Platitudes in Mathematics

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1. Introduction

AJ Ayer claimed that truths of pure mathematics are 'analytic' in the sense that each follows 'simply from the definition of the terms contained in it' (Ayer 1936, ch. 4). Following Potter (2004, pg. 10), I use the term 'postulationism' for this position and positions like it. Postulationism is attractive for the epistemologist of mathematics. If '7+5=12' is entailed by definitions, then presumably one can establish that this sentence is true by deducing it from definitions. And this looks to be the beginning of an attractive, simple explanation of how justified belief in mathematics is possible. Postulationism was popular among the logical empiricists,[1] but has subsequently fallen out of favour; perhaps this is due to the influence of Quine (1951). I am happy to say that postulationism may now be about the make a comeback, thanks to a vigorous defence of one version of postulationism by Agustín Rayo, in his recent book *The Construction of Logical Space* (Rayo 2013).

My task in this paper is to address one particular objection to postulationism, which goes like this. The postulationist may say that mathematicians *today* can show that every polynomial function is continuous by deducing this conclusion from the definition of 'continuous', and other relevant definitions. But now consider mathematicians in 1800, before the term 'continuous' had been properly defined. How were *they* justified in claiming that every polynomial function is continuous? If the postulationist cannot answer this question, her epistemology of mathematics is incomplete. My task in this paper is to present a solution to the problem.

I'll limit my discussion in two ways. First, I will only consider *pure* mathematics. Second, I will consider only *expert* pure mathematicians. I do this not because I think that pure mathematics is more important than applied mathematics, or because I think that experts are more important than outsiders, but simply because I don't want to bite off more than I can chew.

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[1] See (Reichenbach 1924) for another statement of the view.

2. Rayo's Postulationism

Rayo claims that an individual can achieve justified belief in mathematics using what I call 'the stipulations and deductions method', as follows:

> *Step One:* Write down some 'axioms', which contain only logical particles and terms not before used.
> *Step Two:* Find reasons to justify sufficiently the contention that the axioms are coherent.[2] (If the axioms turn out *not* to be coherent, start again at *Step One.*)
> *Step Three:* Stipulate that the axioms are true, and that they implicitly define the non-logical terms that they contain.
> *Step Four:* Introduce some further terms by explicit definition.
> *Step Five:* Prove some theorems by deduction from the definitions.[3]

An example. Poonam, working in isolation, undertakes to learn some number theory. She begins by writing down some axioms. These must contain only logical terms, and new terms, hitherto unused: these might be 'zero', 'successor', 'natural number', 'plus' and 'times'. The axioms might be the Peano axioms, or something similar. Poonam must then investigate whether the axioms are coherent. If she finds sufficiently strong reason to believe that they are coherent, she then stipulates that the non-logical terms in her axioms are to be understood in such a way that the axioms are true. She then introduces more terms ('even', 'prime' etc.) by explicit definition. Then she proves some theorems.

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[2] Postulationists may disagree on the question of what exactly 'coherence' is. Perhaps the most obvious proposals are:

> (a) A set of sentences Γ is coherent just in case there does not exist a proof of $\perp$ all of whose premises are elements of Γ.
> (b) A set of sentences Γ is coherent just in case there is a model $\mathfrak{M}$ such that each element of Γ is true relative to $\mathfrak{M}$.

These views can differ significantly when applied to cases in which the mathematical language used allows second-order quantification. My own preference is for a view more like (b) – though see (SELF-CITATION REMOVED) for details.

[3] When using the stipulations and deductions method, one defines certain *words* by stipulating that certain *sentences* containing those words are true. One might prefer a variation of the method in which it is *concepts* which are defined. Following Rayo, I'll stick to the 'linguistic' version of postulationism, simply because I prefer to avoid difficult questions about what concepts are.

I have little to say now about *Step Four*. The received view is that that having stipulatively defined the term 'even' in the normal way, one is then justified in believing that (1) is true:

(1) A natural number is even just in case it is divisible by two without remainder.

Some more extreme Quineans disagree; their position deserves discussion, but I won't consider it here.[4] I will also have little to say about *Step Five*. The epistemology of learning by deduction is an important topic, but it is not my topic in this paper.

Step Three does require some discussion. Poonam's axioms will presumably entail existential generalizations, and so the idea that she could render them true by fiat may seem fantastical. The postulationist might seem to attribute to Poonam similar powers to those ascribed to God in the book of Genesis. But this is a misunderstanding. The idea is not that Poonam has a quasi-divine power to create abstract objects by fiat. Rather, the idea is that the non-logical terms in Poonam's axioms are *new* terms, of Poonam's own invention; she is therefore free to assign any interpretation to them that she wishes. In particular, she is free to stipulate that the terms are to be interpreted in such a way that her axioms are true. Now one might reasonably worry that there may be *no* interpretation of these terms such that the axioms are true (Sider 2007). However, Rayo has argued that, so long as Poonam's axioms are coherent, there will exist such an interpretation.[5, 6]

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[4] Famously, Quine (1951) argued that notions like 'definition' and 'synonymy' are too obscure to be of any philosophical utility. He did, however, make an exception for stipulative explicit definitions:

> There does, however, remain still an extreme sort of definition which does not hark back to prior synonymies at all; namely, the explicitly conventional introduction of novel notations for purposes of sheer abbreviation. Here the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens. Here we have a really transparent case of synonymy created by definition; would that all species of synonymy were as intelligible.

Some later, more extreme Quineans have thought that Quine should not have made this concession. See Juhl and Loomis (2010) for discussion, especially pg. 214.

[5] Rayo discusses the issue directly in Rayo (2013, ch. 8), though it would be not be too unreasonable to say that the whole book is a sustained criticism of the 'metaphysicalist' presuppositions of this particular objection to postulationism. For an exegesis of this Rayo's position, see (SELF-CITATION REMOVED).

[6] One might protest as follows. Some of our mathematical theories entail that *vastly* many objects exist: Boolos has written that ZFC^2 implies that there are, 'by ordinary lights', '(literally) unbelievably' many objects (Booolos 1998). Now even if ZFC^2 is coherent, there's no guarantee that there exist *that many* objects, and so there's no guarantee that there is any interpretation of the language of set theory on which all the axioms of ZFC^2 are true.

I do not have space to explain Rayo's reply. Instead, I will repeat a Rayovian metaphor. The objection fails, Rayo thinks, because it presupposes that we are 'operating against the background of a fixed domain'

Step Two is also problematic: what reasons could Poonam give in support of the contention that her axioms are coherent? If she already knows some mathematics, she may well be able to establish the coherence of her axioms by providing a mathematical model. For example, if Poonam already knows ZFC set theory she can establish the coherence of her number-theoretic axioms by defining a mathematical model whose domain consists of the finite von Neumann ordinals. But the stipulations and deductions method is of limited interest if it only allows one to learn mathematical theories for which one is already able to specify a mathematical model. So let's suppose that Poonam is not already in a position to describe a mathematical model for her axioms. Then it's hard to see how Poonam could adequately defend the claim that her axioms are coherent. This is a deep and important issue, which I discuss in more detail elsewhere (SELF-CITATION REMOVED). For now, a brief discussion will have to do.

The postulationist might try to sidestep the problem by saying that *Step Two* is not required after all. On this view, once Poonam has chosen her axioms, as long as they are *in fact* coherent, Poonam will then be justified in believing that they are true (at least in the absence of a defeater) even if she has no reason to believe that they are coherent. I will now argue that this position, which I call 'externalism', is problematic.

Here is the simplest version of the externalist position:

> *Crude Externalism*
> Suppose that Poonam has chosen her axioms, and that:
> > (a) her axioms are in fact coherent; and
> > (b) Poonam has no reason to suppose that her axioms are incoherent.
>
> Then Poonam is at once justified in believing that her axioms are true; she need not first show that they are coherent.

(Rayo 2013, pg. 93). Instead, we should recognize that there are many ways of 'carving up the world into objects'. Once we understand how this 'carving' works, Rayo thinks, we will see that so long as our purely mathematical theory is coherent, there will always exist a 'carving' which generates enough objects to verify the theory (Rayo 2013, §1.5).

I realize that this response is mere metaphor. (SELF-CITATION REMOVED) contains (I hope!) a more satisfying discussion of the issue. I would like to thank an anonymous reviewer at *Synthese* for suggesting that I include a comment on this issue.

It is easy to see that this can't be right. Suppose that Poonam's axioms are the Peano axioms together with some difficult truth of number theory, such as:

> (2) $x^n+y^n=z^n$ has no solutions where x, y and z are natural numbers, and n is a natural number strictly greater than 2.

Suppose in addition that Poonam has no reason to suppose that her axioms are incoherent. Then according to crude externalism, Poonam is immediately justified in believing that her axioms (including (2)) are true. But this is clearly absurd: one can't acquire justification for believing difficult mathematical results in this bizarre manner.[7]

In order to avoid this problem, it seems that the externalist will need to modify her position in something like this way:

> *Sophisticated Externalism*
> Suppose that Poonam has chosen her axioms, and that:
> (a) her axioms are in fact coherent; and
> (b) Poonam has no reason to suppose that her axioms are incoherent; and
> (c) Poonam's axioms meet condition *C*.
> Then Poonam is at once justified in believing that her axioms are true; she need not first having to show that they are coherent.

The challenge is to find some well-motivated condition *C*. Now I have no way of establishing that this can't be done, but I am not optimistic. So I think that, for the moment, the externalist is stuck with the view that Poonam must provide adequate reasons for her contention that the axioms are coherent before she is justified in believing that they are true. This brings us back to the question, 'What reasons can Poonam give in support of her claim that her axioms cohere?'

[7] Similar points about made in Boghossian (2003) and Ebert and Shapiro (2009).

This is what Rayo has to say on the topic:

> [A]cquiring a warrant for the internal coherence of an axiom system is a tricky affair. … It might turn on whether one has a good feel for the sorts of things that can be proved in the system … It might also turn on whether one has a good feel for what a model for the axiom system in question would look like. (Rayo 2013, 109-10)

Rayo's discussion is brief, so what follows is as much extrapolation as exegesis. Rayo suggests two ways of providing evidence for the claim that one's axioms are coherent:

> (a) One can acquire some justification for believing that an axiom system is coherent in part by 'getting a good feel for what a model for the axiom system in question would look like'.
>
> (b) One can acquire some justification for believing that an axiom system is coherent in part by getting a 'good feel for the sorts of things that can be proved in the system'.[8]

Let's take a look at these in turn. What does Rayo have in mind by 'model' in (a)? Well, as I've said, one can certainly establish the coherence of a system of axioms by providing a *mathematical* model for those axioms. But Rayo's language ('getting a good feel for') suggests that he's using the word 'model' in a broader and less formal sense here. Rayo doesn't give examples, so I'll suggest a couple on his behalf. Poonam might attempt to justify the belief that her axioms are coherent by specifying a physical model for them – perhaps identifying the natural numbers with an ω-sequence of space-time points. If Poonam does not want to rely on empirical assumptions about the structure of space-time, she may alternatively appeal to a model in which the natural numbers are identified with type strings of strokes (Parsons 1979, 2009):

| || ||| |||| ||||| …

I'll use the term 'pictorial model' for models like this, to distinguish them from the models studied by model theorists (what I've been calling 'mathematical' models). One can investigate this pictorial

[8] Rayo also makes a third suggestion, which is that one can establish that a system of axioms is coherent by giving a consistency proof for the system. I omit this suggestion because, as I hinted in footnote 2, I do not believe that proof-theoretic consistency suffices for 'coherence' in the relevant sense.

model for Peano arithmetic by playing around with token strings, both in imagination and on paper. The term ‘intuition’ is used in diverse ways in the philosophy of mathematics; it seems to me that one reasonable use of this term is to refer to investigation of pictorial models using experimentation on paper and in imagination.

A traditional pictorial model for the axioms of Euclidean plane geometry is an idealized sheet of paper (which is perfectly flat and of infinite extent); in this model, the ‘lines’ are idealized (infinitely thin and infinitely long) possible pencil lines, and the ‘points’ are the intersections of such possible pencil lines. One can, arguably, investigate this pictorial model by ‘intuition’ in the sense briefly described above.[9]

What about (b): what does Rayo have in mind here? Perhaps Rayo’s idea is that, if after great effort one has failed to deduce a contradiction from some axioms, this provides some evidence that the axioms are coherent. For example, the fact that nobody has yet deduced a contradiction from the axioms of Quine’s ‘New Foundations’ (Quine 1937) perhaps provides some evidence for the hypothesis that they are coherent. I suggest that Rayo also has in mind the following. Having identified a pictorial model for one’s axioms, one can refine one’s understanding of the pictorial model by investigating the theorems of one’s system. Having thus refined one’s understanding of the model, one can be more confident that it is indeed a model for the axioms; this provides further support for the hypothesis that the axioms are coherent. An example may help. It was once widely thought that it is incoherent to suppose that a region of space with non-zero volume is wholly composed of points, each of which has volume zero. Someone who took this position might worry that incoherence lurks in contemporary geometry, where it is claimed that the unit cube (which is said to have measure one) is identified with a set of points (each of which is thought to have measure zero). Mathematicians today are not troubled by such concerns. Their intuitive understanding of volume has been refined by the study of measure theory.

[9] One indication of the epistemological significance of pictorial models in mathematics is this: one of the reasons that mathematicians are wary of Quine’s ‘New Foundations’ (Quine 1937) is that, so far, no pictorial model for the theory has been found. As Fraenkel, Bar-Hillel and Levy write:

> From the point of view of the philosophy and the foundations of mathematics, the main drawback of NF is that its axiom of comprehension is justified mostly on the technical ground that it excludes the antinomic instances of the general axiom of comprehension, but there is no <u>mental image</u> of set theory which leads to this axiom and lends it credibility (Fraenkel, Bar-Hillel and Levy 1973, pg. 164, emphasis added)

I don't claim fully to have answered the question, 'What reasons can Poonam give in support of her claim that her axioms cohere?' But I hope this brief discussion will suffice for now. I want to get on to the 'history problem', which is the topic of this paper.

3. The history problem

As I said, Rayo claims that mathematical claims *can in principle* be justified using the stipulations and deductions method. He doesn't make the additional claim that this is *in practice* how mathematicians justify their claims. Indeed, Rayo doesn't address the question of how mathematical beliefs are justified in practice. This is an important gap in Rayo's epistemology of mathematics.

Let's start off by looking at 'mature' mathematical theories, as they are presented in today's textbooks. In these cases, it's plausible that mathematical claims are justified by the stipulations and deductions method – though perhaps steps one to five are not carried out in the specified order. These theories have rigorously specified axioms. The postulationist may claim that these axioms implicitly define the non-logical terms they contain, and that mathematicians are justified in believing that these axioms are true because they have sufficient reason to believe that they are coherent. The remaining terms have rigorously specified explicit definitions. The theorems are justified by deducing them from the axioms and explicit definitions.[10]

One complication is that there is variation in how mathematical theories are axiomatized, and in how mathematical terms are defined. For example, in Cohn (2000) the definition of 'ring' implies that every ring must have a multiplicative identity; in Behrens (1972), the definition of 'ring' implies that there are rings that lack a multiplicative identity. And so it may be said that the definitions used in contemporary mathematics are inconsistent. The postulationist may respond by saying that the term 'ring' introduced stipulatively by Cohn is different from its earlier homonym, introduced by Behrens. Thus, the two different definitions do not contradict one another after all. The postulationist may struggle to produce a systematic account of the individuation of words, but this is a difficult task for everyone, as Hawthorne and Lepore (2011) have emphasized.

[10] The postulationist should add that some mathematical beliefs are justified by non-deductive reasoning. For example, I take it that mathematicians today are justified in believing that π is normal, even though this has not yet been proven.

A second complication. When we discussed the stipulations and deductions method in section two, we imagined that the method would be used by a single mathematician working in isolation. But of course modern mathematics is a collective endeavour. There are interesting questions to be discussed about the division of epistemic labour involved,[11] but I will set such questions to one side for now. I simply assume that it is reasonable to say that the mathematical community *as a whole* uses the stipulations and deductions method.

So far, so good. But the postulationist will have a harder time accounting for mathematical knowledge in branches of mathematics which have not yet reached such a rigorous form. For example, the term 'continuous' was a meaningful term in widespread use in the eighteenth century. However, nobody had a formal definition for the term before Bolzano, who published his definition in a paper of 1817.[12] Just prior to 1817, the term had no stipulative formal definition.

When I say that the term 'continuous' was meaningful prior to 1817, I am not claiming that the term was always used with exactly the sense that it has today; nor am I claiming that the term was perfectly precise. On the contrary, it seems likely that at least some eighteenth century writers used the term in a sense different to its current one[13] and that the term was open-textured.[14]

Now mathematicians at the start of the nineteenth century were justified in accepting the intermediate value theorem:

(3) Suppose that $f:[a, b] \longrightarrow \mathbb{R}$ is continuous, that $f(a)<0$ and that $f(b)>0$. Then for some $c \in [a, b]$, $f(c)=0$.

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11 For example: Can there be *a priori* knowledge by testimony? See Malmgren (2006) for a recent discussion of this issue.

12 Russ (1980) is a translation of Bolzano's paper. Lützen (2003) is a useful summary of the relevant stretch of the history of mathematics.

13 See (Stedall 2008, section 11.2.2) for Euler's use of the term.

14 It is plausible that the modern definition of 'continuous' captures only one precisification of the term from 1800, and that on another precisification it meant something more like what is meant today by 'uniformly continuous'. This is suggested by (Lakatos 1976, pg. 129, fn. 2).

But it seems that these mathematicians could not have justified the belief that (3) is true by inferring it from stipulative definitions, because they lacked an adequate definition of 'continuous'.[15]

The postulationist may respond by accusing me of confusing the 'context of discovery' with the 'context of justification'. She may claim that while mathematicians in 1800 had stumbled across the intermediate value theorem, they were not yet justified in claiming that (3) is true. It seems to me that this response is desperate. By 1800, research in real analysis had continued for decades and mathematicians had a good grasp of the basic notions – *real number, differentiable, continuous* and so on. Much of their reasoning involving these notions was cogent (even by today's lights), despite the fact that it did not meet the standards of rigour required by today's journals. If we had some very strong argument for the hypothesis that the stipulations and deductions method is the *only* method by which pure-mathematical beliefs can be justified, we might be forced to accept the conclusion that mathematicians in 1800 were not justified in believing the intermediate value theorem. However, we have no good argument for this hypothesis, and the fact that it has this implausible consequence gives us ample grounds for rejecting it.

Now for a second example. Cauchy's 1821 textbook *Cours d'Analyse* (Cauchy et al 2009) was more rigorous than previous textbooks on analysis. However, by modern standards Cauchy's work falls short in some respects. In particular, he didn't present an adequate axiomatisation of the theory of real arithmetic. Modern counterparts of Cauchy's book start with a list of axioms, from which the theorems later in the book are derived. Cauchy provided no such list. It doesn't immediately follow that Cauchy failed to provide an adequate axiomatization: although Cauchy didn't present a single list of axioms, he did specify axioms in various places in the book.[16] However, the axioms that he did present were not adequate: Cauchy did not include a completeness axiom. In consequence, some of his proofs were flawed. For example, Cauchy presented (4) as a theorem:

(4) Every Cauchy sequence of real numbers converges to a limit.

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[15] There were informal definitions of 'continuous' prior to Bolzano's paper. For example, in a monograph of 1814, Cauchy said that when a function is discontinuous, 'insensible' changes in the argument give rise to 'brusque jumps' in the value; this is a useful though very sketchy definition of 'continuous' (Grabiner 1981, pg. 93). However, I assume that such informal definitions were not sufficient to determine the meaning of 'continuous' or to permit the demonstration of the important theorems about continuity.

[16] For example, at one point Cauchy says that it is 'a fundamental axiom' that 'the sum of several numbers remains the same in whatever order we add them' (Cauchy et al 2009, pg. 269).

But (4) was not entailed by Cauchy's axioms, and his 'proof' goes blurry at exactly the point where a completeness axiom is needed (Lützen 2003). Cauchy accepted (4) in 1821, and it seems that he was justified, at least to some extent. But Cauchy could not justify (4) by inferring it from stipulative definitions. His rigorously stated definitions and axioms were certainly not sufficient to entail that every Cauchy sequence converges.

Nineteenth century geometry provides further examples. Euclid's axioms for geometry were, by modern standards, inadequate: they don't logically entail all of his theorems. Axiomatisations of geometry that meet modern standards did not exist before the late nineteenth century (see in particular Pasch (1882) and Hilbert (1899)). These authors added to Euclid's axioms some statements which seem rather trivial, such as:

(5) For any points x, y, and z, if x is between y and z, then x is between z and y.

They also added some more difficult axioms – such as a completeness axiom.

I conclude that, whatever the merits of the claim that one *can in principle* justify mathematical claims using the stipulations and deductions method, there are in the history of mathematics counterexamples to the claim that this is always *in practice* how mathematical claims are justified. If the postulationist is to avoid the charge that her epistemology for mathematics is incomplete, she must provide some explanation of how mathematical claims were justified in these historical cases.

4. The Canberran theory

In his paper, 'Psychophysical and Theoretical Identifications' (Lewis 1972), David Lewis offered an account of the metasemantics of everyday[17] psychological terms such as 'pain' and 'believes'.[18] Setting aside the logical details, Lewis's proposal was this. He claimed that we, the users of these terms, all accept a certain psychological theory. The theory consists of 'platitudes' – statements the truth of which is 'common knowledge among us' (pg. 256). The only example Lewis gave in this paper was 'Toothache is a kind of pain'. However, Lewis wrote that some of the platitudes report

17 I say 'everyday' to indicate that Lewis's account is not supposed to cover technical terms from cognitive science.

18 See also Lewis (1970).

‘causal relations of mental states, sensory stimuli, and motor responses’ (pg. 256); so perhaps Lewis would have agreed that these are also platitudes:

> Disgust often causes retching.
> Embarrassment sometimes causes the face to become red.
> Prolonged exposure of the skin to very cold or very hot objects causes pain.
> Sufficient eating causes the cessation of hunger.

These platitudes, Lewis claimed, together determine the semantic values of the psychological terms: the semantic values of the psychological terms are such as to ensure that all, or most, or a weighted most, of the platitudes are true. Thus, Lewis allowed that some of the platitudes may be false even if none of our psychological terms are empty. For example, it may be that some small minority of the platitudes about belief are false, even if eliminativism about belief, pain, disgust etc. is mistaken.[19]

Lewis also claimed that the platitudes have a special epistemic status. According to Lewis, ‘it is analytic that either pain, etc., do not exist or most of our platitudes about them are true’ (pg. 257).

Lewis explained his position by appeal to a ‘myth’. He asked his readers to imagine that at some time in the distant past ‘our ancestors’ operated without any psychological vocabulary. Then ‘some genius invented the theory of mental states’, and declared that her new psychological terms (‘belief’, ‘pain’ etc.) are implicitly defined by the statements that comprise the new theory. Lewis commented:

> But that did not happen. Our commonsense psychology was never a newly invented term-introducing scientific theory – not even of prehistoric folk-science. The story that mental terms were introduced as theoretical terms is a myth … [But it] is a good myth if our names of mental states do in fact mean just what they would mean if the myth were true. I adopt the working hypothesis that it is a good myth. (Pg. 257)

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19 Lewis accepted that (a) some minority of the platitudes may be false. He also claimed that (b) the platitudes are ‘common knowledge among us’ (pg. 256). How are these two claims to be reconciled?
Note that after making claim (a), Lewis wrote ‘let us set aside this complication for the sake of simplicity’ (pg. 252). Later (pg. 258) Lewis explained that he was ‘ignoring clustering for the sake of simplicity’. So my suggestion is that when Lewis made claim (b) he was engaging in simplification or idealization.

In short, Lewis's position was that platitudes have a similar metasemantic and epistemic status to stipulative implicit definitions, even though (as a matter of historical fact) they are not stipulative implicit definitions.

Later philosophers, inspired by Lewis, have elaborated and modified this idea in various ways, and applied it to other vocabularies. For example, Frank Jackson has discussed colour words and ethical terms (Jackson 1998). Philosophers working in this tradition have been called 'Canberra planners'.[20] Antony Eagle has suggested (Eagle 2008) that these ideas can be applied to mathematics. My goal in the rest of the paper is to argue that Eagle's idea is the beginning of the solution to the history problem.

One might argue, to repeat an example from the last section, that the meaning of the term 'continuous' was fixed in 1800 by a number of platitudes containing the term. Perhaps these included:

(6) Every constant function is continuous.

(7) The sum of two continuous functions is itself continuous.

Perhaps there were also some false platitudes containing the term 'continuous'. For example, throughout most of the nineteenth century, standard textbooks included as a 'theorem' the claim that every continuous function is differentiable everywhere, except at isolated points. But this is false, as Weierstrass showed by constructing a counterexample in the 1870s. Many of the 'proofs' of this 'theorem' contained this mistaken assumption (Hawkins 1970, section 2.3):

(8) Every continuous function is piecewise monotonic.

Mathematicians in the mid-nineteenth century assumed (8) but did not attempt to prove it; they did so consistently, even when in good conditions. So it is plausible that (8) was a platitude in the mid-nineteenth century.

20 See Braddon-Mitchell and Nola (2009) for more recent work on the Canberra plan.

The semantic value of the word ‘continuous’, it may be said, was such as to ensure that these platitudes (or most of them, or a weighted most them) are true. It may also be claimed that these platitudes had some special epistemic status. This could be the beginning of an explanation of how mathematicians were able to justify their beliefs about continuity before Bolzano came up with his rigorous definition. The following geometric axiom, mentioned in last section, is a particularly compelling example:

(5) For any points x, y, and z, if x is between y and z, then x is between z and y.

You will not find this statement labelled ‘definition’ or ‘axiom’ in works of mathematics before the late nineteenth century – but it was certainly used as a tacit premise in proofs. And it’s very plausible that it had much the same epistemic status as explicitly articulated stipulative axioms and definitions. I call this proposal ‘the Canberran theory’. It’s attractive, but it needs work. Two issues in particular require discussion.

The first issue is this. The claim that the platitudes have a ‘special epistemic status’ is hopelessly unspecific. An adequate version of the Canberran theory will have to include more detail on this point.

Second, it’s not yet sufficiently clear what counts as a ‘platitude’. Proponents of the Canberran theory will naturally assume that explicitly specified axioms and definitions are platitudes, and that certain other *relevantly similar* statements are also platitudes. But what does ‘relevantly similar’ come to? As Daniel Nolan has pointed out, ‘‘[p]latitudes’ is an expression that is to some extent a placeholder’ (Nolan 2009): it’s far from clear that all of its users mean the same thing by the term. As Nolan goes on to explain, some Canberra planners take it that ‘the platitudes about some X are all the sentences talking about X that we take to be true’ – Nolan calls this the ‘kitchen sink’ account. Other Canberra-planners endorse more ‘restrictive’ accounts, as Nolan puts it. Some clarity on this issue is required.

The first of these two issues is comparatively easy: I’ll present an account of the epistemology of platitude in section five. The second issue is much harder. My own preference is for a ‘kitchen sink’ theory, though I don’t pretend to have a knock-down argument for this position. I’ll discuss restrictive theories in section six, before defending the kitchen sink theory in section seven.

5. The epistemology of platitude

Consider again (5), which may be a platitude about between-ness:

(5) For any points x, y, and z, if x is between y and z, then x is between z and y.

Examples like this motivate the claim that all platitudes are true, *a priori,* and epistemologically basic. This claim is mistaken, however, because some terms are associated with inconsistent sets of platitudes.[21] Historical examples include Frege's definition of 'extension' in his *Grundgesetze* (Frege 1893/1903), and Church's first version of the lambda calculus (Church (1932), Church (1933), Cardone and Hindley (2009)).

The Canberran will have to concede that in cases where the platitudes are inconsistent, they can't all be true, *a priori* and basic. However, she may insist that in cases where the platitudes *are* consistent (or better, coherent) they are true, *a priori* and basic.[22] This proposal is problematic. We can imagine a mathematical community in which the platitudes governing the basic terms of number theory are the Peano axioms, together with (2), which they call 'the Fermat axiom':

(2) $x^n+y^n=z^n$ has no solutions where x, y and z are natural numbers, and n is a natural number strictly greater than 2.

Now the mathematicians in this community are surely not even *prima facie* justified in accepting (2) simply because (2) is a platitude. It's just not plausible that difficult number-theoretic results such as (2) can be justified in this bizarre fashion. This is a reworking of a point made back in section two. There, we concluded that when one uses the stipulations and deductions method, in order to justify the claim that one's implicit definitions or axioms are true, one must first provide sufficient reason to believe that they are coherent. This suggests the following account of the epistemology of platitude (PTO):

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[21] One might argue more directly that not all platitudes are true by claiming that (10) was a platitude in the mid-nineteenth century and observing that it is false. The argument is inconclusive, however, because it is not clear that (10) really was a platitude.

[22] A terminological variant on this view is to insist on applying the word 'platitude' only to *true* platitudes, and then saying that all 'platitudes' are true, *a priori* and basic.

> The mathematicians in some community are justified in believing that their platitudes are true to the extent that they have found reason to believe that they are coherent.

But this can't be *quite* right, for it has the objectionable implication that the mathematicians in some community are justified in believing one of their platitudes, they must also be justified in believing all the others, and to exactly the same extent. It is surely possible for the mathematicians in some community to be justified to a very high degree in accepting one platitude ((9), say) while being justified to a rather lower degree in accepting some other platitude ((10), say):

> (9) Given natural numbers x and y, $x \cdot y = y \cdot x$.
>
> (10) If S is a set of non-empty sets, there exists a function f whose domain is S, such that for all $s \in S$, $f(s) \in s$.

The proposal is easily fixed:

> Suppose that S is a platitude within mathematical community C; then the mathematicians in C are justified in believing S to the extent that (i) they have some procedure (or procedures) for ensuring that their platitudes are coherent; and (ii) the procedure has been (or the procedures have been) followed properly in the case of S.

Now one might protest that in typical cases mathematicians will *not* be able to provide any satisfactory justification for the claim that their platitudes are coherent: it will be *obvious* that the platitudes are not coherent because there are many mathematical terms which are defined by conflicting ways. To repeat an example, in the mathematical literature there seem to be inconsistent definitions of the term 'ring'. My response to this objection is that these are cases of *homonymy,* not *inconsistency* (see section 3).

A second objection. It can be argued that platitudes from different branches of mathematics contradict one another. For example, it may be said that the platitudes of the theory of real arithmetic imply (PTO):

(11) For all x, there exists some y such that $y < x$.

At the same time, the platitudes of number theory seem to imply:

(12) There exists some x such that for all y, $\neg y < x$.

And so the platitudes of real arithmetic seem to contradict the platitudes of number theory. I claim that the conflict between real arithmetic and number theory is merely apparent. The quantifiers used in different branches of mathematics are restricted; for example, when doing number theory one typically uses quantifiers that range over only natural numbers. What's more, the non-logical terms of number theory are defined by different platitudes to the homophonous terms from real arithmetic, and so I suggest they mean different things. We could write '$<_{NT}$' for the 'strictly less than' relation in number theory, and '$<_{\mathrm{RA}}$' for the 'strictly less than' relation in real arithmetic.

Thus (11) and (12), when properly understood, are not in conflict:

(11′) Given any real number x, there exists some real number y such that $y <_{RA} x$.
(12′) There exists some natural number x such that for any natural number y, $\neg y <_{\mathrm{NT}} x$.[23]

A third objection. Imagine that a perverse logic teacher tells his students that he has in mind ten sentences, and asks them to establish that these sentences are coherent; he then refuses to tell the students what the sentences are. Of course, the students cannot complete the exercise. They need to know what the sentences are before they can even begin to think about whether they are coherent. In much the same say, it might be thought, mathematicians in the early nineteenth century could not possibly have provided evidence for the claim that their platitudes were coherent, because they had not yet identified the platitudes as such – and some of the platitudes may not even have been articulated.

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[23] I risk being accused of over-intellectualization at this point. It can be argued that (11′) and (12′) are rather refined, theoretical claims – but platitudes are supposed to be boring, obvious truths. The first point to make in response is this: my topic in this paper is the mathematical knowledge of expert mathematicians, not the general public. And (11′) and (12′) *are* obvious for expert mathematicians. More importantly, despite the connotations of the word 'platitude', platitudes need not be obvious. Providing sufficient reason to believe that one's platitudes are coherent can be *very* difficult; in such cases, the truth of the platitudes can be highly non-obvious. I would like to thank an anonymous reviewer at *Synthese* for bringing these issues to my attention.

Consider again the two methods which can, according to Rayo, be used to justify the contention that some platitudes are coherent:

> (a) One can acquire a warrant for believing that an axiom system is coherent in part by 'getting a good feel for what a model for the axiom system in question would look like'.
> (b) One can acquire a warrant for believing that an axiom system is coherent in part by 'good feel for the sorts of things that can be proved in the system'.

I claim that both methods can be used even when one's platitudes have not yet been articulated and identified as platitudes. Let's look at (a) first. As I've said, mathematicians in the mid-nineteenth century had not yet articulated all of the platitudes of Euclidean geometry. For example, they had yet to articulate the platitude (5):

> (5) For any points *x*, *y*, and *z*, if *x* is between *y* and *z*, then *x* is between *z* and *y*.

Nevertheless, they did have models for the platitudes of Euclidean geometry: for example, an idealized sheet of paper is a pictorial model for plane Euclidean geometry. By allowing these models to guide their reasoning in appropriate ways, they were able to ensure that the premises used in their proofs – even tacit premises – were largely coherent. Now let's consider (b). I suggested in section two that part of Rayo's proposal is that one can acquire some evidence that one's axioms are coherent by trying, and repeatedly failing, to derive a contradiction from them. This is a technique one can use to provide evidence that one's platitudes are coherent, even if one can't articulate the platitudes. One can attempt to prove a contradiction, given the prevailing standards of proof, even if one can't characterise these standards by giving a list of axioms and definitions. I also suggested that Rayo's claim is that by deriving theorems from one's system of axioms one can deepen one's understanding of one's model or models for those axioms. Again, this is possible even if one's platitudes have not been articulated.

6. Restrictive theories of platitude

Recall Nolan's distinction. According to the 'kitchen sink' theory of platitude, 'the platitudes about some X are all the sentences talking about X that we take to be true.' According to 'restrictive' theories, on the other hand, in typical cases only some of the 'sentences talking about X that we take to be true' are platitudes; some other 'statements about X that we take to be true' are not platitudes. Proponents of the two positions will be called 'kitchen sinkers' and 'restrictivists'.

Restrictive theories may seem more plausible than the kitchen sink theory. Contrast these two geometric statements:

> (13a) If A and C are two distinct points on a line, then there exists at least one point B between A and C.
>
> (13b) A regular heptadecagon can be constructed with straight edge and compasses.

There are conspicuous epistemic differences between (13a) and (13b). One would expect (13a) to turn up as a premise in a deductive justification of (13b), but it would be downright bizarre to give a deductive argument for (13a) that included (13b) as a premise. Indeed, it is not clear that a deductive argument is needed for (13a) – it's the sort of statement that can properly be justified by intuition alone. Not so for (13b): anyone who attempted to justify (13b) by appeal to a pictorial model would be accused of 'hand-waving'. Now perhaps these epistemic differences are in part due to the fact that (13a) is, by stipulation, an *axiom* of geometry while (13b) is not. But this can't be the whole story. Even before (13a) had been identified as an axiom, it was used a premise in deductive arguments for statements like (13b), but early nineteenth century mathematicians would surely have looked askance at a deductive argument for (13a) which used (13b) as a premise.

Other pairs of statements contrast in a similar way:

> (14a) Every natural number has a successor.
>
> (14b) There is no largest prime number.

(15a) The series a_0, a_1, a_2, a_3 converges to l just in case for any $\varepsilon>0$ there exists some N such that for all $n\geq N$, $|a_n - l|<\varepsilon$.

(15b) $(\pi/4) = 1 - (1/3) + (1/5) - (1/7) + (1/9) - (1/11) + \ldots$

These points can be used to motivate a restrictive theory of platitude. Plausibly, we should accommodate these observations about the epistemic differences between the 'a' statements and the 'b' statements by saying that the 'a' statements are platitudinous while the 'b' statements are not.

A proponent of this restrictive approach might attempt to give necessary and sufficient conditions for platitudinousness; for example, one might consider:

> A statement *S* is a platitude within mathematical community *C* just in case the relevant experts in *C* assume (perhaps tacitly) that *S* is properly defended by appeal to pictorial models, and not by deduction.

However, we should not *require* the proponent of a restrictive position to produce an answer to the question 'Which statements are platitudes?' with this form. There are many philosophically useful categories for which we are unable to provide informative necessary and sufficient conditions. A proponent of a restrictive theory of platitude might reasonably answer the question 'Which statements are platitudes?' by describing the differences in the epistemic status of platitudes and non-platitudes; the description need not be as tidy as a list of necessary and sufficient conditions.

So restrictive theories of platitude have a good deal of *prima facie* appeal. But I am sceptical. It seems to me that claims such as those above about the epistemological differences between platitudes and non-platitudes do not survive scrutiny.

Let's take a closer look at the claim that while platitudes can be used as premises in deductive arguments for non-platitudes, it is illegitimate to use a non-platitude as a premise in a deductive argument for a platitude. Consider the axiom of choice:

(16) If S is a set of non-empty sets, there exists a function f whose domain is S, such that for all $s\in S$, $f(s)\in s$.

This statement is ‘labelled ‘axiom’ in textbooks; it is used as a premise in deductive arguments but it not usually justified deductively. It is often defended by appeal to intuition. And so, I think, the restrictivist should say that (16) is a platitude in today’s mathematical community. Now consider the trichotomy law for cardinal arithmetic:

> (17) Given any two sets A and B, either the cardinality of A is strictly greater than the cardinality of B, or the cardinality of B is strictly greater than the cardinality of A, or the cardinality of A is identical to the cardinality of B.

(17) is not classified as an axiom or definition; it is standardly justified by deduction; mathematicians do not allow themselves to assume (17) without deductive argument on the grounds that (17) is highly intuitive (though it is). So I suppose that the restrictivist will want to say that (17) is not a platitude. However, when a defence of the axiom of choice is called for, it is often pointed out that (17) entails (16) – this was established in Hartogs (1915). Thus, (17) is used as a premise in a deductive argument for (16). The restrictivist will have to admit that this is a counterexample to the claim that non-platitudes cannot properly be used as premises in deductive justifications of platitudes.[24]

The restrictivist might respond (perhaps inspired by Maxwell (1963, 402)) by saying that whether or not a statement is a platitude varies with context. In most contexts, the axiom of choice is a platitude, and the trichotomy law is not – and so usually the trichotomy law is deduced from the axiom of choice, and not *vice versa.* But in certain exceptional cases the trichotomy law is a platitude and the axiom of choice is not; in these exceptional cases it is appropriate to deduce the axiom of choice from the trichotomy law. It seems to me that this misrepresents the evidential relationship between the axiom of choice and the trichotomy law; the received view among mathematicians is that these two statements are mutually supporting.[25]

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[24] Other examples are easy to find. When defending the principle of induction for natural numbers, mathematicians will sometimes show that it is entailed by the well-ordering principle. The claim that every non-empty set of reals that is bounded above has a supremum is today typically taken as an axiom of real arithmetic, but sometimes it is defended by deduction from the claim that the real line is Dedekind-complete, which is usually presented as a theorem. And so on.

[25] I have reached this position by discussing the matter with mathematicians. Admittedly, this is not a rigorous procedure. Perhaps some experimental philosophy would be warranted here.

So the restrictivist makes a mistake when she claims that one can't support a platitude by deducing it from a non-platitude. We should also reject the claim that it is improper to defend non-platitudes by appeal to pictorial models. When mathematicians sneer about 'hand-waving', I suggest, their point is not that hand-waving arguments are of no evidential value; rather, their point is that a proof is typically preferable to a hand-waving argument. It is clear, I think, that hand-waving arguments do have some evidential value. For one thing, they guide research agendas: it is not uncommon for a mathematician to devote months to the attempt to prove a conjecture because she has been antecedently convinced by a hand-waving argument that the conjecture is true. What's more, sometimes mathematicians assess their axioms by investigating whether the theorems of the system can be motivated by appeal to pictorial models. For example, when making the case against Quine's New Foundations*,* Fraenkel, Bar-Hillel and Levy point out that some of the theorems of the system are highly unintuitive: for example, it is a theorem of NF 'that there is a set y which is not equinumerous to the set $\{\{x\}|\ x\in y\}$ of its singleton subsets' (Fraenkel, Bar-Hillel and Levy 1973, pg. 163).

So the restrictivist claims about the epistemological differences between platitudes and non-platitudes do not withstand close scrutiny. The supposed boundary between platitudes and non-platitudes, like a boundary-line on a pointillist painting, disappears when one looks closely. For this reason, I think that it is very hard adequately to motivate a restrictive theory, and so I prefer a 'kitchen sink' theory.

7. The kitchen sink theory

I don't claim to have refuted restrictive theories definitively, but I hope that the discussion in the last section is at least sufficient to convince the reader that the kitchen sink theory deserves serious consideration. Combining the kitchen sink theory with my account of the epistemic status of platitudes, we get:

> Suppose that *S* is a statement accepted within mathematical community *C*; then the mathematicians in *C* are justified in believing *S* to the extent that (i) they have some procedure (or procedures) for ensuring that the statements they accept are coherent; and (ii) the procedure has been (or the procedures have been) followed properly in the case of *S*.

In this section, I consider some of the procedures that mathematicians use to ensure the coherence of their mathematical theory.

I assume that any reasonable theory of coherence will imply this general principle:

> *The Coherence/Entailment Principle*
> If a set of statements Γ is coherent and if Γ entails S, then $\Gamma \cup \{S\}$ is also coherent.

The kitchen sinker can use this rule to explain the role of deduction in mathematical theorising. Suppose that one's total mathematical theory is the set $\Gamma \cup \{S\}$. By *The Coherence/Entailment Principle,* one may be able to justify the claim that $\Gamma \cup \{S\}$ is coherent in two steps: first, one deduces S from Γ; second, one provides reasons for believing that Γ is coherent. We should also expect any reasonable account of coherence to imply this:

> *The New Predicates Principle*
> Suppose that a set of statements Γ is coherent, and does not contain the n-ary predicate R. Suppose also that the open formula $\varphi(x_1, \ldots, x_n)$ does not contain R. Then $\Gamma \cup \{\ulcorner \forall x_1 \ldots \forall x_n (Rx_1 \ldots x_n \leftrightarrow \varphi(x_1, \ldots, x_n)) \urcorner\}$ is also coherent.

This allows the kitchen sinker to explain the special epistemic role of statements which are used as stipulative explicit definitions of predicates. If one introduces a new predicate R into one's mathematical lexicon by stipulating that the sentence $\ulcorner \forall x_1 \ldots \forall x_n (Rx_1 \ldots x_n \leftrightarrow \varphi(x_1, \ldots, x_n)) \urcorner$ is to be its definition, it is guaranteed that one will not introduce an incoherence into one's theory. Thus, no justification is required for a statement of this kind.[26, 27]

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[26] In some cases, the 'new' term will be a replacement for some pre-existing term – and may be pronounced and spelled in the same way as its precursor. For example, when Bolzano gave his definition of 'continuous', arguably he replaced a pre-existing term with a homonym. Carnap used the term 'explication' for this process:

> The task of making more exact a vague or not quite exact concept used in everyday life or in an earlier stage of scientific or logical development, or rather of replacing it by a newly constructed, more exact concept, belongs among the most important tasks of logical analysis and logical construction. We call this the task of explicating, or of giving an explication… (Carnap 1947, 8-9.)

[27] Explicit stipulative definitions of proper names, operators etc. can be dealt with in the same way. This is left as an exercise for the reader.

Having justified some of one's mathematical claims by deduction, and having set aside stipulative definitions, one may attempt to justify those statements that remain using models – pictorial and perhaps also mathematical. To repeat some examples from section two, the following 'type' sequences of strokes form the domain of a pictorial model for the arithmetic of the natural numbers:

| | | | | | | | | | | | | | | …

A standard pictorial model for Euclidean plane geometry is an idealized sheet of paper. Justifying statements using pictorial models such as these will typically involve what I have been calling 'intuition' – experimentation on paper and in imagination.

Some statements are very easy to justify by appeal to pictorial models; for example:

(5) For any points *x*, *y*, and *z*, if *x* is between *y* and *z*, then *x* is between *z* and *y*.

The term 'intuitive' is sometimes used for statements like this. Other statements are next to impossible to justify by appeal to pictorial models; such statements are 'unintuitive':

(2) $x^n+y^n=z^n$ has no solutions where x, y and z are natural numbers, and n is a natural number strictly greater than 2.

Because 'intuitive' statements like (5) are easily motivated using pictorial models, deductive arguments for such statements are often not given. Since unintuitive statements like (2) cannot be justified by appeal to pictorial models, they will typically be justified by deduction. Thus, it is natural in mathematics to give deductive arguments for unintuitive statements using intuitive statements as premises.

Some statements are intermediate between these extremes of (2) and (5); intuitiveness comes in degrees. Consider, for example, the Jordan Curve Theorem:

> (18) Let C be a simple closed curve in the plane. Then its complement consists of exactly two connected components. One of these components is bounded and the other is unbounded, and the curve C is the boundary of each component.

This statement is easy to motivate by appeal to a pictorial model: nobody bothered to offer a deductive argument for (18) until the late nineteenth century, presumably because it seemed unnecessary to give a deductive argument for a statement so obvious. Even so, it is reasonable to worry that, despite its purported obviousness, there might be a 'monstrous'[28] counterexample. And so we value deductive arguments for (18) with premises that have still more powerful intuitive support.

Our judgments about which statements can be properly justified by appeal to pictorial models are context-dependent. For example, in some contexts we may be sufficiently confident in our geometric intuition that we are willing to accept all Hilbert's geometric axioms on the basis of intuition alone; in another context (perhaps a context in which the reliability of geometric intuition has been challenged) we may seek to demonstrate the coherence of Hilbert's axioms by providing a set-theoretic model.

The kitchen sinker also has the resources to account for the special role that axioms play in mathematics. Axiomatization, she may say, is a strategy that mathematicians use to ensure that the statements they accept are coherent. The strategy, briefly, is this. First mathematicians survey the statements they accept in some branch of mathematics, and bestow upon some of them the title 'axiom'. A 'proof' for a statement *S* in the relevant branch of mathematics is then a deductive argument whose conclusion is *S* and whose premises are all either axioms or statements for which a proof has already been found. It is agreed within the community that mathematicians will not be expected to produce justifications for axioms (except perhaps during periods of philosophical reflection), and that the canonical way of justifying other statements is by giving a proof. All other arguments are to be regarded as less than fully satisfactory. By *The Coherence/Entailment Principle,*

[28] See Feferman (2000) for an illuminating discussion of 'monsters'.

as long as the axioms that are chosen are coherent, the statements which have been proved will also be coherent.

When choosing axioms, mathematicians must take account of several desiderata. First, the mathematicians should be confident that the axioms they choose are coherent. Second, the axioms must be sufficiently strong: at least, the axioms must be strong enough to entail all those statements in the relevant branch of mathematics that are already generally accepted. Third, mathematicians prefer axioms which are easy to motivate using intuition; as I mentioned above, it is most natural in mathematics to deduce less intuitive statements from more intuitive ones. In principle, mathematicians could use Fermat's Last 'Theorem' as an axiom, but the proofs in the resulting system would be, to say the least, peculiar. This does not imply that it will not sometimes be appropriate, when the axioms are challenged, to defend them using deductive arguments.[29]

In the last section I motivated restrictive theories using contrasting pairs of statements:

(13a) If A and C are two points of a line, then there exists at least one point C between A and C.
(13b) A regular heptadecagon can be constructed with straight edge and compasses.

(14a) Every natural number has a successor.
(14b) There is no largest prime number.

(15a) The series a_0, a_1, a_2, a_3 converges to l just in case for any $\varepsilon>0$ there exists some N such that for all $n \geq N$, $|a_n - l|<\varepsilon$.
(15b) $(\pi/4) = 1 - (1/3) + (1/5) - (1/7) + (1/9) - (1/11) + \ldots$

It is not difficult for the kitchen sinker to explain the differences between the 'a' statements and the 'b' statements. (15a) is a stipulative explicit definition, and we've already seen that the kitchen sinker can account for the special epistemic status of stipulative explicit definitions. (13a) and (14a) are, by stipulation, axioms, and I've already suggested an account of the epistemic role of

[29] … or, indeed non-deductive arguments. Zermelo (1908) contains, in effect, a non-deductive argument for the axiom of choice, variants of which are still commonly used today. I believe that it is possible for the kitchen sinker to give an account of such arguments, but I won't attempt this important task here.

axiomatization on behalf of the kitchen sinker. (13a) and (14a) are highly intuitive, while (13b) and (14b) are not. (15a) is perhaps not as intuitive as (13a) and (14a), but it is more intuitive that (15b).

It might be suggested that in saying that (13a), (14a) and (15a) are 'intuitive' while (13b), (14b) and (15b) are not, I am reintroducing the platitude/non-platitude distinction under a new name. But this is a mistake. According to the kitchen sink theory, statements may be arranged along a *scale,* with the more intuitive statements at one end and the less intuitive statements at the other. This is importantly different to a *categorical* distinction between platitudes and non-platitudes. From the point of view of the kitchen sinker, restrictive theories of platitude arise when one replaces the gradable notion of intuitiveness with a categorical distinction, and then confuses that distinction with the three-fold distinction between axioms, stipulative explicit definitions, and other statements.

8. Conclusion

I don't pretend to have definitive argument in favour of the kitchen sink theory over more restrictive theories. Perhaps further work will uncover a well-motivated restrictive theory, or a more powerful argument for the kitchen sink theory. The Canberran proposal needs work in several other respects, too. For one, I have had only a very little to say about the role of non-deductive arguments in mathematics. And I have had nothing at all to say about the mathematical knowledge of non-experts, or about applied mathematics. So more work is needed.

But I do hope to have convinced you that the history problem is not fatal to the postulationist position: the postulationist may say, with some confidence, that some version of the Canberran theory will explain how mathematical knowledge is possible in the absence of articulated axioms and definitions.

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