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 (Burgess & Rosen, 1997).

In this paper, I'll review (what I take to be) a kind of emerging consensus response to this 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. I'll argue that one can address certain lingering worries for this approach by appeal to the apparent usefulness of a 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 challenges (to state our best theory of the world without quantifying over objects we don't believe in), mathematical nominalists have proposed elaborate logical regimentations of scientific theories which avoid quantification over mathematical objects. However, worries about the nature and meaningfulness of these nominalistic reformulations can be raised. In (Burgess & Rosen, 1997) Burgess and Rosen put this point forcefully

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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. Burgess and Rosen suggest 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 what scientists should say. For example, nominalistic regimentations of a physical theory would generally not be accepted by physics journals.

Thus, it might seem, typical nominalist logical regimentations of math and science (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 what I take to be an emerging consensus response to Burgess and Rosen's dilemma among untroubled friends of metaphysics (proposed by Hellman in (Hellman, 1998) among other places). This response suggests that metaphysics provides a legitimate further layer of analysis below physics, just as physics provides a layer of explanation and analysis below chemistry.

Although I find this reply appealing, I think it's sufficient to satisfy natu-

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

ralistic philosophers initially attracted to Burgess and Rosen's dilemma for two reasons. First, such naturalists will almost certainly reject claims that philosophy/metaphysics provides a legitimate further layer of analysis below physics (and the other sciences). Second, Burgess and Rosen supplement their main argument in (Burgess & Rosen, 1997) with specific criticisms of classic philosophical motivations for nominalism, which Hellman understandably doesn't try to refute in his book review (Hellman, 1998), but which a defender of nominalism along his lines would ultimately have to answer.

Accordingly, in this paper I'll propose a variant on the above emerging consensus response to Burgess and Rosen, which promises to address both these worries. In §3 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. And it suggests nominalists could reply to Burgess and Rosen by arguing that their non-face-value paraphrases of math and science are *continuous with foundational debates in mathematics*, rather than part of a metaphysical project which provides a legitimate layer of analysis below the sciences.

Then in §4 I'll suggest that certain arguments for potentialist set theory suggest a route to general mathematical nominalism could be used to flesh out the above proposal in a way that addresses some lingering worries). This potentialist motivation for nominalism is independent from the classic philosophical motivations Burgess and Rosen criticize. And it is (fairly) similar in character to the kinds of intra-mathematical motivations accepted in foundational mathematics – far more so than traditional philosophical motivations for nominalism. So, nominalist paraphrases motivated in this way can plausibly be claimed to form part of foundational mathematics (or something closely related to it)

So, overall, I'll suggest that nominalists could draw on the existence of foundational work inside mathematics (plus some more controversial ideas about set theoretic potentialism) to answer Burgess and Rosen's dilemma to put a more naturalist friendly spin on Burgess and Rosen's dilemma. Note that I won't try to assess familiar Quine-Putnam indispensability arguments challenging 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 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][4].

> **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,

[2]See (Berry, 2022) 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.

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

[4]See (Dorr, 2010; Sider, 2011; Hellman, 1998)

> and philosophical reasons are to be taken just as seriously as mathematical and scientific ones[5].
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> 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 (Field, 1980) 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[6].

As Hellman points out in (Hellman, 1998), the (metaphysics friendly) nominalist can cite an analogous 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,

[5]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 meta-scientific 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?" (Hellman, 1998)

[6]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.

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 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, plausibly, speaking this way in biology journals doesn't commit one to any position on 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 about heat on which their biological theory comes out true[7]. And 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

[7] So, for example, we might say this biologist is 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).

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's (Sider, 2011) suggests a nice (though optional) addition to this strategy for responding to Burgess and Rosen's dilemma. In (Sider, 2011), 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 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[8]. 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).[9].

Accordingly, 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

[8]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." (Sider, 2011)

[9]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." (Sider, 2011)

working mathematicians, physicists etc. should say or what they implicitly mean.

Thus, I take it, from a traditional pro-metaphysics point of view, Burgess and Rosen's dilemma isn't very serious.

However, a pair of worries remains. First, philosophers of a naturalist bent likely won't be willing to accept philosophy as providing a legitimate further layer of analysis below physics and the other sciences. For example, they might argue that philosophy's poor track record (its apparent failure at producing large scale agreement on the truth about major questions, compared to the sciences (Chalmers, 2015)) makes it unreasonable to regard it as providing a legitimate further layer of analysis below the sciences. Accordingly, they may suggest that the relationship of metaphysics to physics is more like the relationship 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* (Burgess & Rosen, 1997) Burgess and Rosen individually discuss and interestingly criticize all of the major traditional 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, a nominalist paraphraser who answers Burgess and Rosen's dilemma in the way sketched above would also have to address these individual criticisms[10].

[10]As an alternative to Sider's proposal, one could develop the basic proposal above in a more holist and (perhaps) naturalist-friendly way, by saying that philosophy is continuous with the sciences (rather than a project with distinctive aims of revealing metaphysically joint-carving natural kinds). Daly and Liggins' (Daly & Liggins, 2011) develops something like this approach. Daly and Liggins advocate favoring a form of naturalism that sees philosophy as continuous with the sciences and therefore treats philosophical arguments as counting for something. And they contrast this with "deference" naturalism, which draws a sharp distinction between philosophy and the sciences and says we should always favor the latter when there's a conflict.

I think this approach is attractive (given the famous difficulty of demarcating philosophy from the sciences) and reduces some naturalists' concerns. However, I don't think it's enough

3 Continuity With Foundations of Mathematics

In the remainder of this paper, I will argue that we can avoid -or significantly reduce - both the worries above (and so better satisfy naturalist interlocutors) by supplementing the above emerging-consensus reply to Burgess and Rosen's dilemma, with some considerations about foundational work in mathematics and the appeal of potentialist understandings of set theory.

In particular, in this section, I'll argue that we can see attempts to nominalisticaly 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 argue that certain (admittedly controversial) ideas from the literature on potentialist set theory suggest a motivation for mathematical nominalism that's particularly well suited to support the above claim of continuity. This new(ish) path to general mathematical nominalism is distinct from (and independent of) all the traditional philosophical motivations

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to satisfy most naturalists on its own. For note that the traditional philosophical motivations for nominalism (arguments from materialism, empiricism etc.) seem very different in character from paradigmatic arguments in mathematics and the sciences – whether or not we take there to be a crisp boundary between philosophy and science. And we don't need sharp disciplinary boundaries to think that (ceteris paribus) more some contentious bit of reasoning resembles reasoning with a good track record (like mathematics and the sciences) the more we should trust it, while the more it resembles reasoning with a bad track record (like astrology) the less we should trust it. So, track-record based motivations for skepticism about the classic philosophical motivations for mathematical nominalism remain.

Daly and Liggins acknowledge this worry and reply that nominalists can/should deny that mathematics (and mathematically infused sciences) have a better track record than philosophy, since they think people in these disciplines commonly assert false claims.

However, I'm suspicious about this reply for two reasons. First, I'd question whether nominalists (even if they reject the hermeneutic horn of Burgess and Rosen's dilemma) should agree that mathematicians and scientists commonly accept falsehoods. For, they could instead say mathematical/scientific talk is typically indeterminate or disjunctive in its ontological commitments (as I've suggested biologists' talk about heat flow typically avoids committing them to any position on the existence of caloric fluid). Second, even nominalists who agree that mathematicians and scientists assert many false claims which commit them to the existence of mathematical objects, tend to maintain that they simultaneously acquire many important true beliefs about *something else* (e.g., what Balaguer calls the 'for all practical purposes' truth of mathematical/scientific claims (Balaguer, 2023), or corresponding if-thenist versions of mathematical or scientific claims). And they can point to this accumulation of agreement on many significant truths (alongside many claims that are technically false because of their ontological commitment) as reason to think paradigmatic mathematical and scientific methods have a better track record than paradigmatic philosophical ones.

for nominalism that Burgess and Rosen criticize, and intuitively far closer in character to the motivations typically accepted in foundations of mathematical contexts. .

Accordingly (putting these two ideas together) I want to make the following basic proposal. Mathematicians accept something analogous to the abovementioned 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 there are familiarly mathematical (or closely related) motivations for favoring nominalistic logical regimentations of set theory (and thence perhaps of other mathematical talk) in these foundational contexts[11].

To begin to develop this picture, first note that providing foundations for various mathematical subdisciplines is already an accepted and apparently fruitful part of mathematical practice. 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. When doing such foundational work, mathematicians are allowed to employ logical formalizations that don't correspond to the surface grammar of sentences which would occur in journals devoted to the area of mathematics they are formalizing. And these logical regimentations of pure mathematical statements are allowed to be rather complex, like the logical regimentations of applied mathematics our critic is objecting to.

For example, consider the way that practicing mathematicians have pur-

[11]Accordingly (to relate my proposal more directly to Burgess and Rosen's dilemma) I claim a nominalist could say that they're making a revolutionary proposal about what should be said in *foundational* mathematical contexts – or at least something closely related (c.f. mathematical work on regimenting physics like Noether's theorem that 'every differentiable symmetry of the action of a physical system has a corresponding conservation law' (*Noether's theorem*, 2024)) – rather than a hermeneutic or revolutionary proposal about what's said in any kind of non-foundational mathematics or science journals.

sued 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.

This suggests that nominalists can (in principle) appeal to mathematical practice to justify their rejection of both horns of Burgess and Rosen's dilemma[12]. For, contemporary mathematical practice itself seems to clearly allow that there can be good mathematical reasons 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? (Given the dialectical aims of this paper) I won't try to say anything very insightful about this here. But roughly we might think of 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)[13]. But one lays oneself

[12]Chihara makes a somewhat similar point in (Chihara, 2007). I am indebted to his work but see the discussion of contrasts between my argument and Chihara's in a footnote below.

[13]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.

open to relatively pedantic or strange questions arising from (approximately) within mathematics, e.g., questions that connect very disparate parts of one's web of mathematical beliefs. And one tries to apply one's concepts crisply to these questions which might normally not be 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 ordinary 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 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 (and nominalists can somehow make a case for favoring their paraphrases in foundational contexts), nominalists can answer Burgess and Rosen. For, they can say they are advocating nominalistic paraphrases as the best thing to say in the special pedantic context of foundational debate. And if we accept all this (and take a Quinean approach to ontological commitment[14]) it seems only natural to say that our 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 non-foundational journals)[15].

But can nominalists plausibly argue that nominalist logical regimentations should be favored in the special contexts of foundational mathematics? In the next section, I'll argue that certain (admittedly controversial) ideas about potentialist set theory suggest a motivation for mathematical nominalism that's independent of classic philosophical arguments for nominalism and sufficiently similar in character to accepted motivations in the foundations of mathematics to make this plausible.

4 Potentialist Set Theory

Before explaining my proposed naturalist friendly route to nominalism, I must give some quick background on potentialist set theory and its motivations. In

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

[15] Chihara makes a somewhat similar proposal in (Chihara, 2007). There he appeals to the 20th century development of nonstandard analysis (a foundation for calculus using infinitesimals rather than standard definitions of continuity and limits) to argue that (contra Burgess and Rosen) logical re-regimentations can have significant illuminating power as well as pedagogical and research-inspiring value, without being either what mathematicians (or scientists) implicitly mean or what they normally ought to say.

Accordingly, it may be helpful to flag places where I take my proposal in this paper to differ from and (for some purposes) improve on Chihara's. I will mention two.

First, my story appeals to the legitimacy and naturalistic good standing of central and paradigmatic parts of foundational mathematical practice (mainstream foundations of analysis), rather than somewhat more controversial claims about the legitimacy of nonstandard analysis as a part of mathematics.

Second, Chihara uses his more controversial premises about foundations of mathematics to motivate a bolder reply to Burgess and Rosen which includes a kind of pluralism about mathematical foundation. Chihara's strategy is bolder in the sense that it would allow the nominalist to concede that their formalization isn't even what we should say in most foundational contexts. And it is pluralist in the sense that it presents nominalist paraphrases as one valuable perspective among many. In contrast, my proposal includes no such pluralism and thus can be more comfortably deployed by naturalists who want to claim a distinctive favored status for their nominalistic paraphrases.

I personally think Chihara's proposal is quite interesting. However, it's worth noting that we don't need any of Chihara's bolder claims (about the legitimacy of nonstandard analysis or foundational pluralism in mathematics) to answer Burgess and Rosen's dilemma.

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[16].

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 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[17]. 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

[16] 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.

[17] 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.

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[18].

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 reasoning about set theory is still truth preserving (Field, 1989)). However, these views face a challenge about accounting for common tendencies to favor taller over shorter interpretations of set talk[19] (and perhaps also about whether existence facts can be vague [20]).

Another option championed by figures like Putnam and Parsons (Putnam,

[18]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.

[19]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.

[20]The contrary claim is used, for example as a premise in Sider's *Four Dimensionalism* (Sider, 2001) 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.

1967) (Parsons, 1977) 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, 1967) Putnam 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 (Hellman, 1994) and Berry (Berry, 2018) 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 (Parsons, 1977, 2005, 2007) and recently developed by Linnebo (Linnebo, 2010, 2013, 2018) and Studd (Studd, 2019) takes 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. 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 potentialism 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[21]. 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[22].

Indeed, one might even argue Linnebo (Linnebo, 2018) and Studd (Studd, 2019) 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 the literature on potentialism provides a powerful (not to say uncontroversial!) motivation for nominalism about set theory[23], that are quite different from classic philosophical arguments Burgess and Rosen criticize, and closer to the kinds of response to paradoxes which have driven previous choices about foundations of mathematics.

Furthermore, accepting nominalism about set theory can, in turn, provide

[21]Interested readers can confirm that not only the paraphrases of standard set theoretic claims but also the axioms proposed to justify these in (Linnebo, 2018; Studd, 2019) don't carry any such commitment.

[22]See (Linnebo, 2018; Hellman, 1994; Studd, 2019) 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?'

[23]However, see (Berry, 2022), for more details and the reasons why I ultimately personally don't accept this argument and favor nominalist potentialist set theory but not nominalism about other kinds of mathematical objects.

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 mathematical talk – both in pure mathematics and the sciences. Thus, if (contra Quinean indispensability worries)[24] nominalist paraphrases of pure mathematics (and scientific theories that quantify over mathematical objects) can be found, one might think these should be favored in the context of mathematical foundations (or closely related reconstruction of mathematical reasoning in physics[25])[26]

Accordingly, I claim the above argument (from Burali-Forti to potentialism to nominalism) suggests a possible motivation for nominalist reformulations of set theory (and thence the rest of pure and applied mathematics) which can address both the worries mentioned in section 2. This motivation is independent from the traditional motivations for nominalism (via empiricism, materialism, Occam's razor etc.) which Burgess and Rosen provide in (Burgess & Rosen, 1997). And it intuitively resembles motivations given within foundations for mathematics sufficiently well to support claims that nominalist paraphrases are part of (or at least continuous with) foundational mathematics[27]. Hence, I think,

[24]See (Hellman, 1994; Berry, 2022) for discussion of how classic Quinean indispensability worries play out in the context of trying to provide nominalist paraphrases for scientific theories using mathematics that attractively cohere with potentialism about set theory.

[25]To motivate the continuity between foundations of mathematics and foundations of physics (and hence the existence of such foundations of applied mathematics), consider work like Noether's theorem(*Noether's theorem*, 2024)

[26]Note that both (Hellman, 1994), and (Berry, 2022)'s versions of potentialism about pure set theory naturally generalize to potentialism about set theory with ur-elements, and hence could plausibly be used to paraphrase scientific statements (if various familiar concerns from the Quinean indispensability literature can be answered).

[27]One might object that contemporary foundations of mathematics is entirely concerned with how various mathematical practices can be cashed out using Zermelo-Fraenkel set theory (as per the Bourbaki program), and therefore responds to motivations quite different from

this motivation has a decent chance of being acceptable to naturalists who would reject analogous claims for philosophical motivations for nominalism, which are more paradigmatically philosophical in character.

Admittedly, both traditional philosophical arguments for nominalism and my potentialist motivation can be said to resemble foundational mathematics in being concerned with answering 'pedantic and strange questions'. And perhaps some naturalists would be satisfied with that level of similarity (now that their attention has been directed to foundational mathematics as a specific area which can be claimed to be continuous with mathematics).

But I would say that *the kind of* pedantic or strange questions that motivate this path to nominalism (e.g. about Burali-Forti paradox worries about naive conception of the height of the hierarchy of sets) are intuitively similar to motivations considered in foundations of mathematics–in a way that traditional philosophical motivations are not. For example, both potentialist arguments for nominalism and classic analyses of continuity and limit in the foundations of mathematics aim to resolve a tension that arises within mathematical practice (where various prima facie appealing lines of mathematical reasoning turn out to conflict), by replacing a paradox-generating naive mathematical concept with something more precise that avoids this tension and can do the same mathematical work– but doesn't generate the relevant paradox. In contrast, many

the concerns about Burali-Forti paradox etc. that I've suggested could be used to motivate nominalism.

However, I would resist this picture of contemporary foundations of mathematics by noting the existence of lively contemporary debates about, e.g., whether set theory or category theory provides a better foundation for mathematics (Marquis, 2023). And, even among those who favor a set theoretic approach to mathematical foundations, there's continuing mathematical work on how to explicate the height and width of the hierarchy of sets (c.f. Woodin's idea that V=ultimate L (Woodin, 2017) and Hamkins' set theoretic multiverse (Hamkins, 2013)).

Accordingly, I claim, current foundations of mathematics doesn't just include attempts to interpret various areas of mathematics within set theory. It still includes lively debates about the best formulation of set theory, and other possible foundations for mathematics. This opens the door to arguing that the Burali-Forti based route to nominalism I've discussed resembles motivations currently accepted in foundations of mathematics (far more than traditional philosophical routes to nominalism do). I will try to make this case in more detail immediately below.

traditional philosophical arguments for mathematical nominalism arise from attempts to reconcile our understanding of mathematics with some powerful general principle (e.g. materialism, empiricism, Occam's razor) that's largely motivated by considerations outside mathematical practice.

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.

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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