Deductive Theories and Non-Deductive Knowledge

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Abstract: I start by briefly considering three different epistemological conceptions associated with deductive theories. I then relate the conceptions to the debate about whether non-deductive knowledge of mathematical claims is possible. The hope is that getting clearer about the different types of deductive science sheds light on whether a mathematical statement can be known without anyone having a proof of it.

Prologue

Carlo Cellucci's Heuristic Philosophy of Mathematics is a very apt prompt for a conference. There is much to admire in his latest book *The Making of Mathematics*. I certainly agree with Cellucci that there is plenty of philosophical interest in the making and discovery of mathematics. Although my contribution does not touch directly on his work, it is animated by a kindred spirit. My aim is to champion the relevance and importance of non-deductive evidence in mathematics. Far from being 'merely' on the discovery side, this sort of evidence matters greatly to issues of justification and knowledge.

1. Introduction

A deductive theory consists of a set of axioms closed under deducibility. The theorems of the theory are all and only the sentences deducible from the axioms. Different deductive theories, however, can be associated with different epistemologies. Let's look at three examples, which are not meant to be exhaustive.

1. A traditional requirement to place on a deductive theory is that its axioms are self-evident and its theorems follow from the axioms by evidence-preserving rules of inference. An epistemological gloss on Euclid's method in *The Elements*, this picture of mathematical justification is sometimes called Euclidean foundationalism or the Euclidean Programme.[1] Call this type of deductive theory 'Euclidean-deductive' for short. An example of a modern theory which, suitably regimented, approximates this ideal is **arithmetic**.

2. On the second picture, the axioms of a deductive science are justified by a combination of intrinsic and extrinsic evidence. A claim is intrinsically justified if it flows from a conception of its subject matter. It is extrinsically justified to the extent that it allows us to draw desirable consequences, forge connections between different areas, make for better explanations and the like. An example of a modern theory that fits this conception is **set theory**. For want of a better expression, call this 'deductive-ZFC', given that ZFC is the standard contemporary axiomatisation of set theory.

[1] The phrase 'The Euclidean Programme' is taken from Lakatos (1962) and is the title of Paseau and Wrigley (2024).

3. The last of the three is an epistemologically minimal conception. On this picture, the axioms of the deductive theory do not have to be self-evident (as in a Euclidean theory), nor intrinsically or extrinsically justified (as in a deductive-ZFC theory), nor meet any other additional requirements. They are simply any collection of statements, closed under deducibility. Let's call this 'deductive-in-shape'. An example of such a theory is the one I introduce below and call **Bobology**.

This essay splits into two halves. The first half takes a closer look at each of the three conceptions and spells them out. In the second half, I relate the conceptions to the debate about whether non-deductive knowledge of mathematical claims is possible. The hope is that getting clearer about the varieties of deductive theory will shed light on this issue.

2. More on the three conceptions

Our three conceptions of a deductive theory are: Euclidean-deductive; deductive-ZFC; and deductive-in-shape. Let's take them in turn.

2.1 *Euclidean-Deductive*

Wes Wrigley and I have devoted a short book, *The Euclidean Programme*, to characterising and assessing the Euclidean conception. Here I shall briefly summarise the conception's main points. First, a caveat: evidently, Euclideanism is inspired by the perceived methodology of Euclid in *The Elements*. But the idealised picture of mathematical inquiry inspired by *The Elements* shouldn't be confused with what Euclid himself is up to in that book. Just because an ideal is inspired by something doesn't mean that it closely corresponds to it (compare the modern notion of democracy with its Athenian ancestor).

The Euclidean conception may be articulated in terms of justification.[2] The conception has three tenets or core principles. The first is that axioms are true; the second that axioms are self-evident, so that if a subject clearly grasps one then she is maximally justified in believing it; and the third tenet is a sort of flow principle, along the following lines: if a conclusion is deducible from some premises, and a subject (a) clearly grasps this and (b) is maximally justified in the premises, then she is maximally justified in the conclusion as well. Combine the three principles in this way and you get the top-down conception of deductive epistemology par excellence.

The Euclidean conception also contains some other principles besides these three core ones. One is that the axioms have a finite presentation. The second is that axioms should be general propositions, and the third that they should be independent of one another. The fourth and final one is the requirement of completeness: all truths (or perhaps all knowable or all known truths) of the relevant domain can be deduced from the axioms.

Wrigley and I do not claim that all and only these seven principles are clearly articulated by every writer who has been attracted to Euclideanism in its millennia-long history. Of course not. Rather, the seven principles are a rational reconstruction of a view which many philosophers and mathematicians have found attractive and which, one could argue, has dominated philosophical thought about mathematics until fairly recently.

Among contemporary theories, the best approximation to the core Euclidean tenets is arguably the formal regimentation of arithmetic that is first-order Peano Arithmetic (PA). The

[2] *The Euclidean Programme* does it in a more abstract way.

axioms of this theory are all true of the intended model (the natural number structure), the PA axioms themselves are arguably self-evident or close to self-evident, and idealised versions of ourselves who deduce a conclusion from some premises using first-order-formalisable rules are arguably as justified in it as we are in the conjunction of the premises (though see §5.1 below). Note, however, that PA does not satisfy all of the further Euclidean requirements. In particular, it fails to meet the requirement of completeness for familiar Gödelian reasons: not all arithmetical truths are derivable in PA by the First Incompleteness Theorem, nor are all known truths. Examples of PA-undecided known arithmetical claims include Goodstein's Theorem and the consistency of arithmetic.

2.2 Deductive-ZFC

Contemporary philosophers of set theory tend to think of set-theoretic axioms as justified by a mixture of intrinsic and extrinsic considerations. The well-known iterative conception of set justifies some of the ZFC axioms, for example the Pair Set Axiom (if *a* and *b* are sets then the set $\{a, b\}$ exists). Whether it justifies all of them, and which of the proposed axiom extensions it justifies, is open to debate.[3] Be that as it may, it is also generally recognised that at least some, if not all, the ZFC-axioms are also justified extrinsically, for example because they imply the standard theorems of arithmetic and analysis (as interpreted within set theory). As to the details of the exact intrinsic/extrinsic mix behind each axiom, this is the sort of question methodologists of set theory discuss and dissect.[4]

2.3 Deductive-in-shape

The third and most anaemic conception of a deductive theory sees it as no more than the deductive closure of some axioms. A toy example illustrates this thinnest of conceptions. Suppose I take a handful of facts about my friend Bob and call them 'axioms'. They might be mundane physical facts about Bob such as how tall he is, the shape of his nose, his shoulder width, and so on. I then take the set of implications of these 'axioms' and call it *Bobology*. Bobology may not be a particularly interesting theory but it is deductive-in-shape, by definition. Flippant though Bobology sounds, we'll return to it in §5.3 to illustrate an important point.

A theory that is deductive-in-shape but meets no further epistemological conditions, you might complain, is not an interesting one. I agree. I include deductiveness-in-shape because it is the minimal starting point, on which others build, and because it will be instructive to relate it to non-deductive knowledge of mathematics.[5]

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[3] The first question is for example discussed in my (2007), reacting to Boolos (1971, 1989).

[4] These details will not be relevant here. A now several-decades-old but still very relevant study of the justification behind set theory's axioms and axiom candidates is Maddy (1988).

[5] It's worth mentioning in passing that any list of sentences closed under deducibility can be turned into a theory that is deductive-in-shape. The most trivial way is to take all sentences as axioms. An only slightly less trivial method is available, with the added feature that the axioms become independent, when two conditions are met: (i) we can enumerate the list of sentences; (ii) we can determine whether finitely many of the given sentences imply another given one. Given (i), start enumerating the sentences. When you reach the n^{th} one, s_n, then, given (ii), determine whether s_n follows from the previous axioms, drawn from $s_1, \ldots, s_{n-1}$. If not, add s_n to the axiom set; if so, don't add it. Evidently, the resulting set of axioms is both independent and implies all the sentences from the original list.

3. Connections

We've introduced three types of deductive theory: Euclidean-deductive; deductive-ZFC; and deductive-in-shape. Before moving on to non-deductive knowledge, let's take a moment to relate the three notions to one another.

The connection between the first two and the third is obvious. If a science is either Euclidean-deductive or deductive-ZFC then it is deductive-in-shape. This is because each of the first two properties supplements being deductive-in-shape with something else: it bolts on another condition. The first conception adds that the axioms are self-evident and true and the rules of inference justification-transferring. The second adds that the axioms are justified by some mixture of intrinsic and extrinsic evidence in the way ZFC's are.

The link between being Euclidean-deductive and being deductive-ZFC is not quite as straightforward. A theory that satisfies the three tenets of Euclideanism (truth, self-evidence, flow) seems to be intrinsically justified. For self-evidence can be thought of as evidence on any conception, or at least on any conception of the axiom's subject matter, with the result that a self-evident axiom is justified intrinsically. If being deductive-ZFC encompasses any combination of intrinsic and extrinsic justification, then a Euclidean-deductive theory is also deductive-ZFC.

In the other direction, being deductive-ZFC does not imply being deductive-Euclidean, as demonstrated by contemporary set theory. For example, even those who think ZFC's Axiom (Scheme) of Replacement has some intrinsic support would not go so far as to claim self-evidence for it. When bent into deductive shape, theories in natural science are typically like ZFC in this respect. Special Relativity, for instance, is founded on two postulates: the invariance of the laws of physics in all inertial frames, and the constancy of the speed of light in vacuo. But neither of these postulates is self-evident, and indeed the second is counterintuitive to anyone brought up on Newtonian mechanics. (Of course, Special Relativity is not usually presented in *strictly* deductive fashion, but to make the point we need only imagine it recast this way.)

4. Non-deductive reasoning

The above is the first, and far from the last, word on the three conceptions. And the trio don't exhaust the list. In particular, I haven't included deductive theories in which the axioms are true by stipulation, classic examples being theories in algebra (e.g. the group axioms are stipulative of the group concept). I shall come back to these theories in §5.1. My main interest in this essay lies not in further exploring the various types of deductive theories but in relating them to non-deductive knowledge.

To build up to the issue at stake, consider a radical and revisionary idea: that there can be non-deductive *knowledge* of mathematical propositions. I should add: non-deductive and *non-testimonial* knowledge, since there is of course plenty of non-deductive knowledge of mathematics that takes the form of testimonial knowledge—for example it's how most of us know that Fermat's Last Theorem is true. This sort of knowledge is ultimately empirical because it depends on your source's reliability. The question, then, is whether you can know a mathematical statement without having a proof of it, once we set aside testimony; or to put it another way, whether you can know a mathematical statement without you or anyone else having a proof of it. To say that you can goes squarely against the way mathematicians speak, since they typically equate p's being known with there being a proof of p. This is the quasiuniversal, orthodox view, not just in mathematics but in its philosophy. I am one of the very few to dissent from it, because I believe that, in the best cases, non-deductive evidence can yield knowledge of a mathematical proposition (Paseau 2015).

My aim in the second half of this essay is not to rehash the arguments for non-deductive knowledge of mathematics but to relate them to the various conceptions limned earlier. Still, it will be worth giving a summary of the considerations in play. It will also be useful to have a couple of labels for the two opposing views.

> *Traditionalism*
> For no mathematical proposition *p* is it possible for me to know that *p* without someone or other having proved that *p*.
>
> *Revisionism*
> For some mathematical propositions *p*, it is possible for me to know that *p* without someone or other having proved that *p*.

Four clarifications are in order. First, to state the obvious, both claims are stated about me for vividness, but generalise of course to anyone whatsoever. Second, both Traditionalism and Revisionism understand the notion of proof in the informal sense of mathematical practice. The Traditionalist is *not* committed to the implausibly strong claim that to know Pythagoras's Theorem we must have grasped a formal derivation in formal, axiomatic geometry. (Otherwise very few people indeed would know the theorem non-testimonially, and no one before the modern era would.) Moreover, ultimate axioms from which proofs proceed might be a broader class than informal versions of the sort of axioms that typically make up formal theories. For example, '1 + 2 = 3' might count as an axiom of arithmetic, even though it is not an axiom of its formal regimentation PA. The third clarification is that, as I hope is clear, Revisionism allows for plenty of deductive knowledge of mathematical propositions. The Revisionist simply says that there is non-deductive *as well as* deductive knowledge. (The qualification 'non-testimonial' will henceforth be implicit.) Fourth, Traditionalism as stated is compatible with the sceptical idea that there is simply no mathematical knowledge, or that our usual deductive means of trying to acquire it doesn't succeed. To avoid lengthening the discussion by chiselling away at the definition of Traditionalism to incorporate all the required qualifications, let me simply say that I mean to exclude this sort of scepticism. Similarly, Revisionism as stated is compatible with my coming to know that *p* because God has in an unmistakeably authoritative manner told me that *p*, although one has actually proved that *p* (including God, since we can suppose that He need not prove anything to know it's true). Again, I mean to exclude this sort of case.

The observation at the heart of the Revisionist's case is that, in the best cases, there is a vast and varied mass of non-deductive evidence for the mathematical proposition *p* in question. Examples might be Goldbach's Conjecture or the Riemann Hypothesis, or the claim that some number is prime on the basis of primality testing and other evidence. So great, so varied, so convincing, is the evidence in the best cases that it seems mistaken to suppose that we don't in fact know that *p*. The analogue of the sort of evidence amassed in these cases would, if *p* were empirical, indisputably suffice for knowledge that *p*. In fact, in the best cases, the cumulative force of the non-deductive evidence behind *p* is considerably stronger than the evidence for empirical propositions which, philosophical scepticism aside, we would not hesitate to say we know.

What sort of evidence do I have in mind? Elsewhere, I have summarised some of the non-deductive evidence for Goldbach's Conjecture (GC), which states that every even number from 4 onwards is the sum of two primes. Here I'll summarise those summaries.[6] There is a good deal of enumerative inductive evidence for GC; this evidence falls in the range of what are reasonably believed to be potentially hard cases—smaller numbers; various slightly weaker claims than GC have been proved; the density of counterexamples to GC is provably zero; a heuristic argument based on the Prime Number Theorem, coupled with the enumerative inductive evidence, strongly suggests that GC is true; and so on. Much other evidence can be adduced, notably from random models of the primes.[7] As Gowers (2023, pp. 62–63) points out, one can write down an approximate formula for the number of ways in which an even number can be written as the sum of two primes that coincides almost exactly with what random models of the primes would predict.

This *prima facie* strong case for non-deductive knowledge of claims such as GC can be strengthened by arguments of a more philosophical flavour. I offered four such in my 2015 article, which I'll also now summarise. The first is that deduction does not seem to possess any special knowledge-conferring property that non-deductive arguments lack. The second is that analyses of knowledge in general epistemology that have gained some traction fall into two camps: they either allow that knowledge of mathematics may be obtained by non-deductive means; or they do not apply to knowledge of mathematics. More positively: our best general accounts of knowledge support Revisionism. The third is that knowledge of some axioms is obtained by non-deductive means, so that even some seemingly deductive knowledge of mathematical *p* is in fact non-deductive (more on this below). The fourth is that, since we can have non-deductive knowledge of physical facts, we can exploit linkages between physical and mathematical facts (e.g. physical and pure geometry) to acquire knowledge of mathematical facts.

I will not re-make or re-assess most of these arguments here, though the third will resurface in a slightly different guise in §5.3. What I wish to focus on instead, in §5, is the question of how Revisionism sits with each of the conceptions.

Three final points. One valid question is just how much non-deductive knowledge of mathematics we have or might have on the Revisionist picture. For all that Revisionism says, it might be in principle possible but in practice extremely difficult to acquire such knowledge. As my examples of the Goldbach Conjecture, the Riemann Hypothesis and primality testing indicate, I don't think this sort of knowledge is that rare. They all seem to me to be good candidates, as a detailed account would show. (Needless to say, the case for Revisionism does not turn on any given one of these being known non-deductively.) The general point is that Revisionism would lose most of its interest if non-deductive knowledge of mathematics were possible but the bar was set so high as to be virtually unattainable.

A second point concerns a potential objection. Someone could object that Revisionists have simply misunderstood the meaning of the word 'knowledge' as applied to mathematics. But

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[6] See my (2015) and (2023) for more detail, my (2015) for a little more on primality testing, and Franklin (1987, 2021) or Gowers (2023, esp. pp. 70–73) for some of the non-deductive evidence behind the Riemann Hypothesis.

[7] In which the number appears N with probability $1/\log N$ (the same 'probability' that a number N has of being prime, given the prime number theorem). These models are interestingly discussed in a philosophical context in D'Alessandro (2022).

there is absolutely no independent reason to believe this. The general idea that dissent to principles involving the word *W* amounts to not knowing the meaning of *W* has recently come under fire.[8] Moreover, it would be very odd to suppose that Revisionists have misunderstood the word 'knowledge' given that their use of it accords with the overwhelming majority (if not all) of the leading accounts of knowledge in general epistemology. For it would have the consequence that these epistemologists don't know what they are talking about when they talk about knowledge. So I shall put this objection to one side.

The third point concerns a related objection. Someone might complain that Revisionism is deeply anti-naturalist because it accuses mathematicians of widespread error. Almost to a person, mathematicians use '*p* is known' synonymously with '*p* has been proved' in mathematical contexts. Keep an ear out for this the next time you go to a mathematical talk and you're likely to hear it.

In earlier work (Paseau 2015, p. 781), I considered this objection and gave the following response to it. Naturalism strives to respect expert scientists' opinion about matters they have expertise in. Quantum physicists about quantum physics, evolutionary biologists about evolutionary biology, and so on. Mathematicians are not epistemologists; their expertise is not about knowledge or justification or any other epistemic notion or in the use of associated words. Of course, attributing widespread error to mathematicians in their use of these terms requires some sort of explanation (the undue influence of Euclideanism may be part of the story—see below). But it does not offend against the spirit of naturalism.

A referee for this volume has pointed out that there is a different sort of response available here. When mathematicians use the word 'knowledge' and cognates in a mathematical context, perhaps they use it to denote a concept that tracks the distinctive proof-based norms of mathematical inquiry. In other words, perhaps they use it to mean 'know on the basis of proof'. Their claims about this species of knowledge are indeed by and large correct, so we can avoid an error theory. On this response, the only thing that needs further explanation is why mathematicians or those who write about them such as philosophers of mathematics haven't generally noticed that mathematicians seem to mean something different when they use the word 'knowledge' in a mathematical context to what the rest of us—and presumably mathematicians themselves—mean in non-mathematical contexts.

At the time of writing, I'm not sure which is the best response. But I'm confident that one of the two is, and they both deal with the objection.

5. Revisionism and the three conceptions

5.1 *Revisionism and Euclidean-deductive*

On the Euclidean picture, the argument for Traditionalism goes roughly like this. Mathematical proofs bestow on their conclusion an epistemological quality—the highest form of evidence, transmitted from the axioms by inference—that no non-deductive argument could ever hope to achieve. Non-deductive evidence falls very short of this gold standard, and so cannot lead to knowledge. We might call this the 'gold standard' argument.

How good is the argument? Let's not mince words: it's shoddy. Its premise is false and its conclusion does not follow from its premise. Let me explain why.

[8] See in particular chapter 4 of Williamson (2007).

The premise is false because mathematics as a whole is not Euclidean.[9] It satisfies neither the self-evidence requirement nor the flow principle, which means that it falls short of two of the three core principles of Euclideanism. (I shall not discuss the four subsidiary principles.) This fact is generally appreciated for foundational mathematics. As these points should not be very controversial, I shall be relatively brief.

Equating foundational mathematics with set theory,[10] it is clear that axioms such as Infinity or (the axiom schema of) Replacement are not self-evident. The large cardinal axioms regularly deployed in higher set theory fall very short of the ideal of self-evidence. Even some axioms outside set theory that were previously thought to be self-evident are now no longer seen as such. A notorious example is Euclid's own Parallel Postulate, which was thought to be self-evidently true of actual space. Yet it now seems neither self-evident nor true of actual space; at best, it is a stipulation that the geometry we are interested in is Euclidean. Another example from *The Elements* is Book I's common notion that the whole is greater than the part, which on one understanding at least clashes with modern set theory. Since this sort of point is generally appreciated, I shall not belabour it.

A staunch defender of Euclideanism might retort that this leaves many other areas untouched. In particular, as we said earlier, arithmetic approximates the Euclidean ideal. But arithmetic, although developmentally foundational and presupposed throughout mathematics, is just a small part of modern mathematics. The Traditionalist must show that all mathematics is based on self-evident axioms. This is quite implausible. Even number theory trades in much more advanced and less secure areas of mathematics than elementary arithmetic. Think for example of analytic number theory or Wiles' original proof of Fermat's Last Theorem, which made use of large swaths of mathematics.

The staunch Euclidean might try another tack. She might say that stipulated axioms, say in group theory or ring theory or field theory, are self-evident. (Some people call these algebraic theories, but I prefer not to because many theories outside algebra, e.g. analytic topology, have stipulated axioms.) The idea is that stipulations do not allow for doubt: if you lay down the group axioms and say that that is what you mean by a group, you can be as sure as you'll ever be that groups satisfy the axioms you've just laid down. Stipulations are self-evident.

The problem with this reply is, contrary to the impression sometimes given by some philosophers of mathematics, there's much more to these areas than what follows from their respective axioms. Think about group theory, for example. The group axioms are typically given on the first couple of pages of a textbook in group theory. After a few trivial exercises in chasing consequences of the axioms (e.g. proving that the group identity is unique), we start invoking assumptions from other parts of mathematics, not given by the group of axioms; as soon as you start proving things about homomorphisms, for instance, you are invoking background assumptions about functions that go beyond group axioms. Beyond first baby steps in group theory, you just hit groups with every tool at your disposal, from any part of mathematics that proves useful. So to establish that the resulting theorems are maximally evident, you'd have to show that all these tools come from Euclidean parts of mathematics. But they are not, or to put it dialectically, that is precisely what was to be proved.

[9] The next few paragraphs summarise some material from Paseau and Wrigley (2024).
[10] The moral would not change if the foundation were something else, e.g. category or topos theory instead.

Another problem with the response is that even if the axioms are stipulated, showing that a mathematical structure satisfies them is by no means an easy task—it can be far from self-evident that it does. Take the algebraic numbers, i.e. solutions of polynomials with rational coefficients. To apply results from field theory to them, we must convince ourselves that the algebraic numbers form a field. It is fairly obvious that if α is an algebraic number then so is $1/\alpha$; but a not entirely trivial argument is needed to prove that if α and β are algebraic numbers then so are their sum and product. This example is drawn from undergraduate mathematics, but of course more sophisticated mathematics can be mined for examples that make the point even more convincingly.

The second reason for rejecting the gold standard argument's premise is that Euclideanism's flow principle is also false. Let's put the case in terms of rational credences, assuming there is a close link between rational credences and justification. (The linking principle is something like this: suppose you have deduced *q* from *p* and that your rational credence in *q* is less than your rational credence in *p*; then your justification for *q* is less than your justification for *p*.[11]) It is fairly clear that a deductive argument need not, and typically will not, be rational credence-preserving. A reflective subject, aware of her limitations of reasoning power, memory, attention span, etc., who follows a deductive argument to its conclusion should typically give this conclusion lower credence than she gives the conjunction of the premises. If the subject knows that in any ten-minute period there is a 1% chance she will let an error slip, the credence she should give to a conclusion of an argument she has been working through for a couple of hours and whose premises she collectively believes to degree 0.9 should be considerably less than 0.9 (if that is her only evidence for the conclusion).

That may be granted. But the reply might be that deductive arguments are rational-credence-preserving for *ideal* subjects. To which the response is: why should ideal subjects not be subject to inferential uncertainty? Suppose that an idealised agent can be rationally certain that they have correctly applied an inference rule when they have and can always spot mistakes when they have not. We can think of such an agent as infallible in a certain sense; if they think that *p* is a logical consequence of some set of premises according to a given notion of logical consequence, they are correct. Still, an ideal subject might well give credence less than 1 to the proposition that some deductive rule is truth-preserving. In other words, they would not be *philosophically* infallible about whether a given notion of logical consequence is correct. I recognise, of course, that there is one legitimate sense of 'ideal agent' in which such a subject is logically omniscient, and perhaps for them deductively valid arguments are rational-credence-preserving. But the question is then how such agents relate to us. Since *we* are not ideal agents, the Euclidean picture does not tell us how we know mathematics.

We can put the point a different way. The mathematician Tim Gowers thinks it possible 'at least in principle, to justify a mathematical principle not just beyond all reasonable doubt, but beyond any doubt at all' (assuming one does not take 'the drastic step of doubting either some very basic axioms or some very simple rules of deduction'). Gowers immediately, and quite correctly, adds the qualification that the sort of arguments that appear in typical research articles are not formal proofs but 'more like blueprints that are sufficiently detailed to convince experts that formal proofs exist'. So the way I would put it—and I think Gowers would agree—is that even if an idealised reasoner who ran through a formal proof of *p* would believe that *p* beyond doubt, we, who at best have blueprints for such proofs' existence,

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[11] The principle focuses on the one-premise case for simplicity but is easily generalised.

cannot do so. In real-life cases there is almost always some doubt, even reasonable doubt, about whether a given 'blueprint' corresponds to a 'formal proof' or not (to put things in Gowers' language).[12]

In sum: mathematics is not Euclidean because (a) not all of its axioms are self-evident, and (b) implication does not always preserve justificatory strength, at least for thinkers like us or even mildly idealised versions of us. The gold standard argument's premise is false.

The argument is also invalid. And I don't just mean deductively invalid (clearly it is that), but invalid in a broader sense: it's not a rationally compelling inference. The short version of the objection: just because a gold standard for knowledge exists, why can't a silver standard also exist? Suppose you are convinced that there is a way of coming to know that *p* that is particularly commendable, peerless even. Why should that mean that there is no other way of coming to know that *p*? Why should the best be the enemy of the good?

The 'best shouldn't be the enemy of the good' idea is inherently plausible, a platitude even. That there is a stronger form of knowledge-generating justification for *p*—along one clear dimension—doesn't preclude there being a weaker one for the selfsame *p*. It is also borne out by our epistemic practice. When it comes to observable facts, we privilege perception. Suppose I have strong indirect grounds to believe Ali didn't attend a wedding reception: the married couple told me Ali wasn't invited, Ali herself told me that morning she would be out of town, etc. Still, my indirect evidence can be trumped by simply seeing Ali at the reception. But even though perception is privileged, it would be a wildly sceptical claim to suppose that we cannot know observable facts in a non-perceptual way. We can do so in various 'indirect' ways, by reasoning or via testimony. Even if I was not there to observe Ali's absence at the wedding reception, I can know that she was absent if numerous reliable and independent reports of her absence reach me. Even in the mathematical case, we are perfectly happy (philosophical scepticism aside) to grant testimonial knowledge. You can know that *p* because you're reliably informed that Andrew Wiles, Grigori Perelman, Maryam Mirzakhani or some other mathematician has proved that *p*. Euclideans might take this route to knowledge to *p* to be inferior to inferring *p* from the self-evident axioms that imply *p*, but they cannot very plausibly deny that it is *a* route to knowledge.

All in all, the Euclidean case for Traditionalism is unconvincing. Euclideanism is instructive, however, because it suggests a diagnosis of where the pressure for Traditionalism has come from. Of course, this isn't the whole story, but I strongly suspect it's at least part of it. (This is a story which, to be clear, is much more detailed and complicated than the very summary treatment I can give it here.) Here's my tentative diagnosis: the main reason for resistance to Revisionism is a sort of vestigial Euclideanism in thinking about mathematics. There may no longer be many (any?) Euclideans in the philosophy of mathematics today, but some Euclidean modes of thinking and ways of speaking persist. In particular, the rejection of any non-deductive route to knowledge of mathematics is the result of a centuries-long Euclidean conception of mathematical epistemology. It is only natural that this epistemological picture, so prevalent and so drilled into generations of mathematicians, should have made its way into mathematics' self-conception.

5.2 *Revisionism and deductive-ZFC*

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[12] Quotations in this paragraph are from Gowers (2023, p. 57).

Let's explore what the second conception has to say about non-deductive knowledge. On this picture, how might we argue for Traditionalism?

Suppose that axiom set *Ax* has intrinsic evidence behind it, or even embodies a particular conception of its subject matter. The Traditionalist might point out that non-deductive evidence for *p* is not evidence that *p* is part of the unfolding of this subject matter. But this point by itself doesn't go very far towards establishing Traditionalism. As the Revisionist sees it, non-deductive evidence can support a claim *p* that is part of the unfolding of its subject matter. And this non-deductive evidence can lead to knowledge.

The Traditionalist might add that non-deductive evidence is not as strong as the evidence that comes from deriving *p* deductively from the intrinsically supported *Ax*. For example, non-deductive evidence for GC is not as strong as the evidence a proof of GC from the intrinsically supported Peano Axioms would provide. But this would be a version of the gold standard argument already encountered in §5.1 in connection with Euclideanism. We can lay it to rest in the same way: if the best is not the enemy of the good, still less is the better the enemy of the good. There are many routes to knowledge, not all on an epistemic par. And to insist that any route to knowledge of *p* from a domain with an intrinsically supported set of axioms must be deductive is of course to beg the question.

What if *p* doesn't follow the axioms? Then *p* isn't part of the unfolding of *p*'s subject matter. It's not at all clear why, on this picture, one would want to deny the status of knowledge to someone who comes to believe *p* in this non-deductive way.

Traditionalism also sits ill with the fact that many axioms in mathematics have extrinsic as well as intrinsic evidence behind them. Let's describe an actual historical example in a schematic way.[13] Suppose a set of set-theoretic axioms, *O* (for 'old'), is given. Think of *O* as making up Zermelo's 1908 axiomatisation of set theory. A new axiom candidate, *N* (for 'new'), is then proposed. Here, think of the introduction of the Axiom Scheme of Replacement in the early 1920s. Suppose *N* is motivated extrinsically, because of its mathematical power: it allows us to prove some desirable results we could not prove without it. This was in fact how it was introduced in the 1920s. Neither Skolem nor Fraenkel motivated Replacement intrinsically by reference to the iterative conception of set. Indeed, although a proto-version of that conception may be found in Mirimanoff, it is not cited in the relevant works by Skolem and Fraenkel, and the iterative conception only emerges later, in Zermelo and in Gödel's work, and the conception is formalised even later than that, in the hands of Shoenfield and Boolos. Fraenkel himself has this to say: 'the intuitive or logical self-evidence of the principles chosen as axioms [of set theory] naturally plays a certain but not decisive role; some axioms receive their full weight rather from the self-evidence of the consequences which could not be derived without them'.[14] Putting it schematically, the justification for *N*, is, at this point, before its elevation to axiomhood, non-deductive. *N* may of course have some intrinsic justification too, but, just as in the scientific case, if it is believed in part because of its consequences, its overall justification is non-deductive. Replacement itself was later regarded as having some intrinsic plausibility and has been

[13] The following reworks some of the discussion in section 6 of my 2015 article. The historical story sketched here is sufficiently well-known that I have not provided references; some of the original papers may be found in the van Heijenoort (1967) collection.
[14] Cited in Lakatos (1976, p. 25).

argued by some to be part of or implied by the iterative conception in a loose sense,[15] or at least to harmonise with it. But that does not change the fact that, at least when introduced, Replacement was in part justified extrinsically.

It is important to appreciate that, in this schematic example, the extrinsic evidence behind *N* is not merely evidence for turning a previously known principle into an axiom. It is evidence for *N* itself, i.e. evidence that led the mathematical community to know that *N*. And clearly some of the grounds on which *N* is known are the same sorts of non-deductive grounds that revisionists believe provide us with non-deductive knowledge of other mathematical claims. As with Replacement, the Revisionist maintains that some statements—such as *N* at the time of its introduction—are known non-deductively, not merely that axiom status is bestowed upon some already-known statements for non-deductive reasons.

In short, the schematic case supports Revisionism. As the Revisionist sees it, there is no significant difference in some of the evidence for the axiom *N* and the present evidence behind GC. This puts pressure on the claim that we do not know GC, assuming mathematicians knew *N* when it was introduced.

The Traditionalist might object that Replacement is also supported intrinsically, but that GC, however, isn't. Although GC is simple to state, she might continue, it has no inherent intrinsic plausibility. To this, the response is twofold. The first is that Replacement was not regarded as intrinsically plausible when it was introduced. The conceptions that have been used to justify it—a liberal version of the iterative conception and the Limitation of Size doctrine—were later developments. So even axioms don't have to be intrinsically plausible. The second point is that Replacement is in the same boat as many first principles of science. Many of them—conservation of energy, Special Relativity's postulate about the invariance of laws of nature in inertial frames, etc.—have at least some intrinsic plausibility. But no one thinks that because these scientific principles are intrinsically plausible, they are known deductively. Clearly, scientific principles are known non-deductively, even when they are intrinsically plausible.

5.3 *Revisionism and deductive-in-shape*

On the thinnest conception of a deductive theory, there is no reason whatsoever to deny the possibility of non-deductive knowledge of its subject area. For there need be nothing more to the deductive theory than its deductive shape. In particular, there's nothing to exclude another way of coming to know its subject matter, which may in fact be roughly the same way as we come to know the axioms of the deductive science.

Bobology illustrates this point. This theory, recall, consists of some 'axiomatic' facts about my friend Bob closed under implication. The relevant facts, let us imagine more precisely, have to do with Bob's size and shape. Bobology is thus the applied geometry of Bob's body: his height, the shape of his features, and his measurements more generally. We may call facts deduced in this way Bobological. Bobology, observe, does not include any information about Bob's weight, or his hair colour, or his psychology, or anything else to do with Bob that cannot be deduced from his body shape.

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[15] Including by me (see my 2007). In the presence of the other axioms, instances of first-order Replacement are all implied by the Reflection scheme.

We might acquire two sorts of information about Bob in a non-Bobological way. The first sort of information is non-Bobological. For example, we might ask Bob to step on a scale and discover that he weighs 80 kg. Clearly, our evidence for this fact is not Bobological, since we cannot deduce it from Bob's body shape. The second sort of information is Bobological but not obtained in a Bobological way. The axioms of Bobology might not specifically include the fact that Bob's right hand is bigger than his left hand, though they imply it (the proof might be long and complicated or it might be short and simple). Someone completely ignorant of Bobology, or who is only partially aware of its axioms, or perhaps knows its axioms but has not worked out their consequences, might nonetheless come to know this fact about Bob's hands in a non-Bobological way. They might, for instance, ask Bob to carefully place his two hands in prayer, thumb against thumb and pinky against pinky, and observe that the right hand extends beyond the left one. By doing so, they will have gained non-Bobological evidence for a Bobological fact; the evidence was non-Bobological because it was not deduced from Bobology's axioms, although it could have been.

The relevance of all this should be clear. Evidently, the non-Bobological methods just described are acceptable ways of coming to know facts about Bob. Whether the relevant fact is Bobological does not make a difference to its epistemology: Bob's weight or that one of his hands is bigger than the other are facts just as easily knowable as simple Bobological facts. Indeed, the way we come to know Bob's weight or the respective sizes of Bob's hands is very similar to the way we come to know the axioms of Bobology, *viz*. by observing or measuring Bob. Bobological and non-Bobological methods are of a piece: common-or-garden observations about good old Bob.

Now, clearly mathematics is not Bobology. It has a much more distinctive epistemology than the epistemology of Bobology. But the point is that the more mathematics looks like Bobology, the less important a role deductive justification ought to play within it. Whenever we encounter a science of *X* that approaches the Bobological end of things, we should be suspicious of attempts to epistemologically privilege deductive reasoning from its axioms. For it may be that other methods of learning *X*-facts than deduction from the chosen axioms are just as good, if not better in some cases.

Bobology also illustrates a terminological matter. The method of learning Bob's weight by weighing him, however sound, legitimate, and knowledge-producing it may be, is not Bobological. Likewise, one might insist that 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, which is why I called my 2015 article 'Knowledge of Mathematics Without Proof' rather than 'Mathematical Knowledge Without Proof'. Non-deductive knowledge of mathematics may be not mathematical; the Revisionist can concede this terminological point. The substantive question is whether or not the adjective 'mathematical' is like 'Bobological' in being, epistemologically speaking, a more or less empty honorific.

6. Summary

Being Euclidean-deductive is so stringent a conception that mathematics does not meet it. That's one reason why the case for Traditionalism cannot be based upon it. The other is that even if mathematics were Euclidean-deductive, there could still be other ways of coming to know a mathematical proposition than the Euclidean's preferred one.

As for being deductive-ZFC, it's even less clear than in the Euclidean case why lacking intrinsic support creates a barrier to knowing that *p* even where there is a wealth of other evidence for *p* (think of natural science). And at least some axioms are extrinsically supported by the same sort of evidence that Revisionists think ground non-deductive knowledge of a mathematical claim.

Finally, being deductive-in-shape is so thin a conception that the case for Traditionalism cannot be based upon it either. Deductive inference from axioms confers no epistemological virtues on its conclusions if axioms don't enjoy these virtues in the first place.

The discussion suggests a more general dilemma. The more demanding the conception of deductive mathematical knowledge, the easier it is to make the case for the superiority of deductive to non-deductive justification, and thus the easier it is to argue against Revisionism. At the same time, however, it becomes harder to suppose that we do have this sort of deductive knowledge and to match the conception's demanding conditions with actual mathematical practice. There's also the further question of why *all* knowledge, even all knowledge of mathematics, must conform to this standard, even if *some* or even *much* of it does. On the other hand, the less demanding the conception of deductive mathematical knowledge, the harder it is to justify why knowledge of mathematics cannot be had non-deductively. Either way, it's hard to see how Traditionalism could be true. The main thing Traditionalism seems to have going for it is the weight of tradition.[16]

[16] Thanks to participants at *The Heuristic View* conference in Rome in February 2023 for comments, to the conference organisers Fabio Sterpetti and Emiliano Ippoliti for inviting me, and to two anonymous referees for comments.

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