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ORIGINAL RESEARCH

Inference to the best explanation as supporting the expansion of mathematicians' ontological commitments

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Abstract
This paper argues that in mathematical practice, conjectures are sometimes confirmed by "Inference to the Best Explanation" (IBE) as applied to some mathematical evidence. IBE operates in mathematics in the same way as IBE in science. When applied to empirical evidence, IBE sometimes helps to justify the expansion of scientists' ontological commitments. Analogously, when applied to mathematical evidence, IBE sometimes helps to justify mathematicians' in expanding the range of their ontological commitments. IBE supplements other forms of non-deductive reasoning in mathematics, avoiding obstacles sometimes faced by enumerative induction or hypothetico-deductive reasoning. Both platonist and non-platonist interpretations of mathematics ought to accommodate explanation in mathematics and ought to recognize IBE in mathematics, though these interpretations disagree on the ontological commitments that mathematicians ought to have. This paper offers an inductive account of why mathematical IBE tends to lead to mathematical truths.

Keywords Explanation · Mathematics · Platonism · Benacerraf · Imaginary numbers · Ideal elements

1 Introduction

Compared to *scientific* explanation, with which philosophy of science has been seriously engaged for at least seven decades, explanation *in mathematics* has been little explored by philosophers. Nevertheless, mathematicians have long distinguished mathematical proofs that *explain why* some theorem holds from proofs that merely establish that the theorem holds. Fortunately, mathematical explanation has now begun

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to receive greater philosophical attention. I agree with Mancosu (2008, p. 134) that the topic's "recent revival in the analytic literature is a welcome addition to the philosophy of mathematics."

In paying greater attention to explanation in mathematics, we should also consider the sort of confirmatory reasoning that philosophers generally term "inference to the best explanation" (IBE). If there are explanations not only in science but also in mathematics, then presumably there is not only "inference to the best *scientific* explanation", but also "inference to the best *mathematical* explanation". Philosophers (especially scientific realists) standardly regard IBE in science as an important means by which scientists confirm that certain as-yet-unobserved (and perhaps unobservable) entities exist. That various propositions about some theoretical posit, if true, would nicely explain some fact (that is already known and in which the posit does not figure) often makes the proposed explanation more plausible—and the confirmation is stronger insofar as the explanation would be better (or "lovelier", as Lipton (2004) puts it). This paper will examine whether and how IBE plays an analogous role in mathematics: in justifying the expansion of the mathematical domains to which mathematicians are ontologically committed.[1]

Let's see a small part of one example where IBE contributed to mathematicians' warrant in expanding their ontological commitments—in roughly the same way as

[1] This suggestion is not new. It is famously (though briefly) made by Gödel (1964, p. 265). In Sect. 2, I contrast my proposal with the accounts (whether of IBE in mathematics or of the non-deductive reasoning used in mathematics to support the expansion of mathematicians' ontological commitments) given by some other philosophers. Pincock's proposals are much nearer to mine than these other philosophers' proposals are. Like me, Pincock (2012, pp. 295–299) proposes that IBE in mathematics is used to support the expansion of mathematicians' ontological commitments. Like me, Pincock (2012, pp. 210–220) sharply distinguishes this sort of IBE (where the fact being explained is purely mathematical) from IBE in mathematical indispensability arguments in philosophy (where the explanandum is a fact about the physical world). As I am about to do, Pincock (2012, pp. 270–275) discusses the expansion of mathematicians' ontological commitments to complex numbers—though whereas I emphasize the role of IBE arguments in underwriting this expansion (with complex numbers providing the explanations of some facts exclusively about the reals), Pincock emphasizes arguments from the ways that real-number functions could be extended to complex numbers. Of course, these are not incompatible views of how the recognition of complex numbers was historically underwritten; I believe that both of these sorts of considerations were influential.

I am not original in emphasizing the role played by explanatory considerations in the mathematicians' expansion of their ontological commitments to include complex numbers. That mathematicians' recognition of complex numbers was motivated significantly by mathematicians' awareness that complex numbers would nicely explain various facts about the reals has been emphasized by, for instance, MacLane (1986, pp. 118–119): "The complete acceptance of complex numbers came primarily in their many uses in helping to understand other parts of Mathematics. … [T]here are phenomena with real numbers which can be properly explained only with complex numbers." Furthermore, whereas Pincock (2015, p. 13) appeals to IBE in mathematics in connection with explanations of the unsolvability of the quintic, I would look further back (and two exponential powers lower) for IBE in mathematics having historically underwritten the expansion of mathematicians' ontological commitments to include complex numbers. As Birkhoff and MacLane (2010, p. 476) emphasize, it was an important discovery (the so-called "irreducible case" of the cubic) that a general (unificatory, explanatory) formula for finding the *real* roots of cubic equations must use complex numbers. (For more on this expansion of mathematicians' ontological commitments to complex numbers, see note 8; for more on Pincock's views, see note 11.).

On the other hand, while my approach is not unprecedented, appeal to IBE in mathematics is hardly uncontroversial. For instance, Dummett (1994, p. 13) writes that "there is nothing in mathematics that could be described as inference to the best explanation.".

IBE has contributed to scientists' warrant in doing so.[2] Consider the fraction $1/(1+x^2)$. Long division

$$\begin{array}{r l} & 1 - x^2 + x^4 - \ldots \\ (1+x^2) & \overline{)\,1} \\ & -\underline{(1+x^2)} \\ & \quad -x^2 \\ & \quad -\underline{(-x^2-x^4)} \\ & \qquad\quad x^4 \\ & \qquad -\underline{(x^4+x^6)} \\ & \qquad\quad \vdots \end{array}$$

yields the Taylor series $1 - x^2 + x^4 - x^6 + \ldots$. Plainly, for real number x, this series converges only if $|x| < 1$. (When $|x| > 1$, each successive term's absolute value is greater than its predecessor's, so the sum will oscillate in an ever-widening manner.) Consider the fact that the two Taylor series

$$1/\left(1-x^2\right) = 1 + x^2 + x^4 + x^6 + \ldots$$
$$1/\left(1+x^2\right) = 1 - x^2 + x^4 - x^6 + \ldots.$$

are alike in that, for real x, each converges when $|x| < 1$ but diverges when $|x| > 1$. Why do these two series have the same convergence behavior? This similarity is especially puzzling considering that at $x = 1$, $1/(1-x^2)$ becomes undefined whereas $1/(1+x^2)$ behaves soberly. A proof that the first series converges if and only if $|x| < 1$, combined with an entirely separate proof that the second series converges if and only if $|x| < 1$, proves that the explanandum holds. But we may well suspect such a proof of failing to *explain why* the explanandum holds. That is, we may well suspect that the explanandum is no coincidence—that there is a common reason for the two series' common convergence behavior. If we expand our ontology beyond real numbers to include imaginary numbers, then we discover the following theorem (roughly, that every power series has a "radius of convergence" on the complex plane), which supplies a common explanation:

> *Radius-of-convergence theorem*: For any power series $\sum a_n z^n$ (from $n = 0$ to ∞), either it converges for all complex numbers z, or it converges only for $z = 0$, or there is a number $R > 0$ (the series' "radius of convergence") such that the series converges if $|z| < R$ and diverges if $|z| > R$. (Spivak, 1980, p. 524)

[2] My presentation here of this example closely follows my presentations in Lange (2010, pp. 329–331; 2017, pp. 290–292, 331, 344–345; 2019, pp. 3–4, 13; forthcoming1:4–5; forthcoming2:8–9); I have previously used this example in different places to make different points about explanation in mathematics. In Lange (forthcoming1), I use it (in an informal, non-philosophical essay) to motivate the importance of IBE to mathematics. But I have not previously devoted particular attention to the central philosophical topics of this paper, such as the role of IBE in expanding the range of mathematicians' ontological commitments.

If imaginary numbers and real numbers are on an ontological par, then it is no coincidence that the Taylor series for 1 / $(1 - z^2)$ and $1/(1 + z^2)$ have the same convergence behavior, since both functions become undefined at some point on the unit circle centered at the origin of the complex plane (the first function at $z = 1$, the second at $z =$ i).

That this is a genuine mathematical explanation is commonly emphasized by mathematicians themselves. As Spivak (1980, p. 528) says in his classic textbook, the radius of convergence theorem "helps explain the behavior of certain Taylor series obtained for real functions," such as the two I just gave. Earlier in the book, Spivak had already set the stage for this explanation; in introducing these two Taylor series, Spivak had asked why they have the same convergence behavior:

> Asking this sort of question is always dangerous, since we may have to settle for an unsympathetic answer: it happens because it happens – that's the way things are! In this case there does happen to be an explanation, but this explanation is impossible to give at the present time [that is, at the end of Chapter 23]; although the question is about real numbers, it can be answered intelligently only when placed in a broader context. (Spivak, 1980, p. 482).

The "broader context" is eventually supplied by imaginary numbers. They do not appear in the explanandum (and the explanandum can be proved without them), but if they have the same ontological status as the numbers figuring in the explanandum, then they explain the explanandum in a "lovely" way (which cannot be done without them). There are many similar examples. Mathematicians thereby used IBE to support the expansion of their range of ontological commitment from the real numbers to the complex numbers. This role played by IBE in mathematics is the subject of this paper.

In Sect. 2, I will specify what it would be for IBE to figure in mathematical reasoning. For instance, IBE need not be sufficient to justify the acceptance of some mathematical claim; a mathematician who employs IBE need only regard a mathematical claim as deriving some credibility from its potential explanatory power. I will also identify what mathematical IBE adds to other forms of non-deductive reasoning that have sometimes been thought to figure in mathematical practice. I will argue that IBE avoids an obstacle to enumerative inductions in mathematics and that the complex numbers' explanatory potential is a stronger reason for recognizing them than an argument merely from "their mathematical usefulness" (Nagel, 1979, p. 170). None of my arguments will depend on any particular account of how explanatory proofs in mathematics differ from non-explanatory proofs.

According to recent versions of the Quine-Putnam "mathematical indispensability" argument, the role of mathematical objects in certain *scientific* explanations is good evidence for these objects' existence. In such a "Quinean IBE", the explanandum concerns the physical, spatiotemporal world, whereas in the mathematical IBE's that I am examining, the facts being explained are purely mathematical facts. In Sect. 3, I will argue that this difference makes mathematical IBE's invulnerable to some of the most widely discussed objections to Quinean IBE's.

However, this difference also makes mathematical IBE's vulnerable to a challenge that Quinean IBE's do not face. A mathematical IBE is supposed to support an expansion in mathematicians' range of ontological commitment from the mathematics in the explanandum to the mathematics in the explanans. Hence, a mathematical IBE presupposes that mathematicians are already committed to the ontological status of the mathematics in the explanandum. Therefore, unlike Quinean IBE's, mathematical IBE's cannot supply an argument for mathematical platonism without begging the question (as noted by Baker, 2009, p. 613).

Whereas the "Quinean IBE" that I just mentioned is a *philosophical* argument for a *philosophical* conclusion (mathematical platonism), the kind of IBE's that I will be studying are arguments *in mathematics* for *mathematical* conclusions (namely, for mathematical explanations of some mathematical facts). In looking at mathematical IBE's, I am not aiming to argue that they support platonism or any other particular philosophical account of the proper ontological commitments for mathematicians to undertake. My concern is with the role that mathematical IBE's play in justifying the *expansion* of the proper ontological commitment in mathematics from some mathematical domains to broader domains (e.g., from the reals to the complex numbers). I do not maintain that an account of mathematical IBE's reveals what that proper ontological commitment actually is. It does not decide between platonism and its rivals. Therefore, I see mathematical IBE's as arguments that both platonist and various non-platonist accounts (e.g., Aristotelian realist, fictionalist,…) should all recognize as playing important roles in mathematical practice, just as all of these philosophical accounts should recognize the distinction in mathematical practice between explanatory and non-explanatory proofs.

In short, platonists and their rivals disagree about the ontological commitments that mathematicians ought to undertake. I am arguing that IBE in mathematics can help to justify an expansion of those ontological commitments to a broader mathematical domain—for instance (in the example we just saw), can help to justify mathematicians in regarding all complex numbers as on a par with the reals. But (on my view) IBE in mathematics does not show what ontological commitment this "par" involves, only that the commitment should be the same for all of the complex numbers as for the reals.

I will argue (in Sect. 3) that the recognition of mathematical IBE's can play a crucial role for both platonist and non-platonist philosophical accounts even though there would have to be an independent argument for that account's applicability to the mathematical domain from which the mathematicians' ontological commitments are being expanded (e.g., an independent argument for platonism regarding the real numbers). In particular, I will argue that an appeal to mathematical IBE underwrites a response to Field's (1989) version of Benacerraf's (1973) dilemma regarding mathematical knowledge. For platonism, this response amounts to an argument that the fact that our beliefs regarding certain abstract mathematical objects are arrived at by mathematical IBE's helps to explain why those beliefs are so accurate. I will contend that analogous responses (also appealing to mathematical IBE) can be given to analogous explanatory challenges facing non-platonist accounts.

These explanations appealing to mathematical IBE require some account of why beliefs arrived at by mathematical IBE tend to be accurate. Of course, the same question can be asked regarding IBE in science. In Sect. 4, I will argue that one promising account of why IBE is a fairly good guide to truth in science also applies to mathematical IBE. On this account, we often have good inductive evidence (in science and math alike) that certain sorts of facts tend to have certain sorts of explanations, and these expectations inform which potential explanations are "lovely". I will also examine the apparent differences between IBE in math and science regarding the prevalence of brute facts and of the same fact's having multiple explanations.

This paper hardly exhausts the philosophical questions raised by mathematical IBE. As explanation in mathematics receives increased philosophical scrutiny, I hope that some scrutiny accordingly falls on mathematical IBE.

2 What would mathematical IBE involve?

What would an "inference to the best explanation" in mathematics be an inference *to*? What would a proof's power to nicely explain *confirm*? Suppose, for instance, that we want to explain why the two Taylor series (given in the previous section) have the same convergence behavior. Suppose we formulate the radius-of-convergence generalization and although we have not yet proved it, we notice that if it is true, then it (or its proof) would nicely explain why the two Taylor series have the same convergence behavior. By IBE (in the absence of other relevant considerations, such as rival potential explanations and collateral relevant evidence or background knowledge), our discovery of the generalization's "potential explanatory power" (i.e., that it has power to nicely explain, if it is true) should increase our confidence (to some degree) in the generalization's truth.[3] Alternatively, suppose instead that we have already proved the radius-of-convergence generalization by deriving it from the axioms of complex arithmetic. Then (unless we have some doubts about the proof) the radius-of-convergence theorem already possesses maximal credibility. No room remains for us to increase our confidence in it from recognizing that it nicely explains why the two Taylor series have the same convergence behavior. In this case, what remains to be confirmed through IBE?

The answer is that even if the radius-of-convergence theorem has already been shown to follow from the axioms of complex arithmetic, those axioms have not themselves been shown to be explainers of various accepted facts exclusively about the real numbers. In aiming to fit mathematical practice, every philosophical account of

[3] Here is an example of such an IBE argument from the mathematics literature: "If a theory explains much data, then perhaps the theory is true. … Are there a set of random facts that P $\neq$ NP would help explain? Yes. The obvious one: P $\neq$ NP explains why we have not been able to solve all of those NP-complete problems any faster!" (Gasarch, 2014, p. 258) Thanks to Bill D'Alessandro for this example.

Also note the important parenthetical reference in the main text to IBE permitting relevant collateral evidence and background knowledge to override explanatory considerations. IBE is not best understood as some mechanical rule of confirmation. (This point has been emphasized even by van Fraassen (1989, pp. 145–146), a notable critic of IBE.) Rather, IBE involves considerations of explanatory quality serving as *a* guide (not the sole guide) to likeliness. Therefore, IBE requires that explanatory considerations be combined with other information, which sometimes outweighs explanatory considerations or even renders them irrelevant. Unfortunately, IBE is sometimes formulated as purporting to be a mechanical rule of confirmation. For instance, Boyce (2021) shows that collateral information can produce counterexamples to the following principle: If H is part of the "best explanation" of E and C is an indispensable part of that explanation, then E confirms C. However, such counterexamples are accommodated by IBE when IBE is understood in the way that I just mentioned.

mathematics (e.g., platonism, formalism, structuralism, Aristotelian realism, fictionalism,…) ought to recognize the distinction between explanatory and non-explanatory proofs in mathematics. These philosophical accounts (as well as various accounts of explanation in mathematics) may differ on what is required for the axioms of some expanded domain to yield an explanatory proof. At a minimum, whatever ontological status is being accorded the mathematical fact being explained, that status needs to be accorded the mathematical fact doing the explaining. What an IBE argument in mathematics can confirm is that the potential explainer is an explainer and is on an ontological par with what is being explained.

For instance, if the philosophical account takes real numbers to be platonistic abstract entities, then for the axioms of complex arithmetic to explain facts about the reals, the complex numbers must also all be platonistic abstract entities. Under platonism, then, part of what the IBE confirms is that there are imaginary numbers (as described by those axioms) as platonistic entities and hence on an ontological par with the reals in the two Taylor series in the explanandum. Furthermore, even a non-platonistic account must recognize a status that the axioms of complex arithmetic must have in order for them to explain real-number facts—a status that mathematical IBE's can confirm. For instance, suppose that the real-number facts and the axioms of complex arithmetic are facts about certain physical properties (or properties of those properties), as on Aristotelian realism (Franklin, 2008). Alternatively, suppose they are facts about certain fictional entities, as on fictionalism (where a given mathematical fact *p* is understood as roughly that if there had existed certain platonistic abstract mathematical entities, then *p* would have held of them).[4] Under these philosophical accounts, the truth of these mathematical claims is a relatively small achievement (at least compared to what their truth requires under platonism) and is even knowable without using IBE in mathematics. Nevertheless, more than their truth is needed for them to explain. For instance, the predicates figuring in the explanation must stand for mathematically natural properties. A fact involving a gerrymandered, "gruesome" (in the sense of Goodman's (1955) "grue"), or arbitrarily disjunctive property cannot explain since the entities possessing that property (or that would have possessed it, had those entities existed) are not similar in virtue of possessing it. No proof employing a non-natural property genuinely unifies the various cases falling under it, showing that they all arise in the same way; such a proof gives only the misleading veneer of unification, papering over different (natural) properties by using the same predicate to cover instances of all of them.[5] That the predicates figuring in the axioms of complex

[4] Let me emphasize that even a fictionalist should recognize explanations in mathematics. The fact that *p* would have been the case, had there been certain platonistic mathematical entities, can have explanatory proofs and non-explanatory proofs. (Of course, fictionalism interprets those proofs as proceeding from other mathematical facts understood in this same fictionalist way.).

[5] I am invoking here the distinction between what Armstrong (1978, pp. 38–41) and Lewis (1999, pp. 10–13) call "natural" (or "sparse") properties—that is, respects in which things may genuinely resemble one another—on the one hand, and mere shadows of predicates (i.e., "abundant" properties), on the other hand. For instance, an arbitrary disjunction of natural properties is not a natural property since, for instance, being five grams or positively electrically charged is not a genuine respect in which objects may resemble one another. For more on the distinction in mathematics between natural and non-natural properties, see Corfield (2003), Lange (2017), and Tappenden (2008).

arithmetic are mathematically natural properties can be confirmed by a mathematical IBE.

Mathematical IBE's thus have something to confirm even under if-thenist, fictionalist and other non-platonistic accounts of mathematics. For instance, consider the fact that if all complex numbers existed as abstract platonistic objects, then it would be *no coincidence* that the Taylor series for $1/(1 - z^2)$ and $1/(1 + z^2)$ have the same convergence behavior. The fictionalist can embrace this fact (concerning mathematical explanations) only if she regards certain complex-arithmetical properties as mathematically natural so that in both $1/(1 - z^2)$ and $1/(1 + z^2)$ possessing these properties (if various mathematical objects existed), these functions would be similar. (These properties include, for instance, becoming undefined at some point on the unit circle centered at the origin of the complex plane.)

IBE operates in mathematics if the radius-of-convergence theorem's potentially giving a nice explanation of the two series' having the same convergence behavior (over and above the theorem's *entailing* that behavior) makes it more plausible that the axioms of complex arithmetic *explain* this convergence behavior. IBE in mathematics thereby parallels IBE in science. IBE does not require that we go so far as to *believe* that the radius-of-convergence theorem explains the convergence behavior merely because the theorem figures in our best candidate explanation. (In this respect, "*inference* to the best explanation" is misleading terminology, as van Fraassen (1989, pp. 145–146) and others have remarked.) There may be insufficient evidence to warrant believing this explanation. IBE requires only that the quality of the potential explanation supply it with a credibility boost (under certain epistemic background conditions) and that the boost's size reflect how *well* the radius-of-convergence theorem would explain the convergence behavior. In this respect, IBE in math is like IBE in science; according to Lipton (2004, pp. 58–63, 121), IBE in science has us take the "loveliness" of the explanations that a hypothesis would supply (if true) as an important guide (but not our sole guide) to the likeliness of the explanation that the hypothesis would provide.

Let's compare this view of IBE in math to other views regarding the confirmation of mathematical conjectures. It has often been recognized that mathematics incorporates hypothetico-deductive and other forms of non-deductive reasoning. For example, Polya (1954) examines a host of mathematical cases involving induction by enumeration, analogical reasoning, and other kinds of plausibility arguments. Putnam (1975) sees positive instances as having confirmed the four-color map conjecture (before it was proved). Neither Polya nor Putnam, however, mentions IBE as among (what Putnam calls) the "quasi-empirical methods" in mathematics. In contrast, Kitcher (1984) briefly acknowledges IBE in mathematics; having proposed that all explanations (mathematical and scientific) involve unification, Kitcher takes IBE in math as a means of confirming the axioms that entail a given mathematical fact and thereby explain that fact by unifying it with the other facts that those axioms entail:

> We accept new axioms … because by doing so, we can derive a large number of antecedently accepted statements from a small number of statements. The ability to do this is important to us because the unification of a field enhances our understanding of it. (Kitcher, 1984, p. 218)

Maddy (1980) likewise mentions explanatory power as among the considerations that may support a given axiomatization of set theory. Mancosu (2008) takes this one step further; he interprets Feferman as "seem[ing] to think" that if results in one branch of mathematics (e.g., concerning the natural numbers) "which call for an explanation" have a potential mathematical explanation in terms of "more abstract entities" to which mathematicians are not already committed, then the potential explanation may supply "good reason to postulate such abstract entities and to believe in their existence" (Mancosu, 2008, p. 139). This is roughly the form of IBE (under a platonist interpretation) that I saw at work in the Taylor-series example.

Manders (1989) likewise investigates how mathematicians argued for the existence of imaginary numbers. These arguments (on Manders's view) appealed to the fact that when the domain of interest is expanded from the real numbers to the complex numbers, certain "solvability conditions" are satisfied (that is, all equations of a certain form have solutions) in all cases where solvability is consistent with certain other constraints (e.g., the commutativity of certain operations). Manders says that the simplification thereby achieved supplies greater "understanding" of the original domain. By this, he appears to mean that that the expanded domain exhibits greater unification because various uniformities hold there (e.g., all numbers have square roots, all quadratic equations have solutions) that had exceptions in the original domain. Manders does not unpack this greater understanding in terms of additional why questions being answerable in the expanded domain. On my view, the kind of "understanding" that Manders emphasizes should not be identified with mathematical explanation, and so the justification he alleges for domain expansion should not be identified with IBE. In fact, the justification he alleges for domain expansion does not seem very powerful; I see no obvious reason why the ontological status of some mathematical objects is confirmed by the fact that they would enable certain "solvability conditions" to be satisfied. I see no independent way to acquire evidence that those solvability conditions are always satisfied.[6] By contrast (as I will discuss in Sect. 4), in both science and mathematics it often happens that before we have any plausible account of why some known fact holds, we have good reason to expect that fact to have a certain kind of explanation. Of course, our expectation may turn out to be mistaken. But complex numbers cannot be responsible for the behavior of the reals unless they are on an ontological par with the reals—whereas the complex numbers' ontological status seems independent of whether they would enable some "solvability conditions" to be satisfied.

Some mathematical hypotheses have apparently been confirmed by enumerative induction. For instance, Goldbach's conjecture seems confirmed by the absence of counterexamples among the many numbers that have been checked. But such confirmation is vulnerable to the objection (Baker, 2007) that the examined cases are all numbers small enough for us to check (at least by computer). Confirmation of "All F's are G" by every examined F's having been G is weakened insofar as we believe that the examined F's form an unrepresentative sample of all F's. In contrast, as an argument for extending our ontological commitments to all complex numbers, IBE

[6] I take this to be part of the point behind Kitcher's (1989, p. 564) rhetorical question: "Can we assume that invoking entities that satisfy constraints we favor is a legitimate strategy of recognizing hitherto neglected objects that exist independently of us?"

does not face an analogous objection since IBE does not presuppose that the evidence constitutes a representative sample. (That the cheese I left on the table each recent night has disappeared is evidence of a mouse even if the recent nights are unrepresentative in, e.g., being the only ones where I left cheese on the table.)

As we have seen, one point of similarity between IBE in science and in math is that through each, we often justify extending the range of our ontological commitments beyond those involved in accepting the facts being explained. There is another important point of similarity. Just as we do not need to appeal to imaginary numbers in order to prove that the two Taylor series exhibit a given convergence behavior, so likewise for any scientific theory positing unobservable entities, there is a scientific theory having the same implications regarding observable facts but positing no unobservables (Hempel, 1965). However, just as the scientific theory that eschews unobservables lacks the explanatory power of the scientific theory positing them, so a pair of separate proofs (involving no imaginary numbers) of the two Taylor series' convergence behaviors fails to *explain why* the two series are alike in their behavior. Instead, the two separate proofs treat the series' similarity in convergence behavior as a "mathematical coincidence" —roughly speaking, as lacking a common explanation (Lange, 2010, 2017). Hence, that the series' convergence behavior can be deduced without appeal to imaginary numbers does not weaken the confirmation that the imaginary numbers' ontological status receives through IBE (since the imaginary numbers are indispensable to the convergence's "best explanation").[7]

The imaginary numbers' explanatory potential regarding facts about real numbers was important in justifying the belief that imaginary numbers stand on an ontological par with the real numbers. Of course, IBE was not the only kind of argument supporting an ontological commitment to complex numbers. However, the complex numbers' explanatory potential is especially powerful evidence for them.[8] That a theoretical posit in science is a calculational convenience may not be much evidence, since the posit could instead be merely a convenient *façon de parler*. Likewise, that complex numbers are mathematically useful in various ways may not count much. For instance, that complex numbers can be given a geometrical interpretation shows that they will not lead to contradiction, but it does little to confirm that they are ontologically on a par with the reals (Kitcher, 1989, p. 564). By contrast, the explanatory potential of complex numbers is more powerful evidence because complex numbers cannot be responsible for the behavior of the reals unless they are on an ontological par with the reals.[9]

[7] Mancosu (2008, p. 140) also points out that an explanatory proof cannot be replaced with a proof that avoids additional mathematical ontology without loss of explanatory power.

[8] The case of the two Taylor series is just one of many examples where facts about complex numbers beyond the reals were recognized as having the potential to explain facts exclusively about reals. For instance, Euler (*Elements of Algebra*, Part II, §193) saw that complex numbers could explain why the only integral solutions to the elliptic curve $x^3 = y^2 + 2$ are $(x,y) = (3, \pm 5)$.

[9] It might be objected that frictionless planes, ideal gases, and so forth can help to explain the behavior of actual physical objects even if they are not on an ontological par with those objects. In my view, however, the explaining is not done by facts about actual frictionless planes (obviously) or even by facts about what frictionless planes would have been like, had there been any. Rather, what explains are simply facts that certain actual physical objects are certain ways. Since their being those ways makes them (to a sufficient

3 Quinean IBE's, mathematical IBE's, and Field's Benacerrafian challenge

In this section, I will look at the contribution that an appeal to mathematical IBE's could make in responding to a notable epistemic challenge to platonism. However, I will be arguing that our recognition of IBE's role in mathematics cannot favor platonism over its rivals. Furthermore, platonism will not be my only concern; I will argue that an appeal to mathematical IBE's can make similar epistemic contributions in the context of other accounts of mathematics.

The "mathematical indispensability" argument associated with Quine (1951) and Putnam (1971) aims to show that we are justified in believing in the reality of various abstract platonistic mathematical objects. The argument is roughly that these objects are indispensible to our best scientific theories and so the empirical evidence for those theories thereby strongly confirms the reality of these objects. Recent versions of this argument (Baker, 2005, 2009; Colyvan, 2001, 2007) are more precise about the role that a mathematical object must play in a scientific theory in order for empirical evidence supporting the theory to confirm the object's reality: the mathematical object must play an indispensable *explanatory* role. This version of the indispensability argument portrays it as a form of IBE.

This "Quinean IBE" involves mathematical objects figuring in *scientific* explanations; the facts being explained are facts about concrete objects having causal relations and existing in spacetime. In contrast, the IBE's that I have been examining ("mathematical IBE") involve mathematical objects figuring in *mathematical* explanations; the facts being explained are mathematical facts. This difference makes mathematical IBE invulnerable to some of the most widely discussed objections to the Quinean IBE.

One objection (e.g., Shapiro (2000, p. 220), cf. Pincock (2012, p. 211)) is that mathematicians have been persuaded of the existence of imaginary numbers and other entities not by their scientific utility, but rather on grounds internal to mathematics. A mathematical IBE constitutes precisely such grounds. A second important objection is Field's (1980, 1989) argument that there are "nominalistic" reformulations of scientific theories—that is, theories having the same physical consequences as these theories when standardly interpreted, but not requiring the truth of the mathematical claims standardly employed. Hence, mathematics is not indispensable to scientific theories, and so the Quinean IBE does not get started. Field's objection does not apply to mathematical IBE's because the explanandum there is a mathematical fact, not a fact about physical objects. That physics can manage by treating mathematical claims as useful representational aids rather than as truth-apt does not show that mathematics can manage in this way. Field (1980, p. 13) argues that the truth of a mathematical claim, added to a nominalistically formulated physical theory, makes no physical predictions besides those made by the theory alone, and so Field concludes that if a physical theory can be formulated nominalistically, then empirical evidence cannot discriminate between that theory and one supplemented by commitment to mathematical objects. The analogous argument regarding a mathematical theory rather than

Footnote 9 continued
degree of approximation) like frictionless planes, it is helpful to describe those objects in terms of what frictionless planes would be like.

a scientific theory is a non-starter: empirical evidence is not the principal court of appeal in mathematical practice, and a mathematical proposition, added to some other mathematical propositions, can obviously enable additional mathematical predictions to be made.

A third reply often made to the "Quinean IBE" (e.g., Bueno, 2009, Melia, 1998; Baker and Colyvan 2012 replies to it) is that even if mathematics is indispensable to the explanations supplied by our best scientific theories, it does not play the right kind of role in those explanations for the empirical evidence supporting those theories to confirm the existence of mathematical objects. Mathematics serves only as a descriptive or representational aid. As Melia (1998, pp. 70–71) argues, "mathematics is used simply in order to make more things sayable about concrete things. And it scarcely seems like a good reason to accept objects into our ontology simply because quantifying over such objects allows us to express more things." But even if we grant that in scientific explanations, mathematics serves only to assist us in making claims about physical objects, this objection to the Quinean IBE does not apply to mathematical IBE's.[10] In science, the physical world is available to be what mathematics serves as an aid in representing. But there is no obvious candidate for what mathematics serves as a mere tool for representing in purely mathematical work. Even if mathematics in a *scientific* explanation serves merely as an aid in representing facts about physical objects, its role in an explanation in *mathematics* must be entirely different (or else the explanation would not succeed); as merely a tool for representing physical facts, mathematics cannot explain why some mathematical theorem holds. For facts about complex numbers to genuinely explain facts about reals, complex numbers must be on an ontological par with reals.

However, although mathematical IBE's avoid some widely discussed objections to the Quinean IBE, they also encounter an obstacle that the Quinean IBE does not face. The Quinean IBE concerns explanations of facts about physical objects—frequently, about observable objects—and those objects' existence is taken for granted. The Quinean IBE aims to show that we ought to have a similar ontological commitment to the platonistic mathematical objects purportedly posited by the explanations of facts about physical objects. In contrast, mathematical IBE's concern explanations of mathematical facts. In the Taylor-series example, the issue is whether to extend our ontological commitments from the reals to complex numbers. So under platonism, for instance, this mathematical IBE takes for granted the real numbers' existence as platonistic mathematical objects (figuring in the Taylor series in the explanandum). But we cannot take for granted the existence of any abstract mathematical object if the issue is whether to be mathematical platonists or non-platonists of some kind. So even if a mathematical IBE justifies our according the same ontological status to complex numbers as to reals, it cannot show that we are justified in believing in the existence of

10 One way to elaborate this objection to the Quinean IBE is that mathematical objects are not serving as *causes* according to the proposed scientific explanation and so the evidence for that explanation does not confirm their existence. This objection to the Quinean IBE would apply to the mathematical IBE since the mathematics there is not describing causes of the explanandum (a purely mathematical fact having no causes). However, this is not a good objection. Evidence for a scientific explanation often counts as evidence for non-causes it posits. For instance, laws of nature (and features of spacetime structure) are not causes, are often posited by putative scientific explanations, and are often confirmed by evidence for the explanation.

complex numbers as abstract objects unless platonism has already been independently established regarding the reals. In short, whereas the Quinean IBE aims to argue for platonism, we cannot appeal to mathematical IBE's in order to argue for platonism over its rivals without begging the question.

The same point applies even if we are not platonists. Whatever our philosophical account of mathematics, a mathematical IBE aims to justify the *expansion* of mathematical ontological commitments from one domain to another. But it cannot tell in favor of what particular ontological commitment's scope is being expanded; that is, it cannot tell us whether we should be platonists or fictionalists or Aristotelian realists or… regarding the mathematical domain from which the expansion is taking place. (Recall from Sect. 1 that even fictionalists must regard the acceptance of an explanation in mathematics as involving a certain kind of mathematical ontological commitment: to the naturalness of certain mathematical properties.)

However, even if a mathematical IBE can underwrite only an expansion in our ontological commitments from one mathematical domain to another, we should not underestimate its importance. For example, a platonist might maintain that some relatively narrow range of platonistic mathematical objects are epistemically accessible by Quinean IBE's, or by rational insight (Bonjour, 1998), or by direct perception (Maddy, 1980, 1990), or by some other means. Other kinds of mathematical objects, however, are too remote to be accessible in any of these ways. This problem could be addressed by mathematical IBE's underwriting the expansion of mathematical ontological commitments from our independent knowledge of some narrow range of platonistic mathematical objects. Without mathematical IBE's, many parts of higher mathematics that play no indispensable role in any scientific explanations, that even persons with mathematical training cannot access by perception, and into which we cannot have rational insight would become epistemically inaccessible. Mathematical IBE's would then nicely complement other means of acquiring knowledge of mathematical abstract objects.

Analogous points apply under non-platonistic accounts. For example, an Aristotelian realist view (Franklin, 2008), according to which mathematics concerns properties of (actual or merely possible) physical objects (and properties of those properties, and…), faces the epistemic challenge that many mathematical properties seem too far removed from what we can observe for us to have good reason to recognize them. But the "lovely" mathematical explanations that these properties would allow us to give can (by mathematical IBE) provide evidence of instances of these properties. In the same way, fictionalism and if-thenism should regard mathematical IBE's as helping to justify mathematicians in expanding the range of mathematical properties they regard as natural. In this way, even under non-platonistic interpretations of mathematics, mathematical IBE's play crucial roles—even if mathematical IBE's presuppose our having independent grounds for the ontological commitments (of the relevant non-platonistic sort) regarding the mathematics figuring in the facts being explained by the explanations that IBE confirms. Mathematical IBE's expand the range of justified ontological commitments in mathematics just as in science, IBE's justify scientists in expanding their justified beliefs from observable facts to propositions concerning the unobservables explaining those facts. Both platonism and non-platonistic accounts are more defensible if they recognize mathematical IBE's because doing so allows them to account for some of our mathematical knowledge.

To appreciate that mathematical IBE's could still play a crucial role in platonism's defense even if they presuppose an independent argument for platonism regarding at least some initial, narrow range of mathematics, I will look at one very prominent argument regarding platonism. In particular, I will argue that an appeal to mathematical IBE can help us respond to Field's version of Benacerraf's (1973) dilemma regarding mathematical knowledge. Field (1989, p. 26) says:

> Benacerraf's challenge … is to … explain how our beliefs about these remote entities can so well reflect the facts about them. … *[I]f it appears in principle impossible to explain this*, then that tends to *undermine* the belief in mathematical entities, *despite* whatever reason we might have for believing in them.

Field's point is that it counts against the plausibility of a certain body of beliefs that our (alleged) tendency to be reliable in those beliefs is in principle impossible to explain, and the abstract character of platonistic objects suggests that it would be in principle impossible to explain our reliability regarding them. In contrast, even before we had an account of how our perceptual beliefs are reliable, there was no in-principle obstacle to such an account, since the objects of our perceptual beliefs enter into causal relations with us. But

> a realist view of mathematics involves the postulation of a large variety of aphysical entities – entities that exist outside of space-time and bear no causal relations to us or anything we can observe – and there just don't seem to be any mechanisms that could explain how the existence and properties of such entities could be known. (Field, 1989, p. 230)

On Field's view, then, there is an in-principle obstacle to explaining any tendency on our part to have true beliefs regarding mathematical objects.

It seems to me that natural selection might explain why we have a tendency to have true beliefs regarding certain very elementary mathematical facts. The tendency to form correct mathematical beliefs of this kind is selectively advantageous. If a creature sees two dangerous predators in front of her and three more behind her, then she is more likely to survive long enough to have offspring if she believes that $2 + 3 = 5$ (and so concludes that there are five dangerous predators in total) than if she believes that $2 + 3 = 0$ (and so concludes that there are no dangerous predators in total). That a mental faculty for arriving at such elementary mathematical truths would enhance fitness seems indisputable, and that it has actually been selected for seems plausible. As Sinnott-Armstrong (2006, p. 43) says pithily: "People evolved to believe that $2 + 3 = 5$, because they would not have survived if they had believed that $2 + 3 = 4$…. The same goes for the belief that wild animal bites hurt."[11]

[11] Clarke-Doane (2012, p. 314, 326) gives more references to biological and philosophical literature on natural selection for a disposition to generate true simple mathematical beliefs. Pincock (2012, pp. 297–298), in taking IBE in mathematics as having supported expansions in the range of mathematical domains to which mathematicians have justified commitments (see note 1), also proposes a non-IBE rationale for the commitments to (e.g.) ordinary arithmetic from which the expansion takes place.

I acknowledge that the same selectively advantageous work performed by these elementary beliefs about mathematical objects could have been performed instead by beliefs that are not made true by platonistic objects. According to Clarke-Doane,

> for any mathematical hypothesis that we were selected to believe, H, there is a nonmathematical truth corresponding to H that captures the intuitive reason that belief in H was advantageous… . By *nonmathematical truth* I mean … roughly, a truth that does not imply the existence of a relevantly mind-and-language independent realm of mathematical objects. When H is an elementary arithmetic proposition, such as that 1+1 =2, the relevant truths will typically be (first-order) logical truths regarding objects in our environments (it is conceivable that they would also sometimes be mereological or impure set-theoretic truths regarding such objects). (Clarke-Doane, 2012, p. 332).

Perhaps this shows that a creature could have derived the same selective benefits without having elementary beliefs about fairly small numbers—as long as she had various beliefs about logical (or other kinds of) truths about physical objects and she could employ those beliefs as easily as we employ elementary beliefs about fairly small numbers. But that is not immediately relevant to responding to Field's challenge. Field's challenge grants for the sake of argument that we have beliefs about platonistic objects and argues that there is an in-principle obstacle to explaining their reliability. Field (1989, pp. 26–27) likens his challenge to the challenge we would issue to someone with beliefs "about the daily happenings in a remote village in Nepal" and who claims these beliefs to be nearly all true "despite the absence of any mechanism to explain the correlation between those belief states and the happenings in the village." That challenge grants that the individual has beliefs about the village. Insofar as such an explanation is in principle impossible, there is reason to doubt that we have reliable beliefs regarding the remote entities, since our reliability is not plausibly a brute fact. One way to meet Field's challenge, at least regarding elementary beliefs about fairly small numbers, is to point out that if our ancestors were guided by their beliefs about fairly small numbers, then they were likely to do better if their beliefs were true than if their beliefs were false. Therefore, if a tendency to form true beliefs of this kind is heritable, then there was plausibly selection for it. This selectionist explanation would be an explanation of the kind that Field suspects is impossible in principle. It helps to be right about the platonistic objects if we are using those beliefs to guide our interactions with concrete objects—even if those interactions would have been just as successful if we had instead been guided by certain beliefs that do not concern platonistic objects (so that if we had used those other beliefs instead, then our being right about the platonistic objects would have made no difference to our success).

In other words, this selectionist explanation is not undermined by the fact that a creature without mathematical beliefs, but with various true nonmathematical beliefs (of the sort Clarke-Doane mentions), could have thereby derived the same selective benefits that she would have derived from having true elementary beliefs about fairly small numbers. Let's put the point contrastively. The selectionist explanation relevant to responding to Field's challenge explains why we have true *rather than false* elementary beliefs about fairly small numbers (by having beliefs like $2 + 3 = 0$). It does not purport to explain why we have those true beliefs *rather than no beliefs* *at all* about numbers but instead various nonmathematical beliefs (of the sort Clarke-Doane mentions). This specificity to a particular contrast class is typical of selectionist explanations. For instance, a simple selectionist explanation might explain why some butterfly species has camouflage *rather than* no protective coloration or any other device to discourage predation, despite not explaining why the butterfly has camouflage *rather than* a bad taste or some other device to discourage predation. A selectionist account's failure to answer a why question involving the latter contrast class does not prevent it from answering a why question involving the former contrast class.

The selectionist account does not show that our elementary beliefs about fairly small numbers are true. As far as this argument is concerned, those beliefs could be false—not because $2 + 3 = 0$, but rather because they posit abstract mathematical objects that do not exist. That our ancestors were nevertheless successful when they were guided by those beliefs could be explained by nonmathematical truths of the sort that Clarke-Doane mentions. (That is his point in mentioning them.) Even so, the selectionist account can address Field's challenge as far as elementary beliefs about fairly small numbers are concerned (though see just below). That challenge begins by presupposing that we have beliefs about these mathematical abstracta and that there are such things (just as someone might have beliefs about a remote Nepalese village that actually exists). The challenge is to explain how (except by unlikely good fortune or as a brute fact) we managed to capture the facts about these mathematical things. The mechanism I have mentioned is natural selection: a false belief about these things would be selectively disadvantageous (in the absence of a true non-mathematical belief that takes over its role in guiding action).

I do not mean to suggest that this appeal to natural selection suffices as it stands to explain why we have true beliefs about "remote" platonistic entities. There remains the matter of *explaining why* it is selectively beneficial for us to have accurate rather than inaccurate beliefs about the small real numbers taken as platonistic entities. Why should our projects tend to go better if we get the facts right about platonistic entities (any more than our projects would tend to go better if our beliefs about a remote Nepalese village were correct rather than incorrect)? Platonism needs to give an account of how ordinary physical objects relate to these abstract entities (perhaps modeled on how particulars relate to the universals they instantiate).

The point of this excursion into selectionist explanations is that nothing like such an account could respond to Field's challenge regarding mathematical facts *beyond* the most elementary arithmetical and geometrical ones. Our remote ancestors had no beliefs about imaginary numbers or projective geometry's points at infinity—and even if they had had such beliefs, their accuracy would have bestowed no selective benefits in our ancestors' rude conditions. So the reliability of our beliefs regarding those objects cannot be explained by the selective advantages thereby afforded. By contrast, that our beliefs regarding these mathematical objects are arrived at by mathematical IBE's might explain why our beliefs regarding these mathematical objects are so accurate. This explanation (as we saw earlier) presupposes some independent account of the accuracy of our beliefs regarding the explanandum. We have just seen a possible selectionist account of our accuracy there. But even with that selectionist account, it remains crucial to add (in replying to Field's challenge) that we used mathematical IBE's to arrive at some of our mathematical beliefs (such as those concerning imaginary numbers). The selectionist account cannot be generalized to cover the accuracy of our beliefs regarding the mathematical objects figuring in many of our best mathematical explanations.

In short, an account appealing to mathematical IBE's would perfectly complement a selectionist account. That mathematical IBE's (unlike the Quinean IBE), even if successful, could underwrite no more than *expansions* in our ontological commitment from (under platonism) some mathematical objects to others does not make an appeal to mathematical IBE's dispensable. Rather, it might play an important role in accounting (under platonism) for our mathematical knowledge. An appeal to mathematical IBE's appears to pair nicely with arguments (such as the selectionist account) that are applicable only to a very narrow range of mathematical objects—but a range that includes the mathematical objects figuring in the explananda of some mathematical IBE's.

Field himself briefly mentions a possible selectionist reply to his version of Benacerraf's dilemma:

> …the idea would be that evolutionary pressures (biological and/or cultural ones) have led us to find initially plausible those mathematical claims which are empirically indispensable, and that this gives all the explanation of the correlation between our judgements and the mathematical facts that we should want. (Field, 1989, p. 29)

He says that he's "suspicious about this line of response…to the Benacerrafian challenge" (p. 29). One of his "doubts" about it

> is that the amount of mathematics that gets applied in empirical science (or indeed, in metalogic and in other areas where mathematics gets applied) is relatively small. This means that only the reliability of a small part of our mathematical beliefs could be directly explained by the proposal of the previous paragraph. To be sure, one could try to use the reliability of our beliefs in this relatively small part of mathematics to 'bootstrap up' to the reliability of larger parts, by hypothetico-deductive inference within mathematics…But I think that there is substantial room to doubt that such inferences are all that powerful: too many different answers to questions about, say, large cardinals or the continuum hypothesis or even the axiom of choice work well enough at giving us the lower level mathematics needed in science and elsewhere. (Field, 1989, p. 29)

Of course, such bootstrapping is exactly what I am suggesting—but by mathematical IBE rather than hypothetico-deductively. By requiring the *explanation* of some fact, not merely its deduction, IBE supplies a stronger reason for extending our ontological commitments than hypothetico-deductivism can—whether in science or in mathematics. Furthermore, because explanation is a higher bar than deduction, we can grant Field's remark that several different answers to questions about certain mathematical objects such as large cardinals "work well enough at giving us"—at entailing—"the lower level mathematics needed in science and elsewhere". It does not follow that these answers all supply equally good *explanations* of that mathematics. It is a familiar feature of science that explanatory power helps to resolve underdetermination of theory by evidence (e.g., Kitcher, 1993, p. 155). For instance, Tychonic and Copernican astronomical models worked equally well at predicting relative planetary positions (the models being empirically equivalent in this regard), but the Copernican model was enormously superior in its explanatory potential (Kitcher, 1993, p. 211, 254–255). Of course, it could turn out that several different answers to questions about large cardinals would (if true) supply equally "lovely" explanations. In that case, perhaps we must remain forever ignorant of the facts about those objects.

An appeal to mathematical IBE, then, is not made dispensable by a selectionist reply to the "Benacerrafian challenge". Rather, mathematical IBE's strengths and weaknesses nicely complement those of a selectionist account. Part of Benacerraf's challenge is to understand how platonistic mathematical objects could be epistemically accessible to us. Mathematical IBE might give us one means of access to them.

4 Why is explanatory quality a good guide to truth?

According to the previous section, the fact that mathematicians use IBE to expand the range of their ontological commitments could help to explain why the beliefs at which they arrive in this way are so accurate. As we saw, this explanation requires some account of mathematicians' accuracy in their prior range of ontological commitments (whether to platonistic entities or not); that is, it requires some explanation of mathematicians' accuracy in their ontological commitments regarding the mathematical facts that mathematicians are trying to explain (e.g., a fact about real-number Taylor series). Additionally, for mathematicians' use of IBE in expanding their range of ontological commitments to help explain why the beliefs at which they arrive are so accurate, something would need to explain why beliefs arrived at by mathematical IBE's tend to be accurate. This is the challenge I will address in this section.

We face the same challenge regarding scientific IBE's: Why does a scientific theory's power (if that theory is true) to nicely explain a given fact, over and above the theory's entailing (or probabilifying) that fact, make that theory more likely to be true (in the absence of collateral information that overrides explanatory considerations)? Of course, some philosophers (e.g., van Fraassen, 1980, 1989) have denied that a scientific theory's potential explanatory power supplies evidence of its truth. But this is a minority view. Let's see whether an explanation of scientific IBE's reliability might carry over to mathematical IBE's.

One promising account of why in science the power to give nice explanations is a fairly good guide to truth begins by noting that even before we have found a plausible explanation of some fact, we often believe justly and accurately that the fact has an explanation and we often justly have a fairly accurate idea of its explanation's general features. These are the features that we regard in this case as contributing to a potential explanation's quality—what Lipton (2004) calls its "loveliness". For instance, suppose that our car suddenly starts to make weird sounds, to emit smoke, and to stall frequently. Even if we have no idea precisely why it is doing these things, we typically have good reason to believe that there is some explanation (i.e., that these are not brute facts) and that the explanation ascribes these various phenomena to a common cause. Having this feature would then help make a proposed explanation "lovelier" (better). Of course, we cannot be certain that the events have a common-cause explanation; the reason for the stalling might in fact have nothing to do with the reason for the sounds, and these two reasons might have just coincidentally appeared simultaneously. But often in cases like this, the various symptoms have a common cause—and we have good reason to believe that they do before we have identified that cause. That reason derives from our past experiences with various other, similar events (not necessarily involving cars or even other manufactured objects) and the kinds of explanations that we have discovered those events to have.

By the same token, if various languages share some feature, then we often have good reason to expect that this commonality has an explanation and, in particular, that it arises from some other feature that those languages share (such as a common ancestral language, external influence, or physiological feature shared by their speakers). We have good inductive evidence from our discoveries of explanations in other, similar cases that the languages' commonality is likely to have a common explanation. (Once again, this expectation on our part could turn out to be mistaken, but often it is not.) IBE is a good guide to the truth in such cases because we tend to have good inductive grounds for some accurate expectations regarding the explanation of the languages' similarity, and those general features that we thereby expect the explanation to possess are the traits that we regard in this case as enhancing a potential explanation's loveliness. In the case of the languages, we justly regard a potential explanation as lovelier insofar as it attributes a similarity in the languages to a common explainer.[12]

I have focused on this particular loveliness-enhancing feature because it is also exhibited by the mathematical explanation we saw earlier: the reason why the two Taylor series have the same convergence behavior. That explanation attributes this similarity to another similarity in the two functions undergoing Taylor expansion: that they both become undefined at the same distance from the origin of the complex plane.[13] Before mathematicians knew this explanation, they had good reason for having considerable confidence that the two series' common convergence behavior has an explanation that traces this similarity to some other, as yet unknown similarity in the two functions. That is because mathematicians had already discovered a great many similar mathematical facts to have mathematical explanations of this sort. This evidence confirmed the existence of a similar sort of explanation in the case of the two Taylor series and made this feature loveliness-enhancing in this case. Before they discovered the explanation of the two Taylor series' common convergence behavior, mathematicians had expectations regarding what sort of explanation there is, and those expectations were justified in an internalist sense: the evidence supporting these expectations (consisting of other mathematical explanations) was epistemically available to mathematicians.

[12] Lange (forthcoming3) presents an approach along these lines as justifying IBE in science.

[13] My own account of explanation in mathematics (Lange, 2014, pp. 506–507; 2017, pp. 254–268; 2018, pp 1296–127; 2019, pp. 12–13; forthcoming1:3) emphasizes that many (though not all) explanatory proofs derive their explanatory power by tracing a salient similarity among the cases in the explanandum back to some analogous similarity identified by the explanans. In this paper, I have no need to presuppose that my account correctly identifies the source of these proofs' explanatory power. It suffices that this feature is often loveliness-enhancing.

Here is an example of the kind of confirming evidence supplied by other mathematical explanations.[14] Take an ordinary calculator keyboard (see figure).

7	8	9
4	5	6
1	2	3

We can form a six-digit number by taking the three digits on any row, column, or diagonal on the keyboard in forward and then in reverse order. For instance, the bottom row taken from left to right, and then right to left, yields 123,321. There are sixteen such "calculator numbers": 123,321; 321,123; 456,654; 654,456; 789,987; 987,789; 147,741; 741,147; 258,852; 852,258; 369,963; 963,369; 159,951; 951,159; 357,753; and 753,357. As you can easily verify (with a calculator!), every one of these numbers is divisible by 37. Is this "calculator-number regularity" a coincidence (as the title of a recent *Mathematical Gazette* article asks)?[15] In other words, does the calculator-number regularity have an explanation and, if so, what is it?

It turns out to have an explanation given by a proof that proceeds from a property shared by each of these numbers precisely because they are calculator numbers. Here is such an explanation:

> [The calculator-number regularity] is *no* coincidence. For let a, a + d, a + 2d be any three integers in arithmetic progression. Then
> $a.10^5 + (a + d).10^4 + (a + 2d).10^3 + (a + 2d).10^2 + (a + d).10 + a.1$
> $= a(10^5 + 10^4 + 10^3 + 10^2 + 10 + 1) + d(10^4 + 2.10^3 + 2.10^2 + 10)$
> $= 111111a + 12210d = 1221(91a + 10d).$
> So not only is the number divisible by 37, but by 1221 (= 3 x 11 x 37) (Nummela, 1987, p. 147)

This proof exploits the fact that every calculator number can be expressed as $10^5a + 10^4(a + d) + 10^3(a + 2d) + 10^2(a + 2d) + 10(a + d) + a$ where a, a + d, a + 2d are three integers in arithmetic progression. These three integers, of course, are the three digits on the calculator keypad that, taken forwards and backwards, generate the given calculator number. Hence, this explanation traces the calculator-numbers' similarity in being divisible by 37 to another property they share. It is a lovely explanation.

Mathematicians have discovered many similar cases where the reason why apparently disparate mathematical cases possess a common property turns out to be because of some other commonality running through them. Considering this evidence, mathematicians (even mathematics students reading Spivak's textbook and drawing on their own mathematical experience) are justified in expecting the common convergence behavior of the two Taylor series to be explained by some other (as yet unidentified) feature shared by the two functions. Therefore, a potential explanation is lovelier by virtue of attributing the common convergence behavior to another property that the

[14] In my presentation of this example, I closely follow my previous discussions of it (Lange, 2010, pp. 308–309; 2014, pp. 488–489; 2017, pp. 276–279, 286, 353–356; forthcoming2:15–16). I originally learned about it from Roy Sorensen.

[15] The article appears (unsigned, as a "gleaning") on p. 283 of the December 1986 issue.

two functions share. (Once again, this explanation of mathematical IBE's accuracy does not presuppose platonism; even a fictionalist account would have to recognize mathematicians as being justified in drawing on their prior knowledge of mathematical explanations to inform their expectations about as yet undiscovered explanations.)

In sum, mathematical IBE is a good guide to the truth because mathematicians are frequently able to anticipate accurately which mathematical facts have explanations and what sorts of explanations they have. These expectations tend to be accurate because they are arrived at inductively from mathematicians' knowledge of other mathematical explanations. These expectations inform judgments of which potential explanations are lovelier.

Of course, since mathematicians arrive at their expectations inductively, those expectations sometimes turn out to be mistaken; for instance, mathematical facts that appear likely to have explanations sometimes turn out to have none.[16] Furthermore, since this account of mathematical IBE's reliability presupposes the reliability of a different kind of ampliative reasoning in mathematics, it is perhaps not a full explanation of mathematical IBE's reliability. However, to solve the Humean problem of induction (whether in science or in mathematics) is certainly beyond the scope of this paper! Moreover, in explaining mathematical IBE's reliability by appealing to the reliability of another kind of inductive reasoning, this proposal is not unique to mathematics; a similar account could be given of why scientific IBE is reliable.[17] For an account to make mathematical IBE's reliability no more mysterious than the reliability of other forms of ampliative reasoning in mathematics—and no more mysterious than scientific IBE's reliability—constitutes some progress toward explaining why mathematical IBE is reliable. It makes IBE's reliability in mathematics considerably less mysterious than it would be for tarot cards, for instance, to be reliable in mathematics.

This proposal for explaining why IBE tends to lead toward the truth in math and science might appear to suggest that IBE can be deployed much less often in mathematics than it can in science. In science, we believe that virtually all facts have explanations; only the fundamental laws of nature and perhaps certain other, very special facts (perhaps the dimensionality of spacetime or the universe's initial conditions) are brute. In contrast, it may well be that only certain very special mathematical facts have explanations. An ordinary mathematical fact may have a derivation from the relevant axioms,

[16] For example, it is just a coincidence that 31, 331, 3 331, …, and 33 333 331 are all prime. (The next number in the sequence is composite.) Similarly, consider these two Diophantine equations (that is, equations where the variables can take only integer values):

$2x^2(x^2 - 1) = 3(y^2 - 1)$

and

$x(x - 1)/2 = 2^n - 1.$

As it happens, each equation has exactly the same five positive solutions for x: 1, 2, 3, 6, and 91 (Guy, 1988, p. 704). Mathematicians believe that this is just a coincidence—that there is no explanation. (I gave this example in Lange (2010, p. 316; 2017, pp. 278–279, 289; forthcoming2:9).).

[17] My proposed account of why potential explanatory loveliness is a good guide to truth (see note 12) is in the spirit of Sober's (1990) account of why simplicity is a good guide to truth: because the background beliefs that guide us when we are being guided by simplicity are true. (For instance, simpler phylogenetic trees posit fewer mutations, and we know that mutations are rare, so in being guided by simplicity in arriving at phylogenetic trees, we tend to be guided toward the truth.) The accuracy of those background beliefs is, in turn, no great mystery (bracketing Hume's problem) considering that they were arrived at inductively.

but perhaps that derivation is a mathematical explanation only in special cases. Perhaps the only mathematical facts that have explanations (over and above proofs) are mathematical facts exhibiting some striking feature—those revealing some striking symmetry, for instance, or identifying something shared by otherwise apparently disparate cases (as in the calculator-number example).[18] For instance, it may make no sense to ask why 31 is prime (insofar as this question asks for something over and above a proof that it is prime), whereas it may make sense to ask why 31, 331, 3331, 33,331, …, 33,333,331 are all prime (where this question asks for something other than a separate proof of each number's primeness). If any of this is correct, then the cases where mathematicians are able to use their past experience to confirm that a given mathematical fact has an (as yet unknown) explanation may be very special, whereas for nearly every fact that scientists encounter, they are entitled to expect it to have an explanation.

I can embrace this line of thought; perhaps IBE can be deployed less often in mathematics than in science. But this difference should not be exaggerated. For one thing, it does not show that when IBE is applicable in mathematics, it is typically weaker than IBE in science. Even if a mathematical fact must be pretty special for there to be a distinction between its explanation and its mere proof, nevertheless for such a mathematical fact (for which no explanation has yet been found), there may typically be significant evidence that it has an explanation with various features. By IBE, a mathematical conjecture might then derive some significant credence from its potential to offer such a lovely explanation. Furthermore, even if nearly every scientific fact has an explanation, it may be that only special scientific facts have explanations with various special properties. For instance, the fact that Mars, Jupiter, and Saturn all exhibit retrograde motion in the night sky has an explanation involving a common cause: that Earth orbits the Sun inside these other planets' orbits. But there are a host of facts about the solar system having no unifying explanation, such as Mars's, Jupiter's, and Saturn's masses (or their volumes, axial rotation rates, numbers of moons, distances, and even existences). Perhaps in both science and math, certain further evidence must be present for IBE to underwrite awarding greater credence to hypotheses that would (if true) supply explanations involving common explainers.

Another apparent difference between IBE in science and math concerns the same fact having multiple explanations. In science, it might seem, a given fact typically has a single explanation. For instance, suppose that my car's sudden tendencies to stall, to smoke, and to produce weird sounds have a common cause in some malfunction somewhere in the car's internal mechanism. Once we have found that explanation, we do not continue to seek other explanations of the car's behavior; having accounted for the car's symptoms, there is no call for another explanation of those symptoms. In contrast, even after some mathematical fact has been explained, mathematicians standardly seek and welcome additional explanations of it. New explanatory proofs can provide new sorts of payoffs, including new theorems and proof strategies and new insights. Mathematical IBE can award greater credence to a mathematical conjecture

[18] This is true on my account of explanation in mathematics (see note 13). Recall Spivak's (1980, p. 482) remark (quoted in Sect. 1) acknowledging that a mathematical fact may turn out to have no explanation.

by virtue of its power (if true) to nicely explain some fact even after that fact has already been explained.

I am not convinced that this apparent difference between scientific and mathematical IBE is a genuine difference. I agree that mathematicians seek new proofs (even new explanations) of theorems that have already been explained. But oftentimes scientists also seek additional explanations of facts that have already genuinely been explained—and for many of the same reasons as mathematicians do. For instance, in science, a fact that has been explained relative to one contrast class may not yet have been explained relative to another contrast class. To use a canonical example: that Jones had latent untreated syphilis and Smith did not might explain why Jones rather than Smith got paresis, but in view of the rarity of paresis even among those with latent untreated syphilis, we might still want to know why Jones got paresis rather than not contracting it. When mathematicians welcome multiple explanations of the same fact, the different explanations may place the fact in different contrast classes. For instance, an account of why a given method works rather than fails to solve a given problem may not explain why the method works in that case and fails in an apparently similar case rather than working in both.

Here is another example.[19] Let the explanandum be the fact that the derivative at $x = a$ of the sum of infinitely many terms, each differentiable at $x = a$, does not always equal the sum of the terms' derivatives at $x = a$. A proof may explain why this result obtains rather than the derivative of the sum always equaling the sum of the derivatives. But even with this proof, we may still not have explained why the sum of *infinitely* many terms is different in this respect from the sum of *finitely* many terms (which always equals the sum of the terms' derivatives). Different contrast classes call for different explanations, whether in science or in math.

Besides the differences between contrast classes, there are many other reasons why science sometimes seeks multiple explanations of the same fact. One explanation may be "deeper" than another. To explain why a given gas's pressure rose, we may cite the fact that the gas was heated while its volume was held fixed and the law that any gas's pressure rises when it is heated while its volume is held fixed. This explanation does not keep us from welcoming a deeper explanation, such as one that replaces the law with the kinetic-molecular theory of gases and the dynamical theory of heat. This deeper explanation connects the gas's behavior to other gas phenomena explained by the same two theories. Different explanations of the same mathematical fact may likewise reveal the explanandum's connections to different sets of other facts, perhaps from different branches of mathematics. This is an important reason why one explanation of a given theorem does not exclude another.

In addition, a mathematical fact used to explain some theorem may itself be explained by another explanation of that theorem. Similarly, even when we have already identified the malfunction in the car's internal mechanism that explains the car's symptoms, we may still use IBE to award additional credence to a theory by virtue of its power (if it is true) to explain nicely why this malfunction in the car's internal mechanism occurred and hence why the symptoms occurred. In both math

[19] In (Lange, 2018, pp. 1288–1290), I gave this example for a different purpose.

and science, IBE can be used to judge among rival potential additional explanations of a fact for which an explanation has already been found.

5 Conclusion

I have argued that mathematical IBE's can support an expansion in the range of mathematicians' ontological commitments, just as scientific IBE's can support an expansion in the range of scientists' ontological commitments. Both platonist and non-platonist accounts of mathematics should recognize IBE as playing this role in mathematics, even if these accounts disagree about mathematicians' proper ontological commitments.

For instance, Newton used an IBE to confirm that there exists a single force that is responsible for both keeping the Moon in its orbit and causing a body near Earth's surface to fall. Newton noticed that the strength (per unit mass) of the downward force on a terrestrial falling body is the same as the strength (per unit mass) of the Earth-directed force causing the Moon's motion once the force on the Moon is extrapolated (by its posited inverse-square dependence on distance) to its strength at the Earth's surface. Newton believed that the best explanation of the two forces' having the same strength is that they are the same force:

> And therefore that force by which the Moon is kept in its orbit, in descending from the Moon's orbit to the surface of the earth, comes out equal to the force of gravity here on earth, and so … is that very force which we generally call gravity. (Newton, 1999, p. 804)

The best explanation of this similarity between the forces on the Moon and on terrestrial falling bodies is that they have a common explanation: they are the same force.

Just as Newton used IBE to justify adding the universal inverse-square gravitational force to our ontology, so likewise (I have argued) IBE is used in mathematics to confirm that new elements should be added to mathematical ontology. The two Taylor series I have discussed have the same convergence behavior just as the two forces discussed by Newton have the same strength. The existence of the universal inverse-square gravitational force that Newton posited is confirmed by its potential to provide a lovely explanation of the fact that the forces on the Moon and on terrestrial falling bodies have the same strength. Likewise, that all complex numbers exist on an ontological par with the reals (whatever that "par" ontological status is) is confirmed by the potential of complex arithmetic to provide a lovely explanation of the fact that the two Taylor series have the same convergence behavior. In each case, IBE underwrites our regarding the evidence as confirming the existence of a common explainer of what would otherwise be a puzzling coincidence.

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