Non-deductive justification in mathematics[1]

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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 Programme.

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*.[2] 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 focussed on the practice side of things, mine more on the philosophical side. The two articles may profitably be read side by side.[3]

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[1] I am grateful to Michèle Friend for the invitation to contribute to this handbook and to Jim Franklin for helpful comments on a previous draft.

[2] Unless otherwise stated, *p* will be a mathematical proposition.

[3] It is worth noting 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. And even if they are, this Bayesian analysis may or may not involve an objective form of probability. (Franklin mentions this last point on page 8 of his 2021a.)

We begin in section 2 with a case study: the non-deductive evidence behind Goldbach's Conjecture (GC). A famous conjecture in number theory first put forward by Christian Goldbach in a 1742 letter to Euler, it has much non-deductive evidence behind it but remains unsolved. Section 3 briefly looks at scepticism about the value of enumerative inductive evidence in arithmetic. What is the justificatory value of, say, verifying the first trillion instances of GC, all 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 section 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.[4] Testimonial justification is non-deductive because it is not based in any sense on a 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.[5] 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.[6]

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

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[4] 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.

[5] Calling it 'Wiles's proof's 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.

[6] The material here draws significantly on p. 779 of my (2015). N.B. My survey of the non-deductive evidence for GC is far from exhaustive.

('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 generalisation. An induction is something of the form 'A_1 is F, …, A_n is F; therefore all *A*s are *F*'.[7] I shall reserve the term *enumerative induction* for this meaning. (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.[8] (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.[9]

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: (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:[10]

$$\frac{\begin{array}{l} A \Rightarrow B_1, B_2, \ldots, B_n \\ B_1, B_2, \ldots, B_n \text{ all hold} \end{array}}{A \text{ gains in justification}}$$

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[7] Or even, in some cases, of the form 'A_1 is F, …, A_n is F, … ; therefore all *A*s are *F*', since we may know infinitely many instances.

[8] It 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).

[9] The latest results can be found on Tomás Oliveira e Silva's website http://www.ieeta.pt/~tos/.

[10] Compare Mazur (2014, p. 25). Pólya (1956, 1968) is a classic early discussion of non-deductive reasoning in mathematics.

Of course, '$A \Rightarrow B$, B; therefore A' is a formal fallacy, known as *Affirming the Consequent.*[11] 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 $B_1, B_2, \ldots, B_n$ are verified (i.e. the greater n is); the more varied and independent of one another $B_1, B_2, \ldots, B_n$ are; the 'closer' each of $B_1, B_2, \ldots, B_n$ is to A;[12] the more of the $B_1, B_2, \ldots, B_n$ were discovered after conjecturing A and independently of that conjecture; and so on.

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.[13] 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 $N/ \ln N$, 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.[14]

Fourth, the ratio $R_N = \frac{1}{N}$. (numbers $k \leq N$ 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.

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

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[11] Or 'modus morons', as Haack (1976, p. 115) playfully calls it.

[12] For instance, the claim that every even number > 2 is the sum of six primes is closer to GC than the claim that every even number > 2 is the sum of ten primes. Needless to say, how to make precise sense of this notion of closeness is tricky.

[13] This example is also discussed by Baker (2007, pp. 69–70).

[14] 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.

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

where $p_1 \leq \ldots \leq p_m$ 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). Hence the number of distinct sums of primes no greater than $2N$ tends to $\frac{1}{2}.(N/\ln N)^2$. (We divide by two because each sum, with the possible exception of $N+N$, appears twice.) Thus the typical even integer smaller or equal to $2N$ can be written as the sum of two primes in about $\frac{1}{2}.(N/\ln N)^2/N= \frac{1}{2}.N/\ln^2 N$ ways, a quantity that increases with N.[15]

This last argument is admittedly 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.[16]

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*: the evidence that consists of 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 experts' judgements.

3. Scepticism about enumerative induction

The first sort of evidence for GC mentioned in the previous section consists of the first

[15] The argument seems to be mathematical folklore.

[16] Lange (2022) discusses inference to the best explanation in mathematics.

4×10^{18} verified instances of GC. In the absence of supporting reasons, mathematicians may mistrust such evidence for arithmetical generalisations, more so than most other forms of non-deductive evidence. Some philosophers have also expressed scepticism 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.[17] 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 scepticism. 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 sceptic, which a more expansive treatment would develop. The first point, already made in section 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 *favourably* 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 sceptic 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 favour of someone deploying enumerative inductive evidence to confirm it. In any case, as we also saw in section 2, a heuristic argument also points to the same conclusion.

A second and related point, also acknowledged by Baker, was implicit in section 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-scepticism' depends on what's motivating it. In a recent article (Paseau 2021), I distinguished three sorts of size-sceptics and

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[17] 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 (odd *and* even) have that property. But the evidence would consist of infinitely many cases, which could not all be said to be small.

pointed out that some are better motivated than others. In particular, some are better able to respond to the following *frontloading argument*.[18]
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 generalisation 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 generalisation 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 $l = \lim_{n\to\infty} I_n$, where $I_n = v(\{0, 1, \cdot\cdot\cdot n\})$. 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 is chosen to be 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 (depending 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-scepticism. Paseau (2021) discusses in detail which forms of size-scepticism are genuinely affected by it. The conclusion there is that some are but not all.

4. The Last Bastion of the Euclidean Programme

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

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[18] The following adapts the first few paragraphs of section 5 of Paseau (2021). In section 4 of that article, I distinguish three types of size sceptic. (1) The *c-sceptic* believes that an inference based on a sample is (in this respect) weaker than an inference based on another sample that contains larger instances than the first. (2) The *s-sceptic* believes that an inference based on a sample consisting only of small instances is (in this respect) weak precisely because the instances are small. (3) The *u-sceptic* believes that an inference based on a sample consisting only of small instances is (in this respect) weak because the instances are small and therefore unrepresentative. Objections to size scepticism affect these three forms in different ways. Section 7 of Franklin (2021a) also discusses size scepticism.

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 Programme* is given by Imre Lakatos in the following passage, where he contrasts it with the *Empiricist Programme*:

> 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 Programme 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 characterises the Euclidean Programme 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 Programme 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. Its high point came in the seventeenth century; see in particular Pascal's posthumous *On the Geometric Mind* (written in the late 1650s) or even Descartes' *Discourse on Method* (1637). For various reasons, the Euclidean Programme is no longer tenable as a mathematical epistemology for *all* mathematics. Paseau and Wrigley (2023) explains the reasons why for the case of set theory.[19] 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 Programme's ideal of completeness is also, post Gödel, not realisable. Any reasonable axiomatic organisation 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.[20] The justification of its axioms is, it might be said, more intrinsic than extrinsic. To explain this distinction: extrinsic evidence for a principle to consist 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.

In any case, one of the Euclidean Programme'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 Programme 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

[19] The present section overlaps with some material in that book.
[20] As discussed in section 5 of Franklin (2021b) for example.

> 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: 59–60)

It is essential to the Empiricist Programme that certain propositions are identified as being data, and that these have a special epistemological status which explains their role in the programme. Various manifestations of the Empiricist Programme 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 (not to mention that the analogy with the natural sciences would be distorted beyond the point of being informative). 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 Programme. 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 recognise 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.[21]) 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 statable 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

[21] The exact sense of implication in question is not of great importance.

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 in section 5, 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 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.[22] 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 it has to be non-deductive. So 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

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[22] Indispensabilism is the philosophy of mathematics inspired by Quine and Putnam. See Paseau and Baker (2022) and Colyvan (2001) for more recent accounts. Here's a formulation of the famous Indispensability Argument, taken from the first article:

> 1. We ought rationally to be ontologically committed to those objects that are indispensable parts of our best scientific theories.
>
> 2. Mathematical objects are an indispensable part of our best scientific theories.
>
> ---
>
> We ought rationally to be ontologically committed to mathematical objects.

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*.[23] 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 section 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,[24] 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 section 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

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[23] 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.
[24] For general reasons lucidly discussed in Zagzebski (1994).

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).[25] To apply this, note that mathematical propositions are usually thought of as necessary. The standard account of counternecessaries 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.[26]

Even if we reject the standard account of counternecessaries, 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 then 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

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[25] 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.
[26] The standard accounts are derived from Stalnaker (1968) and Lewis (1973). As Baker (2021) notes, following an observation by Ralph Wedgwood, counternecessaries are usually known as 'counterpossibles'. If the analogy with 'counterfactual' is to be exact, however, 'counterpossibles' should be known as counternecessaries. See Baker (2021) for more on these types of conditionals, whatever exactly they should be called.

method that happens to be right in this instance.[27] This 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. In lottery cases, as described above, 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 if not all 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 a 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 whilst 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 knock-down argument for the conclusion that we can know a mathematical proposition without proof.[28] 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 (2022), 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.[29] 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,

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[27] For instance, suppose I believe that $1^3 + 5^3 + 3^3 = 153$ because I recognise it as an instance of the generalisation $a^3 + b^3 + c^3 = abc$, where *abc* is written in decimal notation, which I believe to be true. The generalisation is patently false, but this instance of it happens, quite fortuitously, to be correct.

[28] 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.

[29] 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 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 overgeneralise 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.

References

D.M. Armstrong (1973), *Belief, Truth, and Knowledge*, Cambridge University Press.

A. Baker (2007), 'Is There a Problem of Induction for Mathematics?', in M. Leng, A. Paseau & M. Potter (eds), *Mathematical Knowledge*, Oxford University Press, pp. 59–73.

A. Baker (2021), 'Counterpossibles in Mathematical Practice: The Case of Spoof Perfect Numbers', in B. Sriraman (ed.), *Handbook of the History and Philosophy of Mathematical Practice*, Springer, online.

R. Chisholm (1957), *Perceiving: A Philosophical Study*, Cornell University Press.

M. Clark (1963), 'Knowledge and Grounds: A Comment on Mr. Gettier's Paper', *Analysis* 24, pp. 46–8.

J. Franklin (2021a), 'Bayesian Perspectives on Mathematical Practice', in B. Sriraman (ed.) *Handbook of the History and Philosophy of Mathematical Practice*, Springer, online.

J. Franklin (2021b), 'Let No-One Ignorant of Geometry...': Mathematical Parallels for Understanding the Objectivity of Ethics, *The Journal of Value Inquiry*, vol. and pp. tbc.

G. Frege (1884/1974), *Die Grundlagen der Arithmetik*, transl. as *Foundations of Arithmetic* by J.L. Austin, Blackwell.

E. Gettier (1963), 'Is Justified True Belief Knowledge?', *Analysis* 23, pp. 121–3.

A. Goldman (1967), 'A Causal Theory of Knowing', *The Journal of Philosophy* 64, pp. 357–72.

A. Goldman (1976), 'Discrimination and Perceptual Knowledge', *Journal of Philosophy* 73, pp. 771–91.

S. Haack (1976), 'The Justification of Deduction', *Mind* 85, pp. 113–119.

I. Lakatos (1962), 'Infinite Regress and Foundations of Mathematics', in J. Worrall and G. Currie (eds), *Mathematics, Science and Epistemology*, Cambridge University Press, pp. 3–23.

M. Lange (2022), 'Inference to the Best Explanation is an Important Form of Reasoning in Mathematics', *The Mathematical Intelligencer* 44, pp. 32–8.

M. Lange (2022), 'Why is Proof the Only Way to Acquire Mathematical Knowledge?', *The Australasian Journal of Philosophy* (forthcoming).

D.K. Lewis (1973), *Counterfactuals*, Blackwell.

P. Maddy (1988), 'Believing the Axioms', *Journal of Symbolic Logic* 53: 481–511, 736–764.

B. Mazur (2014), 'Is it Plausible?', *The Mathematical Intelligencer* 36, pp. 24–33.

J. Nagel (2014), *Knowledge: A Very Short Introduction*, Oxford University Press.

P. Ochem & M. Rao (2012), 'Odd Perfect Numbers are Greater than 10^{1500}', *Mathematics of Computation 81*, pp. 1869–1877.

A.C. Paseau (2011), 'Proving Induction', *Australasian Journal of Logic*, pp. 1–17.

A.C. Paseau (2008), 'Justifying Induction Mathematically: Strategies and Functions', *Logique et Analyse* 203 (2008), pp. 263–9.

A.C. Paseau (2015), 'Knowledge of Mathematics without Proof' (2015), *The British Journal for the Philosophy of Science* 66, pp. 775–99.

A.C. Paseau and A.R. Baker (2022), *Indispensability*, Cambridge University Press.

A.C. Paseau and W. Wrigley (2023), *The Euclidean Programme*, Cambridge University Press.

A.C. Paseau (forthcoming), *What is Mathematics About?*, Oxford University Press.

G. Pólya (1956), *Mathematics and Plausible Reasoning, Volume 1: Induction and Analogy*, Princeton University Press.

G. Pólya (1968), *Mathematics and Plausible Reasoning, Volume 2: Patterns of Plausible Inference*, Princeton University Press.

B. Russell (1907), 'The Regressive Method of Discovering the Premises of Mathematics', in D. Lackey (ed.), *Essays in Analysis,* Braziller, pp. 272–83.

R. Stalnaker (1968), 'A Theory of Conditionals', in *Studies in Logical Theory*, N. Rescher (ed.), Blackwell, pp. 98–112.

A.N. Whitehead and B. Russell (1910/1962), *Principia Mathematica to *56*, Cambridge University Press.

T. Williamson (2000), *Knowledge and Its Limits*, Oxford University Press.

L. Zagzebski (1994), 'The Inescapability of Gettier Problems', *The Philosophical Quarterly* 44, pp. 65–73.