usually construed, but also for any application of mathematics to mathematics in the *topical* sense—i.e., wherever some mathematical resources are used in the course of reasoning from and to mathematical statements that can be stated and understood without these resources. The problem of epistemic gain can also be raised for phenomena within science that are not usually treated as applications, including changes of representation.

Of course, the mere fact that we can ask a question such as (G-Why) does not mean that it admits of a substantive answer. By analogy, the fate of (S) in the recent literature helps clarify possible ways of dismissing a problem such as (G-Why), and possible responses. Two kinds of arguments present themselves.

First, one can argue that (S) (resp. (G-Why)) admits of no interesting answer because the success of (resp. the gain afforded by) an application of mathematics is similar to the success of a theory—a brute fact that can only be discovered empirically, by trial and error. In the case of (S), this may be Bueno and Colyvan's view. The only one way of answering a worry of this kind is to exhibit an answer to the question at issue. Recall the problem raised by the systematic geometric interpretability—as opposite-direction segments—of the negative solutions that the algebra can yield. One could hold that the success of this systematic procedure of interpretation is a brute fact, and that any further attempts at explaining it are idle. However, showing that one can *can* in fact shed light on the systematic interpretability of negative solutions by way of a more encompassing mapping between the geometry and the algebra, in terms of *oriented* segments together with suitable orientation conventions, conclusively refutes this point of view. In the end, it is a perfectly consistent position to maintain that Bueno and Colyvan's inferential account is adequate to *describe* (in a way that answers (H)) how scientists use any particular piece of mathematics in practice, while holding that an answer to (S), which may not be available to practitioners and might only be discovered many years later, requires some stronger identity of structure.[7] Likewise, one may be able to exhibit illuminating answers to (G-Why); one cannot exclude their possibility *a priori*.

Second, one may hold that, while there may be informative accounts to be given of the success of (resp. of the gains afforded by) individual applications, the nature of these accounts will vary from case to case and will rely on further mathematical results, so that it does not fall within the purview of a general philosophical account of applications of mathematics. As regards (S), this kind of move has been made most clearly in broader debates on scientific representation, where recent "inferential" accounts have tended to focus on the general question of what makes something an "epistemic representation"—whether this representation be accurate or not—while treating the question of accuracy or success as legitimate, but outside the scope of a general theory.[8] Such exercises in philosophical boundary drawing need not detain us here. Let us attempt at clarifying and answering (G); worries about where any answer properly belongs can wait.

3 Dispensable versus indispensable applications

To sharpen the problem of epistemic gain, we first need to attend to a crucial distinction. Given an application of mathematics as defined above, consider, among all the conclusions it yields, those that could have been stated and understood independently of the application (e.g., traditional algebra-free geometric statements). It may be that some of these conclusions could not have been obtained at all without the application; conversely, it may be that all of them could, in principle, have been obtained already (though perhaps only with more difficulty). Call the application *indispensable* in the former case, *dispensable* in the latter. These two cases raise very different questions and call for separate discussions.

Start with indispensable applications (case 1). The increase in inferential strength these involve can arise in two different ways, although the distinction between the two will depend on delicate questions of logic. On the one hand, the mathematics being applied may allow obtaining conclusions that were not already contained in our (application-free) starting points in any plausible sense, for instance by (explicitly or implicitly) bringing along with it additional axioms or assumptions (case 1.1). On the other hand, the conclusions that are gained may be consequences of our application-free starting points in some sense, but ones that we did not have means to reach *inferentially* (case 1.2). Whether such a case is possible will depend on the logic we use and on what we mean by (logical) consequence, but some assumptions allow for it. For instance, the discussions that followed Field's *Science without Numbers* (Field, 1980) highlighted that if one interprets logical consequence semantically while using a logic in which syntactic and semantic consequence do not coincide, there is space for an application of mathematics that is dispensable semantically (everything it allows deriving is

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[7] Such phenomena are common in physics as well: Wilson (2006, chapter 8) offers numerous examples of applications of mathematics whose success can be accounted for after the fact by developing new "correlational pictures," as he calls them—that is, new accounts of what the mathematics being applied represent empirically.

[8] This point is brought out very clearly by Nguyen (2025) in a recent review of Suárez (2024), a book that summarizes two decades of work on inferential approaches to scientific representation.

already true in any model of the application-free starting points) but indispensable syntactically (some conclusions derivable using the application could not have been derived without it), falling in case 1.2.[9]

When we are in case 1 (be it 1.1 or 1.2), the answer—or at least one major answer—to (G-What) is clear: the gain afforded by the application consists in an increase in inferential strength. Question (G-Why) would then require pinpointing, as far as is possible, where exactly this increase comes from. For instance, in case 1.1, one approach might be to turn to "reverse mathematics" (see Eastaugh, 2024) to pinpoint the exact strength of the existence assumptions that may be smuggled in by the mathematics being applied.

By contrast, my target in this paper is the problem of epistemic gain as it arises when the application of mathematics is deductively dispensable—when all application-free conclusions that can be obtained using the application could already have been obtained without it (case 2). Importantly, recall that the dispensability at issue only concerns conclusions that could be stated and understood without the application (in a formal setting, it would be a conservativity claim). For instance, an application such as the use of algebra in geometric problem-solving will introduce previously unavailable statements involving algebraic equations; however, these are outside the scope of the dispensability claim, which only concerns traditional algebra-free geometric statements.

In contrast to cases of indispensability, what the gains may be of going through a dispensable application is not immediately clear. This is the question I now turn to, before examining why the gains in question might arise.

4 Gains from dispensable applications

What may the gain be of going through a *dispensable* application instead of, well, dispensing with it? This is the what-question of epistemic gain as it arises in dispensable cases. This section will survey possible gains and zone in on a particular one, which eminently cries out for explanation: applications can make it easier to find solutions to problems.

In general, when several ways of reaching a conclusion are available, there can be many reasons for preferring one over another. One path to the conclusion could be more explanatory, say, or more conducive to some kinds of understanding, or perhaps more in line with the expertise of a particular scientific or mathematical community. Over the past decades, a central concern of the literature on mathematical practice has been to clarify the various dimensions along which proofs of one and the same theorem can differ. One proof may appear to show "why" the theorem is true; another may be pure, i.e., not appeal to notions far removed from the statement of the theorem, or conversely show a surprising connection with a different branch of mathematics; yet another may be more surveyable, or more motivated, and so on.[10]

My target in this paper, however, is the intuition expressed in the literature on applicability—that applications of mathematics can yield "inferential" gains. Bueno and Colyvan (2011), quoted in the introduction, write that applications can make it easier to reach certain conclusions that would otherwise be "extremely hard" to obtain. In a similar vein, Field (1980, 28–29) writes that "invoking real numbers (plus a bit of set theory) allows us to make

[9] See Shapiro (1983), Manders (1984), and the discussion in Field (2016).

[10] Classic early papers include Steiner (1978); Rav (1999); Dawson (2006). For surveys, see Mancosu et al. (2023) on explanation, Arana (2024) on purity, and Morris (2024) for a broader range of contributions.

inferences among claims not mentioning real numbers much more quickly than we could make these inferences without invoking the reals."[11]

To refine this idea, the most straightforward approach is to focus on *proof length*. Given Field's use of logical tools, what he likely means, when writing that real numbers allow us to make inferences more "quickly," is that the detour through real numbers allows for *shorter proofs* from real-number-free premises to real-number-free conclusions. Bueno and Colyvan seem to think of scientific reasoning from some empirical premises to some empirical conclusion as made up of a string of "inferences": their talk of the "ease" or "difficulty" of obtaining conclusions may simply be about how long this string of inferences is.

Attempts to make proof length into a well-defined measure face substantial challenges, however. Whether computed in terms of lines or of characters, the length of *formal* derivations is notoriously sensitive to small choices of formalization (Potter 2004, §13.8; Arana and Stafford 2023). Mathias (2002) provides an oft-quoted example: the full expanded length of Bourbaki's term defining the number 1 will differ by a whopping 42 orders of magnitude depending on whether the ordered pair is taken as a primitive or is given its usual set-theoretic definition. Admittedly, formal proofs need not be expanded into primitive symbols to be acceptable: the proofs it would make sense to measure for our purposes should be allowed free use of such shortening devices as explicit definitions, derived inference rules, or the cut rule. Indeed, the introduction of a new concept through an explicit definition can count as an application in the topical sense, and bring with it the kinds of gains we are interested in. Nevertheless, it can scarcely be denied that formalization typically involves many irrelevant choices with a substantial impact on proof length.

If instead of formal derivations, we focus on the length of *in*formal proofs, the problem of irrelevant details of formalization is less pressing. Choices in the presentation of an informal proof are plausibly related to the proof's "ease" in some practically meaningful sense; just spinning out a lemma from a complex proof can make it easier to follow, for instance. But we are merely trading one problem for others. Beyond the inescapable vagueness of any measure of natural language text (not to mention the formulas and diagrams that may be involved), the length of an informal proof will depend in a crucial way on the expertise of its intended audience: informal versions of the same proof in an undergraduate textbook, in a graduate textbook, and in a research paper may differ considerably in their level of detail.

One might be tempted to conclude that proof length is an inherently fuzzy and noisy measure, but nevertheless maintain that it measures the right thing. More fundamentally, one may worry that—for our purposes—it measures the wrong thing altogether. As Detlefsen (1990, 376) emphasized, one should distinguish two kinds of complexity of proofs (see also Arana and Stafford 2023; Dean and Naibo 2025, 4). On the one hand is "verificational" complexity, which tracks how hard it is to check that a given derivation is indeed a correct proof. On the other hand is "inventional" or "discovermental" complexity, which is about how hard it is to *discover* the proof in the first place. For all its limitations, proof length may be appropriate to measure the first, but it is largely orthogonal to the second: a proof may be relatively short, yet quite hard to find; conversely, a proof may be routine but involve a long computation.

[11] While Field believed he was describing a case of dispensability in the sense of case 2 above, his proofs were not in fact sufficient to establish it (his reliance on second-order logic instead pointed to case 1.2). We can leave this point aside, however: it is easy to check that such dispensability does hold in simple cases, which already suffices to raise an instance of the problem of dispensable epistemic gain.

A closer examination of classic examples of applications suggests that the most salient gain they offer pertains to the ease of *discovering* solutions to problems, rather than to the length or speed of these solutions. As a starting point, consider mathematical theorems that first received "impure" proofs, but for which "pure" proofs were eventually sought and found—for instance, number-theoretic theorems that were first proved using "transcendental" means (i.e., real or complex analysis) before receiving "elementary" proofs, such as the Prime Number Theorem or Jacobi's four-square theorem (on the latter, see Arana 2024, 6–10). It is not true in general that the elementary proof is "longer," at least not in any obvious sense. The Erdős-Selberg proof of the Prime Number Theorem is quite short (Erdős, 1949), although it is intuitively more intricate than traditional analytic proofs. One obvious advantage that analytic proofs did have, however, is that they were found first. Of course, this does not imply that they are easier to find in and of themselves; there could be many contingent reasons for their having been discovered first. But it is *prima facie* plausible that the apparent intricacy of the Erdős-Selberg proof is of a nature that makes the proof harder to find (as opposed to hard to *check*).

Next, let us return to the use of algebra in geometry. In principle, Descartes still abided by the strictures of Euclid-style reasoning: a solution was only acceptable if one *constructed* it (e.g., using straightedge and compass), then proved geometrically that the construction satisfied the conditions of the problem. Deriving an equation and finding its roots was not enough: one then needed to construct lines (for instance) corresponding to these roots. Descartes provided systematic rules for doing so, and often omitted that construction step as routine, but was clear that the full solution included it. If that is the case, however, then the whole algebraic procedure becomes redundant from the point of view of justification. The construction—together with the geometric proof that it satisfies the conditions of the problem—is a full solution in and of itself. The point of using algebra, as Descartes himself repeatedly emphasizes, is that it allows *finding* an appropriate construction.

Statements about how a new tool can systematically make the solution of certain problems easier also abound with respect to the introduction of new symbolic calculi or forms of representation (which, remember, can count as applications in the topical sense—the sense that is relevant for our purposes), as highlighted by a number of recent case studies.[12] Yap (2011) examines the congruence calculus in light of Gauss's claims that it is dispensable, but makes straightforward the solution of a whole range of problems that would, without it, have required considerable ingenuity. Waszek and Schlimm (2021) discuss Boole's insistence on problem-solving advantages as the central benefit of his logical calculus. De Toffoli (2023) highlights the central, but in principle dispensable role of arrow diagrams in commutative algebra.

Thus, claims about gains in discoverability—meant as gains, not in *what solutions* can in principle be discovered, but in *how easy* it is to discover them—are common in the mathematical literature, and clearly important. This is so even though the nature of such gains remains rather obscure. It is not clear how to measure them, or even if they can be objectively measured at all; apart from Arana and Stafford (2023), I am not aware of any attempt at tackling the issue head-on. Nevertheless, I'll take the existence of such gains as a datum of mathematical practice, and I'll focus of them from now on—taking them as the best available sharpening of the intuition that dispensable applications of mathematics can afford "inferential" gains.

[12] For broader surveys of relevant work on representations in mathematics, see Schlimm (2025); Waszek (2024).

In a series of important papers, Hunt (2021a,b, 2023, 2025) develops a different approach to the what-question of epistemic gain—different from considerations of both ease of discovery, and proof length or problem-solving speed. Before anything else, note that Hunt's focus is broader than that of the present paper. In general, he compares compatible ways of arriving at one and the same conclusion, and asks what may be epistemically valuable about having several (since, for the mere purpose of obtaining the conclusion, one would be enough). His problem thus encompasses any dispensable use of an application, where the same conclusion will be reachable both through the application and without it, but also includes cases in which no applications are involved, for instance when a mathematical theorem admits of two distinct proofs using the same concepts and tools. Hunt's central claim is that, while different derivations of a conclusion may differ *merely* in practical ways, such as efficiency for some agents, some also differ at a deeper level, namely in their inferential structure—in "what depends on what." As an example, compare once again an algebraic with a traditional Euclid-style solution to a geometric construction problem. From Hunt's perspective, what makes them *epistemically different* is that (part of) the algebraic solution only depends on the quantitative relations between the lengths of line segments, and not on facts regarding the relative positions of points. The algebraic solution might (or might not) be shorter and easier to find; from Hunt's perspective, such "practical" benefits, when they exist, are less important than, and plausibly *downstream from*, the epistemic differences in inferential relations.

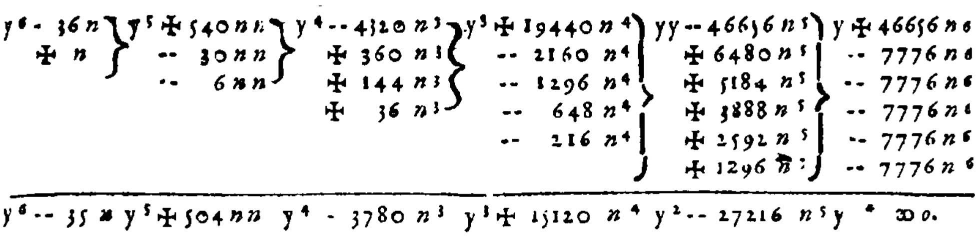

Fig. 3 An algebraic equation in Descartes' *Géométrie* (1637, 378)

By contrast, take the algebraic solution and replace our standard notation by a slightly different one. For instance, like Descartes, we could use vertical brackets instead of parentheses as a grouping device for coefficients (Fig. 3). From Hunt's perspective, this would yield a "trivial notational variant," which might have some practical advantages (in the instance, numerical coefficients are easier to compute, as their component terms are already in an appropriate layout for carrying out additions in place-value notation), but with no change in "what depends on what" when solving problems.[13]

For Hunt, then, the essential benefit that an application can offer consists in teaching us new, differently structured problem-solving plans—which means teaching us new facts about "what we need to know to solve problems." On this view, the "inferential" advantages we started from, whether they be explicated in terms of proof length or of discoverability, are merely symptoms of deeper gains.

[13]Note that Hunt's notation of "trivial notational variant" is, strictly speaking, relative to particular (classes of) problem-solving plans. Only very rarely will two notations be fully equivalent across all possible problem-solving plans in which they might play a role.

Whether we adopt this view or not, what remains to be done is answer the why-question of epistemic gain as it relates to the "inferential" advantages I explicated above in terms of gains in ease of solution discovery. In other words, we need to clarify, as far as is possible, *why* a given piece of mathematics gives rise to gains in how easy it is to discover solutions to certain problems (whenever it does give rise to such gains). If we adopt Hunt's view, this amounts to clarifying how the knowledge of inferential relations that Hunt sees as constitutive of epistemically valuable reformulations relates to gains in discoverability, which is not obvious.

5 The "pre-existing highway effect"

Why, then, can (dispensable) applications of mathematics yield gains in how easy it is to discover solutions to problems? The present section examines a *deflationary* answer, according to which such gains are, in a certain sense, illusory. Insofar as applications provide gains in discoverability—so the answer would go—this is not because they make the relevant solutions easier to find in any *intrinsic* way, but merely because they allow *reusing* knowledge (methods, solutions, etc.) that, contingently, happens to have been developed in other contexts already, but whose relevance was not previously recognized. In other words, the discoverability benefits of applications come merely from enabling a more efficient use of pre-existing knowledge.

A metaphor may make the claim clearer. Imagine that finding the solution to some problem involves crossing a thick forest, more or less along the course of a river. If it so happens that a highway has been built on the far side of the river, we are most likely better off crossing over, taking the highway, then crossing back, instead of hacking our way through impassable thickets on our present side. The reason for this, however, is not that the journey is intrinsically easier on the other side; it merely happens that, contingently, a lot of work has already been done to pave the way over there. If there was no highway on the far side, there would be no reason to cross: we could just as easily find our way on the side we are presently on (perhaps even, eventually, start building a road where we are). For perspicuity, I shall refer to the gain for problem solving that can stem from the use of pre-existing techniques as the "pre-existing highway effect."

Applications of mathematics are often presented in the literature in a way that involves the straightforward use of off-the-shelf mathematical results. Perhaps, one might think, the gain they offer is merely that of pointing to the appropriate shelf. Consider again early 17th-century uses of algebra in geometry. Perhaps they afforded benefits in problem solving *just* because—by way of a suitable river crossing, namely geometric interpretations of addition, multiplication, etc.—they made solution techniques that had previously been developed in a numerical context usable for geometrical problems. On this account, the gains apparently afforded by the algebra would come, not from any intrinsic problem-solving advantage, but from cleverly translating the problem so as to put pre-existing techniques to work: in a counterfactual world with no such pre-existing techniques—with no earlier Arabic and Renaissance algebraists—shifting from a geometrical problem to a symbolical language of equations would have offered no benefits. The resulting picture is the following. The success problem—why is it at all possible to systematically obtain correct geometrical conclusions through algebra?—might be answered structurally: a common structure shared by arithmetic and geometry allows parallel solution techniques—"algebraic" ones—to be used in both. The additional why-question of epistemic gain relative to gains in discoverability would be fully answered by pointing to the contingent fact that by the early 17th century, a corpus of techniques stemming from Arabic and Renaissance algebra happened to be available for arithmetical problems.

The pre-existing highway effect, however, can at most bear upon applications in the *genetic* sense (see Section 1): it cannot work for cases in which a piece of mathematics that did not exist before is developed specifically to tackle a particular (kind of) problem.

Consider the oft-discussed proof by Euler (1741) that one cannot stroll around 18th-century Königsberg in such a way as to cross each of the seven bridges exactly once (Pincock, 2007; Räz, 2018). It is often brought up as an example of mathematical *explanation*; accordingly, it is presented as an application of mathematics that derives an empirical conclusion (i.e., that all attempts at plotting a suitable path through Königsberg will fail) from a graph-theoretical theorem (i.e., that a connected graph admits a path going through each edge exactly once if and only if each vertex, except possibly for exactly two of them, has an *even* number of edges incident to it).

It is clear that, for someone acquainted with graph theory—including the theorem just mentioned—recognizing that Königsberg's neighborhoods and bridges can be seen as a graph will make the solution of the problem easily discoverable, exemplifying the benefits of the pre-existing highway effect. However, the claim to fame of Euler's paper is precisely that *there was no such thing* as graph theory at the time. Yet there is a plausible case to be made that the resources Euler introduces—which constitute an "application" in the topical sense—do make the solution of the Königsberg problem more discoverable, even though they are not indispensable for it.

One chief resource introduced by Euler is ostensibly *notational*, and went hand in hand with a shift from the complicated geography of the city of Königsberg to a more abstract and general formulation (Räz, 2018). Euler used lowercase letters to denote bridges and uppercase letters to denote what we might call neighborhoods—maximal areas of the city that do not include any part of the river or of a bridge. A path through the city can then be represented as a string of (alternately uppercase and lowercase) letters. Euler then noted that, for the purposes of the problem, the lowercase letters labeling bridges could be omitted, so that paths could be represented as strings of uppercase letters. Now, let n be the number of bridges in some city. The problem of whether one can find a path going through each bridge exactly once then reduces to the problem of whether there is a string of $n+1$ (uppercase) letters subject to specific constraints: each letter has to appear a precise number of times (which depends on *how many bridges* are connected to the corresponding neighborhood and on whether the letter appears at an extremity of the string).

Intuitively, Euler's notational devices afford substantial advantages when it comes to tackling the Königsberg problem.[14] However, these advantages cannot be explained away through

[14] As a caveat, note that from a historical point of view, one cannot draw definitive conclusions about Euler's own problem-solving itinerary by looking at his published paper. For all we know, he may have found the solution to the puzzle independently of the particular notations he later crafted for expository purposes. Importantly, Euler's paper had a theoretical goal: he used the Königsberg problem as an example of a little-studied kind of "geometry of position" (*geometria situs*) that did not depend on the measurement of distances. To do so, it was essential to display the Königsberg case as an instance of a more general type of problem. This—rather than the mere solution of the brain teaser—may well be the real purpose of Euler's notations. In using this example, chosen because it is already much discussed in the literature, I am thus going beyond the historical evidence and relying on intuitions about the role that Euler's notation may play in solving the puzzle.

the pre-existing highway effect. They require another approach—if, that is, they are to be explained at all.

6 Attention and expressive restriction

The "pre-existing highway effect" is not enough to account for how applications can make it easier to discover solutions to problems, as we just saw. The purpose of this section is to sketch a second approach, which is inspired by the work of Ken Manders;[15] I shall call it the *attentional account*.

To introduce the main idea, let us return to Euler's seven bridges. The initial puzzle is about walks through the city of Königsberg, with its river and bridges, but also its avenues, alleys, and passageways. As mentioned above, the first step of Euler's solution is to suppress much of this complexity by denoting maximal land areas by uppercase letters, and paths by strings of such letters. Similarly, a typical presentation of the problem today would start off by representing the set-up diagrammatically, with areas represented by points and bridges by lines joining the points. Either way, many details about the city have been eliminated, though not quite the same in each case: Euler's notation represents paths by displaying the sequence of neighborhoods they cross (suppressing details about bridges, which only come into play as constraints on possible paths); the typical diagrammatic representation shows areas and bridges, but does not, in and of itself, represent paths. (Incidentally, one should for this reason be wary of assuming that these representations are just about bringing out *the* graph structure of the problem: Euler's strings of uppercase letters and our graph diagrams do not in fact retain the same elements of the initial problem or represent them in the same way.)

The previous section discussed one potential advantage of such representational shifts: by suppressing most of what is specific about the city of Königsberg, they may allow us to recognize that the problem is similar to others that we have encountered in the past or that we know have been solved elsewhere, thus taking advantage of the "pre-existing highway effect." By contrast, the attentional account aims at explaining why Euler's notational devices may help in problem solving even when no such pre-existing knowledge is available.

The core idea is that Euler's notations help merely because they suppress detail—because they focus attention on a smaller number of properties of the target objects, here paths—and hence reduce the number of propositions about the target objects that can be entertained as potential reasoning steps. Initially, investigating paths through the city of Königsberg might involve thinking about the internal layout of individual neighborhoods, that is, about what kind of itineraries we might chart in between bridge crossings. Representing paths by strings of uppercase and lowercase letters (corresponding to neighborhoods and bridges, respectively), as Euler does at first, already restricts the properties of paths that can be expressed: properties such as "going through the Steindamm thoroughfare" cannot be expressed, so that one no longer attends to them when reasoning on the notation. The further simplification of omitting the lowercase letters standing for bridges eliminates further properties of paths, such as "going through the Honey Bridge as one's first river crossing." Only properties of the sequence of neighborhoods that a path traverses can still be expressed. Thanks to this expressive restriction, the number of facts about paths one can entertain and attempt to prove is

[15] The central idea is already outlined in Manders (1989). Much of Manders's later work, however, unfortunately remains in draft form; see in particular his widely circulated "Euclid or Descartes? Representation and Responsiveness" (1999), to which this section is deeply indebted.

much reduced. The intuition is that this ultimately makes it easier to identify those properties of paths that can lead to a solution (in the instance, properties about *how many times* the various uppercase letters occur in the traversal sequence).

At this point, a crucial caveat is necessary. The focusing of attention on a smaller number of properties of paths permitted by Euler's strings of letters can only help find the solution of the problem because it so happens that (a crucial step of) that solution does not, in fact, depend on the layout of streets and alleys within neighborhoods, or on other properties of paths that have been suppressed. The attentional account cannot predict *a priori* which expressive restrictions may prove especially helpful for the discovery of a particular solution.

In general, the attentional account articulates one way—other than the pre-existing highway effect—in which an application, in the topical sense, can make it systematically easier to find solutions to a class of problems: By shifting reasoning to a setting in which fewer properties[16] of the objects under investigation are attended to, an application can reduce the number of propositions (of any given length)[17] about the target objects that are available as candidate reasoning steps. (For the sake of conciseness, one can say, metaphorically, that it reduces the size of the investigator's "search space.") The fact that there are fewer possible reasoning steps to consider means that, even with the worst possible problem-solving strategy, namely a brute force search, it is easier to find the reasoning steps that constitute an actual solution of the problem (*if*, that is, such steps are actually available among the expressively restricted propositions that remain).

Let us return to the more complex example of early uses of algebra in geometry, as analyzed by Manders. The shift from the initial geometrical configuration to an algebraic equation involves disregarding much detail about the relative positions of points: the only properties of the initial configuration that can still be attended to are what we might call quantitative relations among line segments. This reduces the number of possible reasoning steps drastically. Later on, the algebraic manipulation of the resulting equation might involve disregarding the complex algebraic expressions of the coefficients, perhaps replacing them by new single letters so as to attend only to the degree or the general form of the resulting equation. Eventually, however, solving the initial problem geometrically requires again taking the relative position of points into account: the algebraic solutions need to be interpreted. In other words, different steps of the solution process require systematically attending to different properties of the problem situation. Analyzed in this way, the "application" of algebraic techniques in geometry takes on the appearance of sophisticated techniques of attention management.

Notice that the attentional account does not require that practitioners, when deploying a piece of mathematics in the service of solving a particular problem, be able to spell out, without using the piece of mathematics being deployed, which features of the original problem situation it systematically suppresses and which it helps attend to. Such a gloss may only become available long after a given application has been made good use of. The dialectic here is similar to the one we encountered in Section 2 regarding the *success* problem. As we saw, proponents of a mapping account who hold that some identity of structure is required to explain why a particular application leads to correct conclusions need not require practitioners to know what structure-preserving mapping underlies the success of their application.

[16]Note that, for the purpose of this section, relations are considered as species of properties.

[17]Assuming a finite vocabulary, the number of propositions of any given length will be finite, and the smaller the vocabulary, the smaller the number of propositions.

Likewise, we may explain the epistemic gain afforded by the deployment of a piece of mathematics by the fact that this piece of mathematics shifts which aspects of the problem set-up are attended to, and hence manipulates the investigator's search space—yet admit that the investigator may be none the wiser.

We are now in a position to understand the relation between the attentional account and Hunt's views on the value of reformulations. The attentional account shows how shifting to a new setting can restrict attention to a reduced number of specific properties of the initial set-up, thus reducing the number of candidate reasoning steps facing the investigator and systematically making it easier to find solutions to certain kinds of problems. Now, consider a solution obtained through this route. It will be a derivation of the conclusion using the application, hence will go through propositions concerning the specific properties of the set-up that are singled out by the application. The inferential structure of this solution will thus differ from that of a solution that does not use the application—they will differ, in Hunt's terms, regarding "what depends on what." It is these local differences in inferential plans that Hunt's account focuses on. For instance, an algebraic solution to a geometric construction problem will, at some of its steps, rely exclusively on quantitative relations between line segments. However, the exact propositions and inferences making up the inferential plan of a solution employing the application will vary from case to case. The exact algebraic equations, and the exact transformations performed on them, will vary from construction problem to construction problem; the inferential plans of their solutions will be different. What the attentional account focuses on is what these solutions nevertheless have in common. In the instance, what all algebraic solutions to construction problems have in common is that certain steps of their inferential structure are propositions regarding a certain class of (quantitative) properties of geometric configurations. The reason algebra makes it systematically easier to find solutions to such kinds of problems is that it focuses attention on the properties in this class.

In short, Hunt's account focuses on *local* differences between the inferential plans of solutions to a specific problem, while the attentional account aims at explaining a *global* fact—how applying a given piece of mathematics can systematically make it easier to find solutions to a *family* of problems—and thus needs to appeal to a global *explanans*, namely the fact the detour through the piece of mathematics focuses attention on a *family* of properties of the target problems. The two accounts are fully compatible, indeed complementary.

Conclusions and prospects

Why can applications of mathematics provide inferential advantages? This is what I called the problem of the *epistemic gain* of applying mathematics. The first task of this paper has been to clarify it; to show that it does not reduce to other, more traditional problems of applicability; and to establish that it cuts across the apparent boundary between applications to science and applications within mathematics itself (Sections 1–2). I argued that it is best raised in a broad context, beyond applications of mathematics as usually understood: namely, for any case in which the use of new mathematical resources, not required to understand the initial (empirical or mathematical) problem, affords epistemic advantages. This includes phenomena that might, on their face, be described as mere changes in mathematical representation.

I then focused more specifically on what I called the problem of *dispensable* epistemic gain, i.e., on cases in which the use of a piece of mathematics seems to afford inferential advantages despite the fact that all conclusions obtainable with that piece of mathematics can also be obtained without it (Section 3). The best way to explicate the inferential advantages apparently afforded by the mathematics in such cases, I argued, is as gains in how easy it is to *discover* solutions to problems (Section 4).

My second task has been to argue that the problem of dispensable epistemic gain is not idle: it can receive substantive answers. First, Section 5 examined what I call the "pre-existing highway effect": the idea that a dispensable piece of mathematics may make a difference in the discoverability of solutions if it allows transferring, to the problem being solved, results that are already known elsewhere. While important, I argued that this can only provide a partial answer. Section 6 explored a second solution: that part of what a piece of mathematics may afford the problem-solver are tools of attention management tailored to a class of problems.

How far can this attentional account be taken? This is the main question left open by this paper. The examples of applications discussed above under the rubric of attentional management may appear to be very specific, with a simple abstractionist flavor. The account may well be of wider applicability, however. Manders (1989) has claimed that canonical examples of "domain extensions," such as the move from the field of real numbers to that of complex numbers, or the adjunction of points "at infinity" to the Euclidean plane, might be best understood as unexpected strategies to ensure expressive restriction. For instance, writes Manders, "disregarding the ordering within real algebra itself is difficult, because order remains definable from real-number multiplication"; but the adjunction of $\sqrt{-1}$ makes ordering algebraically nonexpressible (Manders, 1989, 557). Such tantalizing claims would surely repay closer examination.

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