ON NATURALIZING THE EPISTEMOLOGY OF MATHEMATICS

BY

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Abstract: In this paper, I consider an argument for the claim that any satisfactory epistemology of mathematics will violate core tenets of naturalism, i.e. that mathematics cannot be naturalized. I find little reason for optimism that the argument can be effectively answered.

Introduction

It's fashionable in philosophical circles these days to be a naturalist about pretty much everything, at least so it seems. This isn't exactly a new fashion. The 20th century saw movements to naturalize ethics,[1] mind,[2] epistemology, and, along with it, metaphysics.[3] Over the last few decades, there has been a particular push to extend the project of naturalizing everything in sight to mathematics.[4] The success of attempts to naturalize the epistemology of mathematics[5] has been challenged,[6] but in this paper I'm less concerned with the success or failure of any one specific attempt to naturalize mathematics than with the feasibility of the project itself. In particular, I'll examine an argument for the claim that any epistemology of mathematics violates core tenets of naturalism, i.e. that mathematics cannot be naturalized. I set out this argument in §2 and defend its key premise in §3 before concluding with brief remarks in §4. I begin with some preliminaries about naturalism.

1. Preliminaries

The argument I'll consider depends on two background assumptions regarding naturalism. They are:

Pacific Philosophical Quarterly 90 (2009) 63–97

(BA1) A naturalistic epistemology requires a naturalistic metaphysics.
(BA2) A naturalistic epistemology is anti-revisionary.

I call (BA1) and (BA2) 'background assumptions' because I don't state them as premises of the argument (hence 'background'), though one might do so, and I don't here defend them (hence 'assumption') so much as explicate them, though I believe each of the assumptions admits a full defense. If one accepts (BA1) and (BA2), then to the extent that one finds my arguments in §3 convincing one should accept that the project of naturalizing mathematics is fundamentally flawed.

1.1. ON (BA1)

(BA1) strikes me as a near truism of naturalistic epistemology. For definiteness, let's say that a metaphysics is naturalistic just in case the entities (objects, properties, relations, states of affairs, events, etc.) It countenances fall within our scientific worldview.[7] Then (BA1) says that naturalistic epistemology is constrained by the metaphysical allowances of our best science. But since a central aim of naturalistic epistemology is to answer traditional epistemological questions using the resources of our best science, it's not surprising that the metaphysics deployed in answering those questions is restricted to that of our best science.

Naturalistic epistemology is sometimes taken to be any purely descriptive epistemology, i.e. any epistemology that is concerned only with how we in fact acquire those beliefs we take ourselves to be justified in holding, rather than with, additionally, how we ought to acquire beliefs so as to have beliefs we would take ourselves to be justified in holding.[8] On this conception of naturalistic epistemology, Barry Stroud has noted that 'even supernaturalists like Plantinga and Descartes, Locke, Berkeley, and others would still count as "naturalized epistemologists"' (Stroud, 1996, p. 45). Here a supernaturalist is one who '[invokes] an agent or force which somehow stands outside the familiar natural world and so whose doings cannot be understood as part of it' (Stroud, 1996, p. 44). Supernaturalism is opposed to metaphysical naturalism, and so naturalistic epistemologists in the sense that Plantinga *et al.* might be such are not committed to a naturalistic metaphysics, contra (BA1). But, as Stroud also suggests, this sort of naturalistic epistemology is insubstantial: 'Some determinate conception of what the natural world is like is needed to give substance to the claim that one's epistemology . . . is naturalistic' (Stroud, 1996, p. 45). Once the bounds of the natural world are demarcated as above, rough and ready though that demarcation may be, supernaturalism is no longer compatible with naturalistic epistemology. And something along the lines of the demarcation of the bounds of the natural world given above is surely correct from the viewpoint of naturalistic epistemology, given its chief motivation, to wit, to account for our knowledge of the world without invoking mysterious entities or faculties (e.g. Deities, incorporeal substance, or Gödelian intuition).

1.2. ON (BA2)

(BA2) appears more than a mere truism. Epistemological naturalism has it that philosophy – and epistemology, in particular – must be consonant with science, both metaphysically and methodologically. Roughly, the metaphysics countenanced by a philosophical theory must not outrun what's acceptable to the sciences (broadly construed to include the social as well as natural sciences) – e.g. no supernatural entities allowed. And the methods of philosophy must be or be relevantly similar to those of the sciences – e.g. no crystal balls allowed. The metaphysical consonance of epistemology with science underwrites (BA1). The methodological consonance of epistemology with science ensures that whatever changes or revisions take place in science take place on scientific grounds, or at least not on distinctly philosophical grounds.[9] On a traditional Quinean understanding of epistemological naturalism, grounds are distinctly philosophical just in case they are *a priori*. Traditional Quinean naturalism holds that Cartesian foundationalist epistemology is mistaken precisely in its commitment to an *a priori* epistemological standpoint: nothing is *a priori*, so there is no standpoint outside of or epistemically prior to the sciences, broadly construed.[10] *A fortiori*, there is no standpoint outside of science from which it's legitimate to recommend changes in (i.e. revisions to) science. Thus, what's at issue is the integrity of the sciences – the sciences are to be respected and protected against intervention from the outside.

When the players are science, broadly construed, and philosophy in the tradition of Descartes, i.e. first philosophy, this inside-outside distinction is fairly straightforward, even if rough and ready: whatever is or would be *a priori* (and, so, part of first philosophy) is outside; all else is inside. But when the players are mathematics and the non-mathematical sciences, as is the case when the discussion concerns naturalizing the epistemology of mathematics rather than epistemology generally, the inside-outside distinction becomes more problematic. This even if 'non-mathematical science' is construed broadly enough to include naturalized philosophy (or scientific philosophy, as naturalized philosophy is sometimes called). For there is no obvious, established (even if perhaps incompletely understood) distinction like the *a priori–a posteriori* distinction waiting to be pressed into service. How, then, to draw a reasonable inside-outside distinction for the case at hand, that of naturalizing the epistemology of mathematics?

In the interest of staying true to the spirit of naturalism in the Quinean tradition, we try to answer this question in keeping with that tradition. We can identify two principles underpinning the inside–outside distinction in the case of science and first philosophy. One of these came out in the above discussion, viz., the principle that only empirical methods are legitimate. Quine sometimes refers to this as *methodological monism*.[11] The second I'll call the *principle of integrity*:

(PI) A recommendation to revise a practice *P* is legitimate if and only if the outcome of the revision is of antecedent concern to *P*.

The idea is that a practice should be protected from interference motivated by concerns it does not share, as judged according to the results of the interference. Thus, if biology unwittingly posits a mechanism that violates some law of physics, physics may legitimately advise biology to reject this mechanism and look for a replacement because among the concerns of biology is a desire to respect the laws of physics. Similarly, since the sciences are not in the business of accounting for experience in supernatural terms, the recommendation to supplant the theory of evolution with intelligent design is illegitimate and so rightfully rejected by biology.

Something very much like (PI) is explicitly recognized by Quine and other naturalists. For instance, Quine writes that 'naturalism . . . sees natural science as an inquiry into reality, fallible and corrigible but *not answerable to any supra-scientific tribunal*' (1975, p. 72, emphasis added) and Penelope Maddy, self-consciously modeling her approach to mathematics on Quine's approach to the sciences, writes:

> To judge mathematical methods from any vantage-point outside of mathematics, say from the vantage-point of physics, seems to me to run counter to the fundamental spirit that underlies all naturalism: the conviction that a successful enterprise, be it science or mathematics, should be understood and evaluated on its own terms, that such an enterprise should not be subject to criticism from, and does not stand in need of support from, some external, supposedly higher point of view. . . . mathematics isn't answerable to any extra-mathematical tribunal. . . . (Maddy, 1997, p. 184)

I have argued elsewhere that Maddy takes this view too far. In brief: To the extent that Maddy offers a genuine epistemology of mathematics, she holds that only epistemological norms internal to mathematics are relevant to the epistemic status of mathematical claims. Thus she understands naturalizing the epistemology of a practice *P* as showing that the epistemic norms of *P* are defensible according to those same norms. (So, e.g., a defense of the epistemic efficacy of proof in mathematics in terms of God's will would not be part of a naturalized epistemology of mathematics.) This way of viewing naturalism has a number of problems.[12] These problems are not shared by naturalized epistemology in the Quinean tradition, owing to the disciplinary holism embraced by that tradition:

naturalizing epistemology 'proceeds in disregard of disciplinary boundaries but with respect for the disciplines themselves and appetite for their input' (Quine, 1995, p. 16).[13] Naturalizing epistemology in the Quinean tradition is showing that epistemic norms are defensible according to the norms of science (broadly construed). In general, naturalizing the epistemology of a practice *P* in the Quinean tradition is showing that the epistemic norms of *P* are defensible according to the norms of science. This is the sense of naturalizing the epistemology of mathematics with which I'm concerned in this paper. Intuitively, we can think of this as bringing (the epistemology of) *P* under the umbrella of science (broadly construed). Despite Maddy's problems, the basic thrust of her passage (and Quine's above) is sound: naturalism by default accords successful practices an integrity which is inviolate from without. (PI) adds to this thrust that what is external to a practice is a matter of the concerns of the practice.

One might wonder whether Quine would accept (PI) in such an apparently unrestricted form as I've stated it. The worry is that while he might admit (PI) for natural science, he might be inclined to reject it for practices outside natural science. After all, isn't this just where Maddy goes wrong (according to the above remarks), by applying (PI) too broadly? The thing to observe about this worry is that it ignores how broadly Quine construes science. Quine understands science to include social sciences such as economics and history as well as natural sciences such as physics and biology.[14] Indeed, for Quine, 'nearly any body of knowledge sufficiently organized to exhibit appropriate evidential relationships among its constituent claims has at least some call to be seen as scientific' (Quine and Ullian, 1978, p. 3). But an understanding of science this broad legitimizes, for Quine, application of (PI) to any practice manifesting the relevant evidential organization, including practices outside the natural sciences. For present purposes we can rely on our reflective judgments as to which evidential relationships are appropriate (on which, I take it, there is fairly broad consensus). The real trick is to say how practices manifesting the relevant evidential organization relate to one another, how insulated they are from one another. I think Maddy goes off track in making them too insulated; (PI) (and its sister (PI*) below) codifies my attempt to specify a limit to how insulated practices can be that avoids Maddy's excessive insularity while keeping with naturalism.

Methodological monism isn't obviously likely to be of much use in formulating an inside–outside distinction appropriate to naturalizing the epistemology of mathematics. However, (PI) shows promise. To make good on this promise we need at least a rough idea of what separates three types of concerns: (i) concerns exclusive to mathematics, (ii) concerns exclusive to non-mathematical sciences, and (iii) concerns common to mathematics and non-mathematical sciences.

Examples of type-(i) concerns are easy to come by. The continuum hypothesis (CH), Gödel's axiom of constructibility ($V = L$), and the existence of regular limit cardinals other than ω (otherwise known as weakly inaccessible cardinals), are all concerns of contemporary set theory that hold no interest for the non-mathematical sciences. Arguably, much less esoteric mathematical concerns, e.g., to do with the structure of the reals or the distribution of primes, are also of type (i).[15] Similarly, examples of type-(ii) concerns are ready to hand. Physics is concerned with (among other things) interactions between various forces, psychology is concerned with (among other things) the effects of environmental factors on our cognitive processing, and economics is concerned with (among other things) the interaction of wages and unemployment. None of these are among the concerns of mathematics.[16]

Notice that I'm not claiming that mathematicians may not be concerned with type-(ii) issues; neither am I claiming that physicists, biologists, psychologists, economists, etc. may not be concerned with type-(i) issues. Mathematicians may well be concerned with type-(ii) issues, even professionally. There is undoubtedly interesting mathematics associated with type-(ii) issues, and such issues can be a source of research questions for mathematicians. However, if some mathematics is developed for application to a type-(ii) issue and ultimately turns out to be ill-suited to that application, the mathematics need not be abandoned *qua* mathematics. At most, it must be abandoned *qua* mathematics appropriate for a certain application. Euclidean geometry provides a prime example of this. Analogous remarks apply to the potential interest of non-mathematical scientists in type-(i) issues.

Someone might object that mathematics is sometimes more tightly tied to non-mathematics than I allow. For example, one might think that the history of probability theory shows that sometimes the mathematics itself, *qua* mathematics, is rejected subsequent to failed application.[17] When 17th-century probability theory (such as it was) was applied to the issuance of annuities in the Netherlands, the results were disastrous for the insurers (the Dutch state) and consequently, one might argue, probability theory *qua* mathematics was significantly repudiated.[18] This seems to me mistaken. First, correcting the 17th-century practice of issuing annuities required a more accurate mortality table, a more accurate assignment of probabilities of dying over various time (age) intervals.[19] But such assignments are arguably not part of the mathematics of probability any more than the orbital velocities of planets are part of the mathematics of celestial mechanics. In both cases, we have physical phenomena that are describable or representable by mathematical means. But we should not confuse those phenomena with their descriptions or representations. Second, probability theory at the time in question was far from a mature theory. Indeed, episodes such as the indicated annuities failure provided the impetus for developing probability theory. Certainly an applied theory in its infancy developing in response to non-mathematical stimuli does not constitute a repudiation of the theory.

As to type-(iii) concerns, even though the concerns of mathematics and non-mathematical science are largely disjoint the class of type-(iii) concerns isn't empty. For instance, mathematics and non-mathematical science both are concerned to respect various logical laws as well as theorems of arithmetic and analysis. These are, in a sense, negative concerns – concerns not to violate certain constraints on theorizing. There are also positive concerns of type (iii). I have in mind here the aims and interests of those fields where applied mathematics meets non-mathematical sciences, e.g. celestial mechanics, econometrics, and string theory – branches of mathematics that are aimed at developing mathematics for specific applications and the relevant branches of non-mathematical science where that mathematics gets applied.

Given a mathematical or non-mathematical scientific practice *P*, say that the concerns of *P* are *P*-type. Then in light of the foregoing discussion, set theory-type concerns are of type (i), cell biology-type concerns are of type (ii), and economic game theory-type concerns are of type (iii). We can recast (PI) in terms of *P*-type concerns as follows.

(PI*) A recommendation to revise a practice *P* is legitimate if and only if the outcome of the revision addresses a *P*-type concern.

An epistemology is anti-revisionary in the sense of (BA2) just in case it respects (PI*), countenancing only revisions which are legitimate according to (PI*).

1.3. ON (BA1) *AND* (BA2)

Though it will likely be granted that both (BA1) and (BA2) can figure in a naturalistic epistemology, separately or together, one might balk at the idea that both must figure in a naturalistic epistemology. After all, 'naturalism' is notorious for its apparent lack of univocality in philosophical parlance. In particular, one might question the necessity of (BA2) for a naturalistic epistemology. In the previous section, I linked (BA2) directly to Quine's naturalism and suggested that it was a central component of, not merely incidental to, that naturalism. I take it that the source of naturalism in mathematics is naturalism in the Quinean tradition, and the motivation for naturalizing mathematics is (largely) the perceived success of naturalism in the Quinean tradition. Thus, I consider the centrality of (BA2) to Quine's naturalism strong evidence for the centrality of (BA2) to any view legitimately purporting to naturalize the epistemology of mathematics.

That prominent proponents of naturalized mathematics self-consciously take Quine's naturalism as their model and springboard lends support to this position. I already noted the connection of Maddy's attempt to naturalize mathematics to Quine's naturalism. Similar connections to Quine's naturalism are found in the naturalistic views of mathematics advanced by, e.g., Alan Baker, John Burgess, Mark Colyvan, and Michael Resnik.[20] Both Baker and Burgess make remarks that show the sensitivity of their positions to (BA2). Baker notes the 'insight that – given the naturalistic basis of the Indispensability Argument, which rejects the idea of philosophy as a higher court of appeal for scientific judgments – the only sensible way of judging alternatives to current science is on scientific grounds' (Baker, 2001, p. 87). Burgess opposes naturalized epistemology to 'the traditional *alienated* conception of epistemology, on which the epistemologist remains a foreigner to the scientific community, seeking to evaluate its methods and standards – a conception that presupposes other methods and standards of evaluation, outside and above and beyond those of science' (Burgess and Rosen, 1997, p. 33, original emphasis). Both of these cases display the constitutive antipathy of epistemological naturalism to outside interference in science, an antipathy which I contend must be shared by any attempt to naturalize the epistemology of a practice exhibiting the right sorts of evidential structure in order for the resulting epistemology to count as naturalistic in the Quinean tradition. This antipathy and its scope are precisely what (PI*) is supposed to codify and attenuate.

Of course, one might still resist accepting (BA2) as necessary for a naturalized epistemology. In this case, one should take this paper as targeted at any version of naturalism which endorses both (BA1) and (BA2), noting that this covers much of the work on naturalizing mathematics presently being done, including that of many leading philosophers of mathematics.

2. *The argument*

The argument against naturalizing the epistemology of mathematics I have in mind runs as follows:

(1) An epistemology of mathematics ratifies our acceptance of pure mathematics as justified only if it either countenances a non-naturalistic metaphysics or is revisionary.
(2) An epistemology of mathematics does not ratify our acceptance of pure mathematics as justified only if it's revisionary.
(3) Hence, any epistemology of mathematics either countenances a non-naturalistic metaphysics or is revisionary. (from (1) and (2))
(4) Therefore, no epistemology of mathematics is naturalistic. (from (3), (BA1), and (BA2))

As we're taking (BA1) and (BA2) for granted, the only potential weak spots in the argument are (1) and (2). A little thought shows that (2) isn't a problem. If an epistemology fails to ratify our acceptance of pure mathematics as justified, then according to that epistemology we should not accept some significant portion of pure mathematics.[21] That is to call for a revision of mathematics, a revision motivated by epistemic rather than pure mathematical concerns and with epistemic rather than pure mathematical payoffs. Recommending such a revision is illegitimate according to (PI*). Thus, an epistemology recommending such a revision is revisionary in the sense of (2). So the real work in defending this argument comes in defending (1).

In what follows I defend (1) by categorizing philosophies of mathematics according to how they account for mathematical truth and considering the leading views from each category. The idea is to try to find an independently plausible view that falsifies (1), i.e. that provides an epistemology of mathematics which is neither revisionary nor non-naturalistic in its metaphysics. Failing to find such a view doesn't constitute a knock-down argument for (1); however, considering the pool from which candidate views are drawn, it does strongly tell in favor of it.

3. *In defense of (1)*

Suppose we have an epistemology E that ratifies our acceptance of pure mathematics as justified. In particular, suppose that according to E's account of mathematical justification our beliefs concerning pure mathematics which we typically take to be justified do count as justified. The notion of justification endorsed by E must be truth directed; i.e. it must be such that beliefs justified according to that notion tend to be true. This is a near truism of general epistemology. What makes a conception C of justification a conception of *epistemic* justification is at least in large part that beliefs which are justified according to C tend to be true, i.e. that there is some sort of systematic connection between beliefs justified according to C and what is actually the case. Moreover, endorsing the truth-directedness of epistemic justification isn't to countenance reliabilism. Rather it's to recognize a widely accepted conviction that an epistemic notion of justification must be systematically connected to truth, i.e. truth-conducive.

Something like the conviction that epistemic justification is truth-directed undoubtedly underwrites the significance we attribute to Gettier cases. Such cases show us that our intuitive notion of justification lacks a systematic connection to truth; it's possible to be intuitively justified in believing that p, for p to be true, and yet to have the basis of our justification for believing that p disconnected from the basis of p's truth, so that in consequence we don't actually know that p. At the very least, this conviction is widely held in general epistemology. Reliabilist support for the truth-directedness of justification is well known and should be obvious. If one thinks reliability is both necessary and sufficient for justification, then one thinks that justification is truth directed. This is, after all, what it means for reliability to be necessary for justification. But the truth-directedness of justification is also endorsed by philosophers who reject a reliabilist, and indeed any externalist, conception of justification. Laurence BonJour, for example, has maintained that precisely what distinguishes epistemic justification from other sorts of justification (e.g. moral or pragmatic justification) is its connection to truth, even as he has shifted from advocating internalist coherentism to advocating internalist foundationalism.[22]

This raises the question of mathematical truth. More to the point, it raises the question of truthmakers for mathematics – i.e. that in virtue of which mathematical beliefs (statements, etc.) have the truth values they do – whatever truthmakers happen to be like.[23] Let *alethic realism* be the view that mathematical truthmakers, and hence truth values of mathematical beliefs, are independent of our minds, language, and activities. (Shapiro (2000b) labels alethic realism *realism in truth value*.) Let *alethic idealism* be the negation of alethic realism, so that according to alethic idealism mathematical truthmakers, and hence truth values for mathematical beliefs, are in some fashion dependent on our minds, language, or activities. There are different ways to be an alethic realist. One might be a platonist structuralist,[24] a Fregean logicist,[25] or a good old-fashioned object platonist.[26] One might even be a modal structuralist,[27] so long as the modality involved isn't cashed out in such a way that modal facts depend on linguistic, mental, or behavioral facts. There are also different ways to be an alethic idealist. One might be an intuitionist,[28] a logicist in the logical empiricist tradition,[29] or a formalist.[30] Notice, though, that the alethic realist–idealist distinction cuts across the usual realism–anti-realism distinction. Fieldian fictionalism, for instance, is an anti-realist position, since according to that view there are no mathematical entities, and also an alethic realist position, since that there are no mathematical entities doesn't depend on our minds, language, or activities. It follows, according to Field, that the truth values of many mathematical beliefs, though quite different from what we ordinarily think they are, are nonetheless what they are for reasons independent of us.[31] Clearly any view concerning mathematical truthmakers, any view of mathematical truth, will be either a version of alethic realism or a version of alethic idealism.

3.1. NATURALISM AND ALETHIC IDEALISM

Suppose that one holds a version of alethic idealism. In particular, suppose that one is an alethic idealist by being an intuitionist.[32] One who is an alethic idealist by being an intuitionist is a revisionist with respect to mathematics. As is well known, intuitionist mathematics rejects portions of classical mathematics. For instance, in intuitionist mathematics every function on the reals is continuous. So intuitionists must reject the classical theorem that there are discontinuous functions on the reals.[33] Does intuitionistic revisionism respect (PI*)?

There are two cases to consider: intuitionist mathematics can be viewed either as a rival to and potential replacement for classical mathematics or as a discipline which, appearances notwithstanding, has a different subject matter from that of classical mathematics, and so isn't in conflict with it and should not incite changes in classical mathematics. The former view (call it *rival intuitionism*) is held by Brouwer and Dummett, though for different reasons; the latter (call it *tolerant intuitionism*) is held by Heyting.[34] Intuitionist mathematics taken as a rival to classical mathematics clearly violates (PI*). Consider, for instance, the rival-intuitionistic recommendation to reject that there are discontinuous functions on the reals. If accepted, this recommendation would bring a deeper revision to classical mathematics than a simple removal of a heretofore accepted theorem. An explanation, some rational support, for removing the theorem would be required, and such an explanation would come as a story either about the nature of mathematical existence (in the case of Brouwer) or about language acquisition and assertability conditions for language generally (in the case of Dummett). Each of these would be an outcome of the recommended revision, but classical mathematics is antecedently concerned with neither. The question of the nature of mathematical existence isn't a type-(i) or a type-(iii) concern. Similarly for questions about language acquisition and assertability conditions. So one cannot be a naturalist about mathematics by being a rival intuitionist.

What about tolerant intuitionism? Could one be a naturalist about mathematics by being a tolerant intuitionist? While the answer to this question may be of independent interest, it has no significant bearing on our project. A negative answer would be no help to the would-be mathematical naturalist. An affirmative answer would only help in the current situation if 'mathematics' denotes classical mathematics. But how could tolerant intuitionism yield naturalism about classical mathematics? Tolerant intuitionism isn't about the same thing as classical mathematics (whatever that happens to be). So truthmakers for tolerant intuitionistic mathematics are independent of truthmakers for classical mathematics. This is why tolerant-intuitionistic mathematics and classical mathematics are not rivals. Thus, tolerant intuitionism can't yield a naturalistic account of classical mathematics; tolerant intuitionism is simply beside the point as far as naturalizing (classical) mathematics.

3.2. NATURALISM AND ALETHIC REALISM

Suppose, on the other hand, that one holds a version of alethic realism. I'll consider object-platonist, platonist-structuralist, Fregean-logicist, and modal-structuralist variants of alethic realism in turn.

3.2.1. Object platonism

We, of course, have one particularly prominent example of a naturalist who endorses object platonism, viz., Quine.[35] Indispensability considerations lead Quine to conclude that we're ontologically committed to a range of abstract, mathematical objects: numbers, functions, sets, etc. This much is well known. Perhaps less well known is Quine's attitude about just how much of pure mathematics the indispensability argument yields. Quine (1998b) indicates that a great deal of pure mathematics has only recreational value:

> Pure mathematics, in my view, is firmly embedded as an integral part of our system of the world. Thus my view of pure mathematics is oriented strictly to application in empirical science. Parsons has remarked, against this attitude, that pure mathematics extravagantly exceeds the needs of application. It does indeed, but I see these excesses as a simplistic matter of rounding out. . . . I recognize indenumerable infinities only because they are forced on me by the simplest systematizations of more welcome matters. Magnitudes in excess of such demands, e.g., $\beth_\omega$ or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights.[36] (p. 400)

The upshot is that mathematics that goes beyond what's needed for applications in the empirical sciences also goes beyond the reach of the indispensability argument. Consequently, for Quine much of pure of mathematics isn't even truth apt, since for an object platonist truth aptness depends on ontological standing and for Quine much of pure mathematics has no ontological standing. Moreover, there is reason to think that the upper limit of truth aptness on this view is actually quite small. It is widely held that the empirical sciences need no more mathematics than functional analysis, which requires at most entities found in $V_{\omega+\omega}$.[37] As it happens the cardinality of $V_{\omega+\omega}$ is $\beth_\omega$, to which Quine denies ontological standing. So we have here a case that, on Quine's view, mathematical claims concerning entities of rank[38] greater than $\omega+\omega$ fail to be truth apt. But truth aptness is a precondition for knowledge. So on Quine's view much of pure mathematics is excluded from our pool of mathematical knowledge.

This is revisionary, as it fails to respect (PI*). Just as with rival intuitionism, Quine must explain why the set theory that he would leave out of the pool of mathematical knowledge should be left out, and that explanation is going to proceed in terms of concerns alien to set theory.

Those concerns, having to do with explaining and predicting experience, will be of type (ii) and (maybe) type (iii). But they will not be of type (i), so they will not be set theory-type concerns. Hence, Quine might be a naturalist and his naturalism might extend to a portion of pure mathematics, but he's not a naturalist about mathematics.

One way of thinking about what's going on here is by considering how the indispensability argument succeeds (let's suppose) in naturalizing some of mathematics and how it fails to naturalize all of mathematics. The indispensability argument relies on the Quine–Duhem thesis, according to which confirmation accrues only to relatively large bodies of theory and whatever is required by a body of theory that receives confirmation shares in that confirmation. Since certain parts of mathematics are required for doing science, and science is well confirmed, so are certain parts of mathematics. The indispensability argument succeeds in confirming only those parts of mathematics required by empirical science; for definiteness let's fix this at those parts of mathematics concerning entities of rank no greater than $\omega+\omega$. The naturalistic credentials of this success appear to be underwritten by an empiricist conviction that perceptual experience is the ultimate source of confirmation. Naturalistic epistemology aims to account for our knowledge in terms of naturally explicable facts and faculties, facts and faculties studied by or otherwise compatible with the empirical sciences, broadly construed to include the social sciences. The final arbiter of the empirical sciences is perceptual experience. So naturalistic justification (confirmation) at bottom rests on perceptual experience. Naturalistic justification is in some sense a matter of there being a sufficiently strong connection to perceptual experience. The empirical sciences are directly confirmed because they directly confront, explain, and predict experience. The parts of mathematics confirmed by the indispensability argument are indirectly confirmed by their (direct) role in the empirical sciences. The indispensability argument fails to confirm those parts of mathematics concerning entities of rank greater than $\omega+\omega$ because those parts of mathematics *prima facie* fail to be sufficiently strongly connected to experience.

This suggests that the shortcoming of a Quinean approach as a strategy for naturalizing mathematics as a whole might be overcome by arguing for a sufficiently strong connection between those parts of mathematics concerning entities of rank greater than $\omega+\omega$ and experience. Colyvan (2001) gestures at such an argument:

> As for the charge that the [Quinean] indispensability argument leaves too much mathematics unaccounted for (i.e. any mathematics that does not find its way into empirical science), this seems to misrepresent the amount of mathematics that has directly or indirectly found its way into empirical science. On a holistic view of science, even the most abstract reach of mathematics is applicable to empirical science so long as it has applications in some further branch

of mathematics, which may in turn have applications in some further branch until eventually one of these find applications in empirical science. Indeed, once put this way it's hard to imagine what part of mathematics could possibly be unapplied. (p. 107)

Call a part of mathematics that concerns entities of rank up to but not exceeding α *mathematics of rank* α. Here, a part of mathematics P 'concerning' an entity e is understood to mean that e is in the domain of quantification on the standard interpretation of the language of P. For present purposes, I identify mathematical entities with their canonical set-theoretic surrogates. So, for example, the natural numbers are identified with finite von Neumann ordinals and arithmetic is of rank ω. Then the argument suggested in this passage runs as follows.

(1′) For any parts of mathematics P_1^α, P_2^β, P_3^γ of rank α, β, and γ, respectively: if P_1^α has indispensable applications in P_2^β and P_2^β has indispensable applications in P_3^γ, then P_1^α has indispensable applications in P_3^γ.
(2′) Any part of mathematics that has indispensable applications in mathematics of rank $\omega+\omega$ through a chain of indispensable applications as licensed by (1′) has indispensable applications in empirical science.
(3′) It's likely that for every α there is a part of mathematics of rank α that has indispensable applications in mathematics of rank $\omega+\omega$ through a chain of indispensable applications as licensed by (1′).
(4′) So, it's likely that for every α there is a part of mathematics of rank α that has indispensable applications in empirical science.
(5′) Any mathematics that has indispensable applications in empirical science shares in the confirmation of empirical science.
(6′) Therefore, it's likely that mathematics of every rank shares in the confirmation of empirical science.

Call the sense in which a part of mathematics of rank greater than $\omega+\omega$ is indispensable to empirical science in virtue of its participation in a chain of indispensable applications culminating in an indispensable application to a part of mathematics of rank $\omega+\omega$ *extended indispensability*, and call the above argument the *extended indispensability argument*. If (6′) is correct, then the extended indispensability argument arguably justifies us in believing that there are mathematical entities of rank α, for every ordinal α, thus clearing the way for truth aptness for mathematics in its entirety. At least this is the hope of an extended indispensability theorist hoping to naturalize mathematics.

There are problems with the extended indispensability argument. I focus on the most serious.[39] The most reasonable way to understand one part of mathematics having 'applications in' another is in terms of one part of mathematics being used in proving results in another part of mathematics.[40] But if we understand one part of mathematics having applications in another part of mathematics proof theoretically in this way, then work on predicativist mathematics by Solomon Feferman and others[41] shows that (3′) is quite likely false.[42]

The predicativist program pursued by Feferman *et al.* began in earnest with Weyl (1918/1984), where Weyl aimed to develop analysis taking as given only: the system of natural numbers (ω, 0, Sc), where 'Sc' denotes the the successor relation; arithmetical subsets of ω (i.e. subsets of ω definable from a formula all quantifiers of which range over natural numbers); and notions of inductive definition and proof.[43] Feferman has produced a system W formalizing Weyl's system, the details of which need not detain us.[44] For present purposes, two features of W are important.

(W1) W is proof-theoretically reducible to and a conservative extension of Dedekind–Peano Arithmetic (DPA), which is to say, for any sentence σ in the language of arithmetic: (i) DPA proves that any proof of σ in W can be effectively transformed into a proof of σ in DPA and (ii) σ is provable in W only if it's already provable in DPA.

(W2) W suffices for all mathematics, which is indispensable to our best science.

Since (W1) and (W2) yield that whatever mathematical results science needs can be proved in DPA, these features of W imply that mathematics of rank ω suffices to prove all the results which find indispensable application in science. In other words, the indispensability of mathematics beyond rank ω is merely apparent.[45] This is, of course, considerably less ontologically committed than even the interpretation of the indispensability argument that accords ontological standing only to those entities of rank no greater than $\omega + \omega$. Challenges to (W2) have been raised. Those challenges tend to involve 'questions at the margin' involving 'the possible essential use in physical applications of such objects as nonmeasurable sets or nonseparable spaces, which are not accounted for in W' (Feferman, 1992, p. 297).[46] However, plausible responses to these challenges have been given.[47] In short, the theoretical models in which the problematic sets and spaces arise, as well as the question of applying these models in practice, are highly controversial. Thus, their indispensability is at best an open question.

Some might think that the predicativist program won't bear the weight I've put on it, that, e.g., the responses just noted are inadequate, plausible though they may be. So let's set aside predicativism and the problems it raises for the extended indispensability argument. Colyvan's strategy faces other difficulties.

First, it follows from the Reflection Theorem[48] that no matter how much set theory is needed to prove the results which are indispensable to empirical science in the extended sense, it will be only a fraction of the whole of set theory. An exact statement of the theorem is unnecessary. The key point is that reflection implies:

(R) For any finite list $\phi_1, \phi_2, \ldots, \phi_n$ of axioms of ZFC, there is an ordinal α such that V_α models $\{\phi_1, \phi_2, \ldots, \phi_n\}$.

The amount of set theory which is indispensable to empirical science in the extended sense is limited by (R). To see this, let $S = \{\tau_1, \tau_2, \ldots, \tau_k\}$ be the set of mathematical theorems which are directly indispensable (i.e. indispensable apart from the extended sense) to empirical science. S is finite. At any given time, there are only finitely many statements of empirical science[49] and arguments for those statements use only finitely many mathematical results.[50] Moreover, since each τ_i has a proof from the axioms of set theory, there is a set $AX(S)$ of set-theoretic axioms from which every member of S is provable. But now, since proofs are finite and there are only finitely many τ_i, $AX(S)$ is finite and, by (R), there is an ordinal α such that V_α models $AX(S)$. Thus, only entities up to rank α are required for empirical science, which implies that (4′) is false.[51]

Second, even if the arguments of the preceding two paragraphs fail and the extended indispensability argument is correct, naturalized mathematics does not automatically follow. Being justified in believing that there are mathematical entities of every rank isn't the same as being justified in believing all currently accepted mathematics. For instance, one might be justified in believing that all ordinals exist without being justified in believing that some sets are not ordinals. So the correctness of (6′) is compatible with revisionism, and fairly radical revisionism at that. This is so even given a stronger form of (6′) without the qualifier 'likely'. At a minimum, then, it's dubious that extending the indispensability argument along the lines suggested by Colyvan will work to bring all of mathematics into the naturalistic fold. Moreover, it's not at all clear how else one might naturalize mathematics along Quinean lines.

Colyvan has recently been defending recreational mathematics, by which he means mathematics without (extended) indispensable application in our best science, and the naturalist might hope that the arguments used in this defense would be helpful here. Colyvan's basic idea is that '[m]athematical recreation is an important part of mathematical practice' and that 'like other forms of theoretical investigation, [it] should not be thought of as second class or *mere* recreation' (Colyvan, 2007, p. 116). We needn't worry over the details, because there are at least two reasons independent of them that the naturalist's hope for help would be in vain. First, Colyvan's view in (2007) (call this the *recreational view*) is an extension of the view based on the extended indispensability argument we've been criticizing. So the critique the naturalist would like help with applies equally well to the recreational view. Second, though Colyvan confers ontological rights to much more mathematics on the basis of the extended indispensability argument than Quine does on the basis of the standard indispensability argument, the mathematics which escapes the extended indispensability argument, viz., recreational mathematics, is still without ontological rights. Colyvan '[accepts] that applied mathematics should be treated realistically and with unapplied [i.e. recreational] mathematics we have no reason to treat it this way' (Colyvan, 2007, p. 116). Hence, recreational mathematics is still excluded from the pool of mathematical knowledge, and so the same problem that led to Colyvan's extension of the indispensability argument arises anew: excluding significant parts of pure mathematics in this way violates (PI*). So the recreational view provides no help to the naturalist.

There is another object platonist approach to naturalizing mathematics that deserves attention. On this approach, ordinary scientific standards of theory choice legitimize mathematics.[52] The idea is that theoretical virtues such as simplicity, ontological parsimony, fruitfulness, explanatory power, etc.[53] often deployed in choosing between empirically equivalent scientific theories[54] ratify accepting contemporary mathematics understood platonistically. So, for example, where an indispensability theorist counsels accepting whatever mathematics finds indispensable application in our best science and rejecting the rest (often by appeal to the virtue of ontological parsimony), the *scientific standards approach* counsels accepting indispensably applicable mathematics plus whatever non-applicable mathematics enhances the simplicity, fruitfulness, explanatory power, etc. of our best science, even at the cost of significantly enlarging our ontology. In short, the scientific standards approach uses a more balanced application of theoretical virtues than approaches (like the indispensability approach) that appear to allow ontological parsimony to trump other theoretical virtues.[55]

It's not hard to see why the scientific standards approach might be promising for naturalizing mathematics. If it works as advertised it appears to be more or less unconstrained in the mathematics it can deliver, unlike indispensability arguments (standard or extended). Thus it seems well suited to avoid revisionism, thereby respecting (BA2). And as it delivers mathematics on scientific grounds, it seems a good bet to respect (BA1) by countenancing only naturalistic metaphysics. Whether or not this promise is fulfilled is another question.

Notice that the central claim of the scientific standards approach, viz., that contemporary mathematics is ratified by ordinary standards of science, is ambiguous. If we understand science to include mathematics, then the claim is trivially correct and (BA2) is respected. Set aside potential worries raised by the triviality of the claim on this reading. There remains considerable tension with (BA1).

Contemporary mathematics includes theorems such as:

(L) There is a cardinal λ such that $\lambda = \aleph_\lambda$.[56]

Witnesses to (L) are so large and so far removed from experience (and *pace* Colyvan's extended indispensability argument also from any mathematics used in organizing or explaining experience) that they have led at least one otherwise realist philosopher of mathematics to 'suspect that, however it may have been at the beginning of the [set theory] story, by the time we have come thus far the wheels are spinning and we are no longer listening to a description of anything that is the case' (Boolos, 1998, p. 132). On this reading of the scientific standards approach's central claim, there is no reason to be confident that (BA1) isn't violated. At best, the approach owes us an account of what makes theorems like (L) true which respects (BA1). But this is (largely) what the scientific standards approach was introduced to help with. So including mathematics as part of science in the central claim of the scientific standards approach doesn't get the naturalist anywhere.

If, on the other hand, mathematics is not included as part of science in the central claim of the scientific standards approach, the naturalist still doesn't obviously gain anything. According to Burgess, perhaps the most prominent proponent of the scientific standards approach, scientific standards legitimize mathematical entities on grounds of convenience as well as indispensability, ruling out only mathematics which is gratuitous from the standpoint of our best science (construed now to exclude mathematics).[57] How much of mathematics does convenience for science get us? This is a highly non-trivial question, but it's reasonable to think that in order to avoid violating (BA2) it would need to get us at least a minimal non-artificial model of ZFC (either Gödel's L or V_κ for the least strongly inaccessible κ).[58] I find it dubious that considerations of convenience for non-mathematical science get us so much. But for the sake of argument, let's grant that they do. Then we have:

(C) As a matter of scientific convenience, we are entitled to accept that there is at least a minimal non-artificial model of ZFC.

But (C) itself is problematic. If we're not going to come back around to recreational mathematics, convenience has to carry some real justificatory weight here. It must be the case that being entitled to accept that *p* as a matter of scientific convenience is systematically correlated with *p*'s being true. Otherwise, the scientific standards approach doesn't even provide us an epistemology of mathematics, let alone a naturalistic epistemology of mathematics. However, if convenience carries real justificatory weight in this way, then worries of the kind discussed in connection with witnesses of (L) above are back in play. And, as before, the approach owes us an account of what makes theorems like (L) true which respects (BA1). So again the scientific standards approach seems to have gotten the naturalist nowhere.

There's more to be said concerning the scientific standards approach, but I take it this is sufficient to raise serious questions about its usefulness to the naturalist. This being the case, in the interest of brevity we move on to other forms of alethic realism.[59]

3.2.2. Platonist structuralism

The arguments adduced against the (extended) indispensability theorist in §3.2.1. apply equally well to platonist structuralism. The structures recognized by platonist structuralists – number structures, algebraic structures, and so on – have set surrogates. For instance, the natural number structure is canonically represented by ω. And, of course, these set surrogates have ranks. Given this, the notion of a part of mathematics having a rank applies straightforwardly to mathematics construed along structuralist lines and that suffices to put the above arguments in force.

3.2.3. Fregean logicism

The situation with Fregean logicism, which I understand to encompass Frege's logicism as well as the neo-Fregean logicism most prominently advocated by Hale and Wright,[60] is more complicated. A naturalistic epistemology of mathematics, where mathematics is understood in accordance with Fregean logicism, would arguably run afoul of a widely accepted tenet of naturalism, viz., that nothing is knowable *a priori*. If we resist Quine's view that second-order logic is actually disguised set theory, then mathematical knowledge on a Fregean logicist understanding of mathematics depends on knowing logical or conceptual facts, and such facts are arguably knowable *a priori*. Of course, if we accept Quine's view regarding second-order logic, then we have trouble of a different sort: mathematical knowledge according to Fregean logicism would then depend on set-theoretic knowledge (plus knowledge concerning Hume's Principle, the statement that for all concepts F and G, the number of Fs equals the number of Gs just in case there is a 1–1 correspondence between the Fs and the Gs), i.e. on mathematical knowledge. In addition, whether or not Hume's Principle is *a priori* is one of the chief worries of neo-Fregean logicism, with neo-Fregeans arguing in favor of *apriority*.[61] Let's put the question of naturalism and *a priori* knowability aside. There remain questions with respect to both revisionism and metaphysics.

It will be helpful to have a precise way of representing mathematical practices. Philip Kitcher represents a mathematical practice P by a quintuple $\langle L^P, K^P, Q^P, A^P, V^P\rangle$.[62] Here L^P is the language used by practitioners of P; K^P is the set of statements accepted by the practitioners of P; Q^P is the set of live research questions of interest to practitioners of P; A^P is the set of argument strategies deployed by the practitioners of P to obtain or justify the members of K^P; and V^P is the set of views concerning metamathematical issues of P (proper methods of proof and definition in mathematics, scope of mathematics, relative importance of sub-disciplines of mathematics, and so on). We can think of positive concerns of a practice P as being recorded in Q^P. Negative concerns of P, i.e. constraints on theorizing recognized by practitioners of P, show up in V^P. In the interest of perspicuity, we add to P a component N^P representing such constraints. So we adapt Kitcher's approach and represent a practice P by a sextuple $\langle L^P, K^P, Q^P, A^P, V^P, N^P\rangle$. (Representing practices in this way should not be taken to endorse the view that theories or practices are set-theoretic objects. For one thing, the identity conditions of theories and practices are not extensional. For another, taking theories or practices to be sets would invite a charge of circularity in the case of set theory.) Given this representational apparatus, a violation of (PI*) with respect to a practice P is a recommendation to revise P where the revision contributes neither to answering a member of Q^P nor to satisfying a member of N^P.

Fix P_{Acc} to be the practice of currently accepted mathematics. Paseau (2005) recognizes two types of revisionism: *reconstructive revisionism* and *hermeneutic revisionism*.[63] A reconstructive revision of P_{Acc} is a change in the statements, the axioms and theorems, of P_{Acc}, i.e. in the membership of $K^{P_{Acc}}$. Rival intuitionistic mathematics exemplifies reconstructive revisionism. A hermeneutic revision of P_{Acc} is a change in how statements of P_{Acc} are understood or interpreted (in a non-model-theoretic sense of interpretation). Standard reconstrual strategies for nominalizing mathematics[64] exemplify hermeneutic revisionism. As the conception of practice we're using is more fine grained than Paseau's, we expand the notion of reconstructive revision to cover changes not only to $K^{P_{Acc}}$ but also to any of the other components of P_{Acc} (viz., $L^{P_{Acc}}$, $Q^{P_{Acc}}$, $A^{P_{Acc}}$, $V^{P_{Acc}}$, or $N^{P_{Acc}}$).

A recommendation to reconstructively revise mathematics may or may not be legitimate according to (PI*). As we have seen, rival intuitionism provides an example of an illegitimate reconstructive revisionism. But a recommendation to reconstructively revise might be legitimate according to (PI*). For example, set theory was reconstructively revised when the Axiom of Replacement was added to the axioms of set theory. But this was legitimate according to (PI*), since the outcome of this revision was of antecedent interest to set theory. In short, the payoffs of adopting Replacement (e.g., the provable existence of cardinals $\geq \aleph_\omega$ and a nice theory of ordinal numbers) are set-theoretic payoffs.[65]

On the other hand, any recommendation to hermeneutically revise mathematics will run afoul of (PI*). Hermeneutic revision involves a change in the semantics of mathematical language. Call a semantics for a practice P *thin* if its referential claims are taken at face value by practitioners of P. So, for example, a semantics for P_{Acc} is thin if statements such as '" $\sqrt{-1}$ " refers to i' typically don't elicit attempts to spell out what i 'really is.' Call a semantics for a practice P *thick* if its referential claims are not taken at face value by the practitioners of P, i.e. if questions about the nature of referents of terms occurring in statements of P are seriously entertained by practitioners of P. P_{Acc} pretty clearly contains a thin semantics for its language. Mathematicians take all sorts of referential claims at face value in doing their work. But they just as clearly don't press on to inquire just what numbers, sets, functions, etc. really are. Which is to say that P_{Acc} does not contain a thick semantics for its language. Hermeneutic revision involves a thick semantic change; it concerns what mathematical language really means. Thus any recommendation to hermeneutically revise P_{Acc} is a recommendation to 'thicken' the semantics for the language of P_{Acc}. I submit that such thickening augments the membership of both $Q^{P_{Acc}}$ and $N^{P_{Acc}}$. For instance, a hermeneutic revision according to which arithmetic is really about Fs carries with it a recommendation to take seriously questions such as 'Is analysis also about Fs?' and 'Are any F-facts not arithmetic facts,' which is a recommendation to change the membership of $Q^{P_{Acc}}$. Similarly a hermeneutic revision as described carries with it a recommendation to take seriously constraints on theorizing such as 'Avoid results that are inconsistent with arithmetic being about Fs,' which is a recommendation to change the membership of $N^{P_{Acc}}$. None of these changes to P_{Acc} contributes to answering a member of $Q^{P_{Acc}}$ or satisfying a member of $N^{P_{Acc}}$. Otherwise, P_{Acc} would contain a thick semantics. So recommending these changes violates (PI*). Hence, any hermeneutic revision violates (PI*).

Nearly every case of revisionism we have encountered so far in this paper has been a case of reconstructive revisionism. Frege's logicism, however, is subject to the charge of hermeneutic revisionism.[66] According to Frege's logicism, statements of arithmetic are really statements about classes of concepts. For example, the statement '$1 \neq 0$' for the Fregean logicist means that there is no 1–1 correspondence between the class of concepts equinumerous with the concept «$y = Nx : x \neq x$» and the class of concepts equinumerous with the concept «$x \neq x$» (where '$Nx : \phi(x)$' is read 'the number of x's such that $\phi(x)$'). Thus an epistemology of mathematics based on Frege's logicism violates (PI*) and so is inconsistent with a naturalistic epistemology of mathematics.

Whether or not neo-Fregean logicism similarly falls to the charge of hermeneutic revisionism is less clear. In a certain sense, neo-Fregeanism implements a reconstrual strategy: mathematical claims are taken to be claims of second-order logic augmented with Hume's Principle. But given the tight connection between mathematics and logic – by contemporary lights logic is part of mathematics – one might respond that this is at worst a harmless analysis of parts of mathematics in terms of another part of mathematics. And such an analysis isn't obviously of no antecedent interest to mathematics. One might worry that Hume's Principle isn't part of logic, and so that this response to the charge of hermeneutic revisionism falls short of complete of success. However, the real problem with the response under consideration is that it makes the neo-Fregean epistemology of mathematics depend on prior availability of an epistemology of second-order logic, which in the current circumstances is viciously circular: the epistemology of mathematics depends on the epistemology of second-order logic, which, according to the response under consideration, is itself part of mathematics. So at best there are serious questions concerning neo-Fregeanism's anti-revisionist credentials. Thus, neither variety of Fregean logicism fares well with respect to (BA2).

There are also problems with Fregean logicism as regards (BA1). First, it is *prima facie* unlikely that the metaphysics of Frege's logicism qualifies as naturalistic, given that its central entities (e.g. concepts and extensions) inhabit the third realm. As to neo-Fregean logicism, its metaphysical commitments are the metaphysical commitments of second-order logic plus Hume's Principle. How these commitments are cashed out affects whether or not neo-Fregean logicism is a viable candidate for naturalizing mathematics. Here, again, if we take Quine's way and consider second-order logic as set theory in disguise, we run into a circularity problem. We cannot go Frege's way and take the commitments of second-order logic to be Fregean concepts (i.e. properties), since then we're for all intents and purposes back to Frege's logicism and its attendant problems. Of course, one might go a third way, arguing that second-order logic isn't part of mathematics and that we shouldn't be Fregeans about its ontological commitments. In this case we would need an account of the ontological commitments of second-order logic, but if those commitments turned out to be naturalistically acceptable then neo-Fregean logicism might be thought to offer a way of naturalizing mathematics. Moreover, an epistemology of second-order logic, if naturalistically acceptable, would also answer the worries of the previous paragraph. Thus far, then, neo-Fregeanism stands as at least a candidate for naturalizing mathematics. Evaluating the prospects for naturalistic accounts of the metaphysics and epistemology of second-order logic is more than I can do here.[67] However, there is one well-known difficulty for neo-Fregeanism that might upset its candidacy even if the concerns already canvassed don't: the problem of the scope of neo-Fregean mathematics.

Ignoring philosophical worries for a moment, here is what we know about how much of modern mathematics can be accommodated in a neo-Fregean framework, i.e. with respect to the scope of neo-Fregeanism. Owing to work of Boolos, Richard Heck, Crispin Wright, and others[68] we know that the axioms of second-order Dedekind–Peano Arithmetic (DPA^2) are provable in second-order logic plus Hume's Principle (so-called *Frege Arithmetic* (FA)). Following Boolos, this result is known as *Frege's Theorem*. Hale (2001) and Shapiro (2000a) provide ways to obtain the real numbers on the basis of FA plus additional axioms of the same sort as Hume's Principle (so-called *abstraction principles*), and hence to develop real and complex analysis within DPA^2.[69] But, of course, mathematics extends well beyond real and complex analysis, and for neo-Fregeanism to be maintained as a candidate for naturalizing mathematics it needs to encompass the whole of mathematics. In particular, it needs to encompass set theory. Otherwise, it turns out to be a reconstructive revisionist position which fails to respect (PI*).

Without going too far into detail, there are two approaches to extending the Fregean program to set theory.[70] One approach adopts various abstraction principles in place of Frege's ill-fated Basic Law V (the statement that for any concepts F and G, F and G have the same extension if and only if all and only the same objects fall under F as fall under G). The other instead restricts Basic Law V, so as to avoid Russell's paradox. The former approach has been pushed furthest by Kit Fine, in the form of his general theory of abstraction.[71] But even ignoring a lingering difficulty with the so-called *bad company* objection,[72] the natural limit of Fine's theory is third-order Dedekind–Peano Arithmetic (DPA^3), which is equiconsistent with $ZF^- + \wp(\omega)$ the theory one gets by removing the full powerset axiom from ZF (ZF^-) and putting an axiom ensuring the existence of the powerset of ω ($\wp(\omega)$) in its place. The sense in which DPA^3 is the 'natural' limit of Fine's theory is that DPA^3 is what one gets by restricting Fine's theory to second-order logic, as is customary for neo-Fregeans.[73] But setting aside this restriction and allowing variables of every finite type would only yield a theory of consistency strength less than that of Zermelo set theory, Z. In either case, we get much less than the whole of modern mathematics.[74] Hence, the first approach to extending neo-Fregeanism to all of mathematics presently comes up short.

The second approach takes us much further. The set theory Burgess calls *Fregeanized Bernays* set theory (FB) takes us beyond second-order ZFC (ZFC^2) to get (in addition) some small large cardinals.[75] One might argue that we should be satisfied with this, that it covers all of modern mathematics. I would disagree, but let's bracket concerns about whether or how much of the large cardinal hierarchy a theory needs to accommodate to be satisfactory. If a theory accommodates at least ZFC, we'll say it's satisfactory as far as not recommending a reconstructive revision of mathematics. FB is satisfactory in this sense. The question is whether or not FB satisfactorily provides for a naturalistic epistemology of mathematics. A fairly cursory look at FB answers this question negatively.

FB is formulated in monadic second-order logic with primitive notions *extension-of* and *falling-under*. For the former, we have a symbol in the language of FB, '€', so that '$€xF$' translates 'x is the extension of F'. The latter requires no special symbol in the language, as we already have the syntactic device of concatenation: 'Fx' translates 'x falls under F'. The notions of sethood and membership, for which we use the symbols 'S' and '$\in$', respectively, are defined by the following *axioms of subordination*:[76]

(AS1) $\mathrm{S}x \leftrightarrow \exists X € x X$
(AS2) $x \in y \leftrightarrow \exists Y(€ y Y \wedge Yx)$

That sethood and membership are subordinated to extension-of and falling-under in this way is crucial to the Fregean credentials of FB. As Burgess notes, the primacy of extension-of and falling-under is a chief feature of FB making it 'similar to Frege's original theory and different from mainstream axiomatic set theories such as ZFC' (Burgess, 2005, p.185).

The problem, of course, is that this primacy arguably commits FB to Fregean concepts, thus making it hermeneutically revisionist (and so incompatible with (PI*)), or making the naturalistic status of its metaphysics questionable, or both. To be sure, one might define '€' in terms of 'S' and '$\in$'.[77] However, this would still leave us with falling-under as a primitive notion, which *prima facie* carries commitment to Fregean concepts. Moreover, taking 'S' and '$\in$' as primitive, as this strategy does, once again invites a charge of circularity: the epistemology of mathematics relies on an antecedent epistemology of set theory, i.e. of mathematics. All in all, then, it seems unlikely that Fregean logicism is suitable for naturalizing the epistemology of mathematics.

3.2.4. Modal structuralism

According to modal structuralism, mathematical statements are statements about possible structures, as opposed to particular types of mathematical objects. Talk of possible structures is couched in second-order S5, and modal structuralism gives an explicit procedure for reconstruing statements of mathematics as statements of second-order S5. Statements of arithmetic, for instance, are construed as statements about possible ω-sequences.[78] Given a statement σ of (informal) arithmetic, σ can be formalized in a version of DPA^2 the language of which consists solely of a unary function symbol '**s**'.[79] Let σ^* be such a formalization of σ. Then the *modal-structural interpretation* (msi) of σ is:

$$(\sigma_{\mathrm{msi}})\quad \Box\forall X\forall f\,[\bigwedge \mathrm{DPA}^2 \rightarrow \sigma^*]^X(\mathbf{s}|f).$$

Here '$\bigwedge$DPA^2' denotes the conjunction of the axioms of DPA^2, the superscripted 'X' indicates that all quantification in '$\bigwedge$DPA$^2 \rightarrow \sigma^*$' has been relativized to 'X', and '($\mathbf{s}$ | f)' indicates that every occurrence of '**s**' in '$\bigwedge$DPA$^2 \rightarrow \sigma^*$' has been replaced by the second-order function variable 'f'.[80] Since the only non-logical vocabulary in '$\bigwedge$DPA$^2\rightarrow\sigma^*$' is '**s**', this yields a statement of pure second-order S5.

Hellman (1989) extends the modal-structuralist approach to real analysis and set theory, including some large cardinals. Granting the success of these extensions, modal structuralism accommodates at least ZFC and so is satisfactory as far as not recommending a reconstructive revision of mathematics. However, it does not fare so well with respect to hermeneutic revisionism. As we have seen, modal structuralism construes mathematical statements as statements about possible structures; it's a view about what mathematical claims *really* mean, what mathematicians are *really* talking about. This is precisely to engage in hermeneutic revisionism, which is illegitimate according to (PI*). But for the unconvinced, there are yet other problems.

First, as with Fregean logicism above, there are issues to do with *apriority*. For modal structuralism mathematical knowledge depends on knowing the appropriate msi's, i.e. statements of pure second-order S5. So for modal structuralism mathematical knowledge depends on knowing modal facts, and modal facts are arguably knowable *a priori*. So if one takes *a priori* knowledge to be incompatible with naturalism, modal structuralism isn't naturalistically acceptable. Bracket this worry, as we did with Fregean logicism. Still (and second) one might reasonably worry about an epistemology of mathematics that depends on an epistemology of modality. Naturalistic or not, do we really have that much better grasp of modal knowledge than we do of mathematical knowledge? It's not at all clear that we would be gaining much by basing our epistemology of mathematics on an epistemology of modality. Moreover, Hellman himself suggests that the epistemology of modality sufficient for a modal-structuralist set theory is unlikely to be naturalistic.[81]

Finally, the naturalistic standing of the metaphysics of modal structuralism is far from clear. Modal structuralism is intended to be neutral between realism and nominalism.[82] Hellman argues that:

(Eq) σ is true iff σ_{msi}, where the relevant conception of truth is realist, and that this is recognizable by both mathematical realists and modal structuralists, each on their own terms.[83] The idea is that the realist can have her preferred reading of σ in the left-hand side of (Eq), the nominalist can have a metaphysically innocent reading of σ in the right-hand side of (Eq), and the two can agree on the truth-value of σ. On the face of it, this is good news for anyone who would like to press modal structuralism into service in naturalizing mathematics. A nominalistic metaphysics is almost certainly naturalistic. But there is more to it than what we see on the surface.

If one is to make any progress with naturalizing mathematics via modal structuralism, the metaphysical commitments of a mathematical claim σ had better be those incurred by the right-hand side of (Eq), i.e. by σ_{msi}. Since σ_{msi} is a statement of second-order S5, the metaphysical commitments of σ_{msi}, and hence of the modal-structural approach, are just those of second-order S5. Hellman argues that these commitments can be cashed out nominististically, *taking logico-mathematical modality as primitive*. That is, the nominalism yielded by modal structuralism is a modal nominalism.[84] The question, of course, is: Even granting that second-order logic can be satisfactorily nominalized, why think that the relevant modality can similarly be nominalized? I'll not attempt to answer this question here; rather I leave it as one more significant worry concerning the suitability of modal structuralism as an approach to naturalizing mathematics.

Before concluding I want to briefly address an objection one might raise in connection with my use of hermeneutic revisionism in this and the immediately preceding subsection, viz., that though it is in some sense revisionary, hermeneutic revision isn't revisionary in the sense relevant to violating (PI*). The idea is that revising the semantics of mathematics is unlikely to affect mathematical practice much at all, and it's really revisions that affect practice which are at issue in (PI*). So hermeneutic revision needn't conflict with (PI*). This being the case, the naturalist can accept hermeneutic revision. It seems to me there are at least two things to say in response to this worry.

First, I introduced the representational apparatus for mathematical practice in §3.2.3. to help make perspicuous that revising the semantics of mathematics induces a revision in the non-semantic aspects of the practice. Semantic thickening induces revisions to both $Q^{P_{Acc}}$ and $N^{P_{Acc}}$. If this is right, then the objection simply misfires. Of course, one might be unconvinced that semantic revision induces non-semantic revision. This leads to the second response. Even if hermeneutic revision is compatible with (PI*), that does little to undermine my contention that neither Fregean logicism nor modal structuralism is well suited to a naturalistic epistemology of mathematics. This, because my arguments for this contention only partly rely on these views being hermeneutically revisionist. In the first place, there are serious questions as to whether the metaphysics of either view is naturalistic. In the second place, Fregean logicism threatens to be reconstructively revisionist.[85] In either case, regardless of the status of hermeneutic revisionism vis-à-vis (PI*), serious work remains to be done before either Fregean logicism or modal structuralism is in a position to satisfy the needs of a naturalistic epistemology of mathematics.

4. *Concluding remarks*

We have seen in the foregoing defense of (1) that none of the leading philosophies of mathematics, whether alethic idealist or alethic realist, provides for a satisfactory naturalization of the epistemology of mathematics. Each is caught between the demand of general epistemology that justification be truth-conducive and the demands of naturalism that epistemology not be revisionary and metaphysics not go beyond the naturalistic. As foreshadowed at the end of §2, this defense isn't conclusive. I have not considered alethic idealism positions along the lines of formalism, and future developments in modal epistemology or the epistemology of second-order logic might yield answers to worries I raised about neo-Fregeanism and modal structuralism. At present, however, we have only problems with no obviously forthcoming solutions. Moreover, it's not at all clear what sort of solution could yield an account of mathematical truth without simultaneously violating (PI*) or countenancing non-naturalistic entities. It seems that any account of mathematical truth has an attendant story about the metaphysics of mathematics – thick or thin, inflationary or deflationary – and limiting an account to naturalistic metaphysics invariably leads to illegitimate recommendations for revision. Taken all together we have a good inductive case against the eventual overthrow of (1), which strongly suggests that the epistemology of mathematics simply can't be naturalized.[86,87]

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NOTES

1 See, e.g., Boyd, 1988 and Sturgeon, 1985.

2 See, e.g., Armstrong, 1980, 1993; Lewis, 1966, 1972, 1980; Ryle, 1949.

3 Quine is the main, though by no means sole, source here. See, e.g., Quine, 1948, 1951, 1954, 1969a, 1969b, 1969c.

4 See, e.g., Kitcher, 1983, 1988; and Maddy, 1995, 1996, 1997, 1998a, 1998b, 2003.

5 I'll often not specify the epistemology of mathematics, it being understood that by naturalizing mathematics I intend naturalizing the epistemology of mathematics.

6 See, e.g., Dieterle, 1999; Paseau, 2005; Roland, 2007, 2008; Rosen, 1999; and Weir, 2005.

[7] Though this notion of falling within our scientific worldview is obviously imprecise, it should be precise enough for present purposes.

[8] See, e.g., Quine, 1969a; Kim, 1988; and the discussion of the (strong) replacement thesis in the introduction to Kornblith, 1994.

[9] By 'grounds' I intend whatever motivates or rationalizes the revision(s).

[10] For an illuminating discussion of these points, see Hylton, 1994.

[11] See, e.g., Quine, 1975.

[12] Specific to Maddy, see Dieterle, 1999; Roland 2007; and Rosen, 1999.

[13] For more on disciplinary holism, see Roland 2007.

[14] For an explicit statement of this inclusion, see Quine, 1995, p. 49.

[15] One might object that to the extent facts about these issues get applied in the non-mathematical sciences they are of concern outside mathematics. If this objection were to be sustained, I think we could draw a subsidiary distinction between primary and secondary concerns and recast (PI) in terms of primary concerns without loss.

[16] Though, again, one might make a case for mathematics being secondarily concerned with these issues.

[17] Thanks to Hilary Kornblith for alerting me to this example.

[18] See Hacking, 2006, ch. 13 for the relevant history.

[19] See Hacking, 2006, ch. 13.

[20] See Baker, 2001; Burgess, 1990, 1998; Burgess and Rosen, 1997; Colyvan, 2001, 2007; and Resnik, 1997. With apologies to Gideon Rosen, I treat the views on mathematics expressed in Burgess and Rosen (1997) as extensions of Burgess's views expressed in Burgess (1990, 1998).

[21] I here assume that whatever doesn't count as justified according to an epistemology shouldn't be accepted by one who endorses that epistemology.

[22] See, e.g., BonJour, 1985, 1998, 2000; and BonJour's contribution to Bonjour and Sosa, 2003.

[23] Note that I'm not here committing myself to any substantive theory of truthmakers. I'm simply using the term as shorthand for *whatever it is* in virtue of which mathematical beliefs, etc. are true or false. Presumably, the claim that mathematical beliefs, etc. are truth apt isn't contentious – at least not to any party to the current debate.

[24] See, e.g., Shapiro, 1997; and Resnik, 1997.

[25] See Frege, 1884/1980; Hale, 1988; Wright, 1983; and the essays in Hale and Wright, 2001.

[26] See, e.g., Maddy, 1990; Gödel, 1944, 1947/1964.

[27] See Hellman, 1989.

[28] See, e.g., Brouwer, 1912, 1948, 1952; or Dummett, 1973, 1977.

[29] See, e.g., Ayer, 1946; and Carnap, 1931, 1937, 1950/56.

[30] See, e.g., Hilbert, 1925.

[31] See, e.g., Field, 1982, 1988 for Field's fictionalism.

[32] In the interest of brevity, and because for independent reasons neither formalist nor logical empiricist views of mathematics currently enjoy much support, I'll restrict my remarks on alethic idealism to intuitionism. (But see my remarks on the fitness of Carnap's view for naturalizing mathematics in n. 86.)

[33] A standard example of such a function is the Dirichlet function, the function that takes every rational to 1 and every irrational to 0.

[34] See Posy, 2005 for a nice, contrastive discussion of the views of Brouwer, Heyting, and Dummett. A number of contemporary intuitionist mathematicians follow the tolerant-intuitionistic lead of A. S. Troelstra, clearly expressed in (1977): 'In these notes, we shall adopt the intuitionistic viewpoint, not as a philosophy of mathematics that excludes others,

but as the appropriate framework for describing *part* of mathematical experience' (p. 1, original emphasis). See, also, Kreisel, 1965 for a similar sentiment.

35 See, e.g., Quine, 1948, 1954. Later time slices of Quine reject object platonism (see, e.g., 1992), but this doesn't affect the argument in the text.

36 Where α is an ordinal, $\beth_\alpha$ is defined by transfinite recursion on α by (1) $\beth_0 = \aleph_0$, (2) $\beth_{\alpha+1} = 2^{\beth_\alpha}$, and (3) $\beth_\lambda = \sup\{\beth_\beta : \beta < \lambda\}$, for λ a limit ordinal.

37 See, e.g., Feferman, 1992.

38 Intuitively, the rank of a set is the first level where the set appears in the cumulative hierarchy.

39 Other potential problems with the argument include: equivocation on 'applications'; the non-trivial possibility that (2′) is false, given the plausibility that not all mathematics of rank $\omega+\omega$ is indispensable to our best (empirical) science; and a lack of attention to the diminishing strength of the connection between mathematics and experience as the rank of the mathematics increases.

40 I opt for 'part' instead of 'branch' to avoid assuming that the indispensability of an entire branch of mathematics follows from the indispensability of some number of results from that branch. I don't see why the whole of set theory should be indispensable simply because a relatively small part of set theory is. Indeed, if we take that view, the problems of the scope of indispensability arguments evaporates.

41 See, e.g., Feferman and Jäger, 1993, 1996; Ye, 2000; and the papers in Part V of Feferman, 1998. For a nice overview of the project, see Feferman, 2005.

42 Colyvan acknowledges that this work might be a problem for his extended indispensability view (2007, p. 115).

43 Weyl actually uses the positive integers and 1, rather than ω and 0, along with the successor relation.

44 See Feferman, 1988 for those details.

45 Cf. Feferman's remarks concerning the ramifications of the predicativist program for the standard indispensability argument in (1992).

46 See Hellman, 1993; Emch, 1972; and Pitowsky, 1989.

47 For these responses, including discussion, see Richman and Bridges, 1999; Richman, 2000; Streater and Wightman, 1978; Feferman, 1988; and Malament, 1992.

48 See, e.g., Kunen, 1980, ch. IV, §7.

49 Obviously the mathematics needed by empirical science is here excluded. Also excluded are Cambridge statements such as those specifying (say) that some physical system does *not* have physical quantity r for infinitely many real values of r. Of course any statement correctly specifying that a system has physical quantity r_0 gives rise to such a collection of statements. (Water boils at 212 degrees Fahrenheit under standard pressure. So for any real number r: if $r \neq 212$, then water does not boil at r degrees Fahrenheit under standard pressure.)

50 One might worry about schematic scientific laws with potentially infinitely many instances. I take it that such laws are among the τ_i, counted as single statements for present purposes. The fact that such a law may have infinitely many instances provides no more reason to think arguing for it requires infinitely many mathematical results than the fact that the induction scheme for DPA has infinitely many instances provides a reason to think that its proof requires infinitely many mathematical results. (The induction scheme for DPA is provable using finitely much set theory.)

51 The key here is that only finitely many instances of Replacement and Separation are used in the proofs of the τ_i. Indeed, since $V_{\omega+\omega}$ models ZFC – Replacement, finitely many instances of Replacement alone suffices.

52 Thanks to an anonymous referee for raising this issue.

[53] For overlapping but distinct lists of theoretical virtues see Burgess and Rosen, 1997, §III.C.1.a; Colyvan, 2001, §4.3; and Quine and Ullian, 1978, ch. 4.

[54] Theories T and T' are empirically equivalent just in case they have exactly the same observational consequences.

[55] A scientific standards approach is advanced in Burgess, 1998; Burgess and Rosen, 1997; and Colyvan, 2001.

[56] This example comes from Boolos, 1998. The least such cardinal is the union of $\{\aleph_0, \aleph_{\aleph_0}, \aleph_{\aleph_{\aleph_0}}, \ldots\}$.

[57] See Burgess, 1998; and Burgess and Rosen, 1997, §III.C.1.b.

[58] See Kunen, 1980 for definitions.

[59] For a discussion of the scientific standards approach which has points of contact with this discussion, see Paseau, 2007.

[60] I'll keep with this nomenclature where it matters in what follows.

[61] See, e.g., Wright, 1997, 1999.

[62] See Kitcher, 1983, pp. 163–164.

[63] Cf. the distinction between revolutionary and hermeneutic nominalism in Burgess and Rosen, 1997, pp. 6–7.

[64] See Burgess and Rosen, 1997 for discussion of a number of examples.

[65] See Hallett, 1984; and Lavine, 1994 for the history of Replacement's discovery and adoption.

[66] Whether or not Frege himself would consider his logicism hermeneutically revisionist is an interesting question. I won't pursue it here, however, since a negative answer wouldn't obviously undermine my arguments against the suitability of Frege's logicism for naturalism.

[67] One might also challenge the naturalistic status of Hume's principle. I bracket this worry.

[68] For references and discussion, see Burgess, 2005, especially §3.1.

[69] Real and complex numbers fail to be objects in these constructions unless a logic of order higher than 2 is used (see Burgess, 2005, pp. 161–2). If one is wedded to numbers being objects – as Frege himself was, of course – and one prefers to limit oneself to second-order logic, this might present a problem. I bracket this potential difficulty.

[70] For the relevant details, see Chapter 3 of Burgess, 2005.

[71] See Fine, 2002.

[72] See Burgess, 2005, pp. 164–170, 184.

[73] See Burgess, 2005, pp. 170–184.

[74] I here deploy the idea that consistency strength can be used to gauge how much of mathematics is captured by a given theory. See Burgess, 2005, §1.5.

[75] See Burgess, 2005, pp. 190–201.

[76] See Burgess, 2005, p. 185.

[77] See Burgess, 2005, Table B, p. 216.

[78] See, e.g., Hellman, 1990, p. 316.

[79] See Robbin, 1969, chapter 6, especially pp. 145–153.

[80] For the details of this relativization, especially for second-order quantification, see Hellman, 1989, p. 23, n. 19.

[81] See Hellman, 1989, pp. 71–72, especially n. 21.

[82] See Hellman, 1989, §§4 and 6.

[83] See Hellman, 1989, pp. 33–45, 67–71.

[84] See, e.g., Hellman, 1989, pp. 15, 49.

[85] Notice that the obvious superiority of modal structuralism over Fregean logicism with respect to reconstructive revisionism arguably makes it the best account of mathematics from a hermeneutic standpoint.

[86] Two philosophers who have been prominent in the movement to naturalize mathematics, and whose views are almost entirely absent from my discussion, are Maddy and Philip Kitcher. (See the references in n. 4.) I have argued elsewhere that the attempts of both fail, and they fail for essentially the same reasons as the views discussed in §3. (See Roland, 2007 and 2008.) Thus I opted not to address the views of Maddy or Kitcher in the present article. I also omitted discussion of Carnap's logicism. (See the references in n. 29.) This might seem egregious to some, especially in light of a recent resurgence of interest in Carnap – including his logico-mathematical views. (See, e.g., Friedman, 1999; Friedman and Creath, 2007; and Richardson, 1998.) I think there are a number of reasons why a Carnapian philosophy of mathematics is ill-suited to naturalizing mathematics, most involving the antagonistic role played by Carnap's views in the development of the type of naturalism with which I'm here concerned (viz., naturalism in the tradition of Quine). But, more importantly, the mature Carnap abandons the project of epistemology in mathematics; according to Thomas Ricketts, 'in a sense, he gives up philosophy of mathematics' (Ricketts, 2007, p. 211). This being the case, Carnap is engaged in a project quite different from the subject of this paper. Perhaps we should give up on giving an epistemology for mathematics, but then the question of naturalizing the epistemology of mathematics becomes moot.

[87] Thanks to Richard Boyd, Jon Cogburn, Hilary Kornblith, Penelope Maddy, Susan Vineberg, and an anonymous referee for helpful discussion and comments.

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