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Abstract	<p>In mathematics, the deductive method reigns. Without proof, a claim remains unsolved, a mere conjecture, not something that can be simply assumed; when a proof is found, the problem is solved, it turns into a “result,” something that can be relied on. So mathematicians think. But is there more to mathematical justification than proof?</p> <p>The answer is an emphatic yes, as I explain in this article. I argue that non-deductive justification is in fact pervasive in mathematics, and that it is in good epistemic standing.</p>	
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Non-deductive Justification in Mathematics

A. C. Paseau

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Abstract

In mathematics, the deductive method reigns. Without proof, a claim remains unsolved, a mere conjecture, not something that can be simply assumed; when a proof is found, the problem is solved, it turns into a “result,” something that can be relied on. So mathematicians think. But is there more to mathematical justification than proof?

The answer is an emphatic yes, as I explain in this article. I argue that non-deductive justification is in fact pervasive in mathematics, and that it is in good epistemic standing.

Keywords

Proof · Justification · Mathematical knowledge · Mathematical justification · Non-deductive evidence · Empiricism · Non-deductive knowledge · Deductive knowledge · Induction · Foundationalism · Euclidean program

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1 Introduction

The focus of this survey article is on non-deductive justification in mathematics, by which I mean any kind of justification for p other than a proof of p . (Unless otherwise stated, p will be a mathematical proposition.) I shall argue that this sort of justification is pervasive, and that it is in good epistemic standing. My article is complementary to that by James Franklin in the present handbook (Franklin 2021a). His is more focused on the practice side of things, mine more on the philosophical side. The two articles may profitably be read side by side. (It is worth noting, however, that the present article is not committed to an objective Bayesian analysis in the way Franklin's is. For all I say here, justificatory relationships between mathematical beliefs or truths may or may not be cashed out in Bayesian terms, objective or otherwise.)

We begin in Sect. 2 with a case study: the non-deductive evidence behind Goldbach's conjecture (GC). A famous conjecture in number theory, it was first put forward by Christian Goldbach in a 1742 letter to Euler. Although GC has much non-deductive evidence behind it, it remains unsolved. Section 3 briefly looks at skepticism about the value of enumerative inductive evidence in arithmetic. What is the justificatory value of, say, verifying the first trillion instances of GC, all of which are in some sense small? Section 4 ties the rise of non-deductive methods to the decline of the Euclidean ideal in mathematical epistemology. Section 5 considers how one might justify the consistency of mathematics as a whole. The natural, and perhaps only, way to do so is by means of non-deductive evidence. We conclude in Sect. 6 by examining a radical suggestion: perhaps non-deductive evidence is enough for *knowledge* of a mathematical proposition. We expound one of the arguments for this suggestion.

Right at the outset, we need to be clear that most mathematical justification, even knowledge, is testimonial, because it is acquired from other sources (people, books, journals, websites, social media, etc.). For example, I am justified in believing that Fermat's Last theorem is true because I have heard of its proof from various reliable sources. (Or at least from a preponderance of reliable sources. Fermat's Last Theorem states that if $a^n + b^n = c^n$, where a, b, c and n are all positive integers, then $n = 1$ or 2 .) Testimonial justification is non-deductive because it is not based on a mathematical proof of your source's reliability. Any evidence that Andrew Wiles is a reliable mathematician is partly but not strictly mathematical: it compares his mathematical pronouncements (an empirical matter) to the mathematical facts. And evidence that the media and personal channels through which I have heard of Wiles's proof of Fermat Last Theorem's are accurate is not strictly mathematical either. (Calling it 'Wiles's proof' is a simplification; as is well-known, Wiles's original 1993 proof of Fermat's Last Theorem contained a flaw. The 1995 patch-up is owed to Wiles and Richard Taylor.) So most justification of a mathematical proposition p is non-deductive, even if the justification chain ends in a proof. That much is agreed on all hands. A more contested question concerns the extent and significance of justification that does *not* end in a proof because there is currently no proof to be had. It is this sort of justification that is our topic here.

2 A Case Study: Goldbach's Conjecture

Franklin (2021a) illustrates the notion of non-deductive evidence in mathematics with several examples. Section 5 of his article notably presents some of the evidence for the presently unsolved Riemann hypothesis. The present section does the same for GC. (The material here draws significantly on p. 779 of my (2015). My survey of the non-deductive evidence for GC is far from exhaustive.)

Before we get to brass tacks, two notes on terminology. I prefer to talk about “the justification of mathematical statements” rather than “mathematical justification” as it might be insisted that properly mathematical justification must be deductive. As a matter of definition, one might insist, any justification for p that does not take the form of a proof of p is not *mathematical*, sensu stricto. I am not sure that's right; but in any case, it's a terminological fight not worth having, why is why I called my 2015 article on the subject “Knowledge of Mathematics Without Proof” rather than “Mathematical Knowledge Without Proof.”

The second point is that the word “induction” is multiply ambiguous. It has a procedural or ceremonial meaning (e.g., “induction into the Rock and Roll Hall of Fame”) and a physical meaning (“electromagnetic induction”). Even setting these aside, it can mean (at least) three more things. (1) The first is an inference from particular instances to a generalization. An induction is something of the form “ A_1 is F , . . . , A_n is F ; therefore all A s are F .” (Or even, in some cases, an inference to this conclusion from infinitely many instances.) I shall reserve the term *enumerative induction* for this. (2) The second is a broader sense: any non-deductive inference or any form of non-deductive evidence. Bertrand Russell consistently used the word in this way, and I followed him in my (2015). But upon reflection, confusion is avoided and clarity promoted if we simply write “non-deductive” for “inductive” in this sense. That will be my policy here. (This is the sense of induction that is relevant to Hume's problem of induction, i.e., the problem of justifying an inference from observed to unobserved cases. Some recent mathematical results are highly pertinent to this problem, as discussed in my (2011) and (2008).) (3) In its third sense, it forms part of the expression “proof by mathematical induction,” i.e., a proof that all numbers have P by showing that 0 has P and that if n has P then $n + 1$ also does. I shall not be concerned with induction in this sense.

On to GC, then, which states that every even number greater than 2 is the sum of two primes. Number theorists are highly confident of GC's truth on the basis of non-deductive evidence. What form does this evidence take?

First, there is enumeratively inductive evidence for GC. Specifically, GC has been checked for every even number up to about 4×10^{18} and double-checked up to a number not much smaller. (The latest results can be found on Tomás Oliveira e Silva's website <http://www.ieeta.pt/~tos/>.)

Second, various slightly weaker claims than GC have been proved. An example is the ternary Goldbach conjecture, that every odd number ≥ 7 is the sum of three primes. The Soviet mathematician Ivan Vinogradov proved in the 1930s that every sufficiently large odd number is the sum of three primes; in 2013, Harald Helfgott proved the ternary conjecture outright. Two more results in the same vein are:

(a) every sufficiently large number is the sum of a prime and either a prime or the product of two primes and (b) every even number is the sum of no more than six primes. Both of these have been proved, so are bona fide theorems. The idea here is that if a slightly weaker claim than GC is proved, that makes GC more likely to be true. Note in passing that this illustrates our broad use of “non-deductive evidence for p ” to include *proofs* of statements distinct from p but related to it in some interesting way.

This sort of justification exhibits the following pattern (compare Mazur (2014, p. 25); Pólya (1956, 1968) is a classic early discussion of non-deductive reasoning in mathematics):

$$A \Rightarrow B_1, B_2, \dots, B_n$$

$$B_1, B_2, \dots, B_n \text{ all hold}$$

$$A \text{ gains in justification}$$

Of course, “ $A \Rightarrow B, B$; therefore A ” is a formal fallacy, known as *affirming the consequent*. (Or “modus morons,” as Haack (1976, p. 115) playfully calls it.) This is another way of saying that the bare formal bones of this sort of argument are non-deductive.

The schema can be further elaborated. For example, A gains *more* in justification the more consequences B_1, B_2, \dots, B_n are verified (i.e., the greater n is); the more varied and independent of one another B_1, B_2, \dots, B_n are; the “closer” each of B_1, B_2, \dots, B_n is to A ; the more of the B_1, B_2, \dots, B_n were discovered after conjecturing A and independently of that conjecture; and so on. (On closeness: the claim that every even number >2 is the sum of six primes is for instance closer to GC than the claim that every even number >2 is the sum of 10 primes. Needless to say, how to make precise sense of this notion of closeness is tricky.)

Third, let $G(n)$, the Goldbach number of n , be the number of different ways in which n can be written as the sum of two primes. GC can then be expressed as the claim for all even n greater than 2, $G(n) \geq 1$. As Echeverría (1996) points out, computer evidence shows that the function $G(n)$ broadly increases for even n as n increases (with oscillation, but with an increasing trend), so that for instance for even $n \approx 10^5$, $G(n) \geq 500$. In light of this evidence, that $G(n)$ will suddenly drop to 0 is regarded as deeply unlikely.

This third sort of evidence combines neatly with enumerative inductive evidence, because it suggests that smaller numbers are more likely to be counterexamples to be GC than larger ones. (This example is also discussed by Baker (2007, pp. 69–70).) In the case of GC, there are therefore conjecture-specific reasons for thinking that the earliest cases are the “hardest.” GC, incidentally, is by no means unique in this respect. Another example is Legendre’s conjecture, also currently unproved, which states that for every positive integer N there is a prime between N^2 and $(N + 1)^2$. As in the case of GC, the enumerative inductive evidence suggests that not only is Legendre’s conjecture true, but also that the number of primes between N^2 and $(N + 1)^2$ non-strictly increases with N . Incidentally, there are heuristic arguments for this conclusion too, of

the sort we will shortly mention for GC. The prime number theorem implies that the number of primes between N^2 and $(N + 1)^2$ is asymptotic to a quantity which increases with N . So for Legendre's conjecture, as for GC, smaller numbers are "hard" cases – they are more likely to yield counterexamples than larger numbers. (There are other conjectures for which we have specific reason to believe that early cases are *easy* cases. An example is the conjecture that all perfect numbers are even; a natural number N is perfect iff its factors sum to $2N$. Since Ochem and Rao (2012), we know that the smallest odd perfect number, if it exists, must be greater than $10^{1,500}$. In this respect, such conjectures are diametrically opposed to GC and Legendre's conjecture.)

Fourth, the ratio $R_N = \frac{1}{N}$ (numbers $1 < k \leq N$ such that $G(2k) = 0$) has been proved to tend to 0 as N tends to infinity. In other words, the density of counterexamples to GC is zero. Of course, if GC is true then R_N is simply equal to 0 for all $N > 1$.

Fifth, Hardy and Littlewood's formula for the asymptotic number of representations of

$$N = p_1 + \dots + p_m,$$

where $p_1 \leq \dots \leq p_m$ are m primes, has been proved for $m \geq 3$. If true for $m = 2$, it implies GC for sufficiently large even numbers.

Sixth, a well-known heuristic probabilistic argument suggests the same conclusion. The prime number theorem states that the number of primes up to N tends to $N/\ln N$ asymptotically, i.e., the ratio of these two quantities tends to 1 as N tends to infinity (here $\ln N$ is the natural logarithm of N). Using the prime number theorem, one can estimate the number of ways in which an even number N can be written as the sum of two primes. One simple way of doing so based on the theorem estimates that N can be written as the sum of two primes in $\frac{1}{2}N/\ln^2 N$ ways, a quantity that increases with N . (The argument seems to be mathematical folklore.)

This last argument is very rough and ready, and, to stress, heuristic rather than demonstrative. But it can be improved to yield much better estimates that point to the same conclusion: the greater N is, the greater $G(N)$ is likely to be. Such arguments remain heuristic – GC has not been proved – but they make it plausible that the first counterexample to GC, if one exists, will be a "small" number. These sorts of arguments are much more convincing than one might initially think: if the numerical facts did not follow the probabilistic expectation, the thought goes, there would be some totally unknown mathematical phenomenon that would cause the deviation – and there is no reason to expect this.

On the basis of this and other evidence – our list is illustrative rather than exhaustive – mathematicians are close to certain of GC's truth. The non-deductive evidence behind GC is justification for its truth, even in the absence of proof. One might say that the truth of GC best explains this wide range of evidence, and therefore that we should infer its truth on this basis. (Lange (2022a) discusses inference to the best explanation in mathematics.)

In illustrating some of the non-deductive evidence for GC, we have taken into account the strictly mathematical evidence for it. This is, if you like, the first-order

evidence for GC. But for any given p , such as GC, there is also higher-order evidence for p : what other mathematicians make of this first-order evidence for p . To determine how much the evidence supports p , mathematicians also take each other's judgements into account. In any real-life situation, a mathematician's judgement of how much the mathematical evidence supports p will also depend on what other mathematicians make of the same question. When you think about how likely p is to be true, your peers' judgements also matter.

This last point interestingly complicates the picture of mathematical justification, and applies to proof-based and non-proof-based justification alike. We will not dwell on it further here, but simply note that the sum total of the non-deductive evidence for p includes other experts' judgements.

3 Skepticism About Enumerative Induction

The first sort of evidence for GC mentioned in the previous section consists of the first 4×10^{18} verified instances of GC. In the absence of supporting reasons, mathematicians may mistrust such evidence for arithmetical generalizations, more so than most other forms of non-deductive evidence. Some philosophers have also expressed skepticism about the value of enumerative inductive evidence in arithmetic.

Why? The reason usually given is that known instances of an arithmetical conjecture are almost always small. (We say "almost always" because, for example, one could have an argument that all *odd* numbers satisfy some arithmetical property. That would constitute enumerative inductive evidence for the claim that *all* numbers have that property. But the evidence would consist of infinitely many cases, which could not all be said to be small.) For example, in the case of GC, the evidence is potentially biased, as it consists only of the *first* 4×10^{18} natural numbers. Since the size of a natural number significantly affects its properties, our enumerative inductive evidence seems biased with respect to size.

Following Frege (see §10 of his 1884), Alan Baker has given voice to this sort of skepticism. In an article devoted to the subject, he concludes with the following normative and descriptive point (about arithmetic): mathematicians ought not and in general do not "give weight to enumerative induction *per se* in the justification of mathematical claims" (2007, p. 72). But following other writers, Baker allows that circumstantial reasons can come to the rescue of an enumerative induction, in arithmetic as well as elsewhere in mathematics.

We limit ourselves to making three quick points against the skeptic, which a more expansive treatment would develop. The first point, already made in Sect. 2, is well appreciated by Baker and is worth stressing. In several cases, we have good reason to think that early cases are hard cases. We mentioned GC and Legendre's conjecture as examples. For these conjectures, small cases may be biased, but they are *favorably* biased: they are the cases most likely to yield a counterexample. If no such counterexample is found among them, the conjecture has passed an important test, like a climber who has got past the steepest part of the mountain face. As a result, confidence that all remaining cases fall in line can reasonably increase.

In the case of GC, the evidence for thinking that early cases are hard cases is, of course, partly based on enumerative induction. However, unless one is an out-and-out skeptic about enumerative induction, the circle here is virtuous. Enumerative inductive evidence is deployed to show that, so far as GC is concerned (say), the size bias works in favor of someone deploying enumerative inductive evidence to confirm it. In any case, as we also saw in Sect. 2, a heuristic argument also points to the same conclusion.

A second and related point, also acknowledged by Baker, was implicit in Sect. 2. The situation in which *all* we have is enumerative inductive evidence is a rare one. Almost always, this evidence is accompanied by other sorts of non-deductive evidence. Section 2 detailed some of that accompanying evidence in the case of GC.

The third point is that the viability of this sort of “size-skepticism” depends on what’s motivating it. In a recent article (Paseau 2021), I distinguished three sorts of size-skeptics and pointed out that some are better motivated than others. In particular, some are better able to respond to the following *frontloading argument*.

Let E , a finite subset of the natural numbers, consist of our enumerative inductive evidence for a particular arithmetical conjecture. In other words, E is the set of known instances of a generalization over the natural numbers. Let the function v be our evidential function, with domain all finite subsets of the natural numbers. We assume only that v ’s codomain is the closed unit interval $[0, 1]$ with the usual order; the higher v ’s value in $[0, 1]$, the stronger the evidence. Evidential values may be thought of as measuring the subject’s rational degree of confidence in the generalization in question, though without commitment to the whole panoply of probabilistic ideas.

Consider next the following, presumably uncontroversial, evidential principle:

More is Better

If n is not in E then $v(E \cup \{n\}) > v(E)$.

As its name indicates, *More is Better* simply captures the idea that more evidence is better than less; so the evidential value of more evidence is greater than that of less. Next, define $l = \lim_{n \rightarrow \infty} I_n$, where $I_n = v(\{0, 1, \dots, n\})$. By *More is Better*, if $m < n$ then $I_m < I_n$; and since 1 is an upper bound for the I_n , the limit l exists. The real number l itself, of course, may be 1 or smaller than 1, but it has to be greater than 0 (by *More is Better*). So we deduce that $0 < l \leq 1$. Now by the definition of a limit, for any $\varepsilon > 0$, however small, there is an N_ε such that for any $N^* \geq N_\varepsilon$, I_{N^*} is to within ε of l .

Here’s another way of putting it: for ε much smaller than $l - \varepsilon$, almost all the evidential value stems from the first N_ε instances of the enumerative induction. The remaining instances add very little evidential value. The evidential value of any finite amount of numerical instances is therefore concentrated almost entirely in an initial segment. Whatever arithmetical conjecture you wish to test, the value of further instances beyond some finite bound (that depends on the conjecture) will be vanishingly small. An initial segment provides the lion’s share of the confirmation.

The conclusion of this remarkably simple argument appears to contradict size-skepticism. Paseau (2021) discusses in detail which forms of size-skepticism are genuinely affected by it. The conclusion there is that some but not all are.

4 The Last Bastion of the Euclidean Program

What is the structure of mathematical justification? The traditional picture is foundationalist. More specifically, it is a form of foundationalism largely inspired by Euclid's geometrical method in *The Elements* (c. 300 BC). So what was Euclid's method? Starting from some definitions, postulates, and common notions, Euclid derives the geometry of his day theorem by theorem, in a cumulative manner over the course of 13 books. A concise summary of what he calls the *Euclidean program* is given by Imre Lakatos in the following passage, where he contrasts it with the *Empiricist program*:

The Euclidean programme proposes to build up Euclidean theories with foundations in meaning and truth-value at the top, lit by the *natural light of Reason*, specifically by arithmetical, geometrical, metaphysical, moral, etc. intuition. The Empiricist programme proposes to build up Empiricist theories with foundations in meaning and truth-value at the bottom, lit by the *natural light of Experience*. Both programmes however rely on Reason (specifically on logical intuition) for the safe transmission of meaning and truth-value. (Lakatos 1962, p. 5)

The most obvious way to spell out the Euclidean program would be to base it on what Euclid himself has to say about it in *The Elements*. But that would give us very little to go on, because Euclid offers us no philosophical gloss on his method, as many commentators down the ages have noted.

Lakatos offers us more. He characterizes the Euclidean program as follows:

I call a deductive system a 'Euclidean theory' if the propositions at the top (axioms) consist of perfectly well-known terms (primitive terms), and if there are infallible truth-value-injections at this top of the truth-value True, which flows downwards through the deductive channels of truth-transmission (proofs) and inundates the whole system. (If the truth-value at the top was False, there would of course be no current of truth-value in the system.) Since the Euclidean programme implies that all knowledge can be deduced from a finite set of trivially true propositions consisting only of terms with a trivial meaning-load, I shall call it also the Programme of Trivialization of Knowledge. Since a Euclidean theory contains only indubitably true propositions, it operates neither with conjectures nor with refutations. In a fully-fledged Euclidean theory meaning, like truth, is injected at the top and it flows down safely through meaning-preserving channels of nominal definitions from the primitive terms to the (abbreviatory and therefore theoretically superfluous) defined terms. A Euclidean theory is eo ipso consistent, for all the propositions occurring in it are true, and a set of true propositions is certainly consistent. (Lakatos 1962, pp. 4–5)

Now in this passage Lakatos speaks of truth (and meaning) injection; but this is somewhat misleading. The Euclidean program represents an epistemological conception, and the hierarchical path from axioms to theorems is a path the subject, as

opposed to reified truth, follows. The flow-of-truth metaphor is better construed as transmission of an epistemic good of some sort, such as justification say. The picture is then a foundationalist one in which one gains justification for axioms first and thence for theorems by inferring them from the axioms.

Historical proponents of mathematical epistemologies that, to one or degree or another, approximate the Euclidean conception are many and varied. The conception's high point came in the seventeenth century; see in particular Pascal's posthumously published *On the Geometric Mind* (written in the 1650s) or even Descartes' *Discourse on Method* (1637). For various reasons, the Euclidean program is no longer tenable as a mathematical epistemology for *all* mathematics. Paseau and Wrigley (2024) explains the reasons why for the case of set theory. In brief, the standard axioms of set theory are no longer generally regarded as self-evident; at least, not all of them are. And deductive rules that take us from theorems to axioms are also not thought to be certainty-preserving, or even rational-credence-preserving. On top of that, the Euclidean program's ideal of completeness is also, post Gödel, not realizable. Any reasonable axiomatic organization of set-theoretic truths in a deductive system will omit some of them.

Arithmetic much more closely approximates the Euclidean picture than set theory does. (As discussed in Sect. 5 of Franklin (2021b), for example.) The justification of its axioms seems more intrinsic than extrinsic. Extrinsic evidence for a principle consists in its instrumental value, in drawing consequences, forging connections between different areas, making for better explanations, and the like. This is the kind of evidence on which theoretical principles in science are mostly, if not exclusively, based. Intrinsic evidence we may take to be non-extrinsic evidence: the sheer obviousness or plausibility of a principle, as well as how it fits with the broader conception of the subject matter. And many would see the axioms of arithmetic as intrinsically evident.

One of the Euclidean program's tenets that's still very much standing is that deduction is prized as the highest form of justification available for a mathematical proposition and regarded as necessary for knowledge. In fact, in some quarters, deductive justification is thought to be the *only* available form of justification; and for many, non-deductive justification pales in comparison to the real McCoy: proof. To challenge these sorts of proof-centric ideas, or at least weaken their hold, is thus to challenge one of the last bastions of Euclideanism.

Friends of non-deductive reasoning in mathematics will find what Lakatos called the Empiricist program much more congenial than the Euclidean one. This post-Euclidean outlook was perhaps first articulated by Russell, who drew a "close analogy between the methods of pure mathematics and the methods of the sciences of observation" (1907, p. 272). On a thumbnail, the idea is this. We commonly conceive of natural-scientific propositions as being divided into two broad kinds: data and more theoretical principles, ultimately laws. On this (simplified) conception, the data are empirical propositions that we take to be the facts, and the principles/laws are propositions formulated in order to predict the facts. Indeed, the prediction of the data is the primary means of verifying these principles/laws. Whether or not the principles/laws are intrinsically plausible, to a first

approximation we take them to be true if they predict all the data and don't predict anything false.

By analogy, mathematical axioms are supposed to be verified by "predicting" mathematical propositions of some privileged kind identified as the data. Prediction here is simply deductive implication. The justification of these principles or laws would then be extrinsic. This is broadly Russell's view of the matter, which he calls the "regressive method." It plays a major role in his own foundational system; for example, in the introduction to the first edition of *Principia Mathematica*, he (with Whitehead) had this to say about the controversial so-called axiom of reducibility:

That the axiom of reducibility is self-evident is a proposition which can hardly be maintained. But in fact, self-evidence is never more than a part of the reason for accepting an axiom, and is never indispensable. The reason for accepting an axiom, as for accepting any other proposition, is always largely inductive, namely that many propositions which are nearly indubitable can be deduced from it, and that no equally plausible way is known by which these propositions could be true if the axiom were false, and nothing which is probably false can be deduced from it. (Whitehead and Russell 1910/1962, pp. 59–60)

It is essential to the Empiricist program that certain propositions are identified as being data, and that these have a special epistemological status which explains their role in the program. Various manifestations of the Empiricist program will have different ideas about which propositions are properly classified as data, or which propositions are lit by the natural light of experience. Plainly, not just any mathematical truth can be considered a datum, otherwise any true axiom would be self-certifying in an unacceptable way. For Russell and Whitehead, notably, this moral applies to arithmetic as well: the axioms are justified because they entail propositions such as " $1 + 2 = 3$," not the other way round.

The use of non-deductive methods in mathematics therefore tallies better with the Empiricist than the Euclidean program. To put it very roughly, mathematical justification is much more like scientific justification than traditionally imagined. At the very least, it has an important extrinsic component. It is then a short step to the idea that non-deductive evidence has an important role to play in mathematical justification, just as it does in science. Coming at it from the other direction, to recognize the important role this sort of evidence plays in mathematics is to chip away at the epistemological dimension of the empirical science/mathematics divide. Mathematics is much more like science than our philosophical forebears imagined. Just as in science, non-deductive justification has an important role to play.

5 Justifying the Consistency of Mathematics as a Whole

Let's now turn to an apparently unrelated question, whose connection to our main topic will emerge shortly. Set theory is regarded by many as a foundation for mathematics – though in what sense exactly remains a source of controversy. An incontrovertible fact is that almost all mathematics can be carried out in set theory. And we can prove the consistency of virtually all mathematical theories – arithmetic,

analysis, geometry, and so on – in set theory. (The consistency of a theory may here be understood as its not implying every sentence.) If set theory were inconsistent, a proof of, say, the consistency of arithmetic in set theory would be cold comfort, since set theory would also prove that arithmetic is inconsistent – indeed, it would prove anything storable in its language. A set theoretic proof of arithmetic’s consistency is thus best understood as a relative consistency proof: it establishes the consistency of one theory (arithmetic) on the assumption that another (set theory) is consistent. To interpret such a proof as telling us that arithmetic *is* consistent, we need reason to think that set theory itself is consistent.

But now a difficulty looms: we have no proof of this latter fact, that is, of set theory’s consistency. And this for a principled reason: by Gödel’s second incompleteness theorem, if set theory is consistent then it cannot prove its own consistency. Of course, we can take our preferred system of set theory S and extend it to S^+ and then proceed to prove the consistency of S within S^+ . But that only pushes the question back one stage: how do we convince ourselves that S^+ , our new ultimate theory, is consistent? By Gödel’s theorem again, we cannot do so in S^+ (assuming S^+ is consistent).

So far we have assumed that set theory is the foundation of mathematics. But actually this is inessential. The question of consistency can be raised for any putative foundation for mathematics, e.g., category theory as opposed to set theory. And even if you think mathematics has no foundation, you still face the question of why we are justified in thinking mathematics as a whole is consistent. Everything said here can be easily reformulated to accommodate either of these alternative views.

The question of the foundation’s consistency is particularly pressing if you take all justification in mathematics to be deductive, i.e., to be given by proof and exhausted by proof. For, on that view, how on earth are we justified in believing set theory’s fundamental principles? The reply that an axiom has a zero-step proof from the axioms, though true, is hardly consoling. In any axiomatic system, the axioms are trivially provable, for systems consisting of true axioms and of false axioms alike. Any crank can put forward a hare-brained mathematical “system” and give a zero-step proof that each of its axioms is true. So how do we know that the axioms of our *not* hare-brained but trusted (we think) system are true? Or at least why are we justified in so thinking?

Indispensabilists have an answer. Indispensabilism is the philosophy of mathematics inspired by Quine and Putnam; see Paseau and Baker (2023) for a more recent account. In a succinct formulation, the idea is that we are justified in believing the axioms of mathematics as a whole – whatever exactly these are – because they are successfully and indispensably applied in science. Mathematical justification ultimately rests on scientific justification. This is the indispensabilist’s broad answer to the question of how axioms are justified. Plainly, though, the answer is indirect and holistic: to justify the thought that set theory is true, or even consistent, requires no less than a detour through the whole of science, or at least large parts of it. It would be better to combine this holistic justification with more direct evidence for the consistency of mathematics. As we have seen, this evidence apparently cannot be deductive, so has to be non-deductive. In sum, we have here an important role for

non-deductive evidence to play. Such evidence, if it exists, can be used to show that set theory (and hence mathematics as a whole) is consistent.

Does such evidence exist? Most definitely. Maddy (1988) is a then state-of-the-art discussion of non-deductive evidence for the axioms of set theory and their possible extensions. As such, it offers plenty of reasons for believing in set theory's consistency. Chapter 8 of Paseau (forthcoming) is a shorter and less technical survey of some non-deductive evidence for the same.

6 An Unorthodox Claim: Non-deductive Knowledge of Mathematics

We have so far been concerned with non-deductive *justification* and *evidence* in mathematics. We end this article by considering a much more radical idea: that there can be non-deductive (non-testimonial) *knowledge* of mathematical propositions. This claim goes squarely against the way mathematicians speak, since mathematicians typically equate p 's being known with there being a proof of p . (For mathematical p , obviously. In my (2015), I argue that gainsaying mathematicians is much less problematic when they are talking not about mathematics but about its epistemology, as in this case.) This is the quasi-universal, orthodox view. I am among the very few who dissent from it, because I believe that, in the best cases, non-deductive evidence can yield knowledge of a mathematical proposition (Paseau 2015). In this section, I shall present an argument for the unorthodox view, adapted from Sect. 5 of Paseau (2015); my article contains several other such arguments.

Epistemologists have given a good deal of general thought to knowledge. Not just mathematical, but empirical, scientific and moral knowledge, self-knowledge, knowledge of the past and the future, a priori and a posteriori knowledge, and so on. Ever since the publication of Gettier (1963), one particularly prominent concern has been to provide necessary and sufficient conditions for knowledge. Notoriously, none of the myriad proposed conditions has achieved consensus (for general reasons lucidly discussed in Zagzebski (1994)), although many have been thought to be along the right lines, or at least to cover an important range of cases. For brevity, call any analysis that has gained at least some traction in the literature a "right-track analysis." (Some examples will shortly follow.)

The argument for non-deductive knowledge of mathematics is that any right-track analysis falls into one of two categories. *Either* it allows that knowledge of mathematics may be obtained by non-deductive means; call this category *Type A*. *Or* it does not apply to knowledge of mathematics, so *a fortiori* does not privilege deductive, as opposed to non-deductive knowledge of mathematics; call this *Type B*. The underlying thought here is that if (non-testimonial) knowledge of a mathematical proposition could only be deductively acquired, at least *some* right-track analyses would have that implication, when supplemented with some generally accepted principles.

So let's take a look at a few accounts of knowledge. The first and most venerable one is that knowledge is justified true belief. As Gettier (1963) notes, Plato in the

Theaetetus and the *Meno* may have respectively considered and proposed such an account. This account is of Type A: it allows for non-deductive knowledge of mathematics, since a mathematical proposition may be justified non-deductively. For example, the evidence behind GC (some of which we encountered in Sect. 2) justifies our belief in it.

An influential revised account was offered in Goldman (1967). Goldman suggested that a subject knows that p just when her true belief that p is causally connected to the fact that p . Whatever the merits of this account for other domains, it does not apply to mathematics, since the subject matter of mathematics does not have causal powers: the number 53 itself does not causally impinge on our senses any more than a Banach space or an ordered field do. Goldman's "causal analysis" of knowledge is therefore of Type B. Goldman himself could not have been clearer on this point, declaring in the first paragraph of his famous article that "[m]y concern will be with knowledge of empirical propositions only, since I think that the traditional analysis is adequate for knowledge of nonempirical truths [including those of mathematics]" (Goldman 1967, p. 357).

Now that we have seen how this sort of argument goes, it can be extended to other post-Gettier right-track analyses more swiftly. These typically supplement the first two clauses, that p is true and S believes that p , with a third condition that aims to improve upon the justification clause. Here is a sample, along with their classification:

- The belief that p is not inferred from a false lemma (Clark 1963). Of Type A, since non-deductive evidence for p need not contain a false lemma.
- The justification that p must not essentially rest on a false assumption (Harman 1973). Similar to the previous: of Type A, since non-deductive justification for p need not, and in many usual cases will not, essentially rest on a false assumption.
- There is a law-like connection between the fact that p and the belief that p (Armstrong 1973). Armstrong intended it to cover empirical cases only, so this analysis is of Type B.
- The belief that p is produced by a reliable process not undermined by the subject's cognitive state (Goldman 1976). Of Type A, since non-deductive evidence can be the product of a reliable process.
- If it were the case that not- p then the subject wouldn't believe p (Nozick 1981). (Ignoring Nozick's other condition, which is problematic and does not affect the moral. More generally, in presenting these accounts, I have omitted qualifications, refinements, and extra clauses that do not affect the general point.) To apply this, note that mathematical propositions are usually thought of as necessary. The standard account of counterpossibles in the literature takes them to all be true, in which case Nozick's analysis is of Type A – it allows for non-deductive knowledge of mathematics. (The standard accounts are derived from Stalnaker (1968) and Lewis (1973). As Baker (2021) notes, following an observation by Ralph Wedgwood, counterpossibles are usually known as "counterpossibles." If the analogy with "counterfactual" is to be exact, however, "counterpossibles"

should be known as counter necessities. See Baker (2021) for more on these types of conditionals, whatever they should be called.)

Even if we reject the standard account of counter necessities, it seems that Nozick's account is of Type A. For example, we are liable to think that if the Riemann Hypothesis were false, we would lack the evidence we in fact possess for it, and consequently that we wouldn't believe it. Similarly, if the perpendicular bisectors of Euclidean triangles did not meet in a point then trying to confirm this fact empirically—by drawing the lines with the utmost care on a plane surface—would not lead us to conclude that they do. If a particular number were composite rather than prime, the primality test evidence would be different. And so on.

- The subject could not easily have falsely believed that p (this is the safety condition discussed in, e.g., Williamson 2000). This condition can easily be failed by true mathematical beliefs, e.g., if they are the product of happenstance or of an unreliable method that happens to be right in this instance. For instance, suppose I believe that $1^3 + 5^3 + 3^3 = 153$ because I recognize it as an instance of the generalization $a^3 + b^3 + c^3 = abc$, where abc is written in decimal notation, which I believe to be true. The generalization is patently false, but this instance of it happens, quite fortuitously, to be correct. The condition is of Type A: the best forms of non-deductive evidence satisfy it.

Observe in passing that the best inductive mathematical cases are quite unlike lottery cases. Say that p is the proposition that the subject won't win the prize in a large lottery, with N tickets, that she enters. In a lottery case, the subject's evidence for p is insensitive to whether p is true. She has the same evidence and belief in the scenario in which she holds the winning ticket as in the $N - 1$ nearby scenarios in which she holds a losing ticket. Not so for the best cases of non-deductive evidence in mathematics.

Evidently, this is just a sample from a vast post-Gettier literature. But it includes most of the leading candidates; and none of them was chosen with a view to confirming the "either Type A or Type B" moral. In any case, broadening the range of examples would not alter the moral. General epistemology has not brought to light any reasonably popular condition that excludes a non-deductive (non-testimonial) route to knowledge of mathematical propositions. No right-track analyses allow deductive routes to mathematics while ruling out non-deductive ones. As a prominent field of inquiry, mathematics is, and should be, a test case for general epistemology. If the only (non-testimonial) route to knowledge of a mathematical proposition were deductive, you would expect some prominent general accounts of knowledge to have this consequence. That they don't supports the idea that such knowledge can be acquired non-deductively.

Of course, this is not a knockdown argument for the conclusion that we can know a mathematical proposition without proof. (Which, to repeat, is to be understood in the strong sense that no one has a proof of it. As stressed, we often know p testimonially without being able to prove p ourselves.) It is one of many arguments and considerations supporting that idea. To further strengthen it, one ought to consider those as well. A fuller treatment would examine not just these but the

conservative backlash as well, if we may call it that. Lange (2022b), for example, is an interesting recent article that tries to support the standard view by proposing a necessary condition on knowledge that Lange believes (i) is independently motivated and (ii) rules out non-deductive knowledge of mathematics.

A closely related point worth stressing is that, at least in some cases, there is a gap between strong justification for one's true belief that p and knowledge that p . Gettier's refutation of the "JTB" analysis of knowledge showed that justification is not enough for knowledge, even when the belief is true. (Nagel (2014, p. 58) observes that discussion of so-called Gettier cases may be found in Indo-Tibetan philosophy that predates Gettier by centuries.) In fact, one can go further: one of the lessons of so-called lottery cases is that even justification for a true belief that falls short of complete certainty by a tiny but positive margin *may* not be enough for knowing it. In a fair lottery in which there are N tickets, my chances of winning are $1/N$, which for large N is very small, and I may truly believe that I don't hold the winning ticket; but – and this is the key point – I don't know that my ticket won't win.

What do the lottery cases teach us? The fact that a mathematician's non-deductive justification for her belief in the true mathematical proposition p is extremely strong does not, in itself, show that she knows that p . Something more is needed, at least in general. Clearly, though, not all epistemic situations are similar to lottery cases, and one should not overgeneralize from them. Their moral is not that extremely strong justification can *never* transmute true belief into knowledge; only that it *sometimes* fails to do so.

Let's take stock. Suppose you think, like I do, that non-deductive justification can yield knowledge in mathematics. Then you have a job to do. It's not enough for you to simply point out that in many such cases, the evidence for p is overwhelmingly strong. You must produce reasons for thinking that, at least in some cases, it's strong enough for knowledge. The argument in this section that nothing in general epistemology rules it out is just one example of an argument you could give. There are many others.

7 Conclusion

The present article has looked at some of the more philosophical aspects of the role non-deductive evidence plays in mathematics. The role is an important one, and its contours have only started to be investigated in detail in recent decades. There is more philosophical work to be done to understand it better.

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