Is there still a Sense in which Mathematics can have Foundations?

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Abstract. An analysis of traditional mathematical proof is undertaken, with an implicit contrast to formal derivations. The semantic interpretation of mathematical terms plays a role in the former that doesn't appear in the latter. This semantic interpretation–with an accompanying role for intuition-is explained in terms of inference packages, which are psychologically-bundled ways of phenomenologically exploring the effect of several assumptions at once without explicit recognition of what those assumptions are. Although its correspondence with a derivation is the (ultimate) justification for the success of a traditional proof, the certainty that mathematicians experience when they study successful traditional proofs is not due to that correspondence, but rather–for the most part–to the role of inference packages in their reasoning.

1

Introduction. As little as a century ago, a number of prominent philosophers, logicians and mathematicians were obsessed over the purported need for foundations for mathematics, and arguably, related concerns spread widely into the discipline of philosophy, at least into what came to be called 'analytic philosophy'–more specifically–analytic epistemology. My aim–in this paper–is to revisit the issue of foundations for mathematics in order to determine in what sense, if any, foundational concerns should still be regarded as living philosophical issues. Perhaps not unexpectedly, my conclusions will be almost uniformly negative. The possibility of a nearly total erasure of the boundaries between mathematics and the other empirical sciences may be a more surprising side effect of this denial of foundations.

A model for foundations. Euclidean geometry (historically) provided a powerful and influential model for how a foundation for a mathematical subject might look. The model, apart from its striking impact on philosophy, didn't have (indeed–couldn't have had) much influence on mathematical practice itself–at least not until a logic involving relations and quantifiers became available in which axiomatizations with rich mathematical content could be couched. The model is this: A mathematical subject matter M is constituted by a set of fundamental notions. These fundamental notions, in turn, are characterized by a set of axioms. Further notions and theorems are derived, respectively, by definitions and by rigorous mathematical proof. A foundation, so conceived, fully characterizes M by its fundamental notions (via definition) encompassing all the notions operating in M, and by rigorous mathematical proofs from its axioms of all the theorems of M.

Rigorous Mathematical Proof. Thus far unmentioned is the nature of the centerpiece of mathematical practice–rigorous mathematical proof.
Although proofs–historically–were repeatedly scrutinized for implicit assumptions and lacuna, and although some axioms (e.g., the parallel postulate) seemed far too suspicious to be left freestanding (rather than proven on the basis of less contentious assumptions), proof itself remained an epistemic given, and the logic implicit in its operation–if any–remained securely hidden. In part this is because rigorous mathematical proof appeared in the guise of the vernacular, supplemented by coined terminology and diagrams; but the invention of (modern) logic required the simultaneous invention of artificial languages in which the operations of said logic could be made syntactically visible.

The invisibility of the actual process of inference between steps in rigorous mathematical proofs, coupled with the fact that scrutiny of proofs–historically–proceeded by the exposure of additional implicit concepts–and implicit assumptions–perhaps gave rise to the Cartesian impression that were every step in a proof–every concept employed–made explicit, one would see *immediately* how each step followed from the one before it. Unfortunately, inference in the mathematical case–and everywhere else for that matter–seems to require schematic logical rules: ones that apply to classes of arguments. Any attempt to make entirely explicit the application of such schematic rules to sentences in a particular proof leads to an infinite regress.[1]

Epistemic foundations. The model purportedly provided a foundation for mathematics in at least two respects. First, epistemically: It seemed to provide *explanations* for why mathematical results had the peculiar epistemic properties they seemed to have–such truths were established in ways that seemed pro-

[1] [10], [33], [34].

foundly convincing, at least when compared to other ways of establishing truths.[2] Since rigorous mathematical proof was an undisputed medium of inference–itself uncontentiously yielding truths from truths–focus naturally turned to the premises. These were thus required to be self-evident–at least as far as their truth was concerned–and the subject-matter of such premises–Euclid's axioms, for example–bore the burden of explaining the apparent necessity of mathematical truth.[3]

Compositional foundations. The second way that the Euclidean model seemed to provide foundations for mathematics smacks–but only just–of metaphysics: This was that, by means of the definitions and theorems shown, mathematical theorems about more complicated objects result from those about simpler objects, and ultimately from the axioms characterizing the fundamental items initially given. A way of seeing why a result is true of a kind of object proceeds by seeing how that object can be (mathematically) compounded out of other objects about which useful results have already been shown. Triangles are composed of line segments, line segments, in turn, are composed of points–but other sorts of less obvious constructions arise as well: polygons out of triangles, or–more ambitiously–a sphere out of circles (degenerate and otherwise).

Euclidean geometry, of course, utilizes many such constructions: Diagrams in that subject matter are often simply depictions of how one kind of figure can be composed out of others. A familiar example–made so by [31]–involves triangles tucked neatly into squares. But the phenomenon, of course, is everywhere in modern mathematics. In set theory, for example, counting numbers can be taken to be certain sequences of sets, set-theoretical constructions of those in turn can be proven to have the properties we attribute to rational numbers, given–that is–the properties such sets, and the sets functioning as natural numbers themselves, already have; further set-theoretical constructions yield suitable ersatz for the real numbers–and the proofs of this, again, turn on what has already been shown about the ersatz rationals (as well as leaning on accompanying results about set-theoretic constructions). The Baire category theorem illustrates similar concerns: A complete metric space is shown *not* to be the union of a countable collection of nowhere dense sets.

In general, mathematical objects are often explicitly constructed from other ones: Functions are literal constructions out of the objects such functions map to and from (they are sets of pairs of such objects); spaces–of various sorts–are

[2]See chapter 6 of my forthcoming. It's a little sticky to say exactly how the establishing of mathematical truths is epistemically special. Traditional views invoked a priori modes of inference, and the (purported) recognition that such truths are (metaphysically) necessary. It's clear why such invocations are tempting; nevertheless they fail upon close scrutiny.

[3]Even if such postulates seemed evident, if one thought they were about external objects–abstracta, say–one had to explain how one knew they were true. If, on the other hand, one thought they were about constructions of some sort, one had to explain why theorems about such constructions were empirically useful.

constructed from other previously given mathematical objects–functions, points, spaces of various sorts, and so on–and engaging in such constructions enables theorems about the resulting new objects on the basis of theorems about the 'stuff' those objects are constructed from. Nevertheless, 'compositional', 'compounded' and 'constructed', as so used, are pretty much sheer metaphor. I say more about this in *Foundations?*.

Modern versions of the Euclidean foundationalist model. In late twentieth-century versions of the model, the logic is no longer left implicit, but is instead built explicitly into an axiom system by virtue of the kind of language the axioms appear in, and the formal rules such languages are stipulated to obey,[4] e.g., a first- or higher-order language. Apart from that, however, fundamental notions–governed by axioms–are still present. We can, for illustrative purposes, take these to be those of set theory, and thus governed by a set of axioms, say Zermelo-Frankel set theory plus the axiom of choice (hereafter ZFC).

Foundations? Necessarily accompanying any modern perspective on the foundations of mathematics is one of the most famous twentieth-century limitative results: Gödel's theorem. This, unfortunately for the foundational project, offers several blocks against attempts to provide a foundation for mathematics along the lines of a modified Euclidean approach.

First, the hope has been dashed that a set of nonlogical axioms, with accompanying fundamental notions, can be provided even for particular branches of mathematics: Any substantially rich mathematical subject matter is amenable to incompleteness, both in the sense that any decidable axiomatization for it is incomplete, and in the sense that such incompleteness allows the open-ended introduction of new notions that further illuminate the subject matter. Apart from that, there has been a universal tendency to trade in the simplicity and epistemic obviousness of a set of axioms for others possessing increased deductive power. Since not even the consistency of a set of nonlogical axioms of any mathematical significance can be guaranteed, there seems little reason to hope for epistemic foundations–as so described, anyway, in *Epistemic foundations*.

But second, 'constitutional foundations', aren't forthcoming either. I've already mentioned that uses of 'constituted', 'constructed', and so on, in mathematics, are largely metaphorical. A way to see this is to note that such constructions–especially when carried out in a set-theoretic context–offer no natural 'ontological hierarchy' of mathematical objects. Lines, for example, may be constructed from points, planes from lines; but so too, points may be constructed from lines (or spheres) or planes, and so on. Which mathematical objects may be compounded

[4] I'm distinguishing logical axioms, such as **∃-Introduction Rule** or **Cut Rule** (e.g., [37], p. 21), from the specific nonlogical axioms of a postulate system, such as **Axiom of Extensionality** or **Axiom of Power-Set** (e.g., [24], p. 5, p. 19), by calling the latter 'non-logical axioms', and the former 'logical axioms'. At times where context makes it clear, I omit the adjectives.

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out of which turns in large measure on sheer taste: how a mathematician wants to develop a subject deductively–where he or she is starting from, what sorts of mathematics are best presumed available to the implicit audience, and so on.

Even apart from what we might call the 'constitutional arbitrariness' of mathematical abstracta, there is the issue that the tools for such constructions–set theory, say–are themselves not innocent mortar only holding together the various bricks of mathematics; they too involve substantial and open-ended properties. Thus what sorts of objects can be constructed from what depends on what sorts of sets–and set–theoretical constructions–there are, as a glance at the implications (within set theory) of the continuum hypothesis or the axiom of choice reveals; and so this is a matter of what axioms for set theory we choose. Since set theory is incomplete, and since this incompleteness can bear directly on, say, exactly what the contents of the powerset of a set comes to, how rich the set of functions on a manifold is, for example, is affected palpably by the set theory that manifold lives within–what properties such a space of functions therefore has, is subject to changes in set theory.[5]

2

Foundations as a matter of proof. I've suggested that two very natural ways of understanding foundations for mathematics are bankrupt: One can't expect there to be a fundamental set of notions out of which all the notions of mathematics may be constructed by the twin tools of definition and theorem. Nor can one hope for a set of (nonlogical) axioms for mathematics which can be shown to possess valuable epistemic virtues–such as a priori presence (to the mind) or self-evidence, or even manifest consistency. Still, there are remnants of epistemic foundational possibilities in the still-remaining central tool of mathematical practice–proof. Let us, therefore, turn to proof–and its logic–to see if it has elements that can (still) function foundationally.

What the logic of mathematics can do for us foundationally turns on what that logic is. And this is a matter of some debate. Recollect the universally perceived property of proof–as it appears in traditional mathematics–that such proofs are uncontroversial (relatively speaking): collectively recognizable by mathematicians as good or not. The construction of successful proofs often turned–by general admission–on the introduction of new ideas of 'genius'; but the recognition that the new ideas, deployed in proofs, achieved the results claimed for them, was a matter recognized by capable–but otherwise mediocre–practitioners of the mathematical arts.

The algorithmic systems perspective. This suggests that the recognition of *consequence*, of what deductively follows from what–as it appears in tra-

[5] The point generalizes beyond set theory, of course. Should one, e.g., use category theory for one's constructional mortar–incompleteness will intrude as well.

ditional mathematics, anyway–is something naturally generalizable to effective recognizability.[6] However, it does not seem part of the ordinary notion of mathematical proof that a particular fixed canon of logical principles is given.[7] This motivated the perspective of mathematics given in my [2]: Mathematics is an open-ended family of *algorithmic systems*, where each system has a set of proof-procedures that are effectively recognizable–but there are no other restrictions on proof procedures for such systems.[8] Traditional subject matters of mathematics–number theory, set theory, analysis, intuitionistic mathematics, and so on–don't live within any *one* particular algorithmic system; although any particular proof of a result from any such field can itself occur within one particular system, because of incompleteness such fields can (and are) always be enriched by the embedding of algorithmic systems within stronger ones containing additional notions and additional (nonlogical) axioms.

Since there are no constraints on the logic of an algorithmic system, algorithmic systems couched in the languages of higher-order logics also appear: The only restriction is that such systems must contain a decidable (sub-)theory of any such full higher-order logic–since those logics are incomplete.

Whither abstracta? Although there are instances of logical systems with decidable axiomatizations that nevertheless allow characterization of certain mathematical notions that elude first-order characterization, it's generally the case that requiring effective recognizability of the consequence relation defined by the logic governing an algorithmic system leads to a failure to characterize domains of objects that traditional mathematicians took as the subject matters that they were studying. A notorious example is the counting numbers with addition and multiplication: Any first-order axiomatization of the counting numbers holds, as

[6] Practitioners realized, of course, that actual mathematical proofs were not–as they stood–items that one *straightforwardly* recognized as consisting in valid steps. The Cartesian explanation for this was that not every step in such traditional proofs is explicit. To the extent that the steps are explicit, there can be no gainsaying them. Arguably, it was this property of mathematical proof–and this theory about what made it so convincing–that Descartes generalized to the perception of inference generally. See Rule VIII, [12], p. 19.

I understand effective recognizability as requiring that a mechanical procedure exists for recognizing proofs of an algorithmic system: There is a decision procedure for determining whether or not a string of sentences, or a diagrammatic form (if such are part of the proofs in an algorithmic system) is a proof of that algorithmic system.

[7] I am making a *conceptual* point here: it's not part of our *concept* of deduction–in mathematics (or anywhere else, for that matter)–that such and such *particular* logical rules, and not others, are part of the canon of logic for deduction. Indeed, it seems clear, since it was a discovery, and not a trivial one, that there was even one set of logical principles that sufficed for the logic of mathematics, that the notion of consequence–of what follows from what–in the context of mathematics, doesn't include any particular implications about what logical principles–if any–are involved. This fact is compatible, of course, with the discovery that there is a fixed canon of logical principles so involved. See VI below for further discussion of this.

[8] Apart from such proof-procedures not being restricted to any particular logic, as indicated in the second paragraph of footnote 6, they aren't even restricted to language-based systems: They can be diagrammatic in nature. This point will be significant later in the paper.

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well, of non-standard 'unintended' models of arithmetic. That the incompleteness phenomenon isn't *equivalent* to the presence of nonstandard models is illustrated by the fact that second-order Peano Arithmetic, where the second-order quantifiers are understood to range over all and only the subsets of the domain of numbers, although not possessing a decidable axiomatization of its logical axioms, nevertheless is categorical: Any model of second-order Peano Arithmetic is isomorphic to any other.

I've claimed, in focusing on the universally acknowledged ability of (able) practitioners in traditional mathematics to recognize successful and unsuccessful proofs, that effective recognizability of the consequence relation is a *sine qua non* of algorithmic systems. But those who notice that mathematicians understood themselves to be talking about the same objects–up to isomorphism–may well point to *this* aspect of traditional mathematical practice to instead motivate relaxing the effective recognizability of the consequence relation so that categorical characterizations of the targets of mathematical discourse become available.[9]

Motivations for mathematical structuralism. Against proponents of categorical characterizations of abstracta, it may be urged that thinking of mathematical objects as items that 'really exist'–as opposed to fictional items that we manipulate, given our various ends, and according to rules of our own making–has already been undermined by those aspects of mathematical practice mentioned under the rubric *Foundations?* above. For, surely, objects that 'really exist' are ones for which there is a fact of the matter about what they are and aren't constituted of: We would be disturbed to learn that it's equally acceptable–metaphysically–for humans to be taken as constituted of quarks, or for quarks–instead–to be taken as constituted of humans. That we–that mathematicians, anyway–aren't so disturbed at the prospect of points and lines being ontologically interchangeable in this way suggests that mathematical abstracta aren't (really) taken to exist.

Metaphysics–as practiced by philosophers, anyway–is far more adaptable than this last objection anticipates. *Mathematical structuralists* argue that it's structures, and not specific sets of objects, that are the referential targets of mathematical discourse. And such structures, of course, possess exactly the sort of flexible ontological properties that mathematical practice seems to allow mathematical objects to exhibit.[10]

Mathematical practice may seem to offer very little guidance on which of the two views currently on display is the right one. Even given the second-order perspective, the proof-practices of mathematicians at any stage must be restricted

[9] It should be said: Allusions to what traditional mathematicians thought they were talking about doesn't *quite* motivate what the proponent of categoricity has to offer. This is because terms like '0', '1', and so on, are clearly *names*, and so traditional mathematicians thought they were talking about *specific objects*, not just an equivalence class of items structurally similar to the counting numbers. I won't press this particular objection beyond this footnote, however.

[10] For a position like this, see–e.g.–[36].

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to some decidable subset of the full–but undecidable–axioms of second-order logic. And so those proof-practices may be indifferently characterized in a first-order or second-order language. The difference can only be that in one case we take there to be specific targeted structures that mathematicians are actually talking about, and in the other case we don't.

Evaluating the (metaphysical) virtues of categorical characterizations of mathematical structures. But putting it the way I just did misleadingly faults the perspective that drops categoricity of axiom systems as a required aspect of mathematical practice. For the latter doesn't deny the use of noun-phrases on the part of mathematicians when they work within one or another algorithmic system; nor does it deny that mathematicians augment algorithmic systems by embedding them in stronger ones while simultaneously taking terms in the weaker system to co-refer to terms in the stronger ones. The only claim is that there is no straightforward metaphysical way to determine what the best ways are of so augmenting algorithmic systems, and that there is no metaphysical goal–fuller axiomatizations of 'the standard model of arithmetic', say–that such augmentings can be directed *towards*. This should incline one, I think, to treat the first-order perspective as naturally fitting with a view that treats abstracta as fictional items that we–in ways we find convenient–develop ever-stronger axiomatic stories about.

But the crucial issue under consideration *now* isn't what (ontological) attitude one should take towards abstracta. Rather, the issue is this: Granting that treating the first-order formalism as about abstracta–numbers, say–provides no guidance for how one is to augment algorithmic systems, is the same true of (second-order) categorical characterizations of such abstracta? Do such categorical characterizations provide any guidance for how one is to determine additional axioms for second-order logic? The answer is that neither approach is superior in respect to its hospitality to (metaphysical) guidance: That the second-order formalism takes–mathematically speaking–the complete set of logical second-order axioms to be 'given' provides no guidance whatsoever for determining what those principles are. The complete set of second-order principles is metaphysically neutral: Indeed, it's metaphysically neutral in precisely the same sense in which mathematical abstracta themselves are metaphysically neutral. It (nor they) have any epistemic role to play in mathematical practice.

3

The relation of derivations in algorithmic systems to traditional mathematical proofs. Let's return, therefore, to the algorithmic-systems perspective of mathematics. Derivations in such algorithmic systems are finite lists of sentences in entirely formalized artificial languages. Traditional mathematical proofs, on the other hand, occur in the vernacular augmented with a specialized

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vocabulary; furthermore, each step in such an ordinary proof is *not* effectively recognizable as following from sentences appearing earlier in the proof: at least not so in the syntactic sense that's true of derivations in algorithmic systems. So a natural question is: What relationship do the formal derivations of such systems bear to traditional mathematical proofs?

Steiner and Fallis' view of this relationship. One possible (and popular) suggestion is that successful traditional proofs are *translatable* into formal derivations–because traditional proofs are (essentially) abbreviations of such formal derivations.[11] Traditional mathematical proofs, that is, differ from their formal cousins in containing additional 'basic mathematical inferences'. But such basic mathematical inferences are like subsidiary deduction rules insofar as they can be replaced by formal derivational analogues.[12] In describing basic mathematical inferences as like subsidiary deduction rules, it's not assumed by Fallis or Steiner that such basic mathematical inferences are fixed over time: It's allowed that, although such inferences should always be truth preserving, the set of such admissible inferences changes. This is both in the sense that new basic mathematical inferences are added to the list, and in the sense that some basic mathematical inference rules, although regarded as acceptable at one stage in time, may later be regarded as inferences that must be proven to be truth-preserving on the basis of other basic mathematical inferences. It's also not being assumed that such basic mathematical inferences are like the subsidiary deduction rules of logic in being 'topic-neutral'–unspecific in the branch of mathematics they may be applied to. Such rules can be transparent semantic analogues of axioms specific to branches of mathematics–and thus can be rules that don't hold generally.

The model is a nice one insofar as it succeeds in connecting aspects of mathematical practice to formal derivations which underwrite the rigor of ordinary proof in terms of translations to such derivations. It also allows the representation of certain historical shifts in mathematical practice–how principles at one stage (e.g., the Jordan curve theorem) are taken as unproblematical, and then later required to have proofs: Such principles are treated at one stage as basic mathematical inferences, but later denied that status. But it's hard, in terms

[11] [14], p. 49, in support of this position, cites Herbert Enderton ([13], p. 244) to the effect that: 'it is common knowledge that essentially all work in mathematics can be carried out in [a first-order axiomatization of set theory]'. Steiner ([39], pp. 100-102) treats traditional mathematical proofs as translatable into 'Church's applied first-order functional calculus' (p. 96). Alternatively, we might presume that the notion of validity informally at work in mathematics is made rigorous via Tarski's model-theoretic approach, and then–given some other assumptions–invoke the completeness result for first-order logic.

[12] For a formal presentation of subsidiary deduction rules, see, e.g., [22]. Fallis [14], p. 49, writes: 'It must always be possible to replace a basic mathematical inference with a derivation in a suitable formal system'. Steiner operationalizes the practice of translating an ordinary mathematical proof into a formal analogue by introducing a logician (to facilitate the translation) with ([39], p. 100) 'an obsessive-compulsive character, a character incapable of mathematical creativity'.

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of this model, to explain why such changes in attitude occur; one usually invokes changing standards of rigor but this doesn't help. After all, changes in attitude towards such principles *don't* occur because it's recognized by mathematicians that such basic mathematical inferences aren't derivationally justified (if only because, of course, no step in an ordinary mathematical proof seems to be derivationally justified). But this model hasn't any resources–apart from comparisons of ordinary mathematical proofs to the formal derivations they can be translated into–for explaining what it is about ordinary mathematical proofs that inclines mathematicians to regard some examples of such proofs as more rigorous than others.

And anyway, ordinary mathematical reasoning seems to be intrinsically semantic, not syntactic. Although one can always come up with a small list of argument patterns that do seem officially recognized in the mathematical tradition,[13] it nevertheless seems clear that traditional mathematical inference looks like it's driven semantically: by the exploitation of the recognition of properties of the objects the inferences are *about*. That is, mathematical inference seems to depend largely on insights about the purported *referents* of the mathematical terms so appearing in the proofs.[14] Arbib ([1], p. 55) puts the point this way:

> In fact, the usual proof generated by a mathematician does not involve the careful application of a specifically formalized rule of inference, but rather involves a somewhat large jump from statement to statement based on formal technique and on intuitions about the subject matter at hand.

I don't think Arbib's point can be evaded by noticing that he is talking about 'formalized rules of inference' and not a broader class of 'basic mathematical inferences'; for mathematical proof isn't–on the surface, anyway–a matter of careful–or even sloppy–applications of basic mathematical inferences either; Arbib's reference to 'intuitions' acknowledges the recognition–at many stages in a traditional proof–of properties held by the objects the proof is about as what enables the mathematician to recognize those stages as valid steps.[15]

[13] Fallis [14], p. 49, mentions the axioms of ZFC, modus ponens, proof by induction, conditional proof, and proof by cases. It's striking that so many of his examples are logical rules, and not topic-specific mathematical principles.

[14] Recollect the Cartesian perspective: Descartes didn't manifest awareness that attempts to render a proof entirely explicit would require inference *rules*. The 'movement of thought which is continuous and nowhere interrupted', that he predicted ([12], p. 19) would involve movement from one step in a proof to another, based only on the content of the concepts involved *within* each step. Awareness of the need for inference rules on his part, as we've seen, would invite the recognition of a potential regress. (Recall the discussion footnote 1 is appended to).

[15] My use of the term 'ordinary proof' should not mislead. The introduction of formalized derivations in formal languages in the twentieth century doesn't change this aspect of mathematical proof–not even with respect to the study of such formalized languages themselves! I make more of this in a moment.

Consider, as an illustration, the statement of theorem and (part of the) proof from MacLane and Birkhoff [26], p. 339:

Theorem 3.1 *A right R-module A satisfies the ascending chain condition for submodules if and only if every submodule of A is of finite type.*

Proof: First assume ACC, and suppose S a submodule of A not of finite type. Then no finite list of its elements can span S. Choose an element $s_1 \in S$; there must be an element $s_2 \in S$ not in the submodule S_1 spanned by s_1. Similarly, given elements s_1, ..., $s_n \in S$, let S_n be the proper submodule of S which they span. There is an $s_{n+1} \in S$ not in S_n; with Sn it spans a submodule S_{n+1} properly containing S_n. This process continued (by the axiom of choice) produces a sequence $S_1 \subset S_2 \subset ...$ of submodules never ultimately constant, a contradiction to ACC.

Notice that although 'basic mathematical inferences' are alluded to here, e.g., 'the axiom of choice', a great deal of the reasoning turns on the direct indication of properties of objects–in this case, elements and submodules.

The failure of logicism and Hilbert's program as practical attempts to revise ordinary mathematical proof. I often stress the success of the logicist program. After all the magnificent *Principia* project of translating mathematics–as it existed at that time–from the vernacular into a formal language succeeded! The foundational aspirations accompanying that project, of course, didn't succeed–not even when the *Principia* formalism was replaced with a more tractable and intuitively appealing set theory. But the translation of the consequence relation between assumptions and theorems as it occurred in ordinary mathematics survived transplantation to artificial twentieth-century languages–and that is an amazing fact utterly unaffected by Gödel's results. It shows, more than anything else, that some version of the logical rules invented by Frege–in some sense–actually underwrite mathematical inference.[16]

However, there is also a remarkable way that the project failed–apart, I mean, from the thwarting of the foundationalist aims that were so much a part of it. Consider this remark from Frege [15], and compare it to Arbib's [1], above:

> A single [proof] step is often really a whole compendium, equivalent to several simple inferences, and into it there can still creep along with these some elements from intuition. In proofs as we know them, progress is by jumps, ... [and] the bigger the jump, the more diverse are the combinations it can represent of simple inferences with axioms derived from intuition ... [My] demand is not to be denied: every jump must be barred from our deductions.[17]

[16] Strictly speaking, what it shows is that–more or less–any logical system that generates the same consequence relation among sentences that first-order formalisms generate is a logic that can be taken to underlie inference in pre-twentieth century mathematics.

[17] [15], §§90-91. I draw this particular encapsulation of the quote from [17], p. 63.

And consider this remark from Pasch in 1915:

> A statement R^1 is a consequent of B only because its derivation from the latter is completely independent of the meanings of the geometrical concepts occurring in it, so that the proof can be carried through without support from an actual or imagined diagram or from any sort of 'intuition'. A proof which does not meet this condition is no mathematical proof.[18]

After quoting Pasch's remark, Greaves ([17], p. 75) adds: 'This definition of acceptable proof in geometry is still current today'. Is it really? Perhaps this turns on what we're willing to describe as 'geometry'. Consider Spivak [38], p. 30-31. Among the exercises for chapter 1, we read: 'Consider the three surfaces shown below'. Below are three diagrams described as '(A) The infinite-holed torus', '(B) The doubly infinite-holed torus', and '(C) The infinite jail cell window'. We then read '(b) Surfaces (A) and (C) are homeomorphic! *Hint*: The region cut out by the lines in surface (C) is a cylinder, which occurs at the left of (A). Now draw in two more lines enclosing more holes, and consider the region between the two pairs'.[19]

It's clear that Frege and Pasch (and Hilbert, for that matter) were pressing for a significant *practical change* in the proof practices of ordinary mathematics. Traditional proofs in ordinary mathematics that contained large jumps based on intuitive considerations–such jumps indicated symptomatically, although hardly exclusively, by the presence of diagrams for guidance–were to be forever excised from respectable mathematical proof, and replaced by gapless derivations. This, as even an elementary inspection of current mathematics will make manifestly obvious, has *not* happened.[20] Consequently this stronger demand of Frege, Pasch, and company, has been replaced by the (Steiner-Fallis) weaker constraint of 'in

[18] Translated by Nagel [30], p. 238.

[19] This sort of reliance on diagrams is typical in certain areas, algebraic topology, for example (e.g., [19]), or graph theory. In other branches of mathematics such as functional analysis (e.g., [35]), the diagrams are sparser–although never absent.

[20] When philosophers show awareness of the continued presence of these things in mathematical practice, they invoke pedegogy and heuristics: Tennant [40], p. 304-5, writes: 'It is now commonplace to observe that the [geometrical] diagram ... is only a heuristic to prompt certain trains of inference; that it is dispensable as a proof-theoretic device; indeed, that it has no proper place in the proof as such'. Even apart from the fact that 'dispensable' in the above quote can be challenged on practical grounds about the resulting length of the 'heuristic free' proof, there is the fact–indicated in the last footnote–that diagrams appear everywhere in mathematics: in the study of dynamical systems, graph theory, topology, linear algebra (numerous matrices–infinite and otherwise–are diagrammatically depicted as boxes containing numbers or formulas, and proofs often describe or depict manipulations of such diagrams); so it's a highly nontrivial claim that the use of such things are 'dispensable as proof-theoretic devices'. One aim of this paper is to give indications for why–and in what sense–such a view is false. I should add that the role of diagrams in mathematics isn't suitably acknowledged by simply noting the presence of such diagrams on the pages of mathematical texts: One needs to see exactly the role(s) they play in the proofs given (e.g., recollect the quote from Spivak above where the operations the reader is invited to consider are incomprehensible without guidance from the diagrams).

principle' translations. But this leaves unanswered the question of why the original program of changing the *practice* of mathematics failed. I want to suggest that it isn't merely a matter of the fact that such derivations–in practice–are too long.[21] Rather, my suggestion is that the 'objects' that mathematical reasoning is prima facie about are *essential to* the capacity of the mathematician to create and understand mathematical proofs.

What role do objects play in mathematical reasoning? Strictly speaking, it isn't a matter of objects playing a role in mathematical reasoning–it's a matter, at best, of *representations* of objects playing a role. That is, in trying to understand the role of items 'referred to' in mathematics we need no more presuppose Platonism than we need presuppose that fictional characters exist because novelists are constantly thinking 'about' such characters as they write. So, to rephrase the question, what role does such 'object representation' play in mathematical reasoning?

Skipping steps. Before turning to a discussion of this issue, I should note how it connects to another. Fallis' [14] ultimate concern–as the title of his paper indicates–is the presence of 'gaps' in mathematical reasoning: steps deliberately or inadvertantly skipped.[22] If one sees mathematical proofs as similar to formal derivations–differing that is, only in the larger number of 'basic mathematical inferences' that one can use–then there are a number of explanations available for the presence of inferential gaps in ordinary mathematical proofs. Such explanations, though, are required to fit with an obvious datum: that with rare and temporary exceptions, such gaps don't invalidate the traditional mathematical proofs that they appear in. In the vast majority of cases there are derivations corresponding to such proofs.

Exploring the cogency of proof-theoretic explanations of such gaps.[23] First, there are the –at a time–allowable basic mathematical inferences that can be re-

[21] Although I think that is an important and often underrated consideration.

[22] It's worth pointing out that my concern with gaps in mathematical reasoning doesn't exactly take the form Fallis' does. This is because (i) Fallis has largely adopted the Cartesian story although given, as his paper exhibits, ordinary mathematical proofs skip steps, "the Cartesian story must be supplemented in some way" (p. 64). I've already suggested that things are much worse for the Cartesian story, given the danger of regress when the essential role of logical rules is recognized, is incoherent. So gaps in my sense are places where effective recognizability fails between steps in a proof–and arguably that occurs in nearly every step of any ordinary mathematical proof–exempting, perhaps, (certain) steps in sheer computations. But (ii), the translation view Fallis has adopted also causes problems. My view is that the large number of such 'gaps' in ordinary mathematical proofs is overlooked when one is too sanguine about how easy it is to provide translations of ordinary mathematical proofs into formal derivational forms.

[23] I call these 'proof theoretic explanations', because they turn only on the mathematician's grasp of aspects of proof-theoretic form–as opposed to subject matter–to explain the mathematician's ability to reason successfully despite the presence of such gaps in proofs.

placed with explicit formal derivations. Second, there are the gaps due to common understandings among mathematicians that certain details may be left out because everyone is familiar with them.[24] Third, there is the capacity to recognize patterns in forms of proofs–so that details and even subproofs may be left out.[25]

I can hardly deny that these factors are at work.[26] But they don't give the whole story. In particular, there is an interesting aspect to mathematical reasoning that still needs an explanation. This is that–so often–mathematical reasoning involves implicit steps that mathematicians fail to be aware of. They are aware–usually–that steps have been skipped; but often they are unaware of what those steps are.[27] We need a story that explains the ubiquitous presence of tacit reasoning in mathematics. Such a story must be constrained by several data: (i) that the principles involved in such tacit reasoning are often extracted only with difficulty and–sometimes–require striking mathematical creativity: The analysis of the presuppositions in tacit reasoning is (nearly enough) never a trivial task. (ii) That, nevertheless, such tacit reasoning nearly always proves to be *ex post facto* validated.

We are prone to say that–somehow–the mathematician grasps (tacitly) the principles involved without being able to express them explicitly. But this isn't really an explanation–and not even a hint has been given in this remark of where one could look for an explanation. One needs to know–at least in general–how the trick can be done. Avoiding a solution which turns on some sort of perception of the properties of the abstracta themselves–a solution that's no doubt tempting to some–I want to instead sketch a solution that brings together several elements: (i) the object-centered nature of mathematical reasoning–that is, the semantic element that always seems to be present in mathematical proof, and (ii) the use of 'intuition', in mathematics. Along the way, I also hope to explain why mathematicians (rightfully) were and are suspicious of the use of intuition. At the same time I will give reasons for the possibility that successful mathematical work *can't* proceed without employing such intuitions.

[24] See my [5], p. 95-96 for further discussion of these.

[25] The sort of pattern-recognition described here is syntactic: that such and such details–written out in such and such a way–will lead to a result. Everyone sees–syntactically–what the result will be; and so it doesn't have to be given explicitly. Such pattern-recognition to some extent may be learned–and may be an implicit requirement on mature mathematical skill–and so the second and third explanations to some extent overlap.

[26] Although I have to add that the second explanation introduces a lacuna: How are mathematicians convinced of the specific 'basic mathematical inferences'? Sometimes, of course, such inferences amount to (a complex of) axiomatic conditions on a subject matter. But not always.

[27] The striking cases are not when the steps left out invalidate the proof (e.g., the Kershner case that Fallis ([14], p. 51) writes about) but the far more numerous cases of successful proofs, where substantial presuppositions are only much later–sometimes centuries later–recognized to have been at work. The most notable example, of course, is the presence of logical rules in mathematical reasoning! But there are others: One interesting case is (implicit) uses of the axiom of choice by practitioners who later repudiated it. (See [28], for details.) There is also the presence of a number of implicit assumptions in proofs in the Euclidean tradition that were discovered by later practitioners. See [18] and [29] for a discussion of some of these. See my [6] for a discussion of these that complements the general position argued for in this paper.

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4

Inference packages. I want to analyze the use of 'mathematical intuition' as involving what I'll call 'inference packages': Some of the reasoning mathematicians engage in about certain kinds of abstracta involve these 'packages' that are applied as 'black box units', but which encapsulate specific groups of assumptions that aren't (separably) introspectively visible when such inference packages are employed. I'll start by illustrating the phenomenon with a small family of examples, and then speculate a bit about what's involved in such packages.

Imagine triangles on a plane. Notice that if you visualize the movement of triangles on a plane, shifting them about (rigidly) doesn't change the magnitude of their interior angles or their areas. Notice also that if you imagine an equiangular triangle growing in area (in a way that doesn't cost it its equiangularity), the interior angles of the triangle also don't change in magnitude.

Imagine triangles on a sphere. This is a bit different. Although, if you move the triangle around (rigidly), its interior angles don't change in magnitude, nor does its area change, letting an equiangular triangle grow in area without disturbing its equiangularity does change the magnitude of the triangle's interior angles: It gets 'more curved',–around the sphere–and its angles gain magnitude. One can also imagine blowing up the sphere–like a balloon–uniformly, although keeping the triangle the same in its area; in this case its angles flatten and lose magnitude.

Finally, imagine triangles on an ellipsoid (egg). Here the triangle cannot be moved rigidly about without changing the magnitude of its interior angles. We can imagine moving it from the flattest part of the ellipsoid to one of the ends of the ellipsoid, and back again; in this way we can see how the magnitudes of the interior angles of the triangle will increase and decrease.

Embodying several assumptions. Certainly in carrying out the various 'thought experiments' imagined above, we have exploited 'intuition', as it's often described by mathematicians. What's involved? Well, we have the capacity, when thinking about figures in this way–by placing them on surfaces–of exploring the simultaneous effect of several assumptions embodied in the surfaces we imagine, seeing how those assumptions affect what sorts of triangles are possible, and how they change given certain changes in the circumstances. The plane and sphere, for example, embody constant curvature; the ellipsoid doesn't. The plane is infinite; the sphere and ellipsoid aren't. (And so on.)

My suggestion is that the presence of inference packages–capacities to reason using simultaneously many assumptions that have been knit together–as it were–in the representations of objects which can be 'seen' to change this way and

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that way under certain circumstances, but not in other ways, is something that shows up everywhere in mathematical practice. But before making that case, let me officially note–and expand on–a couple of interesting facts about the specific examples that were given.[28]

The assumptions built into an inference package aren't introspectively accessible. First, it's clear that the assumptions presupposed in the imagistic models mentioned are ones that–in general–it's hard to recognize. I don't mean by this, that it's hard to recognize that an ellipsoid doesn't have constant curvature, and a sphere does; I mean that it's hard to tease out the assumptions, or a set of assumptions, which can be taken as the source of the differences in how the triangles on the different surfaces can be transformed. In general, of course, there is never one way to tease out such assumptions (for exactly the same reason there is always more than one way to axiomatize a subject area in mathematics). But even one way of doing so isn't naturally–introspectively–available to those to whom this kind of imagery is otherwise easily available. One can imagine someone failing to realize, for example, that the difference between how the triangles on the ellipsoid act, on the one hand, and how they act on the plane and sphere, on the other, is that the latter two are surfaces of constant curvature, and the former is not. One can (and does) also fail to realize how subtle the notions of continuity are–that are presupposed–in the "simple" act of imagining how the figures move over the surface; or, more simply, that assumptions about whether lines can be extended or not, whether superposition holds or not, and so on, suffice as assumptions to characterize in what ways we imaginatively allow the figures to change (and not change) as we imagine them moving over the surfaces in question.

This sort of introspective invisibility of, as it were, the principles presupposed in our thought experiments–the axioms presupposed, if you will, when we visualize the movement of triangles on various surfaces–is evidence for this capacity employing an 'off the shelf reasoning component' that we can bring to bear to imagine how certain assumptions together affect the objects they hold of. What marks this as an 'off the shelf reasoning component' is precisely the fact that it takes work to extract exactly what assumptions are being held in place when we imagine the objects. The suggestion is that, in this case, certain kinds of visual and kinesthetic capacities are brought to bear on a piece of mathematical reasoning about a kind of mathematical object so that we can quickly see certain

[28]If you want to, however, first notice how flexible we are at knitting different sorts of assumptions together, and determining–at a glance as it were–theorems of those assumptions: think about moving plane figures about on a saddle, imagine deforming various rubbery figures into one another (a donut into a coffee cup, but not into a disc), imagine strings on the surfaces of complex figures, and notice how we can pull some of them into points, or into one another without leaving the surface, although we can't do so with others, e.g., on a donut; imagine slicing figures along smaller dimensions (e.g., two dimensional slices of three dimensional objects); notice how different the two-dimensional figures can look as a result (e.g., ellipses along one axis of an ellipsoid, and circles along the other axis of the ellipsoid).

results that follow from those assumptions embodied in those capacities.

Plug-ins. In describing an inference package as an 'off the shelf component', I didn't mean to suggest that they don't come with various 'plug-ins' that we can change. The initial thought experiment given–using first a plane, then a sphere, then an ellipsoid–illustrates how what looks like essentially the same visual inference package can be applied to surfaces that presuppose different assumptions. In our doing so, however, we imagine *different* surfaces, and move figures along on them: Our ability to do this is not (at least introspectively) enabled by our extracting an axiom that holds of one surface, and replacing it with another so that one surface is thus transformed into another.

How postulating inference packages illuminates the role of diagrams, the representations of objects, and the presence of tacit reasoning in mathematical practice:

(i) It *explains why* diagrams make it easier for us to see ('understand') the proofs they accompany: get the 'gist' of them, realize what the 'big move' is, and so on. After all, one can–and should–wonder why diagrams would help anyone at all. Why isn't it easier for us to understand that something follows from something else only when a totally explicit proof has been given? (Why, in other words, aren't we puzzled as soon as a step–even a small one–is skipped?[29]) Why should the visualizations involved in diagrams provide any illumination at all? The answer–and some answer needs to be given–is that such diagrams trigger the employment of one or another inference package that we've already (largely) got available, and that enables us to package a number of assumptions together that are separately operating in the proof, and quickly see what they imply.

(ii) It explains the ubiquitous semantic presence of 'objects' in mathematical proof–even in post-Hilbert program and post-*Principia* mathematical proof: Such 'objects' are the 'givens'–as it were–of inferential packages. They are what we exercise inferential packages on.[30]

(iii) It explains–as required–the ubiquitous presence of tacit–but successful–reasoning in mathematics: the fact that so often tacit assumptions are only with great difficulty extracted from a piece of reasoning. The 'black box', 'off-the-shelf', availability of such inference packages prevents (easy) introspective recognition of

[29] We can certainly *imagine* creatures who would be puzzled as soon as any step–however small–was skipped: They would be hell to teach.

[30] Qualification: There are (empirical) options about exactly what form the 'objects' in such inferential packages take. For example–in the visual example I opened this section with–perhaps they really are abstractions (templates) of some of the visual properties of physical objects: e.g., the inferential package–that the plane/sphere/ellipsoid examples invoke–actually operates on colorless and textureless representations involving breadthless lines on flat planes, and so forth. On the other hand, maybe it involves a capacity to visualize something much richer; but then we have learned to modify the package in small ways so that it can apply to the idealized figures we've been asked to imagine. Lastly, it may be possible that there is no answer to this question. The phenomenology is simply indeterminate.

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what assumptions are presupposed in that package. Indeed, I suspect we recognize what assumptions–or other–are at work in an inference package by a kind of 'reverse engineering'. It isn't that we actually take apart the inference package (e.g., that we analyze exactly what tacit assumptions are being used when we visualize triangles on a sphere); rather, we design an axiom system in which assumptions are explicit–and show that the axiom system yields the same theorems that the inference package does–e.g., that triangles, according to the axiomatization, have the same properties that our inference package (with, perhaps, suitable plug-ins) attributes to them. Of course, as I mentioned earlier, there are many axiomatizations that yield the same theorems.

(iv) It explains *why* diagrams can be misleading. It's universally noted (and feared) that diagrams–and mathematical intuition, generally–mislead. Indeed, this was the primary motivation for Frege, Hilbert, and others, for avoiding them. The reason why a diagram can mislead is because one can fail to realize (i) that it embodies a special case, and that (ii) one has inadvertantly relied on the special-case details implicitly presupposed by the diagram, instead of the (weaker) presuppositions of the fully general case. The paradigmatic illustration of how this can happen, of course, is the drawing of a specific (acute, say) triangle that has properties not shared by other triangles. Should we crucially rely on the presupposition that all triangles are acute–say–the diagram will mislead us into drawing conclusions that don't hold of all triangles generally.

Brown ([9], p. 33) points out that, nevertheless, diagrams can be valuable because–as he puts it–what can happen with certain diagrams is that the special case that the diagram embodies 'is mathematically equivalent' to the general case 'in the sense that given certain mathematical assumptions ... the various cases are equivalent'. He ([9], 34-37) gives a number of very interesting picture-proofs of the totals of certain infinite sums. (The picture proofs depict ways of carving up boxes or other simple two-dimensional figures so that we can see at a glance how a way of mapping infinite sums into subdivisions of a figure completely fills specific areas.)

My point is this: *Some* diagrams embody assumptions that, given what they are supposed to help prove, are sometimes misleading. The diagrams are helpful because they allow us to bring into our thinking an inference package that enables us to see–at a glance, as it were–what the implications of certain assumptions are. But because the assumptions embodied in the diagram that the inference package is tacitly helping itself to aren't introspectively available to us, we can't see (at a glance) whether those assumptions line up with the ones officially assumed in the proof. Thus it is precisely that the inference package is being used in an 'off the shelf' manner that explains why diagrams can be misleading.[31]

[31] It's important not to underestimate the presence of 'diagrams' in mathematics. The (two-dimensional) way we execute multiplications: by writing summands systematically, along with carrying notation, and so on, is diagrammatic. And again, notice that it isn't transparent to introspection that the procedure should give the right answer (given how we define the operation of multiplication). We teach the trick to children without demanding they understand or can

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There is a sense in which Kant–long ago–may be taken to have anticipated the idea of inference packages. Certainly he argued that intuition was a required part of mathematical proof because the objects of mathematics were given, as it were, not as separable entities that the mathematician could study on an individual basis by means of definitions and analyses of the objects so defined, but rather only as accompanied by an implicit manifold 'structure' that they were part of; and that this is what enabled synthetic inference: in the geometric case–for example–by the intuitive right the mathematician recognized herself to have of constructing diagrams in such and such ways.[32]

However, and whether or not Kant's view actually deserved to be interpreted this way, intuition so described was often seen as something relatively inflexible, so inflexible in fact that–although it was granted that mathematicians have an 'intuitive grasp' of one, two and three dimensions, and that they have an intuitive grasp of two-dimensional non Euclidean geometries when such are embeddable in Euclidean 3-space–mathematicians were taken to have no intuitive access to four or higher dimensions (in the case of Euclidean geometry), or to three or higher dimensions in the nonEuclidean case. The ability of mathematicians to prove results about objects in such spaces was therefore implicitly seen as already operating in a way close to something sheerly 'formal'. Regardless of how Kantian intuition should be understood, I want to make it clear that the inference packages I'm postulating are a great deal more flexible.

What follows, therefore, are some observations about the inference-package hypothesis. I want to stress that a lot of what I have to say is speculation–I especially want to stress this about my remarks about how such inference packages may or may not be related to neurophysiologically-instantiated cognitive systems.

Distinguishing inference packages from subpersonal cognitive processing. It's important, first, to distinguish inference packages from the various sorts of (subpersonal) cognitive processes that underwrite our inferential abilities. For one thing, such cognitive processes are largely unavailable to introspection–but inference packages play a conscious, and sometimes official, role in deduction. This was illustrated by the moving triangle thought experiments of 4 which involved visualizations that we are explicitly aware of. I hypothesize that such inference packages are–generally–built out of cognitive systems that, although operating to support an inference package drafted for use in mathematics, often have well-defined roles elsewhere–for example, in language processing, or in the various kinds of sensory processing we engage in.[33]

prove why it works.

[32] See [21], The Main Transcendental Question, First Part, especially §12, pp. 36-37.

[33] Matters are complicated. Let's restrict our attention to the counting numbers, on which there is a lot of research literature. An early view, e.g., Hurford ([20], p. 3) argues that 'the

Distinguishing representations of objects in inference packages from representations of objects in (subpersonal) cognitive systems. Related to this is that it's important to keep distinct the concepts of mathematical objects that are the consciously recognized targets of inference packages from the representations of mathematical objects that are manipulated by one or another subpersonal cognitive system which nevertheless (neurophysiologically) underwrites our grasp of consciously recognized mathematical concepts. For example, there is surprising evidence ([11], pp. 22-23) of 'the seemingly compressive character of the number line [which is a] feature of the mental representation of numerical magnitudes'. In particular, 'subjective magnitudes obey Weber's law: the same objective numerical difference seems subjectively smaller, the larger the numbers against which it is contrasted'.

There is some worry in the literature that such a compressive subjective distribution of the counting numbers is incoherent, either because it doesn't fit well with our conscious impressions of the properties of such numbers, and their magnitude relations to one another, or because the very number concepts themselves don't coherently admit to being placed on a compressed number scale (i.e., their

number faculty largely emerges through the interaction of central features of the language faculty with other cognitive capacities relating to the recognition and manipulation of concrete objects and collections ...' As a result, he thinks that 'It is therefore not necessary to postulate an autonomous 'faculty of number' as a separate module of mind'. But other researchers have gathered evidence that there are cognitive processes dedicated to primitive aspects of number cognition. See [11] for a discussion of the research and literature as of that date. One important lesson of the research literature is that the subpersonal routines underlying inference, in general, are quite heterogeneous. Thus, even where a module is dedicated to underwriting certain capacities we have with counting numbers, it isn't the case that it underwrites all our counting number capacities. Dahaene ([11], p. 34) writes: 'Modularity is the fundamental concept which emerges from the present review. According to [Dahaene's] proposed model, the ideal of a unique 'number concept' must give way to a fractionated set of numerical abilities, among which faculties such as quantification, number transcoding, calculation or approximation may be isolated'. Three points. First, it isn't–of course–only Dahaene's model that 'fractionates' our abilities with numbers in this way: *All* the models that fit the empirical results about learning difficulties, brain damage effects on competencies, error effects, and response time experiments, are ones which fractionate the cognitive systems brought to bear on various counting tasks. (See, e.g., the discussion of several such models in [11], pp. 27-34, and–in particular–the McCloskey model in [27].) But second, if the 'number concept' is a concept supposedly accessible to conscious reasoning on our part then none of these results actually fractionate *our concept of number*; no more so is my concept of 'picking up a glass of water' fractionated by the neurophysiological facts that show that a number of different and heterogeneous cognitive systems–none of which I'm aware of–are involved in my ability to implement that concept. I stress this because, as noted, inference packages are items we apply consciously to concepts we take ourselves to have. I say more about this in *Distinguishing representations of objects in inference packages from representations of objects in (subpersonal) cognitive systems.* Lastly, my suspicion is that successful inference and practice in other mathematical areas requires even more borrowing from heterogeneous cognitive systems than our grasp of the counting numbers do. (Evidence for this may be found in the sorts of difficulties children have of acquiring the concept of fractions. See [16], pp. 68-70.) Nevertheless, the consciously accessible mathematical concepts that are the targets of mathematical reasoning aren't fractionated precisely because the subpersonal machinations involved in our acquired facility with such concepts aren't included in the (consciously perceived) contours of those concepts.

magnitude relations to one another are mathematically rigid).[34] Dehaene ([11], p. 24), in response, postulates 'two largely distinct modes of number processing: one based on a symbolic code, and the other on a quantity code'. He agrees that it is absurd to talk of a subjective scale of number when 'numbers *qua* symbols' are involved, since they 'enter into objective relations'. But if they are mentally represented as quantities, there is no problem.

Actually, there is no problem in either case. Numbers as representations operated on by cognitive systems–of whatever sort–aren't the sorts of items that can enter into 'objective relations'–only Platonic objects, if any, manage *that* trick. Cognitive-system representations are manipulated howsoever the cognitive architecture governing them manipulates such things; and if evidence arises (based on various experiments of random number selection, and so on) that the properties such representations have–the subjective scale, for example–doesn't agree with the properties we take numbers, for example, to have, that just indicates the gulf between (subpersonal) cognitive representations and the representations of numbers that we are consciously aware of.

One may worry that this move attributes to those representations of numbers –that we are conscious of–properties no objects of *consciousness* should be allowed to have: In the number case, for example, the properties attributed to them are exactly the properties they have! I can't say a lot about this now, but let me offer a couple of elucidations. First, one shouldn't think of the representations of objects that we are conscious of as items which are, as it were, made up of the 'stuffs' of consciousness. Rather, the stuffs they are made up of are precisely the representations of the cognitive systems that enable us to grasp them. But, second, there is another element involved: This is that we don't allow 'mistakes' or misunderstandings–as we officially understand such things–to individuate what we take ourselves to be conscious of. If I imagine a sphere, and translate a triangle across its surface, but claim that its angles change as a result, someone (who knows about such things) will tell me to think about it again. It will *not* be claimed that the particular mental image that I've got in mind *has* the properties I've just attributed to it. In contrast to the mental representations involved in (subpersonal) cognitive systems (that *are*–and should be–individuated, in part, by the errors they induce us to systematically make), the consciously targeted representations of inference packages are officially abstracted away from such errors.[35]

[34] See [11], pp. 24 for discussion of these objections.

[35] In this sense the conscious targets of mathematical reasoning are 'normative': And in this case, 'normative', largely means that the properties such objects are *supposed to have* are the ones that they are taken *to actually have*. It's important to realize that it's virtual objects of this sort that we reason about *and that we find ourselves conscious of* –not the representations manipulated by the actual subpersonal cognitive systems that make possible our inferential abilities.

Are inference packages fixed in the tacit assumptions they presuppose–apart from plug-ins? It might seem natural to think that inference packages contain a limited set of tacit principles (relative to the different ways these might be explicitly axiomatized) that exhaustively characterize the sets of virtual 'objects' that they apply to–and which are the sets of consciously recognized objects we apply such packages to. Such a limitation (in presuppositions) is a correct characterization of the inference packages–however it doesn't extend to the official objects such inference packages are about because the 'normativity' of such objects–as just described–intrudes.[36] Imagine the counting numbers–as we do–as distinct individual objects that succeed one another linearly, and to which we can apply addition and multiplication. Perhaps there is a (tacit) set of axioms that completely describes the inference package that we bring to bear to imagine such things[37] –but nevertheless *any* axiomatization of these things is incomplete. We take ourselves–officially–to be consciously aware of a set of objects, that is, which have been shown to elude axiomatization. On the other hand, we don't see how to extract additional axioms on the basis of the inference package(s) that we currently know how to apply to the counting numbers; this is evidence that the inference package is itself limited to certain presuppositions, and not others. In this sense, we take the objects we consciously–and officially–take ourselves to be studying to transcend any inference package we bring to bear on them, although at the same time, we (also officially) take the inference packages we use to have such objects as their objects.[38]

[36]Perhaps 'normativity', is the wrong way to describe the phenomenon I momentarily describe: Perhaps it's better described as the 'transcendence' of the virtual objects from the inference packages about them. In any case, the phenomenon in question is the phenomenological companion of the fact that mathematical practice stipulates the co-reference of mathematical terms belonging to different algorithmic systems–see, e.g., [2], especially Part II, §7.

[37]I'm assuming, contrary to fact, that there is *one* inference package that can be so brought imaginatively to bear on numbers–surely there are several. This doesn't change anything essential to the point being made.

[38]This is why Gödel's incompleteness proof of any decidable axiomatization of the standard model was–psychologically speaking–a total surprise: Mathematical intuition didn't make evident the incompleteness of the axioms of the counting numbers. It's worth adding that when we supplement axiomatizations (of the numbers) with additional axioms, we do so by bringing fresh concepts to bear–this is true even of Gödelized ways of doing so: We bring a different inference package to bear on them.

It might be thought that we should distinguish three sorts of representations. First, the subpersonal ones that are manipulated by cognitive systems–and that are strictly individuated by the cognitive systems that apply to them, second, those that are the virtual objects manipulated by inference packages–where their individuation conditions abstract away from mistakes, and third, the ones constituted by our stipulative identification of virtual objects across inference packages–where the individuation conditions of these representations abstract away from localization within particular inference packages. The role of the second sort of representation would be this: They would be the representations we could be taken as aware of at a time–as long as we remained within a single inference package. My suspicion is that mathematical flexibility is such that–even within the confines of the comprehension of a single proof, a mathematician may switch inference packages without switching on the objects she takes herself to be conscious of. One should–therefore–be aware of how the 'objects' of conscious awareness are actually being

Individuating inference packages. I've been working hard to distinguish inference packages and the mathematical concepts that *they* are applied to from (subpersonal) cognitive systems and the representations they are applied to. But it's also important to distinguish differences between inference packages and how (Kantian-style) intuition is often understood to operate. Although inference packages look topic-specific, and even look like they are borrowings of specific inference strategies from particular sensory modalities (e.g., vision), I want to deny that nevertheless inference packages are simply individuated according to the various sensory modalities–so that, say, there is *one* inference package corresponding to visualization (generally), another *one* corresponding to our auditory capacities, and so on. Second, I want to deny–as I've already indicated in footnote 37–that such inference packages are naturally individuated according to mathematical subject matters–e.g., that there is one inference package operating when we visualize figures in two-dimensional geometry, a(nother) one when we study counting numbers, and so on.

Regarding the first point. The idea is that howsoever visual processing takes place, there may be distinguishable inference packages, distinguishable ways of drawing inferences about kinds of visual information, all of which are involved in seeing, but which–or some of which–can be *independently* applied by a mathematician to mathematical subject matters. Thus, it may be that one sort of inference package is brought to bear when a mathematician imagines figures in Euclidean space, but an entirely different (although still originally visual) inference package is involved when a(nother) mathematician imagines the comparative rates of growth of integrals of functions by transforming their rates of growth into perceptions of changes in areas. Yet a third inference package may be brought into play when a mathematician tries to characterize an eight-dimensional figure by means of how one-dimensional strings can or can't be deformed into one another without leaving the surface of that figure.

Notice the (entirely empirical) suggestions being made here. First, it's being suggested that the various sorts of inference packages that operate in a group in their 'home domain' (seeing, say) may be piecemeal introduced by a mathematician to facilitate his or her understanding of a certain collection of posited mathematical objects. Further, by no means should these various sorts of inference packages be categorized as simple 'visualizations' of the objects in question. This is why 'mathematical intuition'–very broadly understood, of course–can apply to mathematical objects (e.g., higher-dimensional objects) that we otherwise can't (and don't) directly visualize. Second–and now we're turning to the second qualification–even though, in general, diagrams may be guides to alerting the mathematician (or her brain) that such and such a sort of inference package may be valuable–when piecemeal applied to a mathematical subject matter to gain understanding of a particular proof–it needn't be the case that different mathe-

grasped–through the means of one inference package or another–but it doesn't strike me as valuable in the context of this paper to otherwise single out representations individuated by virtue of belonging to particular inference packages.

maticians bring the same inference packages to bear on the same proofs or the same objects officially studied by those proofs.

Indeed, an important aspect of mathematical creativity is figuring out how to apply an inference package to a subject matter that it's never been applied to–in general there can be many individual ways of getting inference packages to so apply. Recall the point made in I that there are many different ways to build mathematical objects out of others. If someone realizes–mathematically speaking–that a certain kind of mathematical object can be treated as the result of a (mathematical) operation on a different kind of object, or that a certain kind of mathematical object can be (mathematically speaking) composed of different sorts of mathematical objects altogether, this may lead to a fresh way of bringing an inference package to bear on the kind of object so constructed.[39]

6

The topic-specific nature of inference packages. Inference packages are topic-specific for two reasons at least. First, they are often intuitively obvious applications of a mode of inference with a home domain: e.g., visualization, or language processing, and so on. But they are topic-specific, secondly, because of the assumptions encapsulated within them. The first reason for topic-specificity is, from a logical point of view, superficial: Regardless of what 'official' sorts of objects the inference package intuitively presents us with, we can bring them to bear on something else provided the tacit assumptions line up correctly with respect to the new items. This Hilbertian point has long been involved in mathematical practice: It explains why a mathematician can visualize the comparisons of growths of integrals of functions in terms of a comparison of increasing areas, say; mathematicians exploit the reinterpretability of the 'objects' of an inference package precisely to gain flexibility in how such packages can be applied to a mathematical subject matter.

But the second reason for topic-specificity is deeper–this is especially so because 'inference packages' aren't badly named: The tacit assumptions are built into such packages so that they license *inferences*; the tacit assumptions are, as it were, built into the 'consequence relation' of such packages.

Methodological holism in mathematical practice. However, a certain sort of methodological holism has long been part of the practice of mathematics: Any

[39]So if we already have inference packages–language based ones, say–that enable us to manipulate certain mathematical objects notationally (matrices, say); and we discover that such matrices code certain operators in otherwise intuitively inaccessible spaces, we have thus gained an intuitive route to the operators in those spaces: a way, that is, of applying the inference packages–that we hitherto applied to matrices–to those operators. Similarly, ways of decomposing higher-dimensional objects into lower dimensional ones, or at least, classifying such in terms of other (geometric or algebraic) objects, may bring in its train the inference packages currently applicable to those objects.

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branch of mathematics may be combined with (or applied to) any other if the results are fruitful. But to so join two branches of mathematics requires they share their inference rules–their logic, anyway–so that there is no conflict or ambiguity about what can be taken to follow from their joint assumptions. Ordinary mathematical practice has in place a universally used means for facilitating that the inference rules held in common are the same ones: Tacit assumptions in inference packages–when made explicit–are presented in the garb of *nonlogical* axioms.[40] Discharging the tacit assumptions at play in inference packages not only makes an axiomatization of the tacit assumptions explicit, it simultaneously topic-neutralizes that inference package by officially removing the assumptions from the 'logic' altogether.

The logic of (traditional) mathematical proof? This makes natural, however, the question of what the logic implicit in traditional mathematical proof is. To the extent that inference packages are seen as part of the proofs of traditional mathematics, the answer must be that the 'logics' differ from branch to branch of mathematics, if only because the inference rules in such inference packages applied to such branches differ. But we are taking it as methodologically given–on the basis of how the tacit structure of such inference packages, and indeed tacit assumptions in mathematical proofs generally, are treated–that such tacit assumptions (even when operating inferentially in an inference package) aren't to be regarded as part of the 'logic' of mathematics by practitioners. The question therefore is: what's left? What–if anything–that is, is common to the set of allowable inferences when mathematical subjects–generally–are joined together after making explicit–treating as nonlogical, anyway–their tacit assumptions? That whatever-is-left is the logic of traditional mathematical proof *tout court*.

One might be tempted to try to read this so-called logic of traditional mathematics directly off of the vocabulary items of the language it's couched in–the vernacular. But there are very good reasons to think that mathematical inference abstracts away from the exoteric intricacy of the various devices of natural languages: the rich and intricate structure of ordinary language quantifiers, the peculiar non-truth-functional connectives, and so on, seem to play no role in mathematical inference. This is shown by the successful translation of mathematical proof into formalized languages where such *exoteria* are absent.

Alternatively, therefore, recollecting the success of the translation of the mathematics available–at that time–into the *Principia* (and other) formalism(s), one might try to read the implicit logic of traditional mathematics directly off such translations. I suspect the second strategy is better than the first, if only because of the success of the translation program. But there is another way to get at the implicit logic of traditional mathematical practice that I'll explore here.

[40] Notice that this is the very opposite of the methodology involved in the construction of 'model-theoretic logics' (see [8]) where mathematical assumptions–mathematical content, if you will–is built into the logical structure of, say, the quantifiers.

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The move is to recall yet again that mathematicians–in practice–have no conscious grasp of rules of inference. What they do grasp clearly are (certain) properties of the consequence relation itself. The trick, therefore, is to try to read the properties of the logic they must be using off of their grasp of the consequence relation. In doing this, therefore, we should neither focus on proof-theoretic construals of that logic, nor on semantic construals of it.

Implication relations. Is there anything left to focus on? Yes. Mathematicians clearly imagine that sentences in the vernacular–and in particular the ones about mathematical subject-matters that they are concerned with–have implications.[41] They are interested, not only in the implications of such sentences on an individual basis, but also as they occur in groups. The way to think of logical connectives is as operations on objects–in this case sentences expressing mathematical content–that the 'implication relation' is defined on, in particular, as valuable *functional* characterizations of how that implication relation can be made to manifest itself. An analysis of logic exactly along these lines is given by Koslow [23]. I summarize the pertinent aspects of his approach briefly, and then show how to apply it to our problem.

Definition of an implication relation. Koslow ([23], p. 5) defines an implication relation–quite generally–as a relation holding over a collection of objects, S[42] obeying the following six conditions (hereon, '$\Rightarrow$' stands for the implication relation):

1. Reflexivity: $A \Rightarrow A$, for all A in S.
2. Projection: $A_1, ..., A_n \Rightarrow A_k$, for any $k = 1, ..., n$.
3. Simplification: If $A_1, A_1, A_2, ..., A_n \Rightarrow B$, then $A_1, A_2, ..., A_n \Rightarrow B$, for all A_i and B in S.
4. Permutation: If $A_1, A_2, ..., A_n \Rightarrow B$, then $A_{f(1)}, A_{f(2)}, ..., A_{f(n)} \Rightarrow B$, for any permutation f of 1, 2, ... n.
5. Dilution: If $A_1, A_2, ..., A_n \Rightarrow B$, then $A_1, A_2, ..., A_n, C \Rightarrow B$, for all A_i, B, and C in S.
6. Cut: If $A_1, A_2, ..., A_n \Rightarrow$ B, and $B, B_1, ..., B_m \Rightarrow C$, then $A_1, A_2, ..., A_n, B_1, ..., B_m \Rightarrow C$, for all A_i, B_j, B, and C.

These conditions can all be seen to obviously hold of our intuitive notion of

[41] I use 'consequence' and 'implication' interchangeably here; I'm–more or less–now switching to 'implication', however, because that's the term used by Koslow [23], and the subsequent part of 6 relies on some of his work.

[42] For Koslow ([23], p. 3): 'Any nonempty set can be provided with an implication relation'. The objects we will be concerned with, however, are sentences in the vernacular with mathematical content. Koslow's notion of 'implication' is quite a bit broader than ordinary notions of 'implication', which are restricted to items with propositional content. My application of his approach, however, focuses only on items with such propositional content.

consequence, as it operates in ordinary mathematical proof, and indeed, in argument generally. Koslow calls an *implication structure* any collection of objects S, with an implication relation, $\Rightarrow$, defined on it. Let's take those sentences in the vernacular that have mathematical content to be the elements of an implication structure, with the notion of implication defined on it being the ordinary intuitive one that mathematicians recognize themselves to employ. Given any implication structure, operators corresponding to the classical logical connectives can be defined. I'll first illustrate how this is done with the example of conjunction.

Conjunction. A notion of conjunction, $C_{\Rightarrow}(A, B)$, is defined for any implication structure $< S, \Rightarrow >$ by the following two conditions (Koslow [23], p. 108):

C_1. $C_{\Rightarrow}(A, B) \Rightarrow A$, and $C_{\Rightarrow}(A, B) \Rightarrow B$, and
C_2. $C_{\Rightarrow}(A, B)$ is the weakest member of S to satisfy the first condition. That is, for any T in S, if $T \Rightarrow A$ and $T \Rightarrow B$, then $T \Rightarrow C_{1\Rightarrow}(A, B)$.

C_1 is no doubt familiar: It describes the 'elimination rules' for conjunction. But what does C_2 tell us? Well, in the general setting in which Koslow investigates implication structures, it tells us exactly–and no more–than what it says: The conjunction of two elements of S (if it exists) is the weakest member of S to satisfy the first condition. But in the context where we understand S to be the sentences of the vernacular that have mathematical content, it says this: The conjunction, so defined, of two sentences A and B from the vernacular, has no additional content beyond that necessary for it to imply A and to imply B. (Were it not the *weakest* sentence obeying C_1, then it would be saying something more than what's only required for it to imply A and to imply B–it would be saying in addition whatever it needed to say to imply some T–that implied A and implied B–but which, in turn, did *not* imply it.) The functional role of conjunction, that is (and this seems intuitively reasonable), in terms of implications, is this: It expresses something that implies whatever A and B imply, *and it expresses nothing more.*

Two points. First, it should be clear that if one is–generally–interested in the implications of sentences with mathematical content, and in particular, in the implications of *groups* of sentences, it's rather valuable to have the ability to *express* those sentences in conjunctions–in a form, that is, that implies each of the sentences separately, and no more than that. Second, it's worth pointing out that nothing in the initial definition of an implication structure S–nothing, therefore, in our notion of implication (so described)–*requires* that conjunctions for every, and indeed for any, two elements exist. In general, they needn't; with respect to mathematical statements in the vernacular, nothing about our intuitive notion of consequence requires that the conjunction of statements be expressible in the vernacular. Whether or not the conjunctions of sentences (expressible in the vernacular) are themselves in turn expressible in the vernacular needs to be established by an examination of the expressive resources of the vernacular. All that's been established so far is the value of such a thing.

Similar functional conditions are used by Koslow to define operators corresponding to the other connectives: disjunction, negation, the hypothetical, and in contexts where implication structures are appropriately extended to contain predicates and objects, the universal quantifier as well. I'll illustrate the approach with two more connectives, the hypothetical, and negation–the latter because of an interesting complication it raises. Then I'll turn to the specific question of whether there are reasons to believe such operators are expressible in the vernacular.

Hypotheticals. Koslow's ([23], p. 77) definition of a hypothetical operator is as follows:

An operator $H_{\Rightarrow}$ is a hypothetical on the implication structure $I =< S, \Rightarrow>$ if and only if the following conditions are satisfied:
H_1. $A, H_{\Rightarrow}(A, B) \Rightarrow B$, and
H_2. $H_{\Rightarrow}(A, B)$ is the weakest member of S that satisfies the first condition. That is, for any T in S, if $A, T \Rightarrow B$, then $T \Rightarrow H_{\Rightarrow}(A, B)$.

As with conjunction, first, it's clear that the expressibility of something with the property of the hypothetical is valuable: It encodes the minimal content M so that B is a consequence of A and M (whatever that minimal content is); furthermore, just as with conjunction, there is nothing in the ordinary notion of consequence that tells us that such a thing is expressible in the language that mathematics is couched in–despite its value. Again: This must be established by an examination of the expressive resources of natural languages. I'll turn to some discussion of this shortly.

Negation. Koslow's ([23], p. 91) definition of the negation operator is as follows:

Let $I =< S, \Rightarrow>$ be an implication structure. For any A in S, we shall say that $N_{\Rightarrow}(A)$ is a negation of A if and only if it satisfies the following two conditions:
N_1. $A, N_{\Rightarrow}(A) \Rightarrow B$ for all B in S, and
N_2. $N_{\Rightarrow}(A)$ is the weakest member of the structure to satisfy the first condition. That is, if T is any member of S such that A, $T \Rightarrow B$ for all B in S, then $T \Rightarrow N_{\Rightarrow}(A)$.

As before, there is no requirement on implication structures that negations exist. But an interesting additional complication arises. This is that the negation operator, even when it exists, needn't be classical; it's possible for the implication

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structure to support a negation operator that's intuitionistic. It *will* be classical–this is the content of Theorem 12.6 ([23], p. 97)–if and only if $N_{\Rightarrow}$ is a mapping of S onto S, that is, that for every element s_1 in S, there is some element s_2 so that s_1 is the negation of s_2.[43]

Can these functionally-desirable logical operators be expressed in the vernacular? Here is the state of play. I've suggested that much of the (mathematical) content that appears in inferential form in mathematical practice is also–in that practice–treated as nonlogical axiomatizations when made explicit. I've also indicated how Koslow's characterization of logical operators shows how functionally valuable operators–given an antecedent understanding of the consequence relation *alone* (i.e., without an accompanying syntactic or semantic description)–can be characterized. Under certain circumstances these operators yield a classical first-order logic defined over an implication structure. The question is: Do these circumstances obtain when the sentences of a natural languages are treated as elements of an implication structure–do the sentences of the vernacular (restricted, say, to ones with mathematical content), and given the intuitive notion of consequence, enable resulting logical operators that are first-order?

Could the implicit logic in question be intuitionistic? Let's take up an easy issue first. Recall that Koslowean negation is classical if–to put the matter roughly–every sentence under consideration is the negation of some other sentence. This will be true of the vernacular if (i) any sentence in the vernacular can be negated, and if (ii) that iterations of negation are cancelling (e.g., 'It isn't the case that it isn't the case that A', is equivalent to A). The use of proof by contradiction in classical proofs indicates–I believe–both these things.

Are the logical operators expressible in the vernacular? Let's turn to the more important issue of whether, given arbitrary sentences of the vernacular, conjunctions, hypotheticals, and so on, of such things are *expressible* in the vernacular. There are reasons to think they aren't. First, consider this: Must such items be truth functional? It can be shown that that the notion of an implication structure is general enough that even if the items in that structure are truth-bearing (as, presumably, sentences in the vernacular are) the resulting logical operators–even if they exist–needn't be truth functional. They *are* truth functional if how truth values may be distributed among such sentences obey certain conditions; I believe, however, that it can be shown that our ordinary notion of

[43] A further complication (that I won't get into) is the question of whether negation should be characterized in a way that so flagrantly violates relevance intuitions. Although relevance intuitions go a long way towards explaining the interest and surprise of mathematics (see my [3] on this), I think ordinary people accept the principle of absorption, and as a result of that can be brought to accept that a contradiction implies every sentence. See [32], p.103-4 for a nice discussion of this principle.

truth obeys these conditions.[44]

But this creates a quandary: It seems clear that *no* connective, as used in English, is genuinely truth functional. Certainly, uses of 'if ... then', which would be required to express the hypothetical, are notoriously not truth functional; but there are lots of intuitive examples of uses of 'and', and 'or', that seem to show similar facts about *those* connectives. This is significant precisely because of the second 'weakest' clauses in Koslow's definitions–as I've argued, such clauses indicate that there is no extra content accompanying the expression of the (functionally pure) 'and', 'or', 'not', 'if ... then', and so on. But this seems false of locutions in natural language: They are up to more than what's been functionally characterized as valuable–given our antecedent notion of implication.[45]

Perhaps it's somewhat controversial whether terms in natural languages can be found which–at least under certain circumstances–express the functional roles of logical operators as defined by Koslow. Certainly one is tempted to reach for a semantic/pragmatic distinction to isolate the pure functional role of logical operators (in the semantics) away from excess (pragmatic) ones. My suspicion is that this strategy won't work, although I can't get further into the issue now.

If the logic is tacit, then idioms allowing explicit expression of logical rules aren't needed. Instead I want to indicate why none of this actually matters to the (traditional) mathematician, nor to the thesis that ordinary mathematical proofs tacitly presume that the logic is first-order classical (despite, say, the strict inexpressibility of first-order idioms in the vernacular).[46] The strategy is this, and it's facilitated precisely by the tacitness of the logic employed: The mathematician simply reasons as if the logic implicitly available is a first-order classical one. By this I mean both that (i)–as we've seen–the mathematical content tacitly presupposed is exposed–when it is–in the form of nonlogical axioms, and that (ii) mathematical inference patterns, anyway, are often enacted without any explicit expression of logical rules (so that explicit logical *idioms* aren't needed), and finally, (iii) when logical principles, or something amounting to logical idioms, does need to be expressed, it's done so with an interesting twist: The mathematician treats (certain) ordinary idioms of the natural language–in the mathematical context–as if they only express the functional (logical) role,

[44]These conditions include, for example, that every (indicative) sentence is not both true and false, and that each such sentence is either truth or false. Other conditions are also called for: that the set of true sentences is closed under implications, and that the set of false sentences is closed under a notion of dual implication definable from the notion of implication. I submit–but won't argue for now–that there is good evidence that our notions of true and false sentences fit all these conditions. See the relevant portions of Koslow [23], especially chapter 8, section 9.5, and chapter 19.

[45]That the connectives and quantifiers, in English, are richer–semantically–than Koslowean logical operators should be no surprise: Language is flagrantly opportunistic–idioms are always being drafted for multiple purposes apart from what (logical) purists want (or need) them used for.

[46]So, if such idioms can be expressed in the vernacular, that's great; but if they can't be expressed, it doesn't matter.

and nothing more. This is possible if there are idioms in natural language which approximate in what they express, the logical functions.[47]

Conclusion. First-order classical logic emerges as the logic of ordinary mathematical proof, not by way of a distillation of what sort of idioms–obeying what sort of syntactic rules–are available in natural languages, nor as a result of the semantics of particular particles of natural language, but as desirable functions given an antecedent intuitive grasp of the notion of consequence in the vernacular (i.e., that it obeys Koslow's conditions on an implication relation). Making the logic explicit, and embedding it in a syntax that reflects its properties *exactly* waited until an artificial language could be invented with idioms that had the desired properties.

7

Let's return now to inference packages, and how they constrain our intuitive grasp of traditional mathematical proofs. The interesting fact is that we feel we grasp or understand a proof not when we can survey it, but only when it 'makes sense'. This is shown by the fact that even very short derivations, where every step is (derivationally) explicit, needn't as a result be particularly informative. We can, of course, see that the result must follow–because we see how each step must follow from the one before it–but we we don't see, as we might put it, *why* the result should follow from the assumptions.

Inference packages and the sensation of understanding a proof. This intuitive fact is related to how talk of 'explanation' sometimes arises in mathematics. Interestingly, not all traditional mathematical proofs are seen as equally explanatory, although all such proofs are equally successful as proofs–in the sense of compelling their readers to assent to the conclusions on the basis of the premises. It's tempting, therefore, to see the differences perceived in how explanatory proofs are as ultimately a subjective matter, especially since, as I've already indicated, such differences don't seem to have much to do with how explicit the proof is. Rather, they have a lot to do with what 'concepts' are deployed in the proofs: Some sets of concepts seem intrinsically more explanatory than other sets of concepts, even when both sets can be used to prove the same theorems.

[47]It's uncontroversial, I imagine, that 'and', 'or', 'not', and so on, at least *approximate* their logical cousins. 'And', for example, might be seen as expressing its purely Koslowean logical function when how it operates with respect to tense is abstracted away; so too, 'if ... then', or 'because', might be taken to express the Koslowean logical function of the hypothetical provided how those English idioms operate with respect to causation is abstracted away. Notice that this 'abstracting away' can be facilitated by the mathematical subject matter itself–for example, the temporal elements in our ordinary use of 'and', and the causal elements in our ordinary use of 'because' or 'if ... then', can be easily left aside in contexts where one is officially studying 'timeless and causally inert objects'.

I hypothesize that what we need to feel that we 'understand' a proof is the comfortable familiarity of an inference package. When the concepts employed in a proof facilitate the use of an inference package, not only do we feel compelled to the conclusion but we also feel we understand *why* the result follows.[48]

Ordinary proofs aren't convincing due to their being abbreviations of longer items that are–themselves–convincing. Regardless of the success of the above hypothesis about the role of inference packages in the subjective sensation of understanding a proof, there nevertheless has always been a strange mismatch between the epistemic status of ordinary proofs that we often understand, and are convinced by–despite their missing (so) many steps–and the fact that should such an ordinary proof be made totally explicit, the result isn't greater confidence in the result–but less, because of the loss of the sense of what's going on (in the welter of details). For a long time, I explained this mismatch on the grounds that mathematicians were good at *syntactic* shortcuts, that they had good pattern-recognition skills that enabled them to skip proof-theoretic details that they knew–in principle–how to fill in. But if the role of inference packages, and the representations of objects that they bring with them, is like what I've been describing in this paper, this is very much the wrong story. The right story is that the tacit details are utterly buried psychologically by the inference package which itself conveys a feeling of understanding despite what's tacit being made introspectively invisible–we are designed, as it were, to be convinced by the results of our inference packages. This is why when tacit details are teased out–e.g., about what's really involved in notions of continuous motion–the result is often much harder to understand, even by professionals (it's less 'intuitive').

Traditional mathematical proofs don't enhance the surveyability of all the steps they rely on. That inference packages can be used successfully to read and construct proofs, and that they mislead mathematicians as little as they do, are epistemically positive facts; such facts rightfully contribute to our confidence in the epistemic value of traditional mathematical proofs. But it's also true that if surveyability is to be seen as an epistemic virtue, it's one that ordinary mathematical proofs don't really have: Such are 'surveyable' only because *our* peculiar psychological packaging makes them convincing to us in the form we write them; it isn't because all the steps involved in the proof really are–in some sense–surveyed by us. Furthermore, the ordinary mathematical proof, despite its being a form of argument virtually unique in its ability to compel assent among those who can follow it, isn't by virtue of *that* automatically given

[48] Geometrical intuitions and concepts are remarkable in how they seem to provide an 'understanding' in a way that computational algebraic approaches sometimes don't. An example philosophers will be familiar with is the contrast–in the study of modal logics, for example–with how one feels one understands what a modal system is up to when one is given, say, Kripke-style semantics for it; whereas a complete set of simple axiomatic rules–without any such semantics–provides no such feeling of 'understanding'.

epistemic credentials. This was seen early on by those who thought such proofs should be made entirely explicit; in doing so, one purpose would be to determine what assumptions were really involved in such proofs.

The psychological facts that convince us a traditional proof is right aren't directly connected to the reasons that it is right. I claim that we now know what's involved in such proofs: We know–pretty well–what implicit logic was involved in traditional mathematical proof, and we have generalized proof–in the twentieth century–to algorithmic systems of any sort. However, surveyability, as a virtue of ordinary proof, has turned out to be something of an illusion; instead, we know that a proof of a result R is valid if a derivation corresponding to that proof exists. In this way what it is that makes a proof successful, and what it is that causes us to be convinced by a proof have come apart. What makes a proof successful is the existence of a derivation of the appropriate sort.[49] What makes us convinced that a traditional proof is right is not our (conscious) recognition that it correlates to a derivation of the appropriate sort (although it does), but rather a potpourri of the various things going on in the ordinary proof: that certain inference packages move us from some steps to others, that (virtually mechanical or syntactic) computations justify the moves in other steps, that certain results have been established by other mathematicians, or even that certain steps semantically depend on earlier ones. The point isn't that these various factors–including the use of inference packages–are proof-theoretically misleading; to a large extent the fears against intuition indulged in by Frege, Hilbert, and others, were simply melodramatic.[50] Rather, the point is that the intuitive certainty the mathematician experiences when comprehending a successful traditional mathematical proof hasn't directly to do with what it is that makes that proof successful.[51]

[49] The claim that a derivation fails to exist which corresponds to a specific traditional proof can be challenged of course–one can deny that translation of the proof into the artificial language of the derivation has been attempted in the correct way. But, generally, it's recognized that the failure to construct a derivation indicates that the traditional proof fails to begin with. Epistemic warrant for traditional mathematical proofs, that is, now reside in the existence of the formal derivations they are supposed to correspond to. MacKenzie ([25], p. 322-323) notes that his research on the mechanizing of proof 'has been unable to find a case in which the application of mechanized proof threw doubt upon an established mathematical theorem, and only one case in which it showed the need significantly to modify an accepted rigorous-argument proof [a proof of Newton's]'. The point here is not the amazing success of traditional proof; it's that no traditional proof has (any longer) the epistemic credentials to withstand a failure to transform it into an entirely explicit derivation.

[50] To some extent–as subsequent mathematical practice makes clear–the abuse of intuition that the set-theoretic paradoxes embodied were–mathematically speaking–special cases.

[51] If it did, then the mathematician would be convinced of an ordinary mathematical proof only because it provided enough steps that he or she could see–in principle–what steps were missing; and were more steps supplied, he or she would only be that much more convinced. (Instead, as noted, the opposite happens.)

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Contrasting the epistemic surety offered by traditional proofs to that of 'computer proofs'. A traditional mathematical proof is a way of determining that a derivation of certain sort exists. It's a method that doesn't rely on empirical science, although our ability to understand such a proof does rely on numerous empirical facts that we take for granted. Were we to employ a computer to determine that a derivation exists, we would be relying–in that case–on empirical science.[52] Although mathematics is the establishing of theorems in algorithmic systems, there doesn't seem to be much of an epistemic difference between convincing ourselves that such theorems hold on the basis of traditional proof, and doing so on the basis of mechanized proving machines–such as computers. We find one approach–psychologically–far more convincing than the other; but it's not clear we're justified in feeling this way–given, anyway, what's really involved in being convinced by a traditional proof. There would be more of an epistemic difference between the two approaches if traditional proofs were themselves derivations, or at least surveyable abbreviations of derivations; but they aren't.

In addition, there is a parochial element in traditional proof absent from computer-generated proofs; this is that traditional proof–to succeed–must enable us to employ inference packages; or at least it must be short enough that the computations we must engage in to be convinced of it are humanly manageable. But derivations can thus go beyond the reach of traditional proofs in two related ways. The first is any traditional proof corresponding to a particular derivation may be too long for us to recognize its validity by means of human brute force–even when we subdivide the labor among many mathematicians; furthermore, there may be no way of presenting the derivation in the garb of accessible concepts within a traditional proof, so that the corresponding derivation may be established without having to execute all its steps by brute force. In such cases all that can be done is the examination of numerous cases that only a computer can manage.

I earlier noted (in *The failure of logicism and Hilbert's program as practical attempts to revise ordinary mathematical proof*) how ordinary mathematical proof seemed unaffected by the success of the translation of mathematics from the vernacular to artificial languages. As it turns out, there is a way in which these programs can be seen to have succeeded in changing the practice of mathematics–not because at long last they are contributing to a change in traditional mathematical proof; if the foregoing discussion is right, that's not possible: Traditional mathematical proof requires the use of inference packages. Rather, the program of formalization succeeded because it showed how traditional mathematical proof was directed towards the existence of derivations. But this means that *any* method that can successfully show the existence of derivations can contribute to mathematical knowledge. Indeed, mathematical practice may change not because mathematicians will change their proof-practices, but because–more

[52] I draw and discuss this distinction between science and pre-science, in my [2], Part III, §2.

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and more–computers will needed to discover those derivations that humans can't discover within the confines of traditional proof.

Traditional mathematical proof is an amazingly good method for indicating the existence of derivations. But it isn't a method that involves a priori tools, or even the grasping of the nature of derivation in some deep and direct way. Epistemically, that is, it's a method that relies strongly on transferring psychological inference packages from places where they are already extremely reliable; and adapting them into the mathematical context successfully. Were we to perfect our use of computers to help indicate the existence of derivations, they too might prove to be very good methods of doing such.[53]

Any scientific area is an epistemic patchwork: It involves a mixture of knowledge based directly on human capacities–such as observation–as well as tools that take us past such capacities, such as scientific instruments. Our ability to use such instruments, in turn, is a blend of applied scientific theory and hands-on knowhow developed in the context of learning to use such instruments.[54]

I have long stressed the epistemic differences between reliance on an evidential procedure that is (scientific-)theory-driven, and one based only on pre-science; the latter has greater epistemic surety in various respects than the former. But this doesn't stop the evidential practices in any science from becoming a blend of both in such a way that practitioners don't bother to distinguish how their evidence procedures depend on scientific theory as opposed to those that don't.

For a very long time, mathematics was different: Traditional proof relied (and relies) entirely on human capacities; it was even thought that such reliance was *definitive* of mathematics–doctrines of a priori truth implicitly turned on such views. But such views required that surveyability–as an epistemic virtue–would survive the transformation of traditional proofs into entirely explicit proofs. Instead, it's the existence of derivations within the context of families of algorithmic systems that's definitive of mathematics–and this opens the discovery of such derivations beyond the confines of traditional proof. Of course, until ways of recognizing the existence of derivations–other than traditional proof–became possible, the formalization revolution couldn't affect mathematical *practice*; but if computers really can help discover derivations that are otherwise undiscoverable via traditional mathematical proof, then the practice will change.

Mathematical proof practices–as institutionalized in the profession of mathematics –is ultimately a matter of discovering new results; this is regardless of whether we understand 'why' they are true or not: Mathematics remains theorem-

[53] Actually, this is too cautious a remark: Several 'computer proofs', that are standing up to repeated examination, the four color theorem, Kepler's conjecture, the classification of finite groups, seem to indicate we're already there: The problem isn't the epistemic surety of computers when playing essential roles in construction of proofs. Rather, it's figuring out strategies for how they can be used to show other results: The use of computers in mathematical proof hardly eliminates the need for human creativity.

[54] See my [4], Part I, for further details on this way of understanding the epistemology of science.

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driven.[55] If mathematicians find they can discover more of these results by means of computers than they can by means of traditional proof, they will desert traditional proof for that reason alone. The epistemic differences in surety between traditional proof (as anchored in human practices independent of empirical science) and computer proof (insofar as it relies on empirical science)–despite how, if I'm right, it corresponds to our subjective sense of understanding–is nevertheless too subtle a distinction to survive the successful discovery of numerous new theorems with the indispensable use of the computer.[56] As long as computer-aided proof is in practice as epistemically secure as traditional proof–as long, that is, as it isn't discovered that purported computer results have misled us in what derivations we can take as having been established, such 'proofs' will become a standard part of mathematical practice.

General conclusion. All that seemed left of the project of providing a foundation for mathematics is that mathematical proof is required. But the analysis of what (traditional) proof comes to unearthed the centrality of the correspondence of such proofs with formal derivations in algorithmic systems. In turn, this allows (at least in principle) the infiltration of computer proofs into the practice of mathematical theorem-proving, where such computer proofs fail to correspond to traditional proofs. In this way the methodology of mathematical proof blends into the methodology of the empirical sciences generally.

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[55] No traditional mathematician rejected computation-heavy traditional proofs which otherwise seemed not to provide an understanding of why the result shown follows.

[56] Of course this is not yet the state of play in mathematics: I'm making a prediction.

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