Potentialist Set Theory and The Nominalist's Dilemma

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Abstract

Mathematical nominalists have argued that we can reformulate scientific theories without quantifying over mathematical objects. However, worries about the nature and meaningfulness of these nominalistic reformulations have been raised, like Burgess and Rosen's dilemma in [3].

In this paper, I'll review and (what I take to be) a kind of emerging consensus response to these concerns: appeal to the idea of different levels of analysis and explanation, with philosophy providing an extra layer of analysis 'below' physics, much as physics does below chemistry.

I'll then argue that one can address certain worries about to this common response, by also considering a variant which exploits the apparent usefulness of distinction between foundational and non-foundational contexts *within mathematics* and certain (admittedly controversial) arguments for Potentialism about set theory.

1 Introduction

In response to Quinean Indispensability arguments that we must be able to state our best theories without quantifying over objects we don't believe in, philosophers who want to deny the existence of mathematical objects have proposed elaborate nominalistic logical regimentations of scientific theories. However, worries about the nature and meaningfulness of these nominalistic reformulations can be raised. In [3] Burgess and Rosen put this point forcefully by noting that the nominalist appears to face the following dilemma. Nominalistic regimentations of scientific theories must be intended either as *hermeneutic* proposals, clarifying what scientists currently implicitly mean, or as *revolutionary* proposals concerning what scientists should start to say. But Burgess and Rosen argue that typical convoluted nominalistic paraphrases[1] of scientific theories are bad candidates for either job. These paraphrases are

- too psychologically and linguistically unmotivated to be a plausible hermeneutic theory of what scientists currently mean.
- too unmotivated by the standards of the scientific disciplines in question to be a plausible revolutionary proposal. For example, nominalistic regimentations of a physical theory would generally not be accepted by physics journals.

Thus, it might seem, typical nominalist logical regimentations (and any theories they are used to develop) should probably be rejected. Call this Burgess and Rosen's dilemma for the nominalist.

In this paper, I'll argue that certain considerations about mathematical practice and arguments for potentialist set theory may help the nominalist respond to Burgess and Rosen's dilemma. In §2, I'll review a common response to this dilemma among untroubled friends of metaphysics: the suggestion that metaphysics provides a legitimate further layer of analysis below physics, just as physics provides a layer of explanation and analysis below chemistry. This style of response is quite popular and (Hellman proposes a version in his book review[6] of *A Subject With no Object*, and Sider's account[15] of the aims of philosophical analysis has significant affinities for it).

However, this reply alone has two important weaknesses. First, naturalist philosophers will certainly reject the idea of philosophy as providing a legitimate further layer of analysis below physics. Second, Burgess and Rosen provide interesting attempts to debunk of classic philosophical motivations for nominalism

[1] Here I mean paraphrases that are more complex in their logical structure than the Platonist alternative.

in *A Subject with No Object*[3], which Hellman understandably doesn't try to refute in his book review [6], but which a defender of nominalism along his lines would ultimately have to answer.

Accordingly in §3 I will suggest a way of modifying (or supplementing) the emerging consensus response, that promises to avoid or significantly lessen both of these concerns. First, I'll argue that contemporary mathematical practice already, seemingly fruitfully, employs a distinction between foundational and non-foundational contexts. This provides independent, naturalism-friendly motivation for thinking there's an illuminating layer of analysis for mathematical claims which can diverge from surface grammar. Accordingly, nominalists can answer Burgess and Rosen's dilemma in a more naturalism-friendly way, by saying their paraphrases are *continuous with foundational debates mathematics*, rather than by claiming metaphysics provides a legitimate layer of analysis below physics. Then I'll argue that some (admittedly controversial) arguments for potentialism about set theory suggest a motivation for mathematical nominalism which is independent from the classic philosophical arguments for nominalism Burgess and Rosen attack, and more closer to the kinds of considerations typically relevant to foundational mathematics.

So, overall in this paper, I will argue that clear features of mathematical practice suggest a more naturalism-friendly version of Hellman's core response to Burgess and Rosen's dilemma. And arguments for an (admittedly much more controversial) potentialist project suggest an interesting particular implementation of this style of response. Note that I won't try to assess familiar Quine-Putnam indispensability arguments about whether nominalist paraphrases for scientific theories can be found[2]. My aim in this paper is only to address Burgess and Rosen's dilemma and related arguments that such paraphrases shouldn't

[2]See REDACTED for some serious problems I think the nominalist faces, but also for discussion of how the primitive modal notion already needed for potentialist treatments of pure set theory can be useful in making sense of applied mathematics as well.

be accepted, even if they could be found.

2 Analogy with Fundamental Physics

So, let us begin with what I take to be an emerging consensus view. I take something like the following to be a common and natural reaction to Burgess and Rosen's dilemma, for untroubled friends of metaphysics [3].

> **Science-Philosophy Division of Labor Response**: It doesn't matter if nominalistic formalizations of scientific theories lack scientific motivation, because they have plenty of philosophical motivation. They reflect what we should say when doing philosophy (or perhaps, more specifically, fundamental ontology) — which is not necessarily the same as what we should say while doing mathematics or the sciences. We have good *philosophical* reasons for preferring nominalistic regimentations of scientific and mathematical theories, and philosophical reasons are to be taken just as seriously as mathematical and scientific ones[4].
>
> So Burgess and Rosen may be right that nominalistic regimentations of physical theories don't reflect what someone should say when submitting to physics journals (and that success at nominalizing physical theories along the lines of Field's *Science Without Numbers*

[3]We might think of it noting an alternative to both horns of the dilemma or as pointing out an appealing branch within the revolutionary horn of the dilemma.

[4]For example, Hellman suggests philosophical motivations for nominalistic paraphrase as follows,"[The purpose of nominalist reconstruction programs] is to help answer certain meta-mathematical or metascientific questions, not normally entertained in pure and applied mathematical work proper.

How can the essential mathematical content and results of mathematics be understood so that a naturalized epistemology of science and mathematics can proceed smoothly? Cannot this content be understood independently of Platonist ontology? How, if possible, can the seemingly embarrassing questions associated with the Platonist picture be blocked, while respecting and preserving the reasonableness of ordinary practice, including the use of ordinary theories?"[6]

> wouldn't be regarded as publishable scientific progress by the latter). But, if so, this doesn't show that nominalistic formalizations of scientific theories are false. For articles in physics journals are not attempting to speak completely explicitly and literally — not attempting to write in a logically regimented language which exposes the metaphysical structure of reality when we apply Quine's criterion. Articles in scientific journals are instead written in unregimented natural language, which is easier to work with and purposely and helpfully lets one bracket certain metaphysical questions[5].

As Hellman points out in [6], the (metaphysics friendly) nominalist can cite a kind of division of labor within the sciences as a model for this distinction between what we should say in philosophical vs. scientific contexts. For, consider what happens when scientists studying lower-level, more fundamental, disciplines like physics or chemistry non-trivially analyze terms that also occur in higher level sciences like biology or ecology. In doing this, scientists aren't (generally) making revolutionary claims about what scientists working in the higher-level disciplines should say, or hermeneutic claims about what these scientists have implicitly meant (in any sense relevant to linguistics or cognitive science) all along.

For example, imagine a 19th century physicist who believes that heat is molecular motion rather than caloric fluid, and writes down physical theories which are most straightforwardly logically regimented so as to replace talk of objects being warm with talk of molecular motion. Such a physicist wouldn't usually believe the revolutionary claim that higher level scientists (biologists or ecologists) should replace talk of heat with talk of molecules moving. Nor would

[5] I have in mind questions like whether there's an abstract object 'electronhood' or merely a property? For example, writing up a physical theory in a logically regimented language which Quine's criterion can be applied might require one take a stand on this.

she make the hermeneutic claim that biologists and ecologists are implicitly having thoughts whose logical structure corresponds to her analysis and commits them to agreeing with her on the caloric fluid vs. molecular motion controversy.

Rather, she'd allow that biologists theorizing about, e.g., how an animal's ears help regulate its body temperature, can rightly speak in ways that treat heat as an unanalyzed primitive quantity. For people speaking this way in biology journals, doesn't commit them to any position about whether heat should be accepted as a fundamental quantity vs. analyzed in terms of molecular motion when writing a fundamental physical theory of everything. At most, the biologist is committed to there being some correct analysis of informal talk of heat on which their biological theory comes out true[6]. This division of epistemic labor and risk isn't just an apparent feature of current scientific practice, but something that's clearly useful and should be unsurprising.

Similarly, (the untroubled friend of metaphysics may say) metaphysics is its own discipline, with its own level of analysis and distinct explanatory work this analysis is intended to perform. Metaphysics is to physics as physics is to biology and ecology. So what we should say in metaphysics journals can differ radically from what we should say in physics journals for the same reason that what we should say about heat in biology journals can differ radically from what we should say about heat in physics journals.

Sider [15] suggests a nice (though optional) addition to Hellman's response to Burgess and Rosen summarized above. In [15], Sider proposes a theory of the aims of philosophical analysis which (I think) naturally explains why we should expect good philosophical analyses to lack both kinds of scientific support considered in Burgess and Rosen's dilemma.

Specifically, Sider suggests that the project of metaphysical semantics relates

[6]So, for example, we might say they are committed to something like the disjunction of all conceivable logical regimentations of their claim about the animal's ears (corresponding to different options for the physical analysis of heat talk).

to linguistic semantics as follows. Both projects use notions like reference and try to explain why people say the things they do. But metaphysical semantics aims to illuminate relationships between what people say and fundamentalia, while linguistic semantics does not[7]. Furthermore, metaphysical semanticists don't attempt to assign meanings in a way that matches facts about sentences' syntactic form, or illuminates what can be rationally derived from them a priori, or known by conceptual competence alone (as linguistic semanticists often do).[8]

Thus, I take it, from a traditional pro-metaphysics point of view, Burgess and Rosen's dilemma isn't very serious. There's no problem about admitting that nominalized physical theories produced in philosophical contexts to answer philosophical questions (like 'what are the metaphysically fundamental objects?) aren't either what working mathematicians, physicists etc. should say or what they implicitly mean.

However, a pair of worries (or controversies) remains. First, philosophers of a naturalist bent won't be satisfied with this style of answer. They might argue that philosophy (paradigmatically philosophical explanatory demands and methods of argument) doesn't have the track record of success needed to play the role the friend of metaphysics envisages (i.e., providing a lower and more fundamental level of analysis below that relevant to physics). From this point of view, the relationship of metaphysics to physics is more like the relationship

[7]Sider writes, "Metaphysical semantics is more ambitious [than linguistic semantics] in that by giving meanings in fundamental terms, it seeks to... show how what we say fits into fundamental reality."

[8]Sider writes as follows.

> "[A person doing metaphysical semantics] is... not trying to integrate her semantics with syntactic theory...And she is free to assign semantic values that competent speakers would be incapable of recognizing as such, for she is not trying to explain what a competent speaker knows when she understands her language. She might, for example, assign to an ordinary sentence about ordinary macroscopic objects a meaning that makes reference to the fundamental physical states of subatomic particles. And she might simply ignore Frege's ...puzzle of the cognitive nonequivalence of co-referring proper names, since she is not trying to integrate her semantics with theories of action and rationality."

of astrology to astronomy than the relationship of physics to chemistry. So (they might say) considering what we should say *when answering metaphysical questions* isn't considering the answer to any question that's worth asking.

Second, in *A Subject with no Object*[3] Burgess and Rosen individually discuss and interestingly criticize all of the philosophical motivations for nominalism Hellman mentions: access worries, appeals to Occam's razor, and general skepticism about the existence of necessary or abstract objects. So someone who wants to answer Burgess and Rosen's dilemma in the way Hellman proposes would also have to address these specific further arguments.

3 Continuity With Foundations of Mathematics

I will now argue that we can avoid (or significantly reduce) the objections above by also considering a variant on the emerging-consensus reply to Burgess and Rosen's dilemma, which appeals to traditions of foundational work inside mathematics and the literature on potentialist set theory.

In this section, I will propose a way of tweaking the above science-philosophy division of labor answer to Burgess and Rosen's dilemma, to better satisfy those with naturalistic inclinations. I'll argue that we can see attempts to nominalistically formalize mathematical and scientific claims as continuous with foundational work within mathematics, whose legitimacy and fruitfulness is widely accepted. In the next section, I'll add a suggestion that certain (admittedly controversial) ideas from the literature on potentialist set theory can be used to motivate favoring a nominalist approach to set theory (and thereby perhaps to mathematics generally) in relevant foundational contexts.

The basic proposal I want to make goes like this. Mathematicians accept something analogous to the above mentioned division of labor between higher and lower-level sciences, in the form of a distinction between what we should say in normal vs. foundational mathematical contexts. And a nominalist can say that providing a potentialist (and, therefore, nominalist) logical regimentation for set-theoretic claims is quasi-mathematically motivated and in other ways continuous with existing foundational projects within mathematics.

Note that, providing foundations for mathematical subdisciplines is already an accepted and apparently fruitful part of mathematical practice. And when providing foundations in this sense for some area of mathematics, we are allowed to employ logical formalizations that don't correspond to the surface grammar of sentences in journals devoted to these sub-disciplines. Mathematicians already draw a distinction between what it's right to say in normal contexts (including the classroom and typical/mainstream mathematical journals) and what it's right to say in certain unusually pedantic contexts of foundational investigation.

For example, consider the way that practicing mathematicians have pursued set theoretic foundations for analysis. It's unclear whether people reading core mathematical journals implicitly do, or should, normally cash out talk of non-foundational mathematical objects like (say) the natural numbers in terms of assertions about the existence of sets, rather than thinking thoughts with a simpler and more face value logical structure (e.g. simply quantifying over the natural numbers and treating notions like $+, *$ and $<$ as primitive notions). But that's not a problem. For, the project of providing set-theoretic foundations for analysis (as motivated by the need to solve problems and paradoxes within analysis and Bourbaki-type programs for facilitating comparison between different areas of mathematics) doesn't require providing a logical regimentation which is motivated in this way. Note that these logical regimentations of pure mathematical statements can be rather complex, like the logical regimentations of applied mathematics our critic is objecting to. Mathematicians developing set-theoretic foundations for mathematics weren't attempting either a hermeneutic or a revolutionary project in Burgess and Rosen's sense.

Similarly, I want to suggest that philosophers advocating a (potentialist) nominalist understanding of set theory and thence mathematics as a whole can appeal to mathematical practice to justify their rejection of both horns of Burgess and Rosen's dilemma. For, contemporary mathematical practice itself seems to clearly allow that there can be good mathematical reason for adopting logical regimentations for mathematical talk in some contexts which don't correspond to what should be spoken or thought in most teaching, research, scientific or practical contexts.

What contexts *are* mathematical foundational proposals relevant to? Roughly we might think of these foundational proposals as accounts of what one should say in a context with the following features. One has plenty of time (so there is no need for abbreviation) and no need to teach others (so there's no need for technically false simplifications)[9]. But one lays oneself open to relatively pedantic or strange questions, e.g., questions that connect very disparate parts of one's web of mathematical beliefs. And one tries to apply one's concepts to types of questions which have not hitherto been much considered (e.g., taking limits of certain strange functions which are not physically natural but whose limits don't seem obviously undefined).

Theoretically, I think such a division of labor between central and foundational mathematical contexts is rational and should be expected, in much the way that the division of labor between the sciences is. It makes sense that mathematicians would distinguish questions of what should be said in the special foundational context above from what should be said while doing something

[9]Note that my suggestion here isn't that we never use foundational notions, like say set theory, in teaching contexts. Sometimes bringing in the same concepts and definitions which are useful for foundational problem-solving winds can also be very pedagogically helpful. My claim is only that foundational contexts are ones in which the defense 'yes technically that may be right, but I thought I should suppress those details for pedagogical reasons' doesn't apply.

like Kuhnian normal science (where we know how to get right answers by employing familiar ways of talking and techniques). For, on the one hand, it is useful to precisify our terms when reasoning at the edges of normal practice, in cases where paradox threatens or it's desirable to apply concepts from one domain to new areas etc. On the other hand, it's often desirable to continue with an apparently working practice and not commit oneself to any specific foundational analysis of what is going on under the hood. Researchers working in areas where normal mathematics seems to be going well plausibly needn't bother attempting to further analyze their terms, and perhaps shouldn't take the risk of doing so (i.e., shouldn't risk committing themselves to one answer to foundational mathematical questions rather than another).

If this division between normal and foundational mathematical contexts is accepted, nominalists need not be troubled by the fact that most math and science journals wouldn't want to publish nominalist regimentations of otherwise familiar theories, and most scientists and mathematicians aren't secretly thinking in terms of such regimentations. For, they can take nominalized mathematics seriously as a story about what we should say in the special pedantic context of foundational debate. And they can cite motivations continuous with the motivations for accepted foundational projects within mathematics for doing so. And if we say this (and take a Quinean approach to ontological commitment[10]) it seems only natural to say that the ontological commitments reflect what we'd say when speaking the specially pedantic context of foundational discussion (rather than when speaking quickly in the classroom or in journals).

In the next section, I will add to the above picture by arguing that someone who accepts the story about foundational mathematical contexts above, could use certain (admittedly controversial) ideas about potentialist set theory to

[10]I take it that accepting something like Quine's criterion is needed to get the nominalization challenge going in the first place.

motivate favoring nominalistic paraphrases in foundational contexts (in a way quite independent from classic philosophical arguments for nominalism) – and thus respond to Burgess and Rosen in a way that avoids both concerns about the emerging consensus response discussed above.

4 Potentialist Set Theory

So, let me begin with some quick background on potentialist set theory and its motivations. In a nutshell, potentialists try to solve apparent paradoxes about the intended structure of the hierarchy of sets by reinterpreting set theory in modal terms.

Recall that, in response to Russell's paradox (among other things), set theorists embraced an iterative hierarchy conception of sets. On this view, all sets can be thought of forming a hierarchy built up in layers (that satisfy the well-ordering axioms). There's the empty set (the set that has no elements) at the bottom. And each layer of sets contains sets corresponding to all ways of choosing some (or none) of the sets generated below that layer[11].

But what about the height of the hierarchy of sets? Here a puzzle arises that can motivate a potentialist understanding of set theory (and thereby logically formalizing set theoretic sentences in a way that doesn't match how we typically speak). Naively, it is tempting to say that the hierarchy of sets is supposed to extend 'all the way up' in a way that guarantees it satisfies the following principle

> **Naive Height Principle:** For any way some things are well-ordered by some relation $<_R$, there is an initial segment of the hierarchy of sets corresponding to it (in the sense that the objects satisfying R

[11] It follows from this conception (of what I'll call the width of the hierarchy of sets) that if the intended hierarchy of sets contains a set x, it must also contain subsets corresponding to all possible ways some elements from x.

could be 1-1 order preservingly paired onto the layers in this initial segment).

But this assumption leads to contradiction via what's called the Burali-Forti paradox[12]. So, in contrast to the fact that we seem to have a precise and logically coherent conception of the intended *width* of the hierarchy of sets, we don't seem to have any analogous conception of its intended height (that remains once the naive and paradoxical idea above is rejected). And it seems arbitrary to say that the hierarchy of sets just happens to stop somewhere: that it has a certain height which doesn't follow from anything in our conception of what structure the hierarchy of sets is supposed to have[13].

Mathematicians and philosophers have explored various responses to this problem. Practically speaking, it's widely agreed that we should drop the above naive conception of the intended height of the hierarchy of sets but continue to accept the ZFC axioms (which this conception motivates). However, we must then understand the suitability of the ZFC axioms and the meaning of set-theoretic claims, somehow.

One popular family of responses maintains that the intended height of the hierarchy of sets is vague or indeterminate – perhaps with all acceptable options satisfying the standard ZFC axioms for set theory (and truth values for set-theoretic sentences being determined in a supervaluationist way so that classical

[12]If we consider the relation $x <_R y$ 'iff x and y are both layers in the hierarchy of sets and x is below y or y is the Eiffel tower and x is a layer' we see that the above naive conception of the hierarchy of sets cannot be satisfied. We have a sequence of objects that is strictly longer than the hierarchy of sets, contradicting the naive conception of sets. We know the sequence of objects related by $<_R$ is strictly longer than the layers of the hierarchy of sets because it's a theorem of ZFC that no well ordering is isomorphic to a proper initial segment of itself.

[13]Note that the problem here is not simply that it might be impossible to define the intended height of the hierarchy of sets in other terms. After all, every theory will have to take some notions as primitive.

Instead, we find ourselves in the following situation. Our naive conception of absolute infinity (the height of the actualist hierarchy of sets) turns out to be incoherent, not just unanalyzable. And, once we reject this naive conception, there's no obvious fallback conception that *even appears* to specify a unique height for the hierarchy of sets in a logically coherent way.

reasoning about set theory is still truth preserving [4]). However, these views face a challenge about accounting for common tendencies to favor taller over shorter interpretations of set talk[14] (and perhaps also about whether existence facts can be vague [15]).

Another option championed by figures like Putnam and Parsons [13][10] is to embrace potentialism. In a nutshell, potentialists eliminate appeal to the intended height of the hierarchy of sets by reinterpreting set theoretic sentences as making claims about how it would be (in some sense) possible for standard-width initial segments of the hierarchy of sets to be extended.

In Putnam[13] suggests we can interpret set theoretic claims as talking about how it would be possible to have physical objects (like pencil points and arrows) forming intended models of certain axioms for set theory, but leaves the details of what modal notion he wants to invoke somewhat vague. Later work by Hellman[5] and Berry[1] develops Putnam's idea by appeal to a notion of logical possibility (which has been argued to be an independently attractive primitive). Hellman uses logical possibility, plural quantification and mereology (to simulate second-order relation quantification). Berry uses a generalization of the logical possibility operator. These approaches are immediately nominalist about sets.

A different school of potentialist set theory, beginning with Parsons[10, 11, 12] and recently developed by Linnebo[8, 7, 9] and Studd[16] take the core potentialist idea above in a different direction. Rather than thinking about how it would be logically possible for there to be objects satisfying set-theoretic axioms, Linnebo and Studd say that whatever sets exist (if any) exist necessarily.

[14] That is, hypotheses which put a lower bound on the intended height of the hierarchy of sets (provided these seem to be coherently satisfiable together with the conception of the intended width of the hierarchy of sets above) tend to be regarded as true (or at least favored) rather than indeterminate.

[15] The contrary claim is used, for example as a premise in Sider's *Four Dimensionalism*[14] chapter 4 section 9. Note that if the arbitrary stopping point worry above is to be avoided, different options about the height of the hierarchy of sets will tend to come along with different (arbitrarily large) options for the total cardinality of the universe, not just different precisifications of how the term set is supposed to apply within a fixed total universe.

But they cash out set theory in terms of how it would be 'interpretationally' possible for a hierarchy of sets to grow, where this involves something like successively reconceptualizing the world so as to think and/or speak in terms of more and more sets (taller and taller actualist hierarchies of sets).

These latter ways of developing pontentialism are not automatically nominalist. However I think they can be developed to have reasonable (if not totally irresistible) claims to nominalistic acceptability. Linnebo and Studd's formalizations of set-theoretic sentences also avoid commitment to the existence of any abstract objects. For, recall that Linnebo and Studd formalize potentialist set theory as making claims about what's interpretationally possible: how one *could* talk or think in terms of various actualist hierarchies of sets. Such claims don't commit us to the actual existence of sets – or even to their metaphysical possibility[16]. To say that we could think or speak in terms of more layers of sets doesn't imply that we are currently thinking or talking in terms of any sets[17].

Indeed, one might even argue Linnebo[9] and Studd[16] face pressure to accept the nominalist claim that no sets (actually) exist, as follows. If our actual set theoretic talk is best understood potentialistically, then it seems natural to say that we aren't currently actually thinking in terms of any sets. But, in any case, Linnebo and Studd translate ordinary set-theoretic claims as saying things about how we could think in terms of more sets, rather than anything about what sets there actually are, so commitment to the existence of sets is avoided.

Thus, overall, I claim that the literature on potentialism provides powerful (not to say uncontroversial!) motivations for nominalism about set theory, that are quite different from classic philosophical arguments Burgess and Rosen criticise, and closer to the kinds of response to paradoxes which have driven pre-

[16]Interested readers can confirm that not only the paraphrases of standard set theoretic claims but the axioms proposed to justify these in [9, 16] don't carry any such commitment.

[17]See [9, 5, 16] for developments of potentialist set theory which answer to questions like, 'Are we to say that for any potential set X, if X were actual then we could consider a larger potential set? And does this commit us to the existence of potential sets?'

vious choices about foundations of mathematics. Furthermore, accepting this kind of nominalism about set theory can, in turn, provide some motivation for nominalism about other kinds of mathematical objects — though it's debatable how far this motivation goes. Obviously, if you identify all other mathematical objects with sets in the manner of Bourbaki, the inference is immediate. But more generally, it feels appealing to treat set theory and other mathematics similarly in some way, so adopting a potentialist (and therefore) nominalist logical regimentations for set theory provides some motivation to adopt nominalist understandings of other pure mathematical talk as well.

Thus, to sum up, we might avoid (or significantly reduce) the two problems for the emerging consensus response to Burgess and Rosen noted above, by saying something along the following lines. Puzzles about the height of the hierarchy of sets can be used to motivate understanding *pure* set theory nominalistically (and providing appropriate logical regimentations when speaking in foundational contexts). And this in turn (somewhat) motivates nominalism mathematical objects generally. Thus, if suitable nominalist paraphrases for all of applied math can be provided (contra Quinean indispensability worries)[18], Burgess and Rosen's dilemma will present no further problem about the intended status and motivations for this paraphrase.

5 Conclusion

In this paper, I've reviewed and developed what I take to be the emerging consensus answer to Burgess and Rosen's dilemma: appeal to the idea of different levels of analysis and explanation, with philosophy providing an extra layer of analysis 'below' physics, much as physics does below chemistry.

[18] See [5, 2] for discussion of general problems and strategies for providing such paraphrases (continuous with nominalist potentialism about set theory).

I've then argued that we can address certain problems for this view by supplementing it with a slightly different answer to Burgess and Rosen's challenge, which draws on an apparently useful distinction between what to say in foundational vs. non-foundational contexts *within mathematics*. I've further suggested that some (admittedly controversial) arguments for potentialism about set theory can motivate favoring nominalist logical regimentations in foundational mathematical contexts, in a way that's far closer to classic considerations in the foundations of mathematics than traditional philosophical arguments for nominalism.

Perhaps, given how intimately the birth of analytic philosophy was intertwined with interests in the foundations of mathematics, we shouldn't be surprised to get support from mathematical practice for the project of giving non-face value logical regimentations/analyses of our best theories.

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