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2024

On Ontological Innocence

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Princeton, NJ : Princeton University

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ON ONTOLOGICAL INNOCENCE

Alice van't Hoff

A DISSERTATION PRESENTED TO

THE FACULTY OF PRINCETON

UNIVERSITY IN CANDIDACY FOR

THE DEGREE OF DOCTOR OF

PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE

BY THE DEPARTMENT OF

PHILOSOPHY

Advisers: Gideon Rosen and Harvey Lederman

September 2024

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ON ONTOLOGICAL INNOCENCE

Abstract

The three essays that compose this dissertation are centrally concerned with the possibility of ontologically innocent quantification. Roughly speaking, quantified claims are ontologically innocent when they are consistent with the denial that there really are the entities over which they quantify. Quantification of this kind has a somewhat paradoxical quality. Nonetheless, I am inclined to think that ontologically innocent quantification is possible. The primary goal of this dissertation is to take the initial steps towards a defense of this claim.

The first chapter challenges a thesis I call **Equiv**. **Equiv** in effect says that quantification can never be ontologically innocent. I argue that accepting the ontologically pluralist claim that there are multiple ways to be implies the falsity of **Equiv**. This is important because there are reasons to think that proponents of **Equiv** should also be ontological pluralists. If so, then there's good reason to think that **Equiv** must be false simpliciter.

The second chapter is directed against those who argue that higher-order quantification is consistent with the thesis that it is possible to quantify absolutely unrestrictedly over everything. I argue that higher-order quantification is in fact at odds with absolutely unrestricted quantification, unless the relevant higher-order quantifiers are interpreted as ontologically innocent. Since the view that quantification *can* be absolutely unrestricted is *prima facie* attractive, the upshot of this conclusion is that we have reason to think that higher-order quantification might be ontologically innocent.

Although significant, the arguments of the first two essays don't yet explain *how* we might make sense of ontological innocence. My third chapter tries to develop the beginnings of such an explanation. The thought is that the difference between ontologically innocent and ontologically committal quantification might be connected with the *weightiness* of the entities over which the quantifier ranges. Reflecting on what it is to be parsimonious offers a way to unpack this idea: I argue that the entities over which a theory quantifies can differ in the contribution they make to the unparsimoniousness of a given theory. My hope is that this variation can in turn help to explicate differences in ontological *weightiness*.

Acknowledgments

I owe so much to so many. First, thank you to my advisers Gideon Rosen and Harvey Lederman, to whom I owe the deepest debt of gratitude. Their influence is writ large on every page on this dissertation. I am immensely grateful also to Boris Kment who read drafts of both the first two chapters; Kris McDaniel, Alex Skiles, and Eyal Tal who read and commented on a draft of the first chapter; and to David Builes and Hans Halvorson for helpful conversations that shaped Chapter 3 in particular. I wish also to underline my gratitude to David Builes and Boris Kment who, in their capacity as dissertation readers, produced pages of incisive, thought-provoking commentary on the initial draft.

I presented many of the ideas in Chapter 1 at the Metaphysical Mayhem Conference at Rutgers University, New Brunswick in July 2023 and I am thankful to all those who attended that presentation for their questions and insights. I owe particular thanks to Diego Arana, Jordan Bridges, Jonathan Schaffer, and Alex Skiles for organizing the conference and giving me the opportunity to present my work. Similarly, an earlier version of Chapter 3 was presented at the Eastern Division of the American Philosophical Association's 2022 Meeting in Baltimore, Maryland. I am especially indebted to Martina Botti who commented on my paper at that meeting and whose feedback decisively shaped my thinking on the topic. Subsequently, this work formed the nucleus of my job talk and I am grateful to all those who attended various iterations of said talk, including Jacob Barrett, Haley Brennan, Kieran Britt, Eliya Cohen, Hans Halvorson, Chris Howard, Brendan Kolb, Sarah Raskoff, Michael Smith, Eyal Tal, and Hannah Tierney. I also owe thanks to José Luis Bermúdez for his probing but

important question during a version of the talk I gave at Texas A&M University in March 2024.

I want to especially thank the other members of my PhD cohort and above all Bridget Brasher, Haley Brennan, and Brendan Kolb who have been steadfast sources of insight and advice during my time at Princeton. Finally, I am profoundly grateful to Eliya Cohen; so many of the issues I try to discuss in these essays arose in conversations we have had together over the past 6 years.

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Introduction

Ontological innocence is best introduced with an example. Consider the following claims:

(1) There's an absence of leadership at the company.

(2) There are several ways to tie a Windsor Knot.

These are perfectly ordinary and familiar assertions. But we don't generally think that absences or ways to tie knots are among the entities that compose our world. Indeed, it is a little difficult to see how the existence of absences is even possible. If we take these judgments seriously then sentences like (1) and (2) seem to be saying something false. Yet, we don't tend to think that claims of this kind can be falsified by reflecting on whether there could be absences. If I wanted to dispute (1) I would point to examples of leadership within the company. But it would be silly to object that (1) could never be true because absences don't exist. So our examples suggest a sort of prima facie puzzle: there are sentences that seem to say truly that there are some things, even though we don't really think there are things of the relevant kind.

We should not exaggerate: the puzzle is not a deep one. There are plenty of cases like this where we say true things using phraseology that might be misleading if interpreted hyper-literally. Unsurprisingly, then, a number of different resolutions to

puzzles of this kind are available. Perhaps the logical structure of the relevant claims comes apart from their grammatical structure. Or maybe these are just examples of loose talk, claims that we wouldn't assert if we were seriously trying to give a scientific account of our world. Alternatively, we might think that there is a way to reconcile the literal truth of sentences like (1) and (2) with the fact that there aren't *really* absences or ways to tie knots. The concept of ontological innocence finds its home in this last kind of strategy. Since there are (I assume) no true contradictions, the proponent of this kind of approach will have to find a way of distinguishing the sense in which there are absences from the sense in which there aren't. "Ontological innocence" names the feature of (1) and (2) that makes them (ostensibly) consistent with denying that absences or ways to tie knots exist.

We can make this precise by introducing additional ideology. We begin with the existential quantifier "$\exists$". It is routine to interpret this quantifier by saying that "$\exists x Fx$" is true when there is something that is F. Accordingly, claims about what there is/are are typically regimented using existential quantifiers.[1] Quantified claims are ontologically innocent if they are consistent with the denial that the things over which they quantify really exist and are ontologically committal otherwise.

As our discussion so far might suggest, there are real questions about whether ontological innocence is even coherent. The aim of the essays in this dissertation is to explore these and related questions. In general, I hope to lay the groundwork for a vindication of ontological innocence. In what follows, I'll briefly set out the motivations for a view of this kind, describe how the three essays that follow belong to this

[1] This is a slight simplification. Strictly speaking, "there is/are" sentences in English are not examples of natural language quantification. (See e.g. Szekely 2015, which interprets such sentences using a feature-placing ideology). Linguistics notwithstanding, I assume throughout that the intended philosophical content expressed by such sentences should be regimented quantificationally. This assumes that the logical and grammatical structures of the relevant claims can come apart.

project, and finally explore some consequences of incorporating ontologically innocent quantification into our philosophical toolkit.

Broadly speaking, there are two kinds of considerations that might incline someone to accept the possibility of ontologically innocent quantification, one ontological and the other metaontological. The first kind of argument for ontological innocence is likely to appeal to those who favor minimalist ontological views. Those with a taste for desert landscapes typically find themselves forced to tread a dialectical tightrope: they must find a way to adequately account for the phenomena that led others to posit a richer ontology without abandoning their own minimalism. The problem is that, in many cases, the truth of the relevant quantified claims falls outside the purview of the philosopher. Mathematics, for instance, generates existentially quantified theorems. Assuming, as I do, a broadly naturalist methodology that denies that the findings of natural science call out for philosophical justification, this creates a quandary for philosophers who are skeptical about the reality of mathematical objects. They lack the epistemic authority to challenge the quantified claims of the mathematicians, but, unless quantification can be ontologically innocent, the truth of the relevant claims seems to rule out the possibility of nominalist views that deny the reality of mathematical objects.[2]

This brings us to the second kind of motivation for ontological innocence. Philosophers with realist inclinations may also have reasons to think quantification need not always be ontologically committal. As I'm understanding it here, realists are committed to the idea that there are important metaphysical facts and that metaphysicians have a role to play in identifying these facts. If, though, quantification is invariably

[2] This recapitulates at a very high-level of abstraction what I take to be the key idea of Burgess and Rosen 1997.

ontologically committal, then the kind of methodological naturalism to which I just alluded tends to imply that there is little to no role for the philosopher. Natural science provides the true theories of our world and the ontology falls immediately out of the quantified claims implied by these theories. Philosophers need only get out the way. Some may see in this a reason to *favor* the view that quantification is never ontologically innocent, but for others the conviction that metaphysics has some independent contribution to make here gives us reason to adopt views that preserve what Kit Fine calls the "autonomy of ontology".[3] Ontologically innocent quantification makes room for this kind of autonomy by opening up the possibility that the truth of quantified claims need not yet settle the metaphysical facts.

A second kind of realism consists in the claim that there is some kind of independent reality that our theories are attempting to describe. Taking this idea seriously seems to suggest that what ultimately matters is not the truth of a certain claim but the reality *that makes that claim true*. Of course, we have no way of describing this reality outside of language. Nonetheless, if we think that what matters is not our assertions per se but their truthmakers, then we may be willing to acknowledge the possibility of an in-principle gap between the truth of certain claims and the features of the world that the relevant claims attempt to describe.[4] But a gap of this kind suggests that quantification might sometimes be ontologically innocent.

Let's turn then to the essays themselves. While there is a substantial thematic continuity between the essays and various points at which their arguments intersect, they are nevertheless intended as freestanding pieces. The first essay, "Ways of being quantified" takes up a question about the coherence of ontological innocence. To pose this

[3] Fine 2009, p. 159.

[4] Judgments like these tend to favor views that tie ontological commitment to truthmaking, see Cameron 2019. For a contrary view see Schaffer 2008.

question I introduce some vocabulary: let's say that ontology concerns *being* and that something *exists* if it is the value of a variable bound by an existential quantifier in a true quantified claim. Ontological innocence it turns out is coherent just in case it is not the case that to be just is to exist.

My main claim in this essay is that if we are ontological pluralists—that is, if we think there are various ways to be—then we cannot accept that to be just is to exist. This matters because there are reasons to think that we should be *quantificational* pluralists—to think that is, that a maximally perspicuous description of reality will contain multiple distinct quantifiers, corresponding to distinct kinds of existence. If to be just is to exist then if we are to be quantificational pluralists then we must be ontological pluralists also. It turns out, then, that if the argument of the first essay succeeds, quantificational pluralists must deny that to be just is to exist.

This argument has an important limitation: it assumes that there is a generic quantifier that ranges over things that otherwise belong to distinct ontological categories. But there are a number of contemporary philosophers who favor systems of higher-order quantification that rule out generic quantification of this kind. My second essay, "Against higher-order unrestrictedness", tries to fill this gap. I argue that those who favor the kind of higher-order view I just described—so-called type-theorists—must think that there is no way to quantify absolutely unrestrictedly unless they are willing to accept that higher-order quantification could be ontologically innocent. This matters because the possibility of absolutely unrestricted quantification is a presupposition of a number of plausible metaphysical views. As a whole, then, the essay suggests that, unless type-theorists are willing to make some unpalatable metaphysical concessions, they too should accept the possibility of ontologically innocent quantification.

Together, my hope is that these two essays lay the foundation of a defense of the coherence of ontologically innocent quantification. Yet although they imply that we should accept that quantification is sometimes ontologically innocent, they don't yet show us *how* ontological innocence might be possible. Put differently, establishing that quantification can be innocent doesn't yet put the notion in good standing: we are still owed an explanation of the difference between ontologically innocent and ontologically committal quantification and an explanation of why it matters. My third essay, "Parsimony and complexity" takes up this task, building on some passing remarks in the first essay.

The idea is that we might begin to shed some light on ontological innocence by connecting it to a kind of ontological weightiness. In essay 3, then, I argue that a true account of parsimoniousness should allow that different entities count to different extents towards the unparsimoniousness of theories. If parsimony is a measure of the kind of ontological demand imposed by a theory, then this gives us one way of beginning to cash out the idea that entities can differ in their ontological weightiness. Other things being equal, more "weighty" things impose a greater cost in terms of parsimoniousness. A particular attraction of this strategy is that it draws on what I take to be naturalistically respectable resources to explicate ontological innocence. This is particularly important because claims about innocent quantification often have a vaguely occult or unscientific quality that limits their appeal.

Together, then, I hope that these essays begin to lay the foundation for a plausible metaphysic of ontological innocence. They are, of course, limited in various respects. Essay 3, for example, argues only that entities differ with regards to the extent to which they make theories unparsimonious. The next stages of this argument, connecting

differential contributions to parsimoniousness to genuine ontological differences must wait for another day. Nonetheless, their incompleteness notwithstanding, there is, I submit, an intriguing tangle of metaphysical issues connecting the essays. To take one example, a theme in both the first and second essays is the way that ontological innocence may help us to make sense of debates about the possibility of a completely unrestricted quantifier, ranging over absolutely everything. If we can make sense of ontologically innocent quantification, then we have available to us a view that holds that while it is always possible to quantify over an even broader domain (in which case no quantifier is absolutely unrestricted), it *is* possible to quantify unrestrictedly over all the things to which we are ontologically committed.

Chapter 1

Ways of being quantified

According to philosophical orthodoxy the study of ontology is importantly connected to quantification. Although dominant, this view is hard to state precisely because, by the lights of its proponents, it is a trivial tautology. To articulate the view as a substantive thesis, we introduce two terms: "being" which names the subject matter of ontology and "existence" which names the status G possesses when G is in the range of an existential quantifier (i.e. when $\exists x Gx$). (I'll use "having being" and "existing" for the corresponding verb forms.) Importantly, I am not understanding the stipulated content I have just attributed to "being" as a *definition*. Instead to avoid tendentiousness, I mean to leave the question of definition open and thus to use "being" in a somewhat open-ended way. I take it that we can nonetheless grasp the term sufficiently for present purposes by specifying "being" ostensively, by reference to our practice of ontology.[1]

Granted these stipulations, the orthodoxy that I want to investigate becomes the claim that having being just is existing. Call this **Equiv**. In practice, the proponents of

[1] I assume here that we have some prior grip on "ontology". We are, for example, able to sort philosophy books (at least roughly) into ontological and non-ontological categories. James Woodward's *Making Things Happen* is not a book about ontology; Peter van Inwagen's *Material Beings* is.

Equiv typically adopt a stronger thesis—**Equiv***—according to which it is a conceptual truth that to have being is to exist. **Equiv*** is plausible in part because it is hard to see *how* there could be a conceptual difference between being and existing. We grasp ontology, the thought goes, only because we implicitly tie it to quantification. Alternatively, we might favor **Equiv*** because, like Quine, we see connecting quantification and ontology as the best alternative to views that treat singular terms as the locus of ontological commitment. (An approach that seems to make claims like "Pegasus doesn't exist" somehow incoherent.)[2]

This paper focuses in particular on a tension between **Equiv*** and the view that there are many ways to have being, which is known as "ontological pluralism" or "pluralism" for short.[3] Paradigmatic forms of the view hold that physical and mental things each have being in their own unique way, or that God has being in a way that mere mortals do not. As these examples suggest, pluralism has been historically influential—it was plausibly the view of Aristotle, Aquinas, Descartes, Frege, and Russell among others—and it is currently enjoying a mini-resurgence.[4] For the purposes of this essay, though, I take no stand on the merits of pluralism. As we'll see shortly, there are reasons to be interested in pluralism in relation to **Equiv**/**Equiv*** regardless of the attractions the view may possess.

The tension between **Equiv** and pluralism may seem unsurprising since it looks as though there is a quick argument from ontological pluralism to the falsity of **Equiv**. The thought is that while there is only one kind of existential quantification, according

[2] Quine 1948, p. 22. Rayo and Yablo 2001, p. 82 attribute to Arthur Prior the converse view that quantificational claims are only as committal as their instances. Cf. Prior 1971, pp. 36–8.

[3] Given the stipulation I introduced above this is equivalent to saying that ontological pluralists believe that the subject matter of ontology is irreducibly plural. We should understand the foregoing *de re* as the claim that the thing(s) that is(/are) the subject matter of ontology is(/are) irreducibly plural.

[4] Historical discussion can be found in Turner 2010, n.3, p. 15, McDaniel 2017, pp. 12–47, and Caplan 2011. These three works are part of a growing contemporary literature on pluralism, which includes also Builes 2019, Merricks 2019, Rettler 2020, and Turner 2012.

to pluralism there are various ways to have being. Thus being and existing must be distinct.[5] But this argument is *too* quick: proponents of pluralism have a simple response. They can argue that in a metaphysically perspicuous framework there are many kinds of existential quantification just as there are many ways of being (see Turner 2010, pp. 7-8ff; Turner 2021, p. 185). This dissolves the quick argument against **Equiv**.[6] Yet as we will see, there is a second, more subtle, route from pluralism to the falsity of **Equiv** that cannot be answered in the same way.

This conclusion matters in part because there are good reasons to take seriously views which insist that an adequate metaphysical framework must employ multiple quantifiers. (One such reason derives from the so-called extensibility arguments favored by those who deny that it is possible to quantify over absolutely everything (e.g. Linnebo 2010), since on this view for any given quantifier it will be possible to construct a distinct quantifier with a yet larger domain (see Fine 2006). A second reason arises for those who favor higher-order approaches to metaphysics that comprise multiple quantifiers ranging over distinct types (Fritz and Jones 2024).) Given **Equiv** such views will lead to ontological pluralism. For if being just is existing, then if there are many kinds of existential quantification, and therefore (by **Equiv**) many ways to exist, it follows that there are many ways to be, which is the defining tenet of ontological pluralism. If the arguments of this paper succeed, though, this combination of views is unstable.

In other words, if metaphysics requires that we theorize using multiple quantifiers, then **Equiv** must be false and there are reasons to think that metaphysics might indeed impose such a requirement. Although the conclusion of this chapter is conditional,

[5] See e.g. van Inwagen 1998 for a version of this idea.
[6] Turner 2012, p. 420.

then, I don't take its dialectical force to be contingent on the plausibility of the ontological pluralism. If (as I assume) metaphysics requires a multiplicity of quantifiers, then the defender of **Equiv** cannot consistently reject ontological pluralism. To show that their view is also inconsistent with ontological pluralism is therefore to show that **Equiv** is false. For reasons that I'll briefly elaborate on below, the falsity of **Equiv** counts against certain deflationary views about metaontology. Accordingly, while ontological pluralism might seem like a substantive commitment on which to object to deflationary metaontogical views, appearances notwithstanding, I take myself to be giving something close to an immanent critique of such views for the reasons elaborated in this and the previous paragraph.[7] The big picture, then, of which this paper is just one part, suggests both that **Equiv** is false and that its falsity has important metaontological implications.

1.1 Being and quantifying

Equiv and **Equiv*** are inspired by a Quinean approach to metaontology that is orthodox in contemporary analytic metaphysics. In a slogan, the Quinean proposal is that to be is to be the value of a variable bound by an existential quantifier (Quine 1948). This idea turns out to be open to interpretations with importantly different emphases. In this section, I isolate the interpretation in which I am interested and suggest why it might conflict with pluralism. In what follows I will use the term "ontological commitment" to mean commitment to some entity's having being.

[7] More precisely, I require one substantive "external" premise: namely that a perspicuous metaphysical framework must comprise multiple basic quantifiers.

Let's start by considering a specific question—whether or not numbers belong in our ontology. Euclid's theorem states that there are infinitely many prime numbers. According to a view defended by John Burgess and Gideon Rosen, who understand themselves to be employing a Quinean approach, our ontological question is therefore answered by ordinary mathematics. Euclid's theorem quantifies over primes, prime numbers are numbers, so it follows from the theorem that there are numbers (Burgess and Rosen 1997). Of course, there are alternative responses available: we might treat mathematical discourse as non-literal or insist that Euclid's theorem isn't strictly speaking a theorem at all, even though we can regard it as such in all non-metaphysical contexts. Burgess and Rosen offer a critique of these approaches based on a kind of methodological naturalism. The nub of their proposal is that what matters for the evaluation of mathematical claims are the standards employed by mathematicians. Since neither of the aforementioned metaphysical alternatives has met with significant uptake in mathematical contexts neither clears the naturalist's threshold for belief. From the perspective favored by Burgess and Rosen this is enough reason to set such alternatives aside. I'll assume here that this critique succeeds (for a more detailed discussion, see note 8); the following arguments therefore assume that the truth of Euclid's theorem gives us a prima facie powerful reason to admit numbers to our ontology.[8]

[8] Paradigmatic forms of methodological naturalism hold that philosophers ought to defer to the epistemic practices of natural science. It is unobvious, though, what kind of deference the naturalist owes to mathematics. Mathematics is clearly not an empirical discipline, yet at the same time it is something that scientists seem to take seriously. There are two standard ways to resolve this question: one takes the legitimacy of mathematics to piggy-back on the authority of natural science. Mathematical entities and methods are confirmed insofar as they are "indispensable" to natural science. The second approach attributes epistemic autonomy to mathematics, whose conclusions are taken to need no independent justification, via natural science or otherwise. Burgess and Rosen implicitly rely on a version of the second idea. For discussion of these issues see Colyvan 2024; Maddy 1997; Maddy 1998; Sober 1993.

For reasons I won't explore here, both of these approaches seem to me to be limited. I prefer a hybrid view that is intermediate between these two poles. Mathematics, we can say, is authoritative because

Ted Sider suggests a slightly different way to approach such questions. While he too favors a Quinean methodology, he introduces an important complication, observing that our evaluation of theories is sensitive to the language in which they are formulated. We should focus, then, on theories articulated in a *joint-carving* language, where a language is joint-carving to the extent that its expressions correspond to perfectly natural features of the world. Such a language might include the predicate "electron" but will not include predicates like "grue". Sider argues that it is not only predicates that can be more or less joint-carving, quantifiers can also vary in this regard (Sider 2011, pp. 85–104). Just as gruesome predicates don't help us to describe the world as it really is, so un-joint-carving quantification could also fail to be ontologically perspicuous. Quantification, he concludes, is ontologically committal only when the quantifier in question is joint-carving.[9] This approach seems to leave open a different kind of answer to that defended by Burgess and Rosen: maybe mathematical theories are not articulated in a joint-carving language. In that case, the truth of various mathematical theorems need not settle the ontological question.[10]

Burgess and Rosen are skeptical of this approach: summarizing considerably, they suggest that there are two ways of thinking about what someone who pursued a strategy like Sider's to argue that there aren't really any numbers might be doing. According to them, a philosopher of this ilk is *either* arguing that mathematicians don't really

scientists take it to be. As per the first of the approaches surveyed above, it does indeed derive its epistemic credentials, ultimately, from natural science. (This constitutes my answer to Rosen's "authority problem", Rosen 1999, 471ff.) Crucially, though, in deferring to mathematics, natural scientists do not insist that we ought to apply empirical standards to mathematical objects. As in the second view, then, we get to say that the appropriate standards for mathematical knowledge are whatever standards mathematicians themselves employ. Like Burgess and Rosen, then, I am apt to think that philosophers are not in a good position to make revisionary claims about the truth or meaning of mathematical theorems. Cf. Lewis 1991, pp. 58–9.

[9] Sider 2011, especially p. 202.

[10] Sider doesn't *actually* make this claim; the point is rather that his metaontological approach opens up logical space for an argument of this kind.

mean to say that there are infinitely many primes *or* means to suggest that a *mathematically* better theory wouldn't say that there are prime numbers. An alternative view that accepts the mathematical claim taken at face value, but denies that it is ontologically committal is not, according to Burgess and Rosen, a viable option. In particular, they worry that this view relies on a distinction without a difference (Burgess and Rosen 1997, pp. 188–9). They hold, that is,that there is no intelligible content that can be given to the difference between the sense in which there are (i.e. exist) numbers—given the truth of the relevant mathematical theories—and a deeper sense in which there aren't. As I interpret them, this amounts to skepticism that there are any ontological facts if that means anything more than that some quantified claims are true. (It's worth noting in passing that although the Burgess and Rosen view is not universally held, it is also by no means idiosyncratic: see the following footnote for details.[11])

The important point is that the Quinean approach—and therefore also **Equiv**—is susceptible to different interpretations, some of which are more deflationary than others. Crucially, whereas Sider's approach allows for the possibility of ontologi-

[11] See e.g. Burgess 2008, p. 92 which dismisses Jody Azzouni's notion of ontologically innocent quantification (Azzouni 2004) as "self-contradictory"; Hazen 1993, (especially p. 136) which gives an argument for platonism from the presumption that quantifiers are ontologically committal expressions; Lewis 1990 which reads Routley's would-be Meinongianism as intelligible only under the supposition that quantification which Routley regards as "neutral" is in fact [ontologically] "loaded"; Parsons 1990 which argues against the innocence of second-order quantification by appeal to "an ontological intuition a little different from but complementing Quine's" (p. 328); van Inwagen 1998, pp. 235–6 which argues that being and existence are identical; Florio and Linnebo 2021, pp. 164–7 which argues against the innocence of plural quantification by appeal to rival conceptions of ontological commitment both of which take the quantification-ontology tie for granted; and Lycan 1979, p. 290 wherein the author reports his inability to understand "Meinongian"—i.e. ontologically non-committal—quantification.

There are, of course, alternatives to this kind of austere Quineanism. Schaffer 2009, for instance, favors what he regards as a neo-Aristotelian view on which existence assertions are typically true but trivial, while interesting questions about what is fundamental remain. I favor my own approach here because I take it to be closer to an immanent critique of deflationism (see page 19 above). One way to see this is by noting that while both Schaffer and I require a commitment to distinctions of fundamentality, I understand such commitments to be implicit in ontological pluralism (see page 25 below), which I in turn take to be mandatory for proponents of **Equiv** if metaphysics requires a multiplicity of quantifiers. By contrast, Schaffer's view relies on an independent grounding-theoretic ideology to motivate such distinctions.

cally innocent quantification—quantification that is not ontologically committal—the Burgess-Rosen view does not. My interest in this paper is in the stronger interpretation of **Equiv** and **Equiv*** suggested by Burgess and Rosen. On this picture, **Equiv** has deflationary implications that are likely to be uncongenial to philosophers of an ontologically realist disposition. To see this, suppose a theory T in biology, say, quantifies over some entity e. If Burgess and Rosen are right, ontological debate about the status of e will have to be conducted either *among biologists* by giving rival theories to T or *among linguists* by arguing that T doesn't really mean what it seems to say and that what T *really* says is consistent with the negation of what it *seems* to say.[12] (The prospects for advocates of the second kind of approach are particularly poor.)[13] There is no scope, then, for distinctively metaphysical or ontological debate about e.[14] I take this to be a deflationary metaontological conclusion and moreover to be a kind of deflationism that is not necessarily rebutted by views like Sider's.[15] For proponents of this view, ontology is little more than a "glorified taxonomy".[16]

The intuition on which this paper is based is that ontological pluralists are committed to treating ontological distinctions as thick and substantive in a way that is inconsistent with radically deflationist views like Burgess and Rosen's. To see why, consider a toy-example of a pluralist view: suppose that concrete things like my little finger and

[12] Strictly speaking there is an intermediate step here. A philosopher could accept that the truth of quantified claims exhausts ontology while maintaining that we have philosophical reasons to reject the theories of biology, perhaps such theories are merely useful fictions. Here, however, I am assuming a naturalist constraint that rules out views like these.

[13] Burgess and Rosen 1997, pp. 231–6.

[14] This constitutes a rejection of what Kit Fine calls the "autonomy of ontology", denying the coherence of an approach that allows that settling the claims of science and mathematics in a rigorous quantified language need not yet settle questions about what things have being (Fine 2009, p. 159).

[15] Sider has arguments for thinking that we should extend the notion of joint-carvingness to quantifiers—Sider 2011, pp. 86–7—but these may not be persuasive to adherents of the Burgess-Rosen view, since by and large these arguments assume a commitment to the substantiveness of certain ontological or metaphysical debates that may not be compelling to philosophers of a deflationist inclination.

[16] Burgess 2008, p. 95.

abstract things like $\sqrt{2}$ have being in different ways. The pluralist must insist that this proposal differs from the ontologically monist view according to which both $\sqrt{2}$ and my little finger have being in the same way, although the former is abstract while the latter is concrete. Put differently, the pluralist claims that we can make sense of the idea that there are different modes of being and that realizing some mode of being is distinct from merely instantiating a metaphysically interesting property (like being abstract). Pluralism thus commits its proponents to a substantial ontological thesis. My thought is that this commitment is sustainable only if the world is ontologically richer than **Equiv*** allows.

1.2 The conceivability argument

The main argument of this paper can be summarized as follows:

1. If pluralism is true, then there is a non-fundamental, general existential quantifier such that it is not a conceptual truth that this quantifier is a disjunction of the more fundamental quantifiers.

2. If there is a non-fundamental, general existential quantifier such that it is not a conceptual truth that this quantifier is a disjunction of the more fundamental quantifiers, then it is conceivable that a quantifier quantifies over things which do not have being in any fundamental sense.

3. If it is conceivable that a quantifier quantifies over things that do not have being in any fundamental sense, then ¬**Equiv***.

4. ∴ If pluralism is true, then ¬**Equiv***.

Below I defend the premises of this argument. Before doing so, however, it is worth saying something about the ideology in which they are couched. All involve the notion of fundamentality, which is contested in metaphysics and may not be acceptable to all.[17] Yet we need to be able to talk in terms of fundamentality if we are to explore the merits of ontological pluralism. For given an arbitrary quantifier "$\exists$" and a predicate "P" we can always define a restricted quantifier "$\exists_P$". We get a form of pluralism worth discussing only if we say that the more specific quantifiers introduced by the pluralist are not just restrictions of a universal existential quantifier, which in this context is cashed out by saying that the specific quantifiers are more fundamental than any broader existential quantifiers.[18]

The other substantial primitive is "conceivability". Importantly this term should not be understood psychologistically, it describes in the first instance not the feeling that some scenario is credible or epistemically plausible, but rather a kind of modality; to be conceivable is to be conceptually possible. The bounds of conceptual possibility are in turn understood to be fixed by (logical) consistency with facts about what it is to fall under a given concept. An upshot of this is that there is little difference between conceptual truth and truth by definition, assuming (plausibly) that to define a concept is to describe what it is to fall under that concept (see note 25 below). As I am understanding it, then, it is conceivable that Catalonia might be independent or that Ian McKellen might win an Oscar, but inconceivable (*de dicto*) that spinsters might

[17] For discussions of fundamentality see: Bennett 2017, pp. 102–86, Fine 2001, Fine 2009, McDaniel 2017, pp. 195–222, Schaffer 2009, Sider 2011, pp. 105–65, and Wilson 2014. More generally, see Takho 2018 for an introductory survey.

[18] It's worth noting that the issue at hand does not concern fundamental *things*, but rather whether things, fundamental or otherwise, have being in a fundamental sense. "Fundamental" modifies modes of being and not the things that have being in some particular way. Non-fundamental things could conceivably have being fundamentally speaking and vice versa. See section 1.5 below.

be married. By stipulation, T is a conceptual truth iff it is not conceivable that not T; conceptual truth is thus the dual of conceivability.

1.3 Articulating pluralism

My task in this section is to argue for the first premise. Let's start by considering in general terms what it is to be a pluralist about any kind of subject-matter. Plausibly, to count as a pluralist view about a topic X, a theory must hold *both* that there are multiple versions of Xness, but also that *each* such version counts fully as a kind of Xness. A pluralist about knowledge, for instance, must maintain that there are multiple kinds of mental states each of which has a valid and equal claim to being *knowledge*. Otherwise, the pluralist view in question will collapse either into the eliminativist view that there is no such thing as Xness or the monist view that there is just one way to be X (although there may be other closely related phenomena in the vicinity). The same requirement arises for the ontological pluralist. If there are many ways of being (as the ontological pluralist requires), then these ways of being must have something substantively in common in virtue of which they count equally as ways of *being*.

A further question is whether the commonality holding between the various specific ways to have being itself gives rise to a generic way of being. There is reason to favor an affirmative answer to this question. For one thing, this is the line that pluralists themselves seem to have generally preferred.[19] A comparison with alethic

[19] It's worth noting that there is not universal agreement among ontological pluralists on this point: Caplan 2011 rightly draws our attention to the diversity of different forms of pluralism. In his metaphor (which analogizes modes of being to boxes) only some pluralists accept that their various ontological boxes must be contained within a generic ontological box. I think that my argument should put pressure on those views that reject a generic form of being *insofar* as they want to be pluralist about the thing of interest to ontologists. It is not obvious, though, that all the historical figures discussed by Caplan satisfy the last condition. See also note 23 below.

pluralism, the view that there are many ways to be true, helps to show why they might have taken this approach: suppose that there are multiple truth properties, $F_1, ..., F_n$, and further that there is some important respect, T, in which $F_1, ..., F_n$ resemble one another.[20] We can use the higher-order property T to define a more general kind of property F_T, where something instantiates F_T iff it instantiates a property F_i that satisfies T.[21] We can now ask whether F_T should count as a form of truth. The answer to this question should again be affirmative: after all, anything that instantiates F_T is true and it is their satisfying T that constitutes the commonality unifying $F_1, ..., F_n$. We might hesitate here because if we allow that F_T is a form of truth, our view may seem to collapse into a form of alethic *monism*. The natural solution for pluralists is to allow that F_T is a kind of truth, but to insist that it is less fundamental than $F_1, ..., F_n$; it is the latter that are the fundamental alethic states on this picture and since they are multiple, this view counts as a kind of pluralism.[22]

Analogously, it seems that the ontological pluralist should allow for the existence of a less fundamental, generic way of being that includes each specific way of being.[23] The generic way of having being is simultaneously less fundamental than the more specific ways of being and also the commonality that consists in each more specific way of having being being a way of *having being*.[24] Importantly, this generic notion

[20] This corresponds to what is known as "moderate pluralism". So-called strong pluralists deny that there is any unity to the various truth predicates. But strong pluralism faces powerful objections and, for this reason, is not widely believed. See Pederson 2012, pp. 590–2.

[21] F_T might correspond to what Wright calls "minimal truth", Wright 1992, chapter 2.

[22] See Pederson 2010, pp. 106–8 for a view of this kind.

[23] A different possibility is the view that Builes 2019 calls "hybridist", which holds not that the generic way of being is *less* fundamental than the more determinate ways of having being, but that they are all *equi*-fundamental. The hybridist view, though, has significant problems of its own. As Builes points out it involves redundancy at the fundamental level, thus violating a non-redundancy constraint that many philosophers have found plausible. Moreover, hybridism is also vulnerable to several other objections developed by Merricks 2019.

[24] The foregoing line of argument is supported by some ideas developed in the recent literature on pluralism, where it is widely agreed that pluralists need a generic notion of being—see e.g. Merricks 2019; Builes 2019; Rettler 2020. A different kind of pluralist, however, might not accept this claim. According to what we might call neo-Fregean pluralism, the true theory of metaphysics must be given

must also be more than a mere disjunction of the various specific ways of having being. (As I am using the expression, some concept X is a "mere disjunction" just in case it is a conceptual truth that to be X is to instantiate one or more disjuncts of a given disjunction that otherwise have nothing substantial in common.) For mere disjunctions are not enough to establish genuine similarity of the kind required here. Jacques Chirac and the Talladega Superspeedway share the property of being either Chirac or the Talladega Superspeedway, but this alone does not establish that they have anything metaphysically interesting in common. Similarly, if we want to be pluralists about being, it cannot be a conceptual truth that the generic notion of being is a mere disjunction of specific ways of having being, since a mere disjunction of this kind does not count as a genuine commonality holding of the various ways of being (cf. McDaniel 2017, p. 25).[25]

The next stage of the argument involves a kind of reductio. One way to show that pluralism and **Equiv** are inconsistent is to show that if we assume **Equiv**, then

in a higher-order language in which entities belong to specific, disjoint "types" (see e.g. Lederman 2024). In this framework, no two things in different types are in the scope of a single quantifier. Given something like **Equiv***, we get a radical form of ontological pluralism, but the very structure of the view rules out cross-type quantification and thus any generic notion of being. Views like these have problems of their own—the rejection of cross-type quantification imposes powerful expressive limitations. Nonetheless, they are important and interesting in their own right. Since, however, this kind of type-theoretic approach raises a whole host of further issues, I am setting them aside in the present discussion. See chapter 2 below for further discussion of these issues.

[25] A reader might object here: being a cousin is not defined as *being either a first cousin or a second cousin, ...* yet the concept of "being a cousin" is not wholly disjunctive. But it is a conceptual truth that anyone who is a cousin is either a first cousin or a second cousin,...etc. Likewise, even if the generic notion of being isn't *by definition* a disjunction of more specific forms of being, it might still be a conceptual truth that to be generically is also to have being in some more specific way. This objection invites us to distinguish between definitional and conceptual truths. (Cf. Fine 1994, especially pp. 2–3, pp. 13–14, and Rosen 2015.) The idea is something like the following: it is true by definition that to be X is to be Y just in case it is part of the ground of a thing t's being X that t is Y.

This objection does not succeed in the present context: if it is a conceptual truth that something has being generically iff it has some specific way of being, then this must hold in virtue of facts about what it is to fall under the relevant concepts: either t has being generically because this is part of what it is for t to have being in some specific way or vice versa. The second possibility is inconsistent with pluralism (it collapses into a kind of ontological monism), so the pluralist must favor the first possibility. But this closes the gap between definitional and conceptual truth suggested by the objector.

given pluralism, ¬**Equiv**.[26] Accordingly, following the ideology favored by both pluralists and their critics, we can appeal to **Equiv** here to conclude that ways of being are expressed quantificationally. It follows that some G has being$_1$ iff $\exists_1 x Gx$, where the subscripted "1" stands for one of the various ways to have being. A generic way of being is thus expressed by a *general quantifier*. The foregoing argument therefore suggests that:

1. If pluralism is true, then there is a non-fundamental, general existential quantifier such that it is not a conceptual truth that this quantifier is a disjunction of the more fundamental quantifiers.

But this just is premise 1 from the argument of section 1.2.

1.4 Conceiving of being

This section defends premise 2:

2. If there is a non-fundamental, general existential quantifier such that it is not a conceptual truth that this quantifier is a disjunction of the more fundamental quantifiers, then it is conceivable that a quantifier quantifies over things which do not have being in any fundamental sense.

We can begin to motivate this claim by considering cases of "worldly indeterminacy". If there is indeterminacy of this kind, then some entity e could conceivably be blue without being any specific shade of blue. For we can imagine that e's color corresponds to a region of a multidimensional color space (say, hue × saturation × brightness) but

[26] Thank you to Boris Kment for this suggestion.

not to any particular point within this region (Rosen and Smith 2004, p. 188). Cases like these introduce the unfamiliar idea that a determinable—in this case "blue"—could conceivably be instantiated without any of its determinates—here, particular *shades* of blue—being instantiated.[27] The idea underpinning premise 2 is analogously that, if the general quantifier is as premise 1 requires, then something could conceivably have being generically without having being in a fundamental sense. Importantly here we should eschew interpretations of "having being in a fundamental sense" which rigidify that notion to the ways of having being fundamentally speaking that are *in fact* instantiated. For it seems conceivable that there could be various fundamental kinds of being, even if those kinds of being aren't realized in reality—the conceivable outstrips the real. The intended interpretation of premise 2 therefore requires a stronger reading on which something could conceivably have being in a fundamental sense even if it is not in the domain of one of the quantifiers that *actually* correspond to fundamental ways of being.

The analogy with determination is useful because it helps to exemplify this stronger reading and thus to highlight why premise 2 might, on first inspection, strike you as surprising.[28] My burden is to show that if the antecedent of 2 is instantiated then something could conceivably have being generically (i.e. determinably), without having being in any specific (i.e. determinate) sense. From some perspectives the consequent of this conditional is literally contradictory—for instance, for those who understand determinables as disjunctions of their determinates. Even according to weaker views, it is common to assume the impossibility of being determinably *F* with-

[27] Cf. Wilson 2013 who espouses a theory of indeterminacy along these lines.

[28] Note though that ideas like premise 2 are not completely unprecedented. Chapter 5 of McDaniel 2017, defends a similar thought.

out instantiating any determinate of F in part defines the relation of determination.[29] In light of this, it's worth highlighting the conditional character of my argument: my claim is not that we should accept the possibility of metaphysical indeterminacy simpliciter, but rather that pluralists wedded to **Equiv*** must accept such a possibility. We might see this either as a reductio of pluralism or as a surprising consequence of the view. (Assuming they accept premise 1 of the argument, those who favor the first response, should immediately accept the conclusion of the conceivability argument—if pluralism is true, then **Equiv*** is false—as trivial.) Consistent with this, I will argue for 2 in timeworn fashion by assuming its antecedent and then trying to show that its consequent—which requires that it is at least conceivable that something could be determinably F without being determinately F—follows.[30]

Let's begin, then, by distinguishing three views of determinable: on one view, determinables are reducible to their determinates. I take this to mean that determinables are somehow constituted by their determinates in a manner analogous to the composition of wholes by their parts. Standard versions of this idea identify determinables with disjunctions of their determinates. I'll call the kind of determinables described by this view determinables$_1$. A second view takes determinables to be non-reducible to their determinates, but insists that the extension of a determinable is necessarily identical to the extension of the maximal disjunction of its determinates. Call determinables

[29] For views of the first kind see the discussion of reductionism in Wilson 2023; for views of the second kind see e.g. Funkhouser 2006, p. 549.

[30] As may be apparent, it seems to me that to reject this possibility out of hand is unwarranted. Skepticism about metaphysical indeterminacy is one thing, denial of its conceivability is another, far stronger, thing. This is especially true given that there are a number of prima facie coherent examples of how such indeterminacy might arise: Fales 1990, p. 228 argues that certain mental states may be determinably but not determinately F: I imagine a hen is speckled without imagining it has some number n of speckles; Wilson 2013, p. 371 notes that a particle in a quantum superposition might be interpreted as instantiating a determinable spin property (for instance), without instantiating any of its determinates. Such cases seem powerful to me. Nonetheless, as I note above, sharing this judgment is not a requirement for being persuaded of my argument for premise 2.

satisfying this analysis determinables$_2$. Finally, a third view takes determinables to be non-reducible and further holds that the extension of a determinable could conceivably differ from the extension of the maximal disjunction of its determinates. Call things of this kind determinables$_3$.

Assuming now the truth of its antecedent, to establish premise 2, I need to show that generic being is a determinable$_3$. Consider first an argument to the effect that generic being is not a determinable$_1$. The argument here picks up a thread from section 1.3: I pointed there to a tension facing the ontological pluralist: they must insist both that there are multiple kinds of being and that these kinds nonetheless deserve a common name. A partial solution is to allow that there is a generic kind of being, while maintaining against the monist that generic being is less fundamental than other ways of being. This solution is only partial though because if the generic kind is to constitute a genuine form of commonality, as the pluralist requires, then it must be non-arbitrary. To be a pluralist about some concept X, in the sense that the ontological pluralist is a pluralist about being, you must recognize some substantial commonality that unites the different ways to be X. Most of us, for instance, are not grue-pluralists in this sense, since we don't think grue is a "real" property (assuming here a moderately sparse conception of properties). Yet there is presumably some collection of things all of which are either green and observed before the year 2000 or blue. The problem is that grueness is insufficiently joint-carving. Generalizing this idea, it seems that a genuinely pluralist view will need to identify a kind of generic being that is at least partly joint-carving. Call this challenge the "unity problem".[31] I take it that solving

[31] McDaniel 2017, pp. 50–3 discusses a very similar problem: how could being be unified analogically in the absence of a focal specification (a central way to be that is more fundamental than the others). Although he offers two "tests" for analogical unity, McDaniel's view is ultimately that the difference between analogically unified properties and mere disjunctions is potentially "brute", we won't al-

the unity problem requires identifying a commonality that enjoys a degree of conceptual independence from the things that fall under it. Generic being must be something "over and above" the specific forms of being it subsumes, otherwise, nothing would distinguish the assemblage of different forms of being from a merely arbitrary miscellany. And conceptual independence of this kind seems to rule out the possibility of reduction. Therefore generic being cannot be a determinable$_1$.

Clearly, though, this is not enough to establish premise 2. To defend that premise under the intended interpretation I will need to show that:

> **(C):** Conceivably, there is a (general) quantifier that quantifies over things that do not have being in any fundamental sense.

To argue for this, let's introduce some further ideology: let's say that a quantifier Q is *directly ontologically committing* just in case an entity e's being in the domain of Q is individually sufficient for e to have being.[32] I mean "individual sufficiency" here to capture a modal claim—namely, whether membership in Q alone, leaving all other quantifiers and facts aside, suffices for e to have being. We can also express ideas like these ground-theoretically: Q is directly ontologically committal iff it *immediately* grounds the being of the entities in its domain. In both cases, I intend the concept to capture the difference between a view on which everything within the scope of a quantifier Q has being because it falls within the scope of some other quantifier and a view on which anything falling within the scope of Q has being whether or not it falls within the scope of some other quantifier. Officially, though, I am understanding direct onto-

ways be able to explain what a property's unity consists in. As I interpret him, one upshot is that analogical properties are not determinables$_1$.

[32] I help myself here to the possibility that e could "have being". Pluralists, however, should think that such expressions are ambiguous. I therefore intend the phrase to be a shorthand for the claim that "there is some way of being which e has".

logical commitment in terms of the first definition since the appeal to grounding may well be uncongenial to the kind of metaontological deflationists attracted to **Equiv***.

We can now ask whether the general quantifier that corresponds to a generic form of being is directly ontologically committal. If generic being were a determinable$_1$ then the answer would be a decisive "no". According to that picture, the general quantifier cannot be individually sufficient for an entity's having being because it can no more be separated from the determinate forms of being to which it reduces than a whole can be separated from its parts. But I've argued that generic being is not a determinable$_1$. Of course, this doesn't settle the matter: if generic being is a determinable$_2$ then we should again answer the question in the negative.

Suppose then that the general quantifier is *not* directly ontologically committal. It is here that a commitment to **Equiv*** makes a difference. For this option seems to be inconsistent with **Equiv***: to think that the general quantifier is not directly ontologically committal is to think that quantifiers can differ with regard to ontological commitment. Of course, the difference in question concerns the *directness* of the commitments they incur, rather than the *degree* of such commitments. Even if the general quantifier is not *directly* ontologically committal, it is nonetheless conceivable that everything in its domain has being fundamentally speaking. Still, a difference in directness is a real difference nonetheless. If, though, as **Equiv*** suggests, there is no conceptual gap between ontology and quantification, then we should accordingly deny that they can make conceptual sense of two operators that are both equally quantifiers but which differ with regard to ontological commitment. (Given **Equiv*** the foregoing amounts to imagining two quantifiers that differ in whether they are directly quantificationally committal. But I take it that insofar as we grasp "quantificational commitment"

that should seem incoherent.) In other words, the claim that the generic quantifier is not directly ontologically committal commits its proponents to what is, according to **Equiv***, a distinction without a difference, it should therefore be unacceptable to pluralists who endorse **Equiv***.

Suppose, then, that the general quantifier *is* directly ontologically committal. Notice that in this case being directly ontologically committal can't be the commonality that unites the non-generic quantifiers. For the pluralist must insist that the specific ways of having being are fundamental in a way that the generic form of being is not. (Otherwise their view would collapse into *hybridism*, a version of pluralism with numerous disadvantages, see note 23 above.) So whatever unifies the non-generic quantifiers must exclude the general quantifier. The unity of the non-generic quantifiers must therefore amount to something *more* than being directly ontologically committal. But this begins to suggest how something falling within the domain of the general quantifier could fail to belong to the domain of any of the non-generic quantifiers: namely, if it doesn't satisfy the stronger condition characterizing these non-generic quantifiers.

We are helped here by the fact that conceivability claims like **(C)** are relatively weak. There are two ways to argue for such claims.[33] First we could show that nothing rules **(C)** out. Here things seem straightforward: what could stop an entity e in the domain of the general quantifier having being without falling within the domain of a non-generic quantifier? Presumably any such obstacle would have to imply that there couldn't be such an e. But since our general quantifier is directly ontologically committal, it seems to follow from the fact that e falls within its domain that e does

[33] The following distinction corresponds to Chalmers' distinction between negative and positive conceivability, Chalmers 2002, pp. 149–56.

(and therefore can) have being. Second we could try to show that **(C)** is *positively* conceivable by giving a coherent account of a scenario that would make **(C)** true. Arguably this too is unproblematic: in section 1.6 below, we'll consider Kris McDaniel's suggestion that understanding holes as merely generic beings helps to reconcile otherwise contradictory intuitions. If successful, this analysis gives us a case of the required sort. (In fact, I'll argue for a stronger construal of the case according to which holes don't have being at all. This doesn't impugn the coherence of McDaniel's proposal though.) It may be objected that being cannot be indeterminate. Certainly a number of philosophers have defended similar slogans. Critically, though, their thought was that it cannot be indeterminate *how many* things have being.[34] Nothing in our case contravenes this, the claim is rather that a determinate number of things could have being *in an indeterminate way*. It looks, then, like **(C)** is positively conceivable. It seems to follow that if the general quantifier is directly ontologically committal, then **(C)** is true. I've argued, though, that the general quantifier can't not be directly ontologically committal. So **(C)** must be true.

It should be clear why this argument doesn't overgeneralize: recall the alethic pluralism I discussed in section 1.3. Suppose the alethic pluralists is asked whether generic or minimal truth, the determinable notion, is directly alethically committal (i.e. whether a thing's being minimally true is individually sufficient for it to be true or whether it must also be true in some non-generic way). If they accept that it is, then they might be committed to minimal truth's being a determinable$_3$. But unless they accept an analog of **Equiv*** there is no reason that they should feel forced to concede that minimal truth *is* directly alethically committal. Instead, they can coherently main-

[34] See e.g. Sider 2001, pp. 120–32, especially pp. 126–7.

tain that minimal truth is only indirectly alethically committal and thus understand it as a determinable$_2$. The availability of this alternative establishes that the argument in question does not overgeneralize.[35]

1.5 Fundamentality and ontology

In this section, I will argue for premise 3:

> 3. If it is conceivable that a quantifier quantifies over things that do not have being in any fundamental sense, then ¬**Equiv***.

Assume the antecedent of 3. I will now try to show that the consequent follows. If it is conceivable that there exist things that do not have being in any fundamental sense then *either* these things do not have being at all *or* they have being non-fundamentally. The first disjunct implies ¬**Equiv*** and thus the truth of 3. So the opponent of 3 must adopt the second alternative. But while this option might seem to save **Equiv***, I'll argue that appearances here are misleading. The key contention of my argument will be that "non-fundamental" means something different when it modifies ways of being from the sense it has when it is predicated of a quantifier.

Suppose, then, that conceivably the general quantifier—*GQ*—quantifies over things that have being non-fundamentally. Nonetheless, *GQ* is just as much a quantifier as any other quantifier since being an existential quantifier is not a gradable notion. So, if something bound by *GQ* exists, it seems to exist just as much as any other existent. If, though, there are things in the domain of *GQ* that exist to the same

[35] As it happens, there is a view that does treat minimal truth as something like a determinable$_3$—see the discussion of "plain truth" in Pederson 2012, p. 597 and also n.11 p. 597. Note, however, that the motivations for this view are very different from those under consideration; the critical point is that alethic pluralists are not forced *by a version of the foregoing argument* to regard minimal truth as a determinable$_3$.

extent as any other existent but do not have being to the same extent then, by Leibniz's law, **Equiv*** (which asserts that being and existing are identical) must be false.[36]

Admittedly *GQ* might be a non-fundamental quantifier. Wouldn't this then make it false to say that an entity in the domain of *GQ* exists to the same extent as entities in the domain of fundamental quantifiers? Well yes and no. The problem is that the non-fundamentality of a quantifier doesn't seem to inflect its *quantificational* aspect. Consider an analogy. Suppose Biden is in Delaware and that he is thinking. Biden is then thinking in-Delawareishly. But "in-Delawareishness" is not a qualitative aspect of his thinking, nothing about the thinking *per se* would change if he crossed into Maryland. "In-Delawareishness" is a so-called Cambridge property of the thinking, a merely extrinsic and relational property. Analogously "non-fundamentally" seems to be a Cambridge *adverb* of existence. *Qua* quantifier, *GQ* is qualitatively indistinguishable from the fundamental quantifiers,[37] singling it out as "non-fundamental" thus amounts to giving it an arbitrary tag. But, for the pluralist, the non-fundamentality of being is not a merely relational property or arbitrary tag; it reflects a real qualitative difference in a thing's mode of being.

We face a choice: if we want to hold on to our equation of being and existing then we could say that "non-fundamentally" has the same meaning when it modifies "exist" as it does when applied to "being". In that case, though, given that premise 2 commits pluralists to a thing's conceivably having being non-fundamentally, we should admit that to describe some quantifiers as non-fundamental is to ascribe them

[36] Someone might want to argue that something having being merely non-fundamentally does not have being to a lesser extent. Even if this is true, having being merely non-fundamentally surely does mark an important difference in ontological status compared to those things that have being fundamentally. Certainly, an exponent of ontological pluralism should not deny this.

[37] Of course, *GQ* differs extensionally from other quantifiers, but then the fundamental quantifiers also differ extensionally from one another, so this can't be the relevant difference maker.

special ontological properties that lack a quantificational correlate—fundamental and non-fundamental quantifiers in this sense are equally *quantificational* but differ in their ontological import. It follows that **Equiv*** cannot be true, for it rules out ontological properties without quantificational correlates. Alternatively we could insist that "non-fundamental" as a property of quantifiers does not have a distinctive ontological import of this kind. In that case, though, it is false to say that the values of variables bound by non-fundamental quantifiers exist non-fundamentally in the same way that they have being non-fundamentally. In which case, by the reasoning of the previous paragraphs, **Equiv*** is false. Either way, **Equiv*** is falsified. So given the relevant antecedent, the consequent of 3 follows, which is to say that 3 is true.

1.6 From ¬Equiv* to ¬Equiv

I have been defending the premises in the conceivability argument which I presented in section 1.2. Since this argument is valid, if my defense of its premises succeeds, it follows that if pluralism is true, then **Equiv*** must be false. But this isn't yet sufficient to establish my goal, which was to show that pluralism was inconsistent with the thesis that being is existing, i.e. with **Equiv**. In this section, I will show how the arguments against **Equiv*** might nonetheless also point to the falsity of **Equiv**.[38] My strategy will be twofold: first I will show how the falsity of **Equiv*** undercuts potential arguments that might be offered in support of **Equiv**; second I will offer an outline for a positive argument from ¬**Equiv*** to ¬**Equiv**.

[38] It's worth noting that this is a strong claim: it amounts to insisting that **Equiv** is true only if it is conceptually true. Most claims are not like this. But on reflection it may not be so implausible to insist that some fundamental metaphysical claims are either false or conceptually true. A related position claims metaphysical truths are necessary: i.e. if some metaphysical thesis is true then it is necessarily true, see Nolan 2011, pp. 323–334 for discussion.

With regard to the first task, we should observe that if **Equiv*** is false, then **Equiv** becomes a substantive metaphysical thesis for which independent argument must be given. Such arguments may be available: **Equiv** may be more parsimonious than the alternatives or it may provide the most elegant way to pick out the subject matter of ontology. For instance, the friend of **Equiv** will probably need fewer primitives in their ideology. But since **Equiv*** is false, there is no conceptual presumption in favor of **Equiv** (cf. Hazen 1993). Moreover the falsity of **Equiv** opens up the possibility of ontologically innocent quantification and so abductive arguments in its defense must be balanced against whatever theoretical gains are realized by the possibility of ontologically non-committal non-fundamental quantification.

This is all the more significant since the defenders of **Equiv** have not typically given abductive arguments in defense of their view.[39] It does not follow, of course, that such defenses would be unsustainable. But proponents of this kind of approach are nonetheless somewhat handicapped: if **Equiv** is not a stipulation (in which case **Equiv*** would hold also), then—setting abduction aside—it is hard to see what positive arguments could be advanced in its favor. A positive argument would presumably consist of showing that there are reasons to think that being and existence (in the technical sense in which I am employing these terms here) should be identified. But as we will see shortly, the *opposite* looks to be true. Instead, the most promising approach for defenders of **Equiv** is to argue that the alternative, ¬**Equiv**, is untenable. In the

[39] Quine's own discussion (Quine 1948) is a partial exception to this generalization. Some standard non-abductive arguments for **Equiv** via **Equiv*** can be found in the sources cited in note 11 above.

One problem with giving *abductive* arguments for **Equiv** is that such arguments are hard to assess in the absence of well-developed alternatives to **Equiv**. A general objective of this dissertation is to lay the foundations for a rival approach that allows for ontologically innocent quantification. My suspicion is that this approach will reap abductive advantages, but this hypothesis can only be adjudicated holistically. The argument at this point is therefore best understood as a promissory note: I claim that abductive considerations might favor rivals to **Equiv** in the hope that the dissertation as a whole illustrates how this might be true.

final section, though, I will undercut such arguments by showing *how* **Equiv** might be false.

Before doing so, however, let us turn to the second part of the case against **Equiv**, which requires sketching a positive argument from ¬**Equiv*** to ¬**Equiv**. I will suggest that there are a number of apparent counterexamples to **Equiv**, cases where things seem to exist without having being. So long as **Equiv*** is an epistemic possibility, these counterexamples are dialectically inefficacious: the defender of **Equiv** can reject them as conceptually incoherent. But if **Equiv*** is false, then a would-be defeater of this argument by counterexamples is eliminated, leaving the initial cases as evidence against **Equiv**.

The point must be made carefully: **Equiv*** is a stronger claim than **Equiv**, so any counterexample to the latter is a counterexample to the former. We might wonder, then, what dialectical advantages could accrue from appeals to **Equiv*** in this case.[40] I don't have a full response to this concern: it seems intuitive to me that **Equiv*** does bring something dialectically significant to the discussion, but articulating exactly why this should be true is tricky. My hunch is that appeals to **Equiv** in this context are tendentious, but that it would not be (as) tendentious to appeal to **Equiv***. To say why, we'd need a full theory of what it is to beg the question, which is beyond the scope of the present essay. Nonetheless, we can give a *partial* answer: if the conclusion of a deductively valid argument is contained in its premises, then being question-begging must require more than assuming something that entails the desired conclusion, since otherwise (absurdly) any deductively valid argument must in some sense beg the question against its opponent. The dialectical infelicitousness of

[40] Thank you to Boris Kment for highlighting this to me.

question-begging must instead have something to do with the degree of overlap in subject-matter between conclusion and premises. The cases I introduce below are best construed as ground-floor claims about what is or is not, rather than as higher-level judgments about the conceivability of various states of affairs. As such, they share a subject matter with **Equiv** but not with **Equiv*** (or at least not to the same degree). A corollary of this is that—in light of the fragmentary account of tendentiousness I've just sketched—appealing to **Equiv** to answer such cases is tendentious in a way that appealing to **Equiv*** would not be. Thus, with **Equiv*** in abeyance, we lose an important dialectical resource in responding to such cases.

Consider then some of the proposed counterexamples. Some are straightforward: most speakers of English will be happy to say that there are various ways to the store. If pressed to explain their commitment to "ways to the store" though, they are apt to deny that such ways are comprised in their ontology. They will likely say that they didn't really mean that such "ways" *exist*. In the ideology I favor in this paper, such assertions should be parsed as denials that these ways to the store *have being*. Similarly, it is true that "there are many difficulties incumbent in the study of philosophy". But *that* truth is surely not enough to establish that such things as "difficulties" have being.

Other cases are more developed. McDaniel (McDaniel 2017, pp. 140–69) argues that holes, shadows, and other absences have being in a way that is "truly degenerate", they are "almost nothings" and "beings by courtesy". He is motivated by the intuition that holes are somehow less than fully real *and* that there is a hole in the middle of at least one doughnut. I agree with McDaniel that both of the foregoing conjuncts look true. And while his own proposal is not strictly inconsistent with **Equiv**—having being by courtesy still is, ultimately, a way of having being—there is arguably

a stronger intuition that it fails to fully capture precisely because it is consistent with **Equiv**. The intuition is that holes don't have being at all. After all, intuitively to be a hole just is to be some kind of bounded interruption in the being of some substance. We can capture this second intuition by going further than McDaniel: insisting that while there are shadows and holes, these do not have being by courtesy and so do not have being at all. But this conclusion *is* inconsistent with **Equiv**.

Cases like these seem powerful to me; denials that the relevant English platitudes—statements like "there's more than one way to get there"—are true look correspondingly ad hoc. The default response is that such intuitions are *incoherent*. But this response requires the truth of **Equiv*** and I argued above that **Equiv*** is inconsistent with pluralism. In that case, though, the foregoing examples bear with full force against the proponent of **Equiv** who must either give up her thesis, expand her ontology to include a whole host of dubious entities, or abandon her pluralism.

1.7 Conclusion: going beyond Equiv

If the argument I outlined above succeeds, then if pluralism is true there could be things that do not have being. This might seem like a reductio of pluralism, since it is *prima facie* obscure how the fact that there are some *G*s could fail to be sufficient for the *G*s to have being. What extra "ingredient" could be required to bring those *G*s that there are within the fold of ontology? In this concluding section, I will suggest one way to give content to the stipulative distinction between being and existence that I introduced at the beginning of the paper. The point here is not to argue *against* **Equiv**, but rather to clarify the import of the conditional that I hope to have established above: namely that if pluralism is true then **Equiv** is false.

One place to start is with the intuition that supervenient entities are "an ontological free lunch".[41] For those who share such a view, the physicalist who can show that mentality supervenes on physical states has a more *parsimonious* view than their dualist rival for whom mental and physical states are modally independent. Since, though, supervenient things need not be identical to the things on which they supervene, traditional views of parsimony don't support this judgment.[42] For example, according to a version of Ockham's famous razor, what matters for evaluating a theory's parsimony is simply the number of things posited by that theory. But, in part because the Ockhamite verdict looks wrong in the present case, a number of contemporary proposals reject the dictum that everything that exists according to a theory makes an equal contribution to that theory's parsimony (or unparsimony, as the case may be).[43]

Assuming that these proposals are on the right lines, we have a way to make sense of the extra element that might be required for a thing to have being. A theory is parsimonious to the extent that it does not make undue ontological demands of the world. (I take this to be what it *means* for a theory to be parsimonious.) I want to tentatively propose that we can make sense of what else is required for an existent to have being by appeal to the ways that theories may differ in their parsimoniousness. According to this proposal, we can understand something s as having being just in case whenever there exists such a thing as s according to a theory T, then T is ipso facto less parsimonious. This suggestion does of course take us beyond the anti-Ockhamite intuition, which in itself does not require us to assert that supervening entities do not have be-

[41] The phrase is Armstrong's, Armstrong 1997, p. 12,

[42] This holds true whether we are talking about qualitative or quantitative parsimony, since the dualist recognizes more modally independent kinds of states than the physicalist. For discussion see Lewis 1973, p. 87 and Nolan 1997.

[43] Arguments against the Ockhamite approach are given in Bennett 2017, pp. 220–5, pp. 226–8, and Schaffer 2015. For a response see Da Vee 2020. I discuss these ideas more fully in chapter 3, where I suggest that the relevant proposals are right to reject Ockham's proposal, but wrong about how to replace it. See page 45.

ing. Rather the thought is that anti-Ockhamite principles of parsimony are tracking some kind of ontological demandingness and that it could be this demandingness that provides the extra "ingredient" required for something to have being. Importantly, the proposal here is merely meant to provide a conceptual route for ascribing content to the notion of "having being". To make parsimony facts the metaphysical ground of our metaontology seems objectionably backwards. But the proposal here is just that we have an implicit concept of ontological *weight* that is *disclosed* in our reasoning about ontological parsimony and that ontological weightiness could be the difference between mere existence and having being.[44]

It is important not to be misled here. Focusing on the arguments of contemporary anti-Ockhamites might create the impression that I am appealing to a view according to which to be is to be fundamental.[45] This, though, is emphatically not the view I mean to advocate. In section 5 of chapter 3 below I'll tentatively suggest an account according to which the contribution a given entity *e* makes to the unparsimoniousness of theories that posit *e* is weighted *both* by how fundamental *e* is within the relevant theories and also by what I call its complexity. (I have more to say there about what it is to be complex.) This view goes beyond contemporary anti-Ockhamites who focus exclusively on accounting for the role of fundamentality in our judgments about parsimoniousness and have nothing to say about complexity. By contrast, the ap-

[44] The proponent of **Equiv** could also try to take up these ideas: they might say that it is just a consequence of their view that everything over which a theory quantifies matters in evaluating its parsimony. This would, in effect, return us to the Ockhamite proposal. I am supposing though that we have independent judgments about when theories are parsimonious and that these judgments support anti-Ockhamite intuitions that rule out this kind of maneuver.

[45] There's a further complication. As above, see note 18, throughout this chapter I have been focused on fundamentality as a quality of modes of being not of entities. In this paragraph, though, "fundamental" refers to the entity-specific notion. Using "fundamental$_e$" for this concept and "fundamental$_o$" for the predicate of modes of being, the view I mean to be defending in the remainder of this paragraph is that something has being fundamentally$_o$ speaking iff it is ontologically weighty iff it is complex, where to be complex in the relevant sense is not to be identified with being fundamental$_e$.

proach to which I am currently attracted, connects ontological weightiness (and thus being) not to fundamentality but to complexity. The idea—a defense of which falls outside the scope of the present dissertation—is that something has being only if it is ontologically weighty, which is to say if it has a certain minimal kind of complexity. From this perspective, having being and being fundamental are cross-cutting; while we can partially explicate what it is to have being by reference to parsimoniousness, this explication does not suppose that there is any connection between being and being fundamental.

I will close by considering one way in which the falsity of **Equiv** might open up new possibilities. Metaphysicians are concerned, among other things, with the viability of *generality absolutism*: the view that it is possible to quantify over absolutely everything.[46] On its face, generality absolutism seems like a presupposition of many of our metaphysical claims—the thesis that everything is self-identical, for example, seems to quantify absolutely generally. Moreover, it is not obvious whether generality absolutism can be coherently denied (see e.g. Lewis 1991, p. 68). But other arguments appear to tell against the generality absolutist and suggest instead that any domain of quantification is indefinitely extensible.[47] Rejecting **Equiv** opens up hybrid positions that may cut through this impasse: we could be generality absolutists about *ontological* quantification, while allowing that quantification simpliciter is indefinitely extensible. A hybrid view of this ilk does not immediately solve the problem of expressibility that has dogged the opponents of generality absolutism. But it may offer a neat way to finesse the contradictory imperatives generated by the arguments implying indefinite

[46] For discussion see Cartwright 1994, Studd 2019, the papers collected in Rayo and Uzquiano 2006, and Williamson 2003.

[47] See e.g. Dummett 1978 [1963].

extensibility and by the absolutist presumptions of metaphysics. This option, though, is open only if **Equiv** should turn out to be false.[48, 49]

[48] Chapter 2 takes up a similar idea in a slightly different context.

[49] The ideas in this chapter were presented under the title "Quantificational pluralism and (meta-)ontology" at the Metaphysical Mayhem Conference at Rutgers University in July 2023. A version of this chapter is also currently under review for journal publication.

Chapter 2

Against higher-order unrestrictedness

Generality absolutism—"absolutism" for short—is the thesis that it is possible to quantify absolutely unrestrictedly, over everything that there is. Our metaphysical theories seem to be riddled with absolutist presuppositions. Unfortunately, there are a number of arguments that appear to show that absolutism cannot be true.[1] In this paper, I want to focus on one particular challenge to absolutism that arises in systems that permit higher-order quantification.

The "standard" form of quantification with which most philosophers are familiar is first-order. Here the quantifier binds variables that fill the syntactic slots in sentences occupied by names, nouns, or pronouns. Higher-order quantifiers quantify into other syntactic positions: for instance, second-order quantifiers quantify into predicate position, while propositional quantifiers quantify into sentence position. In this picture, different quantifiers are said to range over distinct "types". To ensure the consistency of such quantification, the various quantifiers are standardly taken to be disjoint. (This last claim is not exceptionless: there are so-called "cumulative" systems in which quantifiers of different orders can overlap. Here, though, I will assume we

[1] For discussion see the papers collected in Rayo and Uzquiano 2006, Studd 2019, and Williamson 2003.

are working in a strict, i.e. non-cumulative system—see note 2 below.) But this immediately suggests a way in which none of these quantifiers can be absolute in the requisite sense, for any quantifier there are (in some regard) *x*s that it does not scope over.

It is unclear how seriously we should take this last point. Some absolutists, most prominently Timothy Williamson (Williamson 2003), have seen the possibility of higher-order quantification as posing no threat to the unrestrictedness of first-order quantification. (Indeed Williamson actually relies on the possibility of *sui generis* higher-order quantification to *save* the absolutist from an otherwise compelling objection.) The thought here is that in a language allowing for higher-order quantifiers, there are nonetheless no "things" that are outside the scope of an otherwise unrestricted first-order quantifier since "thing" is a first-order term. The appearance of restrictedness arises only if we use a first-order metalanguage to describe the values (again, this is a first-order term) of higher-order quantifiers. If we were to use a higher-order metalanguage the problem would disappear. Williamson claims: "Quantification into predicate position is simply incommensurable with quantification into name position; the former presents no coherent threat to the absolute generality of the latter" (ibid., p. 458). This suggests the following principle:

> **The unrestrictedness thesis:** In a typed system, that there is something in the domain of a higher-order quantifier does not make the lower-order quantifiers restricted.

In what follows, I will argue against the unrestrictedness thesis. (I'll focus on whether first-order quantification can be unrestricted in a higher-order system, although in principle the arguments will often generalize to other types.) I will suggest that meta-

physicians who are committed to the higher-order framework cannot be generality absolutists unless they are willing to affirm that higher-order quantification is not ontologically serious. (I will have more to say about exactly what I mean by ontological seriousness below.) It's important to note that, technically then, the conclusion for which I am arguing is a conditional: if higher-order quantification is ontologically serious then the unrestrictedness thesis is false.

I'm interested in this conditional because of the light it can shed both on generality absolutism and on the metaontology of quantification. Although I do not argue for it here, I am inclined towards a qualified form of absolutism. The falsity of the unrestrictedness thesis would imply that type-theorists cannot be absolutists. This sets up an argument by modus tollens: if absolutism is true, then if we are type-theorists, we must deny that higher-order quantification is ontologically serious. This line of argument parallels the ideas of chapter 1: as I noted in footnote 24 of that chapter, the argument of that chapter appeals to a premise that cannot be accepted by type-theorists. The broader project of which this paper is just a part aims to show that type-theorists are nonetheless in the same boat as their first-order brethren insofar as they must both reject principles like **Equiv**. (NB: I assume here that if higher-order quantification is not ontologically serious, then **Equiv** must be false.) At the same time, my hope is that recognizing the connections between absolutism and the ontological weightiness of quantification can help to shed light on the debates between absolutists and their relativist opponents. A full exploration of these questions must, however, wait for another time. My official aim in this paper is just to establish the aforementioned conditional claim, namely that if higher-order quantification is ontologically serious, then the unrestrictedness thesis is false.

To defend this conclusion, I will develop an analogy with many-sorted languages (which I'll describe in more detail below). It is, I'll claim, hard to interpret quantification in a many-sorted system as unrestricted. If, as I'll argue, many-sorted and higher-order quantification are analogous, then we should conclude that quantification in a typed system is also restricted. In section 2.3, I'll compare my analogy with a similar distinction between singular and plural quantification. In the process, I will describe the kind of ontological seriousness to which I alluded above. Finally in section 2.4, I'll respond to two kinds of objections to the argument by analogy. I take this to be a negative way of arguing for my conclusions: the legitimacy of the analogy (and so of the conclusions it licenses) is strengthened when we see how it can be defended against various prima facie objections. I'll begin though by saying more about what I mean by higher-order quantification.

2.1 The type-theoretic worldview

To situate the following discussion, it will help to assume a type-theoretic background. Type theory is a framework for studying and employing higher-order logic; I will focus on a strict functional type theory.[2] In this system, types are disjoint syntactic categories. We begin with two basic types: e, the type of individuals (i.e. the things named by first-order expressions), and t, the type of formulas. Other types are generated inductively: if σ and τ are types then $\sigma \to \tau$ is a type (provided $\tau \neq e$).[3] Monadic

[2] I'm following here the system outlined in Bacon 2019, 266–7. As noted above, alternative cumulative versions of type theory have been much discussed in recent literature. See e.g. Linnebo and Rayo 2012, Williamson 2013, pp. 237–8, Florio and Jones 2021, Krämer 2017; conversely, Button and Trueman 2021 criticize cumulativity. Nonetheless, the claim that typed-quantifiers can be unrestricted in a philosophically interesting sense is *most* plausible in a strict setting (cf. though Florio and Jones 2021, pp. 55–8.). Since this is the view I hope to argue against, I take the focus on strict type theory to be a concessive assumption.

[3] This corresponds to a *functional* type theory. Later we will consider briefly a different approach—*relational* type theory (see page 83 below). The restriction that if $\sigma \to \tau$ is a type then $\tau \neq e$ is an

predicates are of type $e \to t$. Binary predicates are of type $e \to (e \to t)$ and so forth. A sentential operator like conjunction is of type $t \to (t \to t)$. There are unique, type-restricted quantifiers for each type σ ranging only over the referents of terms of type σ. Variables are similarly type-restricted and expressions are indexed to a particular type. Type-theoretic languages realize a rough hierarchy of predication: terms of type e are first-order; terms of type $e \to t$ are second-order; terms of type $(e \to t) \to t$ are third-order, and so forth. Loosely speaking then, in strict type theory, nth-order terms form well-formed sentences only when predicated of $(n-1)$th-order terms.[4]

It will help to distinguish two kinds of (un)restrictedness. Let's call the sense in which the absolutist claims that a quantifier in a type-theoretic object language could be unrestricted "ontological unrestrictedness"; this is the kind of unrestrictedness that is philosophically interesting. The unrestrictedness thesis should be interpreted as a claim about ontological unrestrictedness. Even absolutists should concede that their ontologically unrestricted quantifier is still *typewise* restricted. This is just another way of restating the fact that the types here are disjoint.

(The distinction I have just drawn assumes that there is *a* kind of quantifier unrestrictedness that is philosophically interesting. This assumption is intended as an idealizing simplification: philosophers have cared about absolutely unrestricted quantification for a multiplicity of reasons. There is no guarantee that these various reasons support a single construal of what it is to be ontologically unrestricted.

important because it ensures that there is a one-to-one correspondence between functional and relational types. What if we didn't impose this restriction? Then there would be functional types without a relational analog. My hunch is that whether or not there are such types shouldn't matter for unrestrictedness—it's the wrong kind of difference maker. This suggests that we can't save the unrestrictedness thesis by relaxing our initial restriction on what functional types there are and hence that this restriction is dialectically innocuous.

[4] The ordinalization of types is not totally straightforward, since not all types comprise predicates. The more general condition is that terms combine functionally, taking other terms as arguments, and that such combinations are syntactically well-formed just in case the argument belongs to the type in the domain of the relevant function.

Nonetheless, there is a significant overlap in the issues that seem to turn on the possibility of absolutely general quantification that I take to justify the idealization. Consider two theses that have been much discussed in contemporary metaphysics: nominalism—according to which there are no abstract objects—and (mereological) nihilism—according to which there are no composite objects. The first thesis can be made trivial by restricting our quantifiers to concreta, the second by restricting quantifiers to simples. Those who think there are genuine debates to be had wish to rule out such interpretations as unintended.[5] They can do so by insisting that the quantifiers in the relevant theses are intended to be absolutely general. The broader point is that many metaontological views can be trivialized by arbitrary quantifier restrictions. Those who hold that there are non-trivial issues in the vicinity here seem to be committed to the possibility that quantifiers can be unrestricted in a way that rules out this kind of gerrymandering. A quantifier is ontologically unrestricted just in case it is unrestricted in this sense.[6])

Actually, it turns out that strict type-theorists cannot assert within the confines of their system that a given quantifier is restricted in any sense. I assume, though, that they will be willing to accept informal glosses that describe their view as a system of

[5] Rayo 2021 makes this point.

[6] David Builes pointed out to me that this comes perilously close to a definition according to which a quantifier is ontologically unrestricted iff it ranges over all the things that we must take ontologically seriously. He observed that this threatens to make the view I defend—that type theory rules out ontologically unrestricted quantification of this kind if higher-order quantification is ontologically serious—collapse into triviality.

This is an important and challenging observation. Nonetheless, I think there are still non-trivial issues remaining. Even given the proposed definition of ontological unrestrictedness, I am inclined to think that the type-theorist should not immediately give up the unrestrictedness thesis. Their claim should be that debates about nominalism or mereological nihilism are implicitly first-order. They should conclude, therefore, that the things we are required to take ontologically seriously and the scope of ontologically serious quantifiers can come apart. (The first category but not the second is exclusively first-order.) The burden of this paper is to use an argument by analogy (as opposed to tendentious assumptions about the nature of the "true" philosophical metalanguage) to push back against such conclusions. On the view I aim to defend the scope of ontologically serious quantification and the subject matter of our ontological theses cannot help but be coextensive. Since, though, this can be intelligibly denied, it doesn't seem to be utterly trivial.

disjoint quantifiers ranging over entities of a given type. Ultimately, to ask whether these quantifiers are *ontologically* unrestricted is to ask about whether the syntactic restrictions imposed by a particular language carve our world at its joints.[7] If so, then the relevant quantifiers *are* ontologically unrestricted in the requisite sense and the kind of informal glosses I mentioned above can be dismissed as metaphysically unperspicuous. The opponent of the unrestrictedness thesis, though, holds that type-theoretic restrictions are unwarranted. From this perspective, while it is true that we cannot assert that a quantifier is type-wise restricted within the confines of type theory, this limitation prevents us asserting an important metaphysical truth.

Let's unpack this idea. Taking type-theoretic languages seriously (or more specifically, the strict systems on which I am focusing) makes cross-type quantification literally senseless. This leads to a well-known problem of expressibility: describing such views informally, we say that everything belongs to some type, nothing belongs to more than one type, and that quantifiers never range over things of distinct types. But according to the type-theoretic view we can't give meaningful philosophical content to this description since it uses a quantifier ranging over things of any type ("every-

[7] Care is required here: the intended idea is that different *quantifiers* could be joint-carving. Following Sider 2011, (chapter 6, especially p. 96), this means that we can deploy talk of naturalness to apply not just to predicates, but also to expressions of different syntactic types. Joint-carving quantification is *not* quantification that is restricted by joint-carving predicates. Even if the atom/non-atom distinction is joint-carving, it in no way follows that a quantifier restricted to atoms is *ipso facto* joint-carving also. If a predicate carves nature at its joints by delineating a natural kind, then a quantifier carves nature at its joints by delineating an ontological category.

This recalls an idea from chapter 1, see page 23 above: pluralists, I suggested, must distinguish between the view that my little finger and $\sqrt{2}$ have being in different ways and the view that they have being in the same way, although one is concrete and one abstract. In other words, distinct ways of having being are more than just being while instantiating some particular property. Analogously, ontological categories are more than just being while belonging to a natural kind; an ontological category is instead an inflection in the mode of being itself. Like pluralists, type-theorists think there are deep distinctions between different kinds of quantifiers. The claim here is that these distinctions can be consistent with generality absolutism only if these distinctions align with genuine (i.e. perfectly as opposed to merely relatively natural, see Lewis 1983, p. 347) fissures in the nature of being itself.

thing").[8] The significance of this observation here lies not in the bearing it may have on the belief-worthiness of type-theoretic approaches, but instead in the more general idea that to be a type-theorist is to take seriously the idea that there are strict limits to what can be meaningfully asserted. Accordingly, type-theorists should deny that we can make grammatical claims asserting the existence of things of another type and so they should hold that there is no way the unrestrictedness thesis could be false. For, on their view, there is no way to meaningfully say that there is something a given quantifier excludes.[9]

Appearances notwithstanding, the key issue here is not linguistic but metaphysical. There are various languages we might use to limn our world and correspondingly various kinds of syntactic restrictions we might impose on ourselves. Questions about the appropriateness of these restrictions turn in part on logical issues: we probably don't want to adopt a system that is so unrestricted it will lead to inconsistencies. But in choosing to speak languages of a certain kinds, we are also choosing different logics. The package of language and logic that we end up favoring will depend ultimately on the metaphysical worldview that we favor.[10]

We appear to face a kind of stalemate. From an outsider's perspective, the unrestrictedness thesis says something prima facie peculiar. From the type-theorist's per-

[8] We might think that this claim is obviously false: the statement "everything belongs to some type, nothing belongs to more than one type, and quantifiers never range over things of distinct types" is a grammatically well-formed assertion in English and thus a counterexample to the initial claim. But this is too quick: the type-theorist can admit that the English sentence is well-formed. They deny, though, that it expresses the intended idea: the quantifiers in the English sentence are first-order. The type-theorist's contention is that cross-type quantification is nonsensical. Since quantification in the English sentence is entirely first-order, it does not constitute an example showing otherwise.

[9] In cumulative type theories, the analogous claim is that there is no way to make grammatical claims asserting the existence of entities of a lower type.

[10] There's a kind of Quinean holism at work here (Quine 1980 [1953]): if we are convinced of certain metaphysical views, then we will be prepared to adopt logics that make these views defensible. Quine thought that logical beliefs were among the most resilient in our web of belief. In this kind of rarefied metaphysical context, though, this seems false. We are unlikely to have any views about higher-order logic until we begin thinking about related questions. Given this, our logical judgments are up for grabs to the same extent as our other philosophical convictions.

spective, it says something that cannot coherently be denied. The type-theorists' opponents insist that they can step back and cognize the type-theoretic system as a whole; type-theorists insist that this kind of argument either misinterprets their view (by failing to take seriously the distinction between types) or else assumes its falsity. One potential way out of this impasse is developed by Stephan Krämer, who tries to show within the confines of a type-theoretic system that type-restricted quantifiers cannot scope over everything in the relevant sense (Krämer 2017).[11] But Krämer's argument relies on the availability of a typewise unrestricted identity predicate and it is by no means obvious that such a predicate should be available to the type-theorist.[12]

I will therefore try a different approach. Instead of relying on assumptions about the type-theoretic structure in which I am working, I will propose an argument by analogy. This doesn't entirely resolve the stalemate but it does give us a framework to argue about these questions non-tendentiously. Even type-theorists can accept that quantification in many-sorted languages doesn't seem to be ontologically unrestricted. If this is right, then we may be able to make progress here by examining the extent to which type-theoretic quantification resembles sortally restricted quantification.[13] In the next section, I'll explore this idea in more detail.

2.2 An analogy with many-sorted quantification

The argument I have in mind is as follows:

[11] Krämer argues that we should opt for a *cumulative* type theory in which nth-order terms can be intelligibly predicated of any mth-order terms, where $n > m$. This allows him to formulate a cross-type identity predicate, that he then uses to express the claim that there are *more* things than are encompassed in any single type.

[12] Krämer does, of course, offer considerations motivating cumulativity. Still it would be nice to have a different way to challenge the unrestrictedness thesis.

[13] I don't, however, take it to be my burden here to convince the type-theorist—those who are determined to defend the unrestrictedness thesis should probably reject the analogy—but rather to find a non-tendentious way of getting to grips with the question at hand.

1. Sortally restricted quantification is not ontologically unrestricted.

2. Type-restricted quantification is analogous to sortally restricted quantification.

3. If type-restricted quantification is analogous to sortally restricted quantification, then type-restricted quantification is ontologically unrestricted if and only if sortally restricted quantification is ontologically unrestricted.

4. Type-restricted quantification is not ontologically unrestricted. (1,2,3)

The remainder of the paper defends the premises of this argument. I will argue for premise 1 in section 2.2.1. In section 2.2.2, I'll argue for premise 2, and in section 2.2.3, I'll work through an example that shows why premise 3 should strike us as plausible. Subsequent sections develop the arguments for premises 2 and 3.

2.2.1 Sortal restriction

Category mistakes are infelicitous sentences that intuitively involve a confusion of categories. For example "4 is angry", "Wednesday is asleep", or "green ideas sleep furiously" are paradigmatic category mistakes. On the face of it, the infelicity at stake here is very similar to the infelicity involved when type-restrictions are violated: "is running is hot", for instance, predicates a first-order predicate of another first-order predicate. Phenomenologically the error in both cases feels similar.[14] Moreover, both category mistakes and clashes of type are typically thought to be meaningless.[15] There's good reason to think, then, that reflection on category mistakes can shed light on

[14] This is important because it is hard to give an exact characterization of the class of category mistakes, which is plausibly vague, and it is therefore routine to pick out the target of analyses of category mistakes either by reference to paradigms or phenomenologically. See Magidor 2013, pp. 1–5.

[15] Interestingly, though, Magidor has presented reasons for thinking that both could be *meaningful*. See ibid., chapter 3 and Magidor 2009.

clashes of type. That's not to say that clashes of type just *are* category mistakes or vice versa. The former, for instance, are more obviously unsyntactical. (Notice importantly, though, that it doesn't follow from this that they are any less *meaningful* see Magidor 2009, pp. 23–5.) The point is just that they provide us with a helpful analogy in thinking about type-theoretic systems.

Taking category mistakes seriously suggests that it may be helpful to adopt a metaphysics of *sorts*. One way to do this is to use the ideology of "many-sorted languages" in giving our theory of the world. To generate a many-sorted language, we take a standard first-order model and partition its domain into various subsets. Each subset is called a "sort". We assign a natural number n to each sort, and then lay down a rule to the effect that a variable x_i is to be interpreted over sort i.[16] Finally, we may restrict which predicates can combine with variables of this sort. (This will place important constraints on our interpretation of the predicates: if "P" can only be predicated of ith sort variables and "a" labels an entity not in this sort, then it better not be that the denotation of "a" is in the extension our interpretation assigns to "P", since the nonsensical cannot be true.)

Sortal restrictions are not enough to establish ontological unrestrictedness. To see this, imagine first that I speak a language that quantifies over cats and dogs. Then I switch to a many-sorted language in which cats and dogs belong to different sorts. It doesn't seem right to claim that quantification over cats in this language is thereby unrestricted in any interesting sense. A second example reinforces the point. Suppose momentarily that sortally restricted quantification is ontologically unrestricted. And imagine we are working in a language in which there is a sort comprising natural

[16] It is possible to have nested sorts if, for example, everything in sorts i and j is also in sort k. I'm interested here though in systems where the sorts are disjoint and henceforth I'll use "many-sorted" and its cognates to refer to languages of this kind.

numbers. We would then be committed to thinking that quantification over the natural numbers is unrestricted in the philosophically interesting sense. It would therefore be true to say that *absolutely everything* is a number. But if we accept that there are also people, ships, and mountains (as we should) then it isn't true that everything is a number. This is made apparent by the single-sorted translations of the relevant generalizations: "$\forall x_n(\text{Number}(x_n))$" where n is the sort of number terms is rendered in single-sorted terms as "$\forall x(\text{Number}(x) \rightarrow \text{Number}(x))$". But this structure—with the universal quantifier ranging over a condition whose consequent states the intended generalization—is a paradigmatic example of how we express restricted quantification.

Of course, we should be wary of taking examples like these too seriously. Perhaps the cases I described elicit certain intuitions only because I have not identified entities of genuinely different sorts. Notice, though, that while the distinction between cats and dogs does not look like it is metaphysically deep, numbers and mountains are different in a more substantial sense. If there are multiple sorts of things, then numbers and mountains are paradigms of the kind of entities that might belong to different sorts. Alternatively, the proponent of a many-sorted language could always adopt the same strategy as the type-theorist and argue that we judge sorted quantification to be restricted only because we implicitly assume a single-sorted metalanguage. (As above, questions about the metaphysics and the kinds of languages we should impose on ourselves stand and fall together.) Nonetheless, as a matter of fact, I suspect that many will be willing to agree that sortally restricted quantification is not ontologically unrestricted. Henceforth, then, I will take it as given that a quantifier that is merely restricted by sort is not genuinely ontologically unrestricted.

It's worth noting that sortal restrictions are syntactic limitations on what formulas can be well-formed. If "P_j" is a predicate of entities of sort j, then "$\exists x(P_j x_i)$" is grammatically nonsensical in just the same way that "$\wedge(\neg(PS$" is grammatically nonsensical. Yet the syntactic analysis of category mistakes is a minority view and, as I noted above, their more obviously unsyntactical quality is in part what distinguishes clashes of type from category mistakes. Doesn't this, then, undermine the analogy I hope to draw? If taking category mistakes metaphysically seriously was the motivation for using many-sorted languages in the first-place, and such languages present a possibly misleading account of category mistakes, then it certainly looks as though we have less reason to theorize in a many-sorted framework.

This conclusion would be premature. There are two important points: first, we can think that sortal restrictions are a helpful way of eliminating the possibility of category mistakes from a language using syntactic rules without committing ourselves to thinking that the infelicity of category mistakes in natural languages is syntactic. (Put differently: sortal languages could be an appropriate metaphysical response to category mistakes, without being an apt framework for disclosing their linguistic structure.) Second, what matters for the subsequent discussion is the *formal* possibility of languages allowing for sortal restriction, rather than the actual analysis of category mistakes. More precisely, my argument requires that we can make sense of a metaphysics of sorts and sortally quantified languages (as we surely can), and further that we have the intuition that such quantification should not count as ontologically unrestricted. The phenomenon of category mistakes merely makes vivid *why* it might be helpful to introduce sortal restrictions as an analogy for type-restrictions. More important to my argument are the metaphysical and formal analogies between typed-

and many-sorted frameworks. In section 2.4 below I give an extended discussion of these issues. Readers may or may not be persuaded, but the critical point is that it is on these arguments rather than on intuitions about category mistakes that my analogy should be judged.

2.2.2 Many-sorted and type-theoretic systems compared

We've already noted the similarities and differences between clashes of type and category mistakes. I'll now suggest some reasons for thinking many-sorted and type-theoretic systems are analogous. The case I'll present is provisional, as mentioned section 2.4 offers a more extensive metaphysical and logical comparison of the two systems. The point for now is just to give some prima facie reasons for thinking that this is an analogy worth exploring.

Many-sorted and type-theoretic systems are, I claim, importantly structurally similar. Both suggest a division of the world into categories, that they use different labels for these categories (respectively "sort" and "type") is not metaphysically important. Moreover, both require that we accept syntactic restrictions that limit the way terms from different categories can combine. These are the two critical similarities from the point of view of the argument by analogy: the idea is that if a theory's ontology comprises multiple non-empty categories, then insisting on syntactic restrictions is not sufficient to establish that quantification over a given category is ontologically unrestricted. The argument assumes that you find this conclusion intuitive in the case of many-sorted systems and attempts to use this fact to show that you should also accept the conclusion in the case of type-theoretic systems.

We could, however, run the argument by analogy in reverse: if we find the relevant conclusion unacceptable in the type-theoretic case, then perhaps we ought to reject it for many-sorted systems also. I suggested above that this is unlikely to be a popular option. Still, that's not an *argument*. Happily, there are other considerations to which we can appeal. The claim to be defended is that if a theory has a categorical ontology, then syntactic restrictions aren't enough to establish that a quantifier limited to entities of a certain category is ontologically unrestricted. Recall though that my opposition to the unrestrictedness thesis is *conditional*: I claim that if we take higher-order quantification ontologically seriously, then such quantification is not ontologically unrestricted. Generalizing, if our theory includes multiple non-empty disjoint categories, then if we take those categories ontologically seriously, then syntactic restrictions are not enough to establish ontological unrestrictedness. In other words, the idea is that if we take a theory of categories in an ontologically serious way, then we should not impose syntactic restrictions that prevent us speaking univocally of the things we take ontologically seriously.

It is a little hard to assess this last claim until we have said more about what ontological seriousness amounts to; I'll return to this task in section 2.3 below. Note though that I reject construals of "ontological seriousness" that would make the foregoing claim trivial. We *might* think that to take different categories ontologically seriously *just is* to eschew syntactic restrictions that limit cross-categorical talk. That is *not* the view I'll adopt. Instead, I think that we should eschew syntactic restrictions that prevent us speaking univocally of the things that we take ontologically seriously because I assume that metaphysical reality is singular and should encompass everything in our (serious) ontology. In other words, syntactic restrictions are uncalled for

because they do not perspicuously represent what I take to be a fact: namely, that at some level our world forms a coherent whole. For present purposes, I will take the idea that metaphysical reality must cohere as a basic, undefended starting point.[17]

This then is the prima facie case in defense of the analogy. We should acknowledge that there are also, of course, important differences between many-sorted and type-theoretic systems. Three such differences stand out as especially important: first and most prosaically, there are potentially important metalogical differences between many-sorted and higher-order quantification. I will return to this idea in section 2.4.2 and argue at length that it is not an important difference-maker in the present case. Second, the categorical structure suggested by type theory seems to more directly trace syntactic distinctions in language than the structure of sorts. Again, I return to this below, arguing in section 2.4.1 that this cannot be a difference-maker with regards to the analogy either.

Finally, unlike many-sorted systems, higher-order systems seem to realize a hierarchical arrangement of categories. There are first-order terms, second-order terms, third-order terms and so on (see page 52 above). Roughly the idea is that we ascend up this hierarchy everything we make a predicative claim about a lower level. I am skeptical that this could be the kind of disanalogy that undermines the argument I am hoping to make. For one thing, the hierarchy in question is inexact. Quantification over the type of propositions is normally seen as a kind of "higher-order" quantification, but asserting a proposition is not a kind of predication. (It is sometimes suggested otherwise since we can interpret propositions as 0-adic predicates. This formal possibility does not, however, appear to track a deep metaphysical similarity. If anything,

[17] Notice, though, that there are philosophers who would reject this claim, see e.g. Fine 2005. In other work, I have argued against Fine's view. ("Incoherentism", MS.)

propositions are rather more like first-order individuals than in being saturated (to use Frege's metaphor), where predicates by contrast are unsaturated.) Moreover, the direction of this hierarchy does not seem to be a deep fact, since there are straightforward permutations that seem to reverse the ranking of different types without changing anything of metaphysical significance. We could for instance, take the type p of predicates of individuals as primitive and define individuals (i.e. things that standardly belong type e) as entities of type $p \to t$. This would make first-order predicates more basic than individuals. Yet permutations of this kind look like they are merely notational.

To summarize then, there are two important structural similarities between many-sorted and type-theoretic systems that justify treating them as analogous. I argued that there was a general reason to think that quantification in categorical systems of this kind couldn't be ontologically unrestricted, at least not if we are taking it ontologically seriously. Finally, I considered three important dissimilarities between many-sorted and higher-order systems. I postponed discussion of two such dissimilarities for later and argued that the more hierarchical character of type theory did not look like the kind of disanalogy that should bear on the problem at hand.

2.2.3 Introducing the analogy

In this section, I try to exemplify the idea behind premise 3 of the foregoing argument, namely that if type-restricted quantification is analogous to sortally restricted quantification then type-restricted quantification is ontologically restricted if and only if sortally restricted quantification is ontologically restricted. To do so, I'll assume that sortally restricted quantification is not ontologically unrestricted and show how this

poses a prima facie problem for one contemporary defense of the unrestrictedness thesis.

Let's begin, though, by considering in general terms why the apparently *restricted* character of sortal quantification might pose problems for the defender of the unrestrictedness thesis. Suppose that we are working in a two-sorted language and I assert to you that quantification over sort_1 is unrestricted. Not so, say you, since there are also things of sort_2. And suppose that I accept your complaint, but respond by returning with a translation of the two-sorted language into a second-order system. Surely I haven't thereby successfully answered your initial objection.

We can get a better grip on the issue here by working through a detailed example. "Everyone was drinking" I say, reflecting on a rambunctious event. Clearly, though, I don't mean absolutely every human. For it is no objection to my claim that Jim Carey is famously teetotal. My quantifier was restricted to attendees at the party. Salvatore Flores and Nicholas Jones suggest that cases like these offer us a way of getting a grip on what it is to be ontologically restricted in terms of the possibility of counterexemplification. The quantification in my initial statement was restricted because although Carey wasn't in fact a counterexample to my claim, there's a way that the domain of my quantifier could be expanded such that he would have been. Conversely, the quantifier in a universal claim is philosophically unrestricted if there is *no* way of expanding its domain that might lead to counterexamples.

This represents a promising avenue for the defender of the unrestrictedness thesis because the syntactic restrictions of type theory impose a grammatical limit on when there can be a counterexample to a claim, viz. only if the counterexample is a

well-formed expression. Jones and Florio call this a "Russellian" account of unrestrictedness:

> **Russellianism:** A domain is (ontologically) unrestricted for some generalization "$\forall vFv$" if and only if it coincides with the "range of significance" for "F"—that is, roughly, the terms of which F can be meaningfully predicated.[18]

Given an equivalence class of generalizations whose predicates do not differ in their range of significance, we can say that a quantifier is unrestricted simpliciter. As Florio and Jones understand it, a predicate's range of significance coincides with the class of entities of which it can be "meaningfully predicated".[19] This means that the range of significance of a predicate is bounded by syntactic restrictions—it is, I presume, a necessary condition on meaningful predication that the predication in question be grammatically well-formed. For simplicity, we can assume here that in strictly typed systems this is also a *sufficient* condition—i.e. a predicate is meaningfully predicated of any argument of the right type. This allows us to speak in absolute terms of unrestriction given a well-typed quantifier.[20]

Let's focus, then, on the interpretation of a well-typed quantifier in some arbitrary generalization. In strict type theory, there are no predicates "P" applying to first-order things such that the range of significance of "P" encompasses more than the totality of

[18] Florio and Jones 2023 argue that we should adopt a distinction between two different conceptions of ontological unrestrictedness: "maximal strength" and "maximal inclusivity". I have not adopted this ideology here in part because I am skeptical about how deep the distinction goes. (Immunity to counterexemplification, which constitutes maximal strength, is often a hallmark of maximal inclusivity. Arguably though, it is a domain's unrestrictedness that explains its immunity to counterexemplification, rather than vice versa as Florio and Jones's proposal would seem to suggest.) Instead I am supposing that we have a sufficient intuitive pre-theoretic grip on the notion of unrestrictedness that allows us to say that any conception that counts sortally restricted quantification as *ipso facto* ontologically unrestricted has gone wrong.

[19] Ibid., p. 6.

[20] For discussion of the merits of a "relative" conception of unrestrictedness see Button and Trueman 2021 and Florio and Jones 2023.

first-order things. Moreover, all predicates of this type are significant when predicated of any first-order thing. So the first order domain is Russellian for all statements of the form "$\forall x_e(F_{e\to t}x_e)$" where "$F_{e\to t}$" is a predicate of first-order things. Thus Russellianism implies that the first-order domain is unrestricted for any first-order generalization and so we can conclude that the first-order quantifier "$\forall x_e$" is unrestricted. This argument can be extended to quantifiers of any type; quantifiers that range over all and only things of a given type, it seems to follow, are ontologically unrestricted.

The problem is that the very same argument appears to show that quantification over the sort of dogs or the sort of numbers is also ontologically unrestricted. For, if we use a predicate "P", restricted to the sort of dogs, no counterexample to generalizations formulated in terms of "P" can be generated by expanding our domain of quantification, since there are no non-dogs within the range of significance of "P". After all, formulas in which there is a clash of sorts are just as ungrammatical as formulas that violate the type-restrictions in a strictly typed system. We can further imagine a sorted system in which *all* predicates applying to dogs are sort-restricted. (Of course, this kind of limitation may not be very plausible, but then the strictness of strict type theory also generates some counter-intuitive conclusions: for instance, that there is no type-neutral notion of identity.) In such a system, Russellianism implies that quantification over dogs is ontologically unrestricted. But quantification over dogs in any such system does not seem to be ontologically unrestricted. Thus Russellianism is subject to a challenging counterexample. The problem generalizes: unless the defender of the unrestrictedness thesis can find some way of establishing a disanalogy between sorts and types, their proposal will be undermined as well.

2.3 Plurals contra singular quantification

So far, I take myself to have introduced the argument by analogy and given a cursory defense of its premises. In section 2.4 I'll return to elaborate on my defense of premise 2—the claim that sortally restricted and type-restricted quantification are analogous. First, though, I'll examine a different way to resist the argument. The strategy here is to try to show that my argument against the unrestrictedness thesis overgenerates by considering a different kind of analogy. In what follows, I'll introduce this analogy, explore the way it might appear to put pressure on my argument, and try to show why such appearances are misleading.

There are many natural languages, including English, that allow for plural terms. The first clause of the preceding sentence exemplifies the phenomenon. We can regiment this by introducing plural variables, which we write as "$xx, yy \ldots$" and a plural quantifier ranging over these variables—"$\exists xx$". Plural constants are symbolized as "$aa, bb \ldots$". It is also convenient to introduce a predicate "$\prec$" taking one singular and one plural argument, where "$x \prec yy$" is to be read as "x is among the ys". Adding these resources to a standard first-order language gives us a plural first-order language.[21]

Intuitively the possibility of plural quantification does not pose a threat to the unrestrictedness of *first*-order quantification. If my first-order quantifier "Q" ranges over everything that there is then it seems right to say that "Q" is ontologically unrestricted even if we can go on to quantify *plurally* over these very same things. If, then, my argument by analogy implies (counterintuitively) that the possibility of plural quantification *does* threaten the unrestrictedness of the first-order domain then it will have shown too much and should be rejected. Worryingly, however, it looks like my argu-

[21] The semantics for this language can also be given plurally see Florio and Linnebo 2021, pp.123–50.

ment might well have that implication. In a famous paper, George Boolos showed that we could give a truth-preserving translation of expressions in a *monadic* second-order language into a first-order plural language (Boolos 1984; Boolos 1985). Boolos's translation maps second-order quantified claims like "$\exists X \phi$" to plurally quantified claims like "$\exists x x \phi^*$", where "ϕ^*" is the pluralized translation of "ϕ". But the argument by analogy I gave in section 2.2 above seems to imply, *pace* defenders of the unrestrictedness thesis, that monadic second-order quantification *is* a threat to the (ontological) unrestrictedness of the first-order domain. Without a good explanation for why Boolos's translations fail to preserve a quantifier's (un)restrictedness, my argument looks to be in trouble.

Fortunately, the requisite explanation is not hard to find. Briefly, I'll argue that there are two ontological attitudes we can take to plural and higher-order quantifiers—heavyweight and lightweight. Orthodoxy assumes a lightweight approach. This explains why we normally don't take plural quantification to be a threat to the unrestrictedness of the first-order quantifier. By contrast, we standardly take a heavyweight attitude to both sortal and higher-order quantification. This explains why, intuitively, sortal and higher-order quantification undermine the unrestrictedness of first-order quantification, whereas plural quantification does not. Boolos's translations, though, assume that we take identical attitudes to plural and second-order quantification (i.e. either both are heavyweight or both are lightweight). Thus, his translations clash with our intuitions because they rely on different background assumptions. If we revise *our* assumptions, then our intuitions should also change. If conversely, we hold fast to our initial judgments, then we should think that Boolos's translations are ontologically misleading.

We begin the argument I just sketched by contrasting two attitudes we can take to plural quantification: according to the first approach, which I will call "pluralist", the plural quantifier ranges *plurally* over the very same domain as the singular first-order quantifier. According to the second approach, which I will call "substantialist", the plural quantifier ranges over a domain of *sui generis* plural entities that I'll call "pluralities". In other words, on this view, pluralities are real substances, hence the name "substantialist". For the substantialist, the pluralist's distinction between singular and plural ways of quantifying is misleading. According to substantialism, the plural quantifier does not differ operationally from the first-order quantifier, but only in its domain.[22] We can put this distinction at work by considering an example: "the books form a stack" I say. Both the substantialist and pluralist agree that "form a stack" is a special predicate that takes plurally quantified arguments. But for the pluralist, I could just as well have enumerated the things forming a stack using the first-order quantifier. The substantialist thinks otherwise: on their view, we have in addition to each of the individual books, a further entity, a book-plurality, over which the plural quantifier ranges.

Typically plural quantification is not taken to be ontologically committal.[23] That is, on the standard view, once we have quantified over the books singularly, our plural quantifiers do not imply that there exist some further things, but simply provide an alternative framework for talking about the commitments of our first-order discourse. This assumes a pluralist attitude to plural quantifiers. I take the view that such quan-

[22] Of course, you could think that there is a quantifier quantifying *plurally* over a domain of pluralities. But this would be *super*plural rather than plural quantification. And a view that is substantialist about plural quantification, but pluralist about superplural quantification looks objectionably disjunctive. (Note that we can give an adequate explanation of superplural quantification without going substantialist: superplural quantification stands towards plural quantification on this picture as plural quantification stands to first-order quantification.) On superplural quantification see Rayo 2006.

[23] For a dissenting view here see Florio and Linnebo 2021.

tifiers are ontologically innocent to be a lightweight one. In other words, using the terms I introduced in the penultimate paragraph, pluralism supports a lightweight construal of plural quantification, while substantialism supports a heavyweight construal. Metaphysicians attracted to the higher-order framework often eschew the ideology of ontological commitment. Nonetheless, we can make sense of different attitudes they might take to higher-order quantifiers by analogy with the different views we can take of plural quantification. According to the lightweight attitude, just as—according to some—plural quantifiers quantify plurally over the very same things as first-order quantifiers, so higher-order quantifiers quantify in some special way over the same things as first-order quantifiers. On the pluralist view, the plural facts are fully explained by the individuals that there are; analogously, on the lightweight view of higher-order quantifiers the higher-order facts would be fully explained by the individuals that there are. Conversely, according to the heavyweight attitude, higher-order quantifiers behave analogously to first-order quantifiers save that they range over a sui generis domain.

Hitherto, I have been implicitly assuming a heavyweight attitude towards higher-order quantifiers, according to which they are relevantly unlike plural quantifiers (on the more mainstream pluralist view). If we accept this assumption then an ontologically perspicuous model-theory would block Boolos's proposed translations. For instance, according to Boolos, $\exists X_k(X_k a_i)$ iff $\exists x x_k(a_i \prec x x_k)$. If, though, the two kinds of quantifiers differ in their ontological status then this proposed translation cannot be adequate.[24] Some extra ontological ingredient is required to take us from the plurally quantified claim to the second-order claim.

[24] This is not an objection to Boolos; part of his broader project is to show that higher-order quantifiers *needn't* be ontologically committal. It is the difference in our background assumptions rather than some flaw in his proposed translations that blocks their adequacy here.

Of course, perhaps we ought to revise our way of understanding plural quantification and regard plural quantifiers as ranging over a *sui generis* domain of pluralities. This would require adopting an substantialist construal of plural quantification. In that case, Boolos's translations might be adequate—perhaps to be the value of a monadic higher-order variable *just is* to be a plurality—but it no longer looks so counterintuitive to claim that plural quantification might be an obstacle to the unrestrictedness of singular first-order quantification. (After all, there are further things, pluralities, over which we haven't yet quantified.) Alternatively, we might think that we should adopt a lightweight view of higher-order quantifiers. Although this is not the mainstream view, it strikes me as a plausible option with a respectable philosophical pedigree.[25] But I will not argue here for a lightweight construal of higher-order quantification. Instead, as I suggested above, my goal is to establish a conditional conclusion: *if* higher-order quantification is ontologically heavyweight, then the unrestrictedness thesis cannot be true.

Still, we should note that establishing this conditional would be an important and surprising result, since the defenders of the unrestrictedness thesis *do* seem to regard higher-order quantifiers as having ontological weight. The issue is complicated in part because they generally eschew the ideology of ontological commitment (apart from anything else, this ideology assumes a first-order metalanguage).[26] Nonetheless, there are good reasons to think that the proponents of the unrestrictedness thesis understand higher-order quantifiers to enjoy a status very different from that of plural quantifiers (assuming the pluralist view, that is). Williamson, for instance, rejects the

[25] E.g. Prior 1971, p. 37, Rayo and Yablo 2001, Turner 2015.

[26] Williamson 2013, pp. 260–1. For a sustained attack on the ideology of "ontological commitment" see Burgess 2008. For reasons I won't discuss here I think this ideology may have more to it than critics allow.

claim that higher-order quantification is ontologically committal as ill-formed (*ontological* commitment being a notion applicable only to first-order discourse) but argues that higher-order quantified claims are nonetheless philosophically "heavyweight" and bear "metaphysical commitment" (Williamson 2013, pp. 260–1). This seems to overlap substantially with a view according to which all quantifiers are equally ontologically committing, but different quantifiers correspond to different ways of being—first-order being, second-order being etc.[27] Similarly, we find Nicholas Jones, one of the defenders of the Russellian proposal discussed above, claiming that "all orders of existential quantification express existence", while rejecting the view that only first-order quantifiers can express existence claims as unmotivated.[28]

At this point, I take myself to have established premise 1 of my argument. But so far I have had relatively little to say about the second premise. The defender of the unrestrictedness thesis could thus reject the analogy I have drawn by arguing that the sortal restriction is importantly unlike type-restriction. In what follows, I will defend premise 2 by considering two possible ways that sortal restriction might seem importantly disanalogous from type-restriction. I'll argue in both cases that appearances here are misleading and that there is no disanalogy. Along the way the positive case for taking sortal and type-restrictions to be *analogous* will become clearer.

[27] A view of this kind would be ontologically pluralist. See McDaniel 2017 for first-order versions of pluralist views. Although McDaniel does not focus on higher-order ontological pluralism (see though ibid., p. 41) such a view would seem to be one viable implementation of the ontological pluralist's ideas. See page 79 below for further discussion.

Williamson I suspect would reject this kind of interpretation, perhaps taking "metaphysical commitment" to be primitive. But for those of us for whom "metaphysical commitment" is *not* primitive, treating the differential commitments of different orders of quantification as different ways of being looks like a reasonable approach.

[28] Jones 2016, p. 148 and p. 146f respectively.

2.4 Objections to the analogy

In considering various ways that a defender of the unrestrictedness thesis might try to respond to the argument of section 2.2, it's worth noting an important distinction between two approaches to higher-order languages: one logical, one metaphysical. As the name would suggest, the logical tradition (e.g. Shapiro 1991) directs our attention towards the distinctive metalogical features of higher-order logic. Typically, proponents of this approach have in mind logics that are unlike first-order logic in being neither complete nor compact. By contrast, proponents of the metaphysical approach tend to use higher-order languages as object languages for metaphysical theory-building. When they do turn to model theory, the models they offer are typically complete and thus lack the metalogical properties of interest to proponents of the logic-first tradition.[29]

Corresponding roughly to these two ways of thinking about higher-order languages, two strategies are available for defending the unrestrictedness thesis. The first accords to typewise distinctions a unique metaphysical significance. Call this the metaphysics first approach. The second eschews this heavily metaphysical approach, pointing instead to metalogical differences between typewise and sortal restriction. Call this the logic first approach. I will consider both in turn.

[29] The most important metalogical property of second-order systems is that they allow for the construction of categorical models of infinite structures. (A theory is categorical just in case it has a single model up to isomorphism.) Every categorical theory is syntactically complete (i.e. for every formula f in the relevant language either f or $\neg f$ is provable). By Gödel's first incompleteness theorem, no recursively axiomatizable theory can be complete in this sense. But any *semantically* complete theory will be recursively axiomatizable. So no semantically complete theory with infinite models will be categorical. We'll return to these issues, see page 88 below.

For examples of the use of (semantically) complete models in the metaphysics-first tradition see e.g. the use of model-theory to give a consistency proof in Dorr 2016, p. 109ff and the various models developed in Bacon 2024, part IV. A third kind of approach uses higher-order meta-languages to give the relevant model theory. This is the approach hinted at in Williamson 2003 and pursued in Rayo and Uzquiano 1999.

2.4.1 Metaphysics first approaches

There is a simple and a more complex way of arguing that type-restriction is metaphysically disanalogous from sortal restriction. It will be helpful to begin with the simple argument.

The central claim of this argument is that the example underpinning premise 1 of the argument by analogy is misguided because it compares distinctions *within* a type to distinctions *across* types. I suggested, recall, that quantification over numbers was not ontologically unrestricted given that there are also cities, people, and mountains. For the proponent of the unrestrictedness thesis, though, it is no surprise that sortal restrictions *within* a type constitute a ontological restriction—for them, our intuition that a domain comprising numbers but not people is restricted is parasitic on the fact that both numbers and people are things of type e. It will be helpful to envisage the subsequent dialectic as a dialog with **U** playing the part of the defender of the unrestrictedness thesis and **O** playing the role of her opponent.

O: Suppose I accept that you are right about my initial example. Can we not rectify things by considering another version of the case? It doesn't seem to make a difference if we change "number" in the example to "property". But even you should think that properties are distinct from people, cities etc in the right way since in your theory they belong to types like $e \to t$.

U: I reject this characterization of my view. Properties like numbers are comprised in e; they are at best imperfect surrogates for the kinds of things in $e \to t$. As such your amendment in no way helps your case: it is just another way of restricting the quantifier over e.

O: Well but I reject your characterization of *my* view. When I say "property" I mean to describe what is expressed by terms of type $e \to t$ in your system. I grant that to be queen and Joe Biden belong to distinct, sui generis categories. But I simply help myself to a (kind of) first-order meta-language. Since it is my language, I can permissibly stipulate that I am using my word "property" such that "there is at least one property" interprets your assertion that "$\exists x_{e\to t}(x_{e\to t} =_{(e\to t)\to((e\to t)\to t)} x_{e\to t})$".

U: That's all well and good, but I deny that you can use a first-order meta-language to charitably interpret my utterances. My higher-order terms are essentially higher-order. They can be properly characterized only by a higher-order meta-language.

Let's momentarily summarize this discussion. **O** tried to show that the argument for premise 1 could be sustained if we replaced quantification over numbers with quantification over properties (thus addressing the initial complaint). **U** argued that this didn't answer her complaint because properties and numbers both belong to the type of individuals. **O** insisted otherwise. The best response for **U** at this point seems to require her to show that there is a deep metaphysical distinction over which **O** is riding roughshod. This is the import of her claim that her higher-order terms could only be adequately interpreted in a higher-order metalanguage.

Put differently, **U** here is claiming that many-sorted languages are importantly distinct from type-theoretic languages in that the former but not the latter can be translated using a language containing just one quantifier without doing violence to their metaphysical content. (On page 86 below, I return to a similar idea according to which

type-theoretic claims are unlike many-sorted claims in that they cannot be translated by a single-quantifier (i.e. first-order) language in virtue of their *metalogical* properties.) In what follows, I'll suggest that this is a mistake. My thought is not that we *can* in fact give first-order translations of type-theoretic views, but rather that if we take the syntactic restrictions imposed by many-sorted languages seriously, then we cannot offer adequate single-quantifier translations of many-sorted claims. Translations, the idea goes, normally need to be meaning-preserving, so the type-theorist can reasonably insist that they cannot be translated using just a first-order quantifier. But, if we take the view of the many-sorted theorist seriously (and assume, as I am, that the relevant sorts are disjoint, see note 16 above), then the same holds *mutatis mutandis* in her case also and so such would-be translations are equally inapt when it comes to many-sorted theories.

To see how these ideas might be sustained, we need to turn to the more complex argument I mentioned above. The critical claim made by **U** in this case is that type-restricted and sortally restricted quantification are importantly disanalogous because the first is faithful to the structure of reality in a way that the second is not. We might say that type-wise distinctions are unlike sortal-distinctions in that only the former are joint-carving. It is this, according to **U** that explains why single-quantifier translations are uniquely bad in her case. Again the dialectic can be helpfully presented as a dialog between **O** and **U**.

U: The argument for my claim [that type-theoretic quantification is special because it is metaphysically perspicuous in a way that sortal quantification is not] begins with Frege's famous observation that concepts and objects are categorically distinct in that the first are unsaturated (*ungesättigt*), whereas the latter are

saturated. Here we have the beginnings of a plausible form of pluralism. Its plausibility is reinforced by the way it mirrors the syntactic structure of natural language. My view is a kind of *neo-Fregean pluralism* that extends this idea to higher-order languages more generally, maintaining that the differences between expressions of different types mirrors an important difference in the structure of reality.[30]

O: I have some skepticism about the appeal of this picture, but let's temporarily set this aside since it looks like I have a straightforward response. For I claim that I can offer a many-sorted theory that captures the very same picture. Here is a schema for producing such a theory: starting with your type-theoretic regimentation of the idea, we replace type-subscripts with sortal subscripts, and then introduce an "instantiation" relation that combines with expressions of different types.[31] This schema generates a theory that seems to be structurally equivalent to yours. Of course, you have no analog of my instantiation relation, but since my system does not involve higher-order quantification, this relation is unsorted and cannot serve as arguments for other terms. As such, instantiation is effectively syncategorematic in this system. I could also introduce "λ" as a syncategore-

[30] For the general picture Cf. Lederman 2024, pp. 2–3. For the Fregean metaphor see Frege 1984a, p. 140, Frege 1984b, p. 193.

[31] More precisely, we first translate functionally-typed expressions into relational type theory (see page 83 below). We then replace type-subscripts with sortal subscripts. To simplify, we can use the same expressions to name sorts (i.e. the sort of propositions is $\langle\rangle$, the sort of individuals is e), writing in sans-serif to indicate that we are using sortal rather than typed-expressions. Schematically well-formed relationally typed expressions are of the form $A(B_1 \dots B_n)$ where A is of type $\langle \tau_1 \dots \tau_n \rangle$ and $B_1 \dots B_n$ are of types $\tau_1 \dots \tau_n$ respectively (Dorr 2016, p. 50). The sortal translation of "$A(B_1 \dots B_n)$" will thus contain terms of sorts $\langle \mathsf{T_1} \dots \mathsf{T_n} \rangle \mathsf{T_1} \dots \mathsf{T_n}$ respectively. To combine these expressions into a well-formed sortal formula we then take each sortal-term in the foregoing expression to stand in the instantiation relation to the others, i.e. "$\mathsf{I(A, B_1, \dots, B_n)}$". As this illustrates in the sorted system, the instantiation predicate has to be multigrade, i.e. variably-adic. Moreover, in this system, terms of any sort can fill any of the instantiation predicate's argument places. We recover the datum that certain kinds of claims are syntactically ill-formed by requiring that while terms of any sorts can fill any of the argument places, the filling of any one such argument imposes syntactic constraints on both the number of remaining argument places and on the sort of the terms that can fill these arguments. Cf. Shapiro 1991, pp. 74–5.

matic term-forming operator and lay down α, β, η-conversion as rules governing its behavior.[32] The resulting theory is uncannily similar to your higher-order language save in that it employs an instantiation relation. So any advantage of metaphysical perspicuity ought to accrue equally to my sorted system.

The more general thought is that many-sorted languages allow us to articulate a kind of ontological pluralism with the same structure as the Fregean idea that you want to capture. To use a metaphor, Frege's vision is that there are infinitely many distinct, non-overlapping ontological boxes, and no subsuming bigger box encompassing all other boxes.[33] But a many-sorted ideology allows for the development of an identical picture, with each sort corresponding to a discrete ontological box and no cross-sortal quantification of the kind that would correspond to a subsuming box. Of course, this will require a radical kind of sortalist picture on which there are no cross-sortal predicates of any kind—there will be for instance no notion of identity that holds in common of things of different sorts. This may be counter-intuitive. But you too face the very same problem: there is no cross-type notion of identity in a strict type theory either and there's no obvious reason to think that this is any more intuitive in your case than it is in mine. (We've seen this issue before, see page 67 above.)

To put the point differently, while I acknowledge that there are interesting and deep differences between pluralist views on which different ways of being can overlap and those on which they must be disjoint, or between pluralist views that allow for a generic notion of being and those that don't (cf. Caplan 2011), I am not seeing any interesting metaphysical difference between views that distinguish

[32] Thus, to take one example, extensional β-equivalence (Dorr 2016, p. 52) becomes: $\lambda x_i.I(T, x_i)[a_i] \leftrightarrow I(T, a_i)$.

[33] The metaphor is Caplan's, see Caplan 2011, p. 83 onwards.

their ontological categories type-theoretically and those that distinguish these categories using a many-sorted system.

U: Your attempt to capture the Fregean picture critically distorts his central idea and as such fails to offer an adequate formalization of the proposal. Notice that it was an important part of the initial Fregean picture that object and concept fitted together intrinsically, without need of any additional mediators. You must give up this element of the view, since on your picture entities of different sorts can combine only via an appropriate instantiation relation. But that is to say that even if you describe things of some sorts as "saturated" and things of other sorts as "unsaturated", you have nonetheless failed to uphold the intended sense in which concepts differ from objects. So your alternative does not give you a way to appropriate the metaphysical virtues of our proposal. Moreover, your system requires two syncategorematic terms—"λ" and "I" (the instantiation relation)—where I require just one. But proliferating syncategorematic terms in this way is a kind of unparsimoniousness. So there's a second respect in which your alternative doesn't realize the Fregean vision I set out.

O: I concede that you have identified a limitation in my proposal here. But I don't think that *you* are in a position to insist on this failing as a strike against my suggestion. For your system equally falls short of maximal fidelity to the original image of saturation; as such if my departures from this program are enough to disqualify my attempt to capture the Fregean vision, then they should disqualify your system also.

U: That's a bold claim. Can you say more about why I should think my type theory doesn't capture the metaphysical appeal behind the idea that predicates are unsaturated in a way that individuals are not?

O: Yes. The vision you outlined is *dualist*. There are two ontological categories: the saturated and the unsaturated. But your system is *pluralist* to an absurd degree since it comprises infinitely many types. And since the difference between any two types is as great as the difference between e and $e \to t$ we should think that the difference between each category in your system is akin to the distinction between the saturated and the unsaturated.[34] True, your proposal captures the idea of saturation better than mine in some ways, since it allows for unsaturated terms to immediately compose with saturated terms. But in other ways, your proposal falls short: we no longer have the neat partition of reality into saturated and unsaturated in accordance with a term's applicative behavior, but rather an utter fragmentation of ontological kinds.[35]

This is important: your initial, attractive, thought was that there is a categorical difference in the ontology implied by "$\exists x_e(x_e =_{e\to(e\to t)} x_e)$" as contrasted

[34] Frege implies that something is a function (and so presumably unsaturated) just in case it "contains an empty place", Frege 1984a, p. 147. This suggests that all types other than e and t are equally and wholly unsaturated. Perhaps then the ontological differences correspond not to differences in type but just to the deep distinction between the saturated and unsaturated. Thus quantification over $e \to t$ would be ontologically restricted so long as there are other unsaturated types (e.g. $t \to t$) but quantification over $e \cup t$ would be ontologically unrestricted since there are no other saturated entities. There are multiple problems with this kind of proposal: first the strict type-theorist should reject the idea that quantification of $e \cup t$ is well-formed. So she should think that even first-order quantification is ontologically restricted after all. But this gives up the unrestrictedness thesis. Secondly once we have conceded that it isn't the distinctions between *types* but rather the distinction between the saturated and unsaturated that matters, it is harder for the defender of unrestrictedness to insist that the type-theoretic system differs in metaphysically important ways from our many-sorted system. For neither the realm of the saturated nor the realm of the unsaturated correspond to types on this proposal.

[35] **U** could respond here by claiming that her system does in fact capture the distinction since unsaturated expressions correspond to all and only the functional types. But this is merely a notational accident: we could have taken p the type of monadic, first-order predicates as basic, and defined e as $p \to t$. Binary first-order predicates would then be of type $(p \to t) \to ((p \to t) \to t)$. This undermines any proposed account of unsaturatedness in terms of being a functional type.

with "$\exists x_{e\to t}(x_{e\to t} =_{(e\to t)\to((e\to t)\to t)} x_{e\to t})$" and that the metaphor of saturation gives some content to this difference. But I am now supposed to be able to make sense of the idea that there is a vast array of types that are as different from one another as e is from $e \to t$ and I find this objectionable. In particular, the problem is that I am supposed to distinguish infinitely many kinds within the realm of the unsaturated. This is hard to make sense of for two reasons.

First I have not been offered any supplement to the saturation imagery. It does not, for example, seem true to say that the value of a monadic second-order predicate is any less saturated than the value of a monadic first-order predicate: both have, as it were, a single "slot" for the requisite argument. Clearly first- and second-order predicates are different syntactically. But the claim can't be that the bare fact that different terms can be sorted into distinct syntactic categories implies that there are deep ontological differences since you rejected the analogous idea in the many-sorted case. The key thought was that some syntactic distinctions are special in a way made vivid by the image of saturation. But the vision you are outlining now goes far beyond that.

You might say that once you accept the distinction between the saturated and unsaturated, then the proliferation of types follows naturally as an extension of the initial thought. But—and this is my second reason for objecting that your proposal fails to do justice to the Fregean vision—this isn't quite correct. Frege's system relied on a second-order logic. In this system, monadic properties and binary relations belong to the same type, since they are equally values of second-order variables. Your system suggests otherwise: according to your proposal the difference between $e \to (e \to t)$ and $e \to t$ is as great as the difference between e

and $e \to t$. In other words, you have not only failed to supplement Frege's idea, you have in other ways undermined it.[36]

U: Hang on though, you're assuming I favor a functional type theory, but I could equally adopt a *relational* form of type theory. In the relational approach, we begin with the same basic type e of individuals but then allow that for any types τ_n, $n \geq 0$, $\langle \tau_1, \ldots \tau_n \rangle$ is a type. We interpret $\langle\rangle$ as the type of propositions. $\langle e \rangle$ is thus the type of monadic first-order predicates, while $\langle \langle\rangle, \langle\rangle \rangle$ is the type of binary sentential connectives. Arguably, the relational approach is metaphysically more perspicuous because it treats the arguments of a polyadic predicate as "syntactically on a par" in a way that functional approaches do not (Dorr 2016, p. 87). For instance, take the type of binary first-order relations. In a functional type theory, these belong to the type $e \to (e \to t)$. Technically this is a function from individuals to functions from individuals to propositions. But this framing is misleading: a binary first-order relation relates two individuals, not an individual and an individual-function pair. By contrast, in relational type theory binary first-order relations belong to the type $\langle e, e \rangle$, which correctly represents the relation as applying equally to two individuals. Perhaps I can use this kind of approach to allay your concerns.

O: I'm afraid I am skeptical that the shift to a relational approach will make any difference for two reasons: one technical and the other exegetical. The technical

[36] **U** could avoid these complaints by retreating to a simple second-order system in which there are just two types of things. I think this is probably her best option. I do not explore this option further here because it does not seem to be the favored approach among contemporary type-theorists. Even if it could be sustained, it would imply that the distinction between types is metaphysically deeper than the distinction between sorts because the former but not the latter can be explicated in terms of Frege's saturation imagery. This seems like a weak kind of response: the sortalist can invoke a distinction that is arguably in better standing than the Fregean metaphor by suggesting that the distinction between sorts can be explicated as a distinction between concreta and abstracta.

reason is that so long as you accept some standard rules of equivalence—I have in mind what the cognoscenti call $\beta\eta$-equivalence—then, given that we have restricted our functional type theory to types of the form e, t, or $\sigma \to \tau$ where σ and τ are types and $\tau \neq e$, the language of relational type theory is isomorphic to the language of functional type theory.[37] This suggests that "the choice whether to theorise in a relational or functional language is just a matter of taste" (Dorr 2016, p. 94). The exegetical reason is that the functional picture is truer to the Fregean proposal. The Fregean proposal is that we can analyze what he calls "concepts" as functions, which is to say not as relations.[38] Relatedly, the metaphor of saturation makes sense in a functional context, but applies less naturally in the relational picture.

U: Ok, but I never purported to be doing Frege-exegesis. My vision is *neo*-Fregean. So what if it departs from the intricacies of his view?

O: This is a fair point; I don't want to claim that you're beholden to Frege's particular vision. My thought, though, was that you introduced your view as an extension of the metaphor of saturation (a metaphor which happens to be Frege's). That's not yet a justification of the view, but it at least seems to constitute some kind of motivation. At this point, however, you seem to be developing a new kind of ontological pluralism that isn't well analogized by the initial metaphor. In itself that's fine, but this new view should be able to stand on its own merits and so far no defense has been offered.

[37] Dorr 2016, pp. 87–94. Let $\mathcal{R}$ be a mapping from the language of relational type theory to the language of functional type theory and $\mathcal{F}$ be a mapping from the language of functional type theory to the language of relational type theory. Dorr proves that for any term A in the language of functional type theory. $A^{\mathcal{R}\circ\mathcal{F}} \approx_\eta A$ (proposition 2.15) and for any term A of relational type theory $A^{\mathcal{F}\circ\mathcal{R}} \approx_{\beta\eta} A$ (proposition 2.18). Both $\mathcal{F}$ and $\mathcal{R}$ are thus isomorphisms.

[38] Frege 1984a, p. 146.

Of course, this absence doesn't imply that a justification *couldn't* be provided. So it's worth pointing out that your view is not only, as of yet undefended, but also frankly counterintuitive. For example, as I suggested previously, if we treat your proposal as a guide to the metaphysical structure of reality, we are required to think that there is a deep distinction between monadic predicates in type $e \to t$ and binary relations in type $e \to (e \to t)$. (Notice in passing that the relational type-theorist also has to hold that $\langle e \rangle$ and $\langle e, e \rangle$ are wholly different types.) But this seems to be precisely the *wrong* conclusion: it is counterintuitive to think that the number of argument places in a predicate makes a deep ontological difference. Differences in adicity look like an arbitrary consequence of linguistic diversity rather than a deep fact about the world.[39] Furthermore, this proliferation of ontological distinctions is disunifying and as such saps the proposal of explanatory power, at least in the ontological domain. The view is simultaneously unintuitive in a third way. The type distinctions align with the syntactic distinctions of a formal language so vast that only a tiny minority of its types will ever be cognized by any human.

Notice that I can say all of this without denying the utility or interest of type theories. I can coherently think that there are powerful abductive reasons to favor such approaches as a framework for contemporary metaphysics. My claim is simply that these reasons don't distinguish it from many-sorted frameworks insofar as we are concerned with the *ontological* import of type distinctions. I am very happy to say that we should often do metaphysics in a higher-order framework, I would merely allow that we should acknowledge that the existence

[39] Cf. Williamson 1985, p. 262 and Dorr 2004, p. 187.

of other types is a prima facie obstacle to the thought that quantification in any one type could be ontologically unrestricted, just as it is in the many-sorted case.

O appears to have the better of this argument. (As mentioned in note 36 above, **U**'s best option seems to be to retreat to the original Fregean proposal.) But there nonetheless seems to be something to **U**'s claim. Perhaps, then, the difference between many-sorted and higher-order quantification is not *metaphysical*, but *logical*.

2.4.2 Logic first approaches

The defender of the unrestrictedness thesis needs an explanation of why syntactic restrictions are relevant to a quantifier's unrestrictedness in type theory, but not in many-sorted first-order languages. She could argue for such a distinction by pointing to an intuitive connection between unrestrictedness and non-first-orderizability. Theories in a many-sorted first-order language are equivalent (modulo a condition described in the footnote) to a corresponding single-sorted first-order theory.[40] By contrast, theories articulated in a type-theoretic framework need not have an equivalent first-order analog. In other words, sort-specific quantifiers can be reinterpreted as restricted first-order quantifiers by embedding universal claims in the consequent of a conditional whose antecedent plays the role previously filled by sort distinctions, whereas typed quantifiers cannot. It seems to follow that sort-specific quantifiers in a many sorted

[40] Many sorted theories comprising *finitely many sorts* can equally well be expressed in a single-sorted theory, in which sort restrictions are replaced with restricting predicates. A corollary of this is that many-sorted theories with finitely many sorts are Morita equivalent (an extended form of definitional equivalence) to a single-sorted first-order theory. Theories with infinitely many sorts are not equivalent in this sense, but this is not much of a restriction in the present context. See Barrett and Halvorson 2017. (The defender of the unrestrictedness thesis might argue otherwise, pointing out that if many-sorted languages are to analogize type theory, there should be infinitely many sorts as there are infinitely many types. This though seems moot: if the domain of individuals is not ontologically unrestricted in a system in which there are just two types, then it seems that we cannot make it unrestricted merely by introducing a countable infinity of *further* types—if anything, the converse seems true.)

language are not unrestricted after all and so that there is a way for proponents of the unrestrictedness thesis to avoid the counterexample. In what follows, I will argue against the foregoing proposal. For simplicity I will focus on the suggestion that it is the *first-order* type-restricted quantifier ranging over things in e that is ontologically unrestricted and will limit my discussion to second-order languages.

There are two versions of the proposal that I am going to argue against. First we might hold that it is a necessary condition for a quantifier Q to be ontologically unrestricted that there is no truth-preserving translation that takes Q-quantified universals to universals articulated with a restricted first-order quantifier. Second we might prefer the view that it is a necessary condition for a quantifier Q to be ontologically unrestricted that Q is governed by a logic strictly stronger than first-order logic. These two views may well be extensionally equivalent and the response I will give applies equally to both. Both versions of the view, I claim, commit us to drawing invidious distinctions between restricted and unrestricted quantifiers. Since we should reject these distinctions, we should also reject the criterion that gives rise to them.

The suggestion then is that quantification counts as unrestricted in second-order but not many-sorted theories, because whereas many-sorted theories are always equivalent to a recursively axiomatisable single-sorted theory, the same does not go for theories in full second-order logic. The qualifier "full" here is important. To build models for second-order theories, we start with a first-order domain U and then define G, a second-order domain. We can think of G as consisting of subsets, relations, and functions on U. If G comprises *all* subsets, relations and functions on U, then

the corresponding model is said to be "full". Call the triple consisting of U, G and an interpretation function a model.[41]

The models we have just defined are known as "general models". Not all general models are full models, since G need not comprise all such subsets, relations, or functions over U. Importantly, the distinctive logical properties of second-order theories emerge when we restrict our attention to full models. (Often the modifier "second-order" is intended to refer to second-order theories as interpreted by full models, as when we say that second-order arithmetic rules out non-standard models.) If we are considering general models, it turns out that a logic quantifying over predicate position can be semantically complete and compact; by contrast, full second-order logic is (semantically) incomplete and not compact.[42] In other words, the non-first-orderizability of second-order theories relies on the assumption that we have in mind a "full" semantics that considers only the full models. Such an assumption is not implausible: it is only in a full semantics that the logical strength of second-order logic is realized. Nonetheless, it is worth noting that there is no formula f of second-order logic such that f is true only in full models.

Consider then the theory of second-order arithmetic understood as a particular set of sentences, the axioms of second-order Peano arithmetic and their semantic consequences.[43] We can distinguish full and merely general construals of this theory. These won't differ in their axioms, but they will differ in which sentences they include since

[41] A fully higher-order view would eschew the use of set theory to construct G favoring instead a fully higher-order metalanguage; for our purposes, however, it is a harmless and simplifying assumption. See Rayo and Uzquiano 1999 for a higher-order model theory.

[42] Recall that a logic is semantically complete iff every valid formula is a theorem in an appropriate proof procedure. We should distinguish this from *syntactic completeness*: a set of axioms is complete in this sense iff for any formula f it proves either f or $\neg f$. See note 29 above.

[43] This is a slightly unorthodox definition of a theory, since theories are usually understood to be sets of sentences closed under a relevant proof procedure. It is appropriate in this context because full second-order logic has no complete proof procedure; to illustrate the difference between full and general models we need therefore to focus on semantic consequence.

they differ in how they construe semantic consequence. (Normally, by *second*-order arithmetic, we mean to single out the full construal; here we can think of merely general second-order arithmetic as a kind of degenerate interpretation.) Specifically, full second-order arithmetic is *categorical* in that it pins down a single model up to isomorphism. As a result, for any sentence s *full* second-order arithmetic satisfies either s or $\neg s$. By contrast, merely general second-order arithmetic is consistent, recursively axiomatizable and, of course, expresses the theorems of arithmetic. So, by Gödel's, first incompleteness theorem it cannot be that for any sentence s *general* second-order arithmetic proves either s or $\neg s$. In particular, there is a Gödel sentence g for general second-order arithmetic (i.e. the simplest sentence asserting its own unprovability in second-order arithmetic) such that general second-order arithmetic does not prove either g or $\neg g$.[44] Additionally, since general second-order arithmetic is semantically complete it satisfies neither g or $\neg g$. So there is some sentence that is a member of *full* second-order arithmetic, but not merely general second-order arithmetic. This establishes that these two construals amount to genuinely distinct theories.

At this point we are considering a proposal according to which higher-order quantification is only a threat to the unrestrictedness of the first-order quantifier in systems that are first-orderizable. This response to my argument by analogy implies that the *first*-order quantifier in the merely-general theory of second-order arithmetic is restricted ontologically speaking, but that the first-order quantifier in the full theory is ontologically unrestricted. For, while the merely-general theory of second-order arithmetic is first-orderizable, the full theory of second-order arithmetic is not. The problem with this proposal is that this makes the (un)restrictedness of a quantifier turn on the

[44] More precisely g says that it is not provable using a proof procedure that is complete for *general* second-order arithmetic.

kinds of facts that oughtn't, intuitively, to make a difference. Whether the quantifier in our arithmetical theory counts as unrestricted ultimately depends on whether we are willing to use non-full models, which is to say on the comprehensiveness of the *second*-order domain G. It is an upshot of this that, holding fixed the first-order domain U, whether a first-order quantifier counts as unrestricted turns on which elements there are in the second-order domain. But intuitively, either the entities comprised in the second-order domain count against the unrestrictedness of the first-order domain or they don't. The present proposal violates this intuition, since the members of G are sometimes but not always counterexamples to the unrestrictedness of U, depending on what else is included in G. More surprisingly still, it turns out that, broadly speaking, members of G lose their standing as counterexamples to the unrestrictedness of U, when there are more of them.

One complaint someone might have is that whether a domain counts as unrestricted turns (in part) on extrinsic facts about it. This is not a good objection: a moment's reflection shows that the (un)restrictedness of a domain must *always* turn on extrinsic facts. Rather the thought is that whether some entity counts as a counterexample to the unrestrictedness of a quantifier should turn only on the intrinsic qualities of that *entity*. Notice that the initial Williamsonian defense of the unrestrictedness thesis was compatible with this idea. The thought there was that higher-order entities are *sui generis*, categorically distinct kinds of entities and that this difference is misleadingly elided when we use first-order metalanguages. This is an intrinsic difference in the relevant sense. By contrast, the proposal I am currently arguing against is not consistent with this claim: whether an element in the second-order domain bears on the restrictedness of the first-order domain depends on its extrinsic characteristics—

namely, on what else there is in the second-order domain. But I take this to be highly counterintuitive.

A final analogy reinforces the foregoing claim: "all the children are under 5 feet". Have I quantified unrestrictedly over all the children? Well it depends on whether or not Class 2B was within the scope of my quantifier. But it *clearly* does not depend on whether "all the teachers are red-headed" excludes Mr. Adler. Or, to vary the same idea: let C be the class of children over whom I quantified initially. We are abstaining on whether or not C encompasses all children. Nonetheless, whether my *first* generalization (concerning the children's height) was unrestricted does not depend on whether or not my claim "all the subsets of C are finite" ranges over all possible subsets of C. Analogously, it shouldn't be the comprehensiveness of the second-order domain that is the difference maker for first-order unrestrictedness.

2.5 Conclusion

The unrestrictedness thesis is less plausible than it might at first appear. More precisely, I've argued that insofar as higher-order quantifiers are ontologically heavyweight in a way that intuitively plural quantification is not, then type-restricted quantifiers in higher-order languages are not ontologically unrestricted. This matters because if quantifiers in type theories can't be ontologically unrestricted, then the use of higher-order ideologies for metaphysical theorizing comes at the expense of generality absolutism. Yet, as I noted in the beginning, much contemporary metaphysics relies on absolutist assumptions. We are faced with a three-way choice: give up absolutism and accept the ensuing revisionism, interpret higher-order quantification as ontologically *lightweight*, or eschew higher-order ideologies altogether. Williamson 2003 suggests

the choice may be even starker: his argument is that absolutism requires the introduction of higher-order frameworks to undercut an otherwise damaging objection. In that case, eschewing higher-order quantification won't suffice to save absolutism and so just two options remain: ontologically innocent higher-order quantification or an un-absolutist metaphysics.

In light of this, it's worth seeing *how* a theorist who embraced ontologically innocent higher-order quantification could avoid the arguments above. The idea is that in this case, higher-order quantification would be analogous to plural quantification (as it is standardly understood in the lightweight, pluralist interpretation). Thus that such we can quantify over things not in type *e* would be no reason to understand quantification over type *e* as anything less than ontologically unrestricted. Put differently, the syntactic restriction that limits our quantifier would be a joint-carving limitation, since it would coincide with a distinction between what there really is—i.e. the things over which we quantify with a heavyweight quantifier—and something like mere figures of speech—i.e. the "things" over which all other quantifiers range. (Compare: the values of plural terms like "the books" are not entities but just a way of talking about books.) Meanwhile we would be able to identify a relevant disanalogy between higher-order and many-sorted quantification since, according to the current proposal, the former is lightweight, whereas sortally restricted quantification is normally understood in heavyweight terms.

Chapter 3

Parsimony and complexity

In metaphysics, as in modernist architecture, less is sometimes more. That is, we often judge that theories that are simpler in some to be explicated sense are *ipso facto* more belief-worthy.[1] I'll have more to say below about the "ethics of terminology" below,[2] but for now I will reserve the term "parsimony" for this kind of theoretical belief-worthiness. As I'm understanding it, parsimony is one of several theoretical virtues, a list of which should include also things like strength, fit, unity, and elegance. I won't attempt to provide a comprehensive list or argue for the inclusion of any particular virtue, although I do assume that parsimony *is* virtuous in the relevant sense. The idea is that these virtues give us a broadly content-neutral way to evaluate whether or not a theory is good in the sense of belief-worthy.

I am interested in parsimoniousness because I believe that it can provide a naturalistically respectable way to shed light on important ontological distinctions. But for present purposes, my focus is more limited. I will argue that the parsimoniousness of a theory is not settled merely by the number of things over which it quantifies. There is

[1] This is not universally accepted, for a dissenting view see Parsons 1979, p. 660, quoted in Cowling 2013, p. 3890. I will not be considering such positions here.

[2] The phrase is from Quine 1951, p. 66.

a gap between being the value of a quantified variable in a given theory and imposing a theoretical cost in terms of parsimony. When it comes to parsimoniousness, some things count for more than others. (In my concluding section, I will return to consider the metaontological significance of this idea.)

In what follows, I'll argue for the foregoing conclusion. In broad brushstrokes, the intuition is that parsimony measures the kind of demand a theory makes of the world. If we accept that parsimoniousness is a theoretical virtue, then we should think that, other things being equal, it's better for a theory to less demanding in this regard. (The question of why parsimony might matter is a controversial one, on which I will not focus here.)[3] If, then, things can differ in their nature, it seems likely that they can differ in how much of a demand positing them makes of the world. My official argument, though, will turn on a case which I take to favor my view of parsimony. I'll argue, in particular, that rival views give implausible accounts of the relevant case.

3.1 Mapping the terrain

Let's start with a point about vocabulary: I'll assume that simplicity, economy, and parsimony are all kindred notions. The intuition is that there is a single theoretical virtue here, which goes by different monikers (that may perhaps illustrate different aspects of the virtue). It doesn't follow, though, that there is only one way to realize this virtue. In particular, it is common to distinguish between *ontological* and *ideological* or conceptual simplicity. As the name suggests, conceptual simplicity concerns the way a theory may uphold or offend against principles of parsimony in regards to the concepts with which it is formulated. It is rather harder for me to define ontological

[3] See e.g. Huemer 2009.

parsimony given my broader philosophical project. Let's suppose that theories are tools for explaining phenomena. In giving such explanations, it is routine to appeal to various *things*. Brownian motion, for example, is explained in part by positing *atoms*. Ontological simplicity, then, is simplicity with regards to the things posited by a theory.

I will be focusing on the kinds of simplicity that are typically characterized as ontological. It is for this reason that I favor the term "parsimony" which tends to connote this form of theoretical simplicity. Ultimately, I hope to argue elsewhere that reflecting on what it is to be parsimonious can help us to get a better grip on *ontology*. On pain of circularity, then, I'd better not characterize ontological parsimony as an ontological kind of simplicity. (This is the reason for my caginess in defining "ontological parsimony".) Metaphorically, we might say that ontological simplicity concerns the ingredients required by a recipe, whereas ideological simplicity concerns the complexity of the recipe's instructions. In these terms, the conclusion I want to argue for is that simplicity is not just a matter of how many ingredients are required, but turns also on the nature of the ingredients. A recipe for an omelet that calls for a dozen eggs of a wild turtledove is, *ceteris paribus*, more complex than one that calls for thirteen eggs of the familiar store-bought variety.

Within this loosely delineated subject-matter, there is a further cross-cutting distinction between so-called *qualitative* and *quantitative* parsimony. According to the former what matters is the number of types implied by a theory, according to the latter what matters is the number of tokens. Quantitative parsimony is not universally acknowledged as a theoretical virtue: David Lewis famously remarked" "I subscribe to the general view that qualitative parsimony is good in a philosophical or empirical

hypothesis; but I recognize no presumption whatever in favor of quantitative parsimony" (Lewis 1973, p. 87).[4] Yet Lewisian scruples notwithstanding, many of the clearest cases where parsimoniousness makes a difference to believability concern *quantitative* parsimony. Urbain Le Verrier explained perturbations in the orbit of Uranus by positing the existence of an eighth planet—Neptune. The problem could also have been resolved by positing multiple bodies to account for the relevant perturbations but such an explanation would have been deficient in virtue of its quantitative unparsimoniousness and was accordingly disfavored.[5] Moreover, qualitative conceptions of parsimony suffer from a notorious problem: there is no agreement on what exactly should count as a "type". At one extreme, if our type is "existents" then every theory posits just a single type and so is equally parsimonious; at the other, if our types are something like individual haecceities, then qualitative parsimony collapses into quantitative parsimony. Clearly neither of these can be quite right, but even when they are eliminated considerable ambiguity remains. For these reasons, although I hope that my discussion will apply broadly to both qualitative and quantitative conceptions, my focus will be on the latter.[6]

[4] Nolan 1997 and Jansson and Tallant 2017 offer defenses of quantitative parsimony.

[5] Jansson and Tallant 2017, p. 786.

[6] Quantitative conceptions of parsimony do face difficulties of their own when it comes to infinite cardinalities: *ceteris paribus*, positing only the even numbers looks more parsimonious than positing both the even and the odd numbers, but there is no difference in the cardinal (i.e. quantitative) scope of the two posits. Cf. Nolan 1997, p. 340; Jansson and Tallant 2017, pp. 788–9. This suggests that, nomenclature notwithstanding, the relevant virtue here must concern not quantity *per se* but something like superfluity. In this picture, two posits P, Q might differ with regard to quantitative parsimony even if they are of equal cardinality if P is a proper subset of Q. (Cf. Barnes 2000, on "anti-superfluity" vs "anti-quantity" principles.)

A second source of skepticism about quantitative conceptions of parsimony is connected to this first problem: the worry is that if a theory can be infinitely more parsimonious than another, then considerations of quantitative parsimony could outweigh all other considerations. But principles of parsimony do not in fact play this role in contemporary physical practice so something must have gone wrong. Happily, skepticism of this kind is readily addressed: even if $t1$ is infinitely less parsimonious than $t2$, the significance of this difference in parsimoniousness in the overall evaluation of the two theories can be bounded since infinite sequences can have finite upper bounds. Therefore, we can consistently think that i). parsimoniousness is always a *pro tanto* virtue, ii). quantitative parsimony matters, and iii). it could be epistemically rational for physicists to posit actual physical

I am more interested in using principles of parsimony as tools of theory-evaluation than as instruments for theory-construction. This is slighly unusual—the principles of parsimony that I will be investigating are often cast in imperatival form, as guides to theory building—but should be fairly intuitive.[7] To describe the parsimony of a theory T I'll write "$\mathcal{P}(T)$". According to the friend of parsimony if $\mathcal{P}(T) < \mathcal{P}(T^*)$ then we have a pro tanto reason to prefer T to $T*$. We might gloss this preference in difference ways—perhaps it amounts to according a higher credence to T or perhaps it gives us reasons to believe that T^* is false. But we needn't worry too much about these issues: what matters for our purposes is not the nature of theoretical betterness, but the question of what makes a theory count as parsimonious.

A final point: my "$\mathcal{P}(T)$" notation suggests that it should be possible to say in cardinal terms just how parsimonious a theory is. I regard this consequence as a helpful idealization: subsequently I will make claims that assume that we can evaluate parsimoniousness on an interval scale (i.e. that we can make sense of the variation in the *extent* to which one theory is more parsimonious than another). This assumption may be controversial and I will defend it in the appendix below: my suggestion will be that it is innocuous since the relevant conclusions can be recovered using a merely ordinal scale. In making this claim, I rely on assumptions about the comparability of various theories in terms of parsimoniousness; I need not assume, though, that any two arbi-

infinities. This explains, I hope, how we could think that quantitative parsimony is a genuine theoretical virtue without infringing against a methodological naturalism that insists that philosophers should be guided by the actual practice of contemporary physicists.

[7] This distinction corresponds to one drawn by Baron and Tallant 2018, p. 597 who distinguish between "constructive" and "comparative" forms of parsimony. I am interested in what they would "comparative" principles of parsimony. Happily we can transition fairly straightforwardly between constructive and comparative principles: where a constructive principle enjoins us to minimize some quantity, we take the corresponding comparative principle to assign to a given theory a parsimony evaluation that is inversely proportional to the relevant quantity

trary theories can be compared for parsimoniousness regardless of subject-matter and insofar as the notation implies otherwise it should not be interpreted overly literally.

We can now introduce the conclusions I hope to defend:

> **NECE** (*not everything counts equally)*: There are theories T, T', that each posit just a single thing, a, b respectively, such that $\mathcal{P}(T) > \mathcal{P}(T')$

I take this to suggest that in general positing b makes a theory unparsimonious to a greater extent than positing a does. In what follows, I will defend **NECE**. I will begin by introducing some rival views that imply the falsity of the principle, before going on to argue that these rival views fail to do justice to an important test-case.

3.2 The Laser

The Western canon bequeathed us a familiar account of parsimony: Ockham's famous razor:

> **Razor**: Do not multiply existents without necessity.

Two theories giving accounts that are otherwise equivalent can thus be compared for parsimony by counting the entities posited by each. A number of philosophers, including Jonathan Schaffer, Karen Bennett, and Ross Cameron have recently argued for a rival principle:

> **Laser**: Do not multiply *fundamental* existents without necessity.[8]

The idea of Schaffer et al is in effect that if you found the **Razor** attractive, you should prefer the **Laser**. I'll call this the "fundamentalist view". In what follows, I'll develop

[8] Similar principles are also endorsed by Bennett 2017, pp. 220–30 and Cameron 2010, p. 262.

some reasons for preferring the **Laser** to the **Razor**.[9] There is a growing literature on this topic and correspondingly a number of different arguments in both directions.[10] One recent commentator, for instance, identifies *seven* arguments in favor of the **Laser** (and suggests they all fail).[11] The argument that seems most persuasive to me appeals to cases that intuitively seem to be best explained by the **Laser**. Consider first a hypothetical example developed by Schaffer. We imagine two theorists, Esther and Feng. Esther offers a theory with 100 types of fundamental particle. Feng introduces an even more fundamental layer and develops a theory with 10 kinds of string that collectively compose Esther's 100 particles.

Feng's theory, we are supposed to judge is more parsimonious, yet the **Razor** returns the opposite verdict. To see this consider a toy model in which backwards-looking principles of parsimony assign to theories the reciprocal of the quantity the forward-looking form of the principle enjoins us to minimize. In the case of the **Razor**, then, the parsimony of a theory T is the reciprocal of the number of things posited by T, whereas for the **Laser** T's parsimony is equal to the reciprocal of the number of fundamental things posited by T. Given this, the **Razor** returns the verdict that Esther's theory is more parsimonious than Feng's—$\mathcal{P}_R(T_E) = \frac{1}{100} > \mathcal{P}_R(T_F) = \frac{1}{110}$—while the **Laser** returns the intuitive result that Feng's theory is the more parsimonious—$\mathcal{P}_L(T_E) = \frac{1}{100} < \mathcal{P}_L(T_F) = \frac{1}{10}$.

[9] It has been observed that in their *constructive* formulation (see note 7 above), the **Razor** entails the **Laser**. This is clearly not the intended interpretation of the **Laser** and it's interesting to note that the retrospective formulation captures the intended contrast between the two principles (namely, that they will disagree so long as there is at least one fundamental thing). In the prospective case, the problem can be resolved by adding a clause like "but multiply any non-fundamental entities *ad infinitum*" to the **Laser**. See Baron and Tallant 2018, pp. 597–8.

[10] For critical discussions see ibid., Thunder 2021, and Da Vee 2020. The dialectic is relatively involved: Baron and Tallant 2018, for instance, give objections to the **Laser** that are answered by Bennett 2017, pp. 227–8 (see also Da Vee 2020, p. 3687).

[11] Thunder 2021.

In response to this case, a number of commentators have suggested that the intuition is not that Feng's theory is more parsimonious, but only that it is all things considered better.[12] This betterness, it is argued, can be explained in terms of other theoretical virtues. Feng's theory is, for example, more unified than Esther's. But these considerations seem moot to me: proponents of the **Razor** have to think that Feng's theory is less parsimonious than Esther's; that it in some sense demands more of reality in terms of what exists. But that seems wrong; Feng's theory is not all things considered better than Esther's merely because it is more parsimonious. Nonetheless, we should reject the claim that it is any *less* parsimonious than Esther's account.[13]

Simon Thunder gives a different response; he argues that Schaffer's presentation of the case tacitly suggests that Feng's theory is true, in which case proponents of the **Razor** need not think the theory violates Ockham's maxim, since the posited entities are presumably "necessary" for the purposes of theory construction in the intended Ockhamite sense. This is a delicate maneuver for several, related, reasons. For one thing, the "necessity" clause in the **Razor** is somewhat ambiguous.[14] Presumably the thought is that entities are necessary if they are required in order to give adequate explanations or descriptions of the phenomenon under consideration. As Schaffer notes, however, this risks making the principle vacuous (Schaffer 2015, p. 645). For given the proposed construal of "necessary", the **Razor** seems to be entailed by the principle that one shouldn't do anything without good reason. But the **Razor** surely says something more informative. Considerations like these suggest that we might do better to interpret the **Razor** as a merely *pro tanto* requirement, setting aside the "without necessity" condition.

[12] Baron and Tallant 2018, p. 599; Da Vee 2020, p. 3681.
[13] See Thunder 2021, p. 7273 for similar ideas.
[14] Cf. Schaffer 2015, p. 645.

Indeed, backwards-looking non-imperatival principles of parsimony (so called "comparative" principles of parsimony, see note 7 above), which are directed towards questions of theory evaluation, are straightforwardly *pro tanto*. As a comparative principle of theory evaluation the **Razor** makes no mention of questions of "necessity". We might state the relevant principle, call it **Razor*** as follows:

Razor*: For a given theory T, $\mathcal{P}(T)$ = 1/existents according to T

A different kind of principle would evaluate parsimony in terms of the number of *unnecessary* existents according to T. Call this **Razor**†. This though is not typically how the **Razor** has been interpreted. (To the best of my knowledge, no one has objected to Schaffer's case on the grounds that Esther posits no unnecessary existents whatsoever and so has a maximally unparsimonious view.)

Moreover, there are reasons to prefer the **Razor*** over the **Razor**† as a construal of the backwards looking version of the principle. The problem is that the **Razor**† is devoid of dialectical utility. Theoretical virtues, recall, are intended to give broadly content-neutral ways of evaluating different views. But the debate about which entities are necessary in the requisite sense, just is a debate about the true theory of the relevant domain. Put differently, I can agree that my theory posits many things while holding that it has other virtues that outweigh its *pro tanto* costs in terms of parsimoniousness. But to claim that my theory includes unnecessary entities is to beg the question against me. The hope was, though, that theoretical virtues might give us a more objective way to make progress in otherwise intractable debates.[15] To return to

[15] Someone might worry that there is no good naturalist case for this kind of virtue. After all, many of the examples in the literature that motivate the relevant principles concern cases where scientists could have constructed theories that would have been less parsimonious. Perhaps the judgment that we're supposed to have in such cases is not that the alternative scientific theories would have been *worse*, but rather that they would have violated a version of the **Razor**. Such concerns would

Thunder's objection, then, the concern wasn't that Esther's theory violated the **Razor** as a constructive principle, but rather that the **Razor*** delivers a counterintuitive ordering of Esther and Feng's theories. This result holds without any presumption about the respective truth of the two views.

Still perhaps some vestige of the concern remains. Thunder writes:

> Strip away all the admiring language with which Schaffer describes Feng's theory, and what we are left with is a case in which we have two theories, T1 and T2, where T1 posits 100 fundamental entities, and T2 posits 10 fundamental entities and 100 non-fundamental entities. Nothing about this case seems to add anything to dialectic [sic]: those who find the Laser independently plausible will judge that T2 is ontologically simpler in the way that matters, those who find the Razor independently plausible will judge that T1 is ontologically simpler in the way that matters, and undecided parties will continue to be undecided. Thunder 2021, pp. 7273–4

If this is true, then it certainly does seem as though the friend of the **Laser** must look elsewhere for a correct analysis of parsimony. But I don't think it *is* right: it is always open to the defender of a view to assert that an argument by cases appeals to intuitions they don't share. In that sense, any such argument must rely on premises that its opponents cannot accept. Nonetheless concrete cases can help us to get a grip on the significance of abstract principles and in this case, Thunder's summary seems to me to leave out important details. For one thing, both Esther and Feng's theory are

be premature. For one thing, accepting certain maxims for construction strongly suggests that conforming to these maxims makes theories more belief-worthy given the (plausible) assumption that the goal of theory construction is to produce belief-worth theories. Moreover, there are examples of backwards-looking principles of parsimony being deployed in natural science: Sober 2015, pp. 209–15 for example, offers us the example of animal psychologists using principles of parsimony as tools of theory-evaluation.

likely to explain *additional* non-fundamental things; in both theories, the fundamentalia are put to explanatory work. Let's say then that T1 posits 1000 non-fundamental particles and T2 posits 1100 non-fundamental particles. Second, it is important that the 100 fundamental particles posited by T1 are among the 1100 *non-fundamental* particles that T2 explains. T2 subsumes T1. This second fact seems to me to be crucial in explaining why T2 doesn't strike us as any less parsimonious than T1. Nor does it beg the question against proponents of the **Razor**, for they must maintain that expanding a theory by positing a further, more fundamental explainer, invariably makes a theory less parsimonious. Since there is a case that suggests otherwise, the **Razor** should be set aside.

There are also some real-world cases with a similar structure. Compare non-reductive physicalist views about the mental with a strong form of substance dualism. According to our non-reductive physicalist there are two types of things—physical and mental—but only one fundamental type (the physical). According to the substance dualist there are also two types of things and these two types are (suppose) equi-fundamental. There is a reasonable intuition here that the physicalist view is more parsimonious. Perhaps it is otherwise deficient—maybe it doesn't account for philosophical zombies, but it has at least a *pro tanto* advantage in terms of parsimony. If we have this intuition, though, then only the **Laser** delivers the right verdict.[16]

[16] The foregoing argument tacitly appeals to Armstrong's intuition that supervening entities are no addition to being (Armstrong 1997, p. 12). Thunder 2021, pp. 7269–70 claims that the fan of the **Razor** has ways of accommodating this intuition. Since though the "no addition to being" judgment appears to license conclusions about the relative parsimoniousness of theories that the defender of the **Razor** cannot accept, I surmise that he is mistaken on this point. In any case, since my goal is not to defend the **Laser** but only to motivate it, these issues do not matter hugely for my purposes.

3.3 Monism and atomism

In this section, I will suggest that the **Laser** cannot be the correct account of parsimoniousness. First though it is important to set aside an objection that looks like it should put pressure on the **Laser**, but which, in my view, ultimately fails. The objection in question is straightforward: according to the **Laser** only fundamental entities matter when it comes to evaluating a theory for parsimoniousness. But it looks like we often rely on judgments about the relative parsimoniousness of theories that have nothing to say about fundamental reality. So the **Laser** would seem to be incomplete as an account of parsimoniousness; mistaken in virtue of its misguided assumption that we could only want to assess for parsimoniousness theories of fundamental reality.

We can make the point concretely with an example: in *Murder on the Orient Express*, the Istanbul to London train is snowbound in what is now Croatia. In one carriage of the train, one of the 14 passengers is murdered. Hercule Poirot confronts two possible explanations of this murder: the first is that the 12 remaining passengers (himself excluded) and the car's conductor colluded to kill the victim; the second is that an intruder boarded the train at some point, committed the crime and then disembarked. The second theory is simpler than the first, both intuitively and according to Poirot himself. Yet it is not ideologically simpler: both theories can be expressed using a single murdering relation. Nor according to the **Laser** does it seem to be ontologically simpler. Murderers are not fundamental on any plausible theory of fundamentality and neither theory has anything to say about any more fundamental entities. So the **Laser** seems to deliver the false verdict that the two theories are on a par with regards to parsimony. Versions of this thought seem to lie behind a number of common objections to the **Laser**.

But the idea is, I submit, mistaken. We should understand "fundamentality" in definitions of the **Laser** to refer not to what is *absolutely* metaphysically fundamental, but to whatever is explanatorily fundamental by the lights of the relevant theory. Put differently, there are absolute and relative conceptions of fundamentality. A theory that has nothing to say about what is absolutely fundamental can still recognize some portions of the world which it describes as *more* fundamental than anything else in the world according to the theory itself. To construe the **Laser** in this way is in effect to treat it as the view that only exogenous variables—explanatory starting points—count when it comes to assessing parsimony. When we make this amendment, the problem disappears: while murderers may not be absolutely fundamental, they are plausibly explanatorily fundamental in Poirot's theories. And since one theory allows for 12 murderers whereas the other requires just a single murder, the **Laser** supports the intuitive judgment that the 12-murderer theory is less parsimonious.

Nonetheless, I believe that the **Laser** cannot capture the full story when it comes to parsimoniousness. The problem arises when we consider a view like Schaffer's priority monism on which the cosmos is the primitive entity on which all else depends.[17] By the lights of the **Laser** monism should thus count as more economical than an atomist theory that took atoms to be the fundamentalia on which the cosmos ultimately depends. It isn't obvious though that monism *is* any more economical. It is not implausible to imagine that the best versions of the monist and atomist theories, call them M and A respectively, agree on all the things—derivative or fundamental—that there are but disagree about the dependence relations in which they are arrayed. Grounding theorists sometimes present their view metaphorically by suggesting that we should

[17] Schaffer 2010.

imagine that reality comprises arrows leading from grounds to grounded. If we take this metaphor seriously, there is a sense in which *M* and *A* are converses: to get from one to the other we need only reverse the direction in which the arrows point. (The diagram below offers a toy-model of this idea.) Intuitively, though, merely reversing the order of dependence doesn't make for differences in parsimony—films played in reverse, for example, are not significantly simpler than those that run forwards.[18] Yet *M* and *A* differ only with regard to the direction of explanatory priority; the **Laser's** sensitivity to this difference therefore produces misleading conclusions.[19] The problem is that merely directional transitions between theories don't seem to affect the strength of the ontological demand they make of the world.

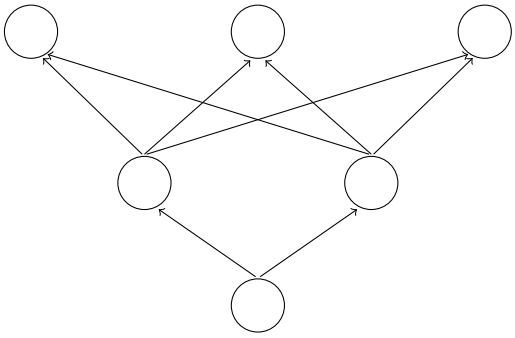


Figure 1 | *M: Priority Monism*

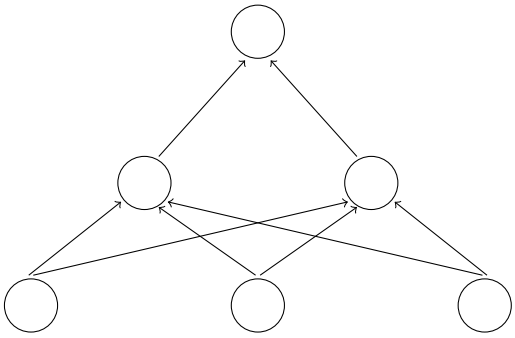


Figure 2 | *A: Atomism*

There's a different way of arguing for the same conclusion. The argument in question begins with what seems to be the strongest consideration favoring (priority) monism. In rough form, the monist's idea is that contemporary physical theories—specifically quantum mechanics—suggest that the cosmos forms one entangled system, containing information that cannot be derived from the physical states and

[18] Audiences readily comprehend, for example, the use of reverse motion techniques in cinematography.

[19] There is an implicit argument by analogy here: if directionality does not make a difference with regards to simplicity when it comes to causal dependence (as in reversed films), then it shouldn't make a difference when it comes to explanatory dependence. We could strengthen this analogy by pointing to the ways that causal dependency often serves as a paradigm of explanatory dependence.

relations of its parts.[20] This suggests that physics favors a monist account of the cosmos. For it is assumed that the fundamental things must be complete in the sense that they can fully account for the derivative entities. But if the properties of an entangled system are not settled by the properties of its parts, then its parts cannot be more fundamental than the system as a whole on pain of violating completeness.

Consider now how a similar argument, appealing this time to classical mechanics, might go. Could the monist argue that in this case their view is to be preferred on grounds of parsimoniousness? It is hard to see how such an argument could be sustained. Whether we describe a physical system as a collection of n interacting particles in a standard three-dimensional Euclidean space or as a single point in a $3n$ dimensional Euclidean space seemingly makes *no* difference in terms of simplicity.[21] Regardless of which framework we choose, the underlying mathematics will be equivalent and so, by scientific standards, there is nothing to choose between them. But in that case, given that the legitimacy of appeals to parsimony relies on the application of similar principles in natural science, it would be a mistake to conclude that a monist formulation of classical mechanics is more parsimonious than its atomist rival.[22] But if

[20] See Schaffer 2010, pp. 50–7.

[21] Cf. Albert 2023, pp. 88–100. Thank you to Brendan Kolb who discussed these ideas with me and drew my attention to the relevance of Albert's work here.

[22] Jill North argues that Lagrangian mechanics should be preferred over Newtonian mechanics since the former contains a lesser degree of "structure" (North 2021, chapter 4). Given that Lagrangian formulations have a somewhat more holistic character than Newtonian mechanics we might consider her to be giving an objection to the foregoing argument. An adequate discussion of this idea would take us beyond the parameters of this project but there are a number of points worth highlighting. 1). North's claims are somewhat orthogonal to the claims I made above because notwithstanding the more holistic character of the Lagrangian formulation, both Lagrangian and Newtonian mechanics can equally be understood as theories describing a multiplicity of particles moving around in three dimensional space. In other words, it is a mistake to read Lagrangian mechanics here as a stand in for a monist theory of classical mechanics. 2). North's suggestion illustrates how our judgments about parsimoniousness are connected to questions about theoretical equivalence. Such questions are controversial in contemporary philosophy of science (for one view see Barrett and Halvorson 2016) and we should expect that those with different views of theoretical equivalence will reach different conclusions about what it is to be parsimonious. More specifically, 3). as she herself notes, North's view is at odds with the orthodoxy in contemporary physics (e.g. North 2021, p. 107). I suspect her idea is that these are *philosophical* judgments and so the attitudes of physicists are not dispositive. This may be right from a metaphysical perspective, but insofar as we are trying to model our parsimony-based

the monist formulation isn't more parsimonious in the classical case, then other things being equal, we should thing that it also isn't any more parsimonious *simpliciter*.

Of course, other things are not equal. The argument of the previous paragraph assumed that we can set aside non-classical quantum behavior. As mentioned above, though, taking quantum mechanics seriously seems to strongly favor monism. Significantly, though, the differences that emerge when we consider quantum phenomena do not seem to be relevant to the *simplicity* or parsimoniousness of M. Interestingly enough, some of the strongest rivals to monistic interpretations of entanglement seem to require backwards causation. And while this might be objectionable, most obviously because it is deeply unintuitive, these objections do not turn on considerations of simplicity.[23] In other words, accommodating quantum phenomena may well favor monism, but it doesn't do so in a way that bears on the relative simplicity of monism and atomism.

At this point, I take myself to have given some powerful reasons for thinking that M and A are on a par with regards to parsimony. But even if you disagree with the foregoing reasoning and want to insist that M *is* more economical than A, you arguably shouldn't think the difference between them is as vast as the difference be-

reasoning on scientific practice (i.e. a more epistemological inquiry), naturalism tends to favor treating the judgments of physicists as fixed points. 4). North's argument is that Newtonian mechanics has a "privileged" (Cartesian) coordinatization since the mathematical formulations of the relevant laws are not invariant under different coordinatizations. This is understood to imply that Newtonian mechanics requires more physical structure than Lagrangian mechanics, since the formulation of the laws in the latter is invariant under different coordinatizations. North's argument draws here on an analogy with a (very plausible) form of argument that draws inferences about the underlying spatiotemporal structure required by a theory by considering what is preserved under various permutations (cf. Maudlin 1994, chapter 2). The analogy notwithstanding, the difference that North has identified between different theories of classical mechanics seems to me to be very thin. On this front, Caspar Jacobs' analysis seems more plausible: "Perhaps the conclusion we ought to draw is not so much that Lagrangian mechanics does not posit a Euclidean metric, but rather that it doesn't care about the representation of that metric, contrary to Newtonian mechanics…I suspect that the form-invariance of the Euler–Lagrange equation does not signal that space has no Euclidean metric, but rather that it is more flexible in how one represents that metric." (Jacobs 2022)

[23] For details see Lewis 2016, pp. 168–73 and pp. 116–19.

tween 1 and the (mindbogglingly enormous) number of mereological atoms in our cosmos. A cursory google search suggests that there may be around 10^{80} physical atoms in the actual universe.[24] The number of mereological atoms is presumably greater still. Schaffer notes that while the **Laser** prices fundamental entities $1:0$ with derivative entities, other variants are possible (see Schaffer 2015, p. 648). We might think we could calibrate this ratio in order to reduce the discrepancy between M and A. There is not, though, a plausible way to amend this ratio that recovers a reasonable comparison of our theories.

To see this, suppose we are considering versions of the theories $M*$ and $A*$ on which there are no intermediate entities: $M*$ and $A*$ recognize only the cosmos and the mereological atoms that constitute it. And let's introduce a third theory $D*$, which corresponds to the dualist view that there are *two* fundamental things. To get a grip on what would be an appropriate discount rate for pricing derivative entities we can ask how many derivative entities $M*$ would need to posit for it to count as overall *less* parsimonious than $D*$. Suppose that $M*$ would be less parsimonious than $D*$ only if it posited the existence of an extra 10 billion people in our world. A second google search suggests that there are 10^{27} chemical atoms in the human body.[25] If the **Laser** is to preserve the conclusion that $M*$ would be less parsimonious than $D*$ only if it posited the existence of an extra 10 billion people in our world, then we must set the ratio of fundamental entities to derivative entities at than $1:10^{-37}$. (This is a preposterously steep discount rate; as we'll see shortly this only makes the problem starker.) The issue now is that given the vast numbers of atoms in the universe, the existence of fundamentalia makes practically no difference to the parsimony of a theory like

[24] `https://en.wikipedia.org/wiki/Observable_universe#Matter_content`. Accessed 06/14/2024.
[25] `https://www.amnh.org/exhibitions/permanent/scales-of-the-universe/atoms`. Accessed 06/14/2024.

$M*$. Let's again assume the same toy-model of parsimony that assigns to theories the reciprocal of the number of fundamental entities they posit and, for the sake of simplicity, we'll also assume that chemical atoms are mereologically simple. If we do so, $\mathcal{P}(M*)$ is approximately $\frac{1}{(10^{80}+10^{37})\times 10^{-37}+1} = \frac{1}{10^{43}+2}$; if we ignore the single fundamental thing posited by $M*$, we get $\mathcal{P}(M*) = \frac{1}{10^{43}+1} \approx \frac{1}{10^{43}+2}$. In other words, notwithstanding our ridiculously steep discount rate, the fundamentalia here make essentially no difference to how parsimonious $M*$ is. Of course, $M*$ is still massively more parsimonious than $A*$; the point is not that discounting skews the comparison between $M*$ and $A*$, but rather that even given extraordinarily steep discount rates, the parsimoniousness of $M*$ considered in isolation on this view is almost entirely dictated by how many derivative things it posits. But this seems inconsistent with the intuition behind the **Laser** which was that fundamental existents matter more when we consider how parsimonious a theory is. In other words, we can recover reasonable judgments via the **Laser** only by applying it in ways that are inconsistent with its intuitive rationale.

It's worth noting in passing that there is an analogous problem for the **Razor**. Consider existence monism, the view that there exists just a single thing—the cosmos.[26] The most plausible versions of existence monism should allow that our world is largely as it seems: the best confirmed theories of natural science are getting something right. The monist's claims concern the metaphysical aspects of this reality; her suggestion is that what naïvely seem to be different things are in fact something like different ways of viewing the cosmos. Now consider a different theory according to which reality is comprised of just two mereological atoms which together fuse to make a third thing. This theory is false, it fails to explain our rich experiences. But it should

[26] See Horgan and Potrč 2008 and Horgan and Potrč 2012.

count as simple. By contrast, existence monism is not a particularly simple theory: the cosmos may be unitary but it is richly structured and internally differentiated. Yet according to the **Razor**, the monist's theory is more parsimonious than the two-atom theory. The **Razor** therefore gets this case wrong and must be incomplete as an account of what it is to be parsimonious.

3.4 Responses

In this section, I will address a fundamentalist response to the objection I have just been developing against the **Laser**. According to proponents of this response, our ambivalence about the merits of monism is explained once we see that its advantages in terms of parsimony are counterbalanced by increases in complexity of some other kind. More carefully, the idea is as follows: I have argued that the **Laser** should be deprecated because it wrongly implies that monist theories are more parsimonious than their atomist counterparts. The fundamentalist responds that this implication is not wrong, it is merely obscured because any gains in terms of parsimony are fully or largely offset by losses in terms of other kinds of complexity. (Alternatively, offering what I understand to be a verbal variation of the foregoing response, they might claim that *ontologically speaking* monism is more parsimonious, but that this is counterbalanced by a different kind of parsimony.) In what follows, I'll argue against this: I will suggest that monism and atomism are effectively on a par when it comes to simplicity. Subsequently I will try to diagnose the intuition that might lead us to conclude otherwise and explain why this intuition does not support the proposed response.

3.4.1 More on why monism and atomism are equally simple

I will argue that *if* monism is not ontologically more parsimonious than atomism, then there is no respect in which monism differs simplicity-wise from atomism. This leaves open a view to which I appealed in section 3.3 that holds that monism is more parsimonious than atomism, but not on the vast scale implied by the **Laser**. It nonetheless suggests that the fundamentalist response under consideration cannot be sustained. The basic idea is that monist and atomist descriptions of the world will be of equal length and complexity, differences in the number of fundamentalia notwithstanding. At first glance, we might be inclined to think otherwise: perhaps the monist account of reality could have the form "Fa", where "a" is the name of the cosmos and "F" is a predicate that fully describes the state of the cosmos at a given point in time.

The problem is that this kind of account is unperspicuous. Given the right kind of language, any theory can be expressed by saying that p, where "p" is the proposition describing how the world is according to the relevant theory. Clearly, though, this is not enough to establish that the relevant theory can give a simple, elegant description of the way the world is. Analogously, adequately describing the world on the monist view requires describing a location in a high-dimensional space of possibilities. Doing so, the thought is, will inevitably have to draw on considerable descriptive resources. Of course, atomists also need a rich descriptive ideology; the point is that the two views are on a par here.

This may be obscured by an unfortunate ambiguity that arises in talk of fundamentality. Concepts are fundamental or primitive if they are basic in the sense of being undefined. For instance, we can formulate classical extensional mereology in terms of parthood or in terms of overlap. If we go the first route, we can define "overlap" as

the property that holds of two things when they share a common part. If we go the second route, we define parthood by saying that a thing a is a part of b just in case everything that overlaps with a also overlaps with b. Similarly, we could develop set-theory either in terms of the membership relation or in terms of subsethood. These are examples of how a theory might be developed from different primitives. By contrast, a thing is fundamental in the sense intended here iff its existence is ungrounded by the lights of the relevant theory.

Suppose then that the existence of the cosmos is grounded by atoms a, b, c and that these atoms are either F or G. This complex tangle of facts (e.g. that $(Fa \wedge Ga) \wedge \neg(Fb \wedge Gb)$ etc.) also grounds the intuitively more complex properties of the cosmos. Notice, though, that it need not *ipso facto* follow that the concepts that *express* these more complex properties are derivative. Let's use "κ" to refer to the cosmos and "P" to describe some complex property. Even if it is true that the fact that $P\kappa$ is grounded in the fact that $Ga \wedge Fb \wedge \neg Fc$ it doesn't follow that Pness is defined in these terms. Less abstractly, the redness of my sweater may be grounded in its being scarlet, but it isn't the case that what it is for it to be red is for it to be scarlet. Grounding entities, then, doesn't eliminate the need for primitive ideology.[27] Since the atomist and the monist agree on everything except the grounding structure, they will presumably need the same primitive ideology since the worlds they are describing are all but indiscernible empirically speaking. ("All but" since there are perhaps empirical considerations arising out of quantum mechanics that favor monism.) But in that case, they should count as equally ideologically parsimonious.

[27] Similar disanalogies between a concept's being primitive and an entity's being fundamental are highlighted in Da Vee 2020, 3682f. Da Vee suggests (rightly in my view) that this undermines Schaffer's argument for the **Laser** by appeal to an analogy between ontological and ideological parsimony. See Schaffer 2015, pp. 649–51.

It seems to follow that if there is any difference in *ontological* parsimony between monism and atomism, then if these ontological advantages of monism are to be offset, it must be by some other, third kind of simplicity/complexity that is neither ontological nor ideological. But it is hard to see how this could be. For one thing, the invocation of yet more kinds of simplicity/complexity seems to me to be itself unparsimonious.[28] It also lacks a clear naturalist basis: it certainly isn't obvious that a threeway distinction between regards of simplicity falls out of the norms of natural science. We might instead try to motivate the proliferation of varieties of simplicity by reflection on possible cases. Consider 30 red balls. Here are two theories: i). the red balls are arranged randomly, ii). the red balls are arranged in a straight line. Perhaps you have the intuition that ii) is somehow simpler. This isn't ideological simplicity as that concept is normally understood: both theories can be articulated in terms of the concepts of being red, being a ball and being space-wise located. Nor is it ontological simplicity: in both theories there are exactly thirty red balls. So if you have the aforementioned intuition you might be compelled to introduce a third kind of simplicity.[29] I doubt though that this will help in the present case. For it isn't obvious why if we countenance this kind of quasi-geometrical simplicity, we should think that monist theories fare worse than atomist theories on this front.[30]

[28] It is a kind of meta-unparsimoniousness: a lack of parsimony in our theories about theoretical simplicity.

[29] One way to develop this idea would be appeal to information-theoretic concepts of complexity. The most well-known of these is Kolmogorov complexity according to which (roughly) the complexity of a string is a function of the length of the program in a privileged language required to reproduce the string in question. Since in ii) the location of the balls is highly patterned, the relevant information is highly compressible. In a 2D plane, for instance, the distribution of the balls might be given by the following rule $\forall b_i(y(b_i) = 1 \wedge x(b_i) = i)$, where $b_i, 1 \leq i \leq 30$, is one of the red balls and $y(b)$ is a function from balls to their y-coordinates and likewise for $x(b)$. By contrast, the geometric properties of the balls in i) are highly incompressible. So stating i) will typically require more informational resources and so it will count as more complex. (For details on Kolmogorov complexity see Prokopenko, Boschetti, and Ryan 2009; for more general remarks on the applications of computational complexity theory particularly to assessing theoretical simplicity see Aaronson 2013, §7.)

[30] Suppose, for example, we take seriously a gloss according to which this kind of simplicity is the inverse of Kolmogorov complexity as suggested in note 29 above. In the vast majority of programming

This isn't an exhaustive survey. Perhaps there is a third kind of simplicity, whose introduction is philosophically well-motivated and with regards to which A and M clearly are different. But the prospects look dim. On this basis, I am going to assume such a possibility can be set aside. I am encouraged in doing so by the observation that we can get to the same place by a slightly different route. For notice that when it comes to monism and atomism we are dealing with two very similar views. If these are to differ simplicity-wise with regards to something other than ontological parsimony we must think that reversing the grounding-order changes the simplicity of a theory in two (potentially cross-cutting) ways. This, though, is hard to swallow: it is a general phenomenon that the proposition that two things are R-wise related is equivalent to the proposition that these two things are related by a converse of the R relation. For example, if John is Mary's husband, then Mary is John's wife.[31] So we should be able to reformulate both the monist and atomist theories in terms of a converse of grounding.[32] Instead of saying that the cosmos grounds its parts, the monist now says that the parts are grounded in the cosmos. Call this M'. M' is equivalent to and just as simple as M, the original theory formulated using the grounding relation. Yet M' is also structurally indistinguishable from A, the rival view to M. (It's not equivalent to A because it differs in other non-structural ways.) So this structure can't be a difference-maker when it comes to simplicity. But there doesn't seem to be much else that could distinguish M and A, save the difference in what is fundamental which is already factored into our assessments of ontological parsimony.

languages, M and A ought to count as equally Kolmogorov-complex, since they are essentially the same theory with directionality reversed, in which case A won't be any more Kolmogorov-simple than M.

[31] See Williamson 1985 and Fine 2000.

[32] I am assuming here a relationalist conception of grounding. The alternative treats ground as a sentential operator. I don't think this should make much of a difference for the argument I am giving (we should be able to construct a converse operator etc. etc.) See deRosset 2023, pp. 35–8.

3.4.2 Unification

There's one other distinction between the monist and atomist theories to which we should attend. Namely that monist theories seem to have a pro tanto advantage in terms of unification, since they have a single explanandum where atomist theories have many explananda, and are thus more unified and therefore more explanatory. This observation is *prima facie* consistent with the observations I have been making, since the relevant virtue is not a virtue of simplicity and there's no reason to think it is offset by any corresponding losses in terms of complexity. By my own lights, though, reasoning of this kind is not especially satisfactory since it engages in what I regard as the invidious exercise of trying to neatly delineate distinct theoretical virtues. The gap between unification and simplicity, for instance, is not nearly as large as it can be made to seem. According to Sober, for example, common cause explanations are simpler than explanations that proliferate coincidences (Sober 2015, pp. 102–19). When we ask *why* simplicity of this kind is theoretically virtuous we learn that it is because it is unifying.[33] If unity and simplicity are distinct virtues, though, this kind of answer should be a misnomer. This is grist to the fundamentalist's mill: she can argue that these observations suggest that the reasoning of the previous subsection notwithstanding, the respect in which monist theories improve on their atomist rivals is indeed a gain in simplicity. I've argued at length though there is no great difference in how simple monism and atomism are. So something must have gone wrong.

There are two possibilities.[34] One option is that monism is indeed more unified than atomism and that unification is, in this instance, a genuinely distinct kind of

[33] Cf. Sober 2015, p. 102.

[34] This is a simplification: I don't consider, for instance, the possibility that unification is not a genuine theoretical virtue.

virtue. After all, consistent with arguing that the boundaries between theoretical virtues are indeterminate, we could think that there are nonetheless determinate paradigms of particular virtues. Alternatively, perhaps there is no real difference with regards to unification, appearances notwithstanding. I prefer a version of the second approach. If monism is to be preferred to atomism on the grounds of unity, then it should be true that a monistic account of classical mechanics should be preferred to an atomistic interpretation on the grounds of unification. But, as above, it seems to me that setting aside issues of entanglement, there's nothing to recommend monism over atomism as an account of our physical reality. This sets up a prima facie puzzle though: for the monist theory certainly *looks* more unified on its face. If unification is generally a virtue, why wouldn't it be in this case?

The answer to this question that I find most compelling appeals to a kind of use/mention distinction. The rough idea is that there is a difference between explaining by unifying and asserting that there is a unified explanation. The distinction between explaining and asserting that there is an explanation is straightforward: insisting that there is some explanatory connection between various phenomena is not yet enough to explain them, for we don't bring explanations into existence just by describing them as such. To succeed an explanation must instead help us to see *how* the explanans gives rise to the explanandum. Analogously, the assertion that several events have a common explanation is not itself a unified explanation of these phenomena. Declaring, for example, that the Battle of the Somme and the October Revolution share a common explanation doesn't yet explain these events. This suggests that being unified is a kind of theoretical virtue when it is predicated of explanations, but not when it's predicated of statements of explanation.

The conclusion of the previous paragraph will only help us to say something about why monism's apparently more unified character doesn't constitute a theoretical virtue if it turns out that monism and atomism are best regarded as making assertions about explanations, rather than as substantive explanations in their own right. And this should be controversial: it is standard to assume that explaining how something is grounded constitutes a genuine explanation. My contention, though, is that neither monism nor atomism *explain how* the various parts of our world are grounded.[35] For simplicity, I'll focus on monism since it is monism that ostensibly enjoys an explanatory advantage. I nonetheless take the relevant considerations to support the analogous conclusions in the case of atomism.

As I've stated it, monism assumes that grounding can relate *things*: the cosmos, an individual in the world, grounds other individuals, including ultimately the mereological atoms. This is consistent with the way that Schaffer, the leading exponent of priority monism, presented the view and also with at least one iteration of Schaffer's views about grounding.[36] This makes it harder to see grounding as a kind of explanation in the sense required for unification to be relevant: generally, we use expressions like "because" or "in virtue of" to give explanations. The former connects sentences, the latter connects sentences and singular terms, in neither case do explanations relate two individuals.[37] Even if we overlook this, putative explanations using a singular term to pick out the cosmos look defective. Call the cosmos "Mos" and our mereological atom "Tom". I find it very hard to hear "Mos grounds Tom" as any kind of

[35] My argument for this conclusion in the next three paragraphs closely follows Ted Sider (Sider 2020, pp. 77–82) who defends a position in this vicinity.

[36] The issue is complicated because Schaffer's views on grounding have evolved in the years since 2010, when his seminal defense of monism was published.

[37] Trogdon 2013, p. 102. Of course, in *describing* explanations we might use the verb "explains" which can take two singular arguments. I would contend, though, that statements of this kind aren't themselves explanations, so much as assertions about explanations.

explanation. It is more normal to think of grounding as relating facts (or similarly, as an operand on propositions). Accordingly, we might conclude that monism should be interpreted as claiming that fact that the cosmos exists explains the fact that this mereological atom exists. I *can* hear this as explanatory. But I think I can do so because I am implicitly assuming some further facts about the cosmos: the cosmos can explain the existence of my chair, but that's not just because the cosmos exists but also because of how it is. We can see this again using the names we introduced earlier: consider "the fact that Mos exists explains the fact that Tom exists". Maybe that's true, but I still want to know *how* and that desire on my part is testament to the unexplanatory character of the original statement.

This then is the critical point: for claims about the cosmos to be explanatory they need to say something about its nature. But notice that once we do so, we are no longer appealing to a single explanans but to many. Perhaps Mos explains Tom because some region of Mos has a certain energetic profile. But if Tom is part of some other assemblage of atoms, then explanations of this assemblage will need to appeal to different aspects of Mos. This though upsets any appeal to unification. There's a final possibility: perhaps we could say simply that Mos is P, where "P" is a predicate that fully describes Mos. The problem now is that explanation is hyperintensional: "P" simply *names* Mos's nature. We might as well say "Mos cosmoses" (using the verb "to cosmos"). To know *how* Mos cosmosing could ground the existence of other things we need to know a lot more about what it is to cosmos, which is to say we need to know many more facts about Mos. But again this undercuts monism's unified character.

A final wrinkle: a monist might at this point appeal to some kind of mediating laws. This appeal would be well-motivated: Schaffer, the arch-monist, later came to

endorse a view on which grounding requires laws of metaphysics.[38] Perhaps if we filled the laws of metaphysics in, we could see how the cosmos might be explanatory. The same problems seem to recur, however: so long as we are working in a strictly monist paradigm, the antecedents of the laws can only pick out Mos's cosmosing. As such, the relevant laws would be condemned to simply recapitulate the unexplanatory connections I examined above.

3.5 Three accounts of Complexity

In this section, I want to focus on the lessons we can draw from the shortcomings of the **Laser** when confronted by monist theories. My contention will be that the **Laser** and other familiar accounts of parsimony ignore an important kind of structure which I will shortly introduce. We need to attend to this structure in order to give a better account of what's going on when monism and atomism are compared for parsimoniousness.

Compare the priority monist's cosmos and the mereological atoms that are fundamental for the atomist. The former is intuitively a lot more complex. If we think, loosely, of parsimony as a measure of the kind of demand a theory makes of the world then it's tempting to think that positing a complex thing makes a greater demand than positing a relatively simple thing. This suggests a revised picture of parsimoniousness. In this picture, we add to the **Laser** a weighting of each fundamental entity in terms of its complexity. Call the result "complex parsimony" **(CP)**:

> **(CP):** $\mathcal{P}(T)$ is inversely propositional to $\sum_i C(x_i)$ for fundamental entities $x_1, \ldots x_n$, where $C(x_i)$ is a measure of the complexity of x_i.

[38] Schaffer 2017.

We might add to **CP** a version of Schaffer's pricing proposal. This would mean that the number of derivative entities posited by a theory also counts towards its parsimoniousness, albeit at a discounted rate. The logic of **CP** suggests that if they are counted in this way, derivative entities should also be weighted in proportion to the extent to that they have internal structure.

This is not intended as a final analysis, but rather as a stepping stone for further research. It should be clear, though, how it might allow us to avoid the kind of problem that the **Laser** encountered. In principle it will be possible to increase a theory's parsimoniousness by trading in multiple derivative entities for fewer fundamental things. But no cheating is permitted. If these entities recapitulate internally the same structure realized by the tangle of relations among the derivative entities, there will be no net gain. The only way to improve parsimony-wise is if the fundamental entities in question are not so complex as to cancel out any numerical gains. And this looks like the right result: if derivative entities really can be traded in for relatively simple fundamentalia, it does seem that we have genuinely advanced in terms of parsimoniousness.

I have not yet given an account of complexity. In the remainder of this section, I will survey three different proposals. For the purposes of this paper, any of the proposals would suffice. As we'll see, each implies (correctly in my view) that the monist's cosmos is much more complex than the atomist's simples and thus explain why atomism and monism don't differ meaningfully with regard to simplicity even though monism posits far fewer fundamental entities. Nonetheless, in the context of the broader philosophical project of which this paper is a part, there are reasons that favor what I'll call the qualitative account of complexity. Although it's possible to

give a sketch of these reasons, a full defense of the qualitative account falls outside the scope of the present investigation.

Let's start though with what I'll call the structuralist account of complexity. The basic intuition is that an entity is complex to the degree that it encodes information that can equally be encoded by a complex (external) structure holding between several distinct entities. The complexity of an external structure is in turn understood to be a function of the density of the relations and connections that hold between the nodes that realize the relevant structure. We can introduce the idea by considering the work of Jason Turner, Turner 2016. Turner defends a form of factualism (the view that our world is a world composed exclusively of facts) and shows that the content expressed by structured facts could equally well be conveyed in terms of unstructured facts standing in a rich array of quasi-geometric relations. The complete view is extremely involved, but here we need only sketch the beginnings of the idea.

We start with the observation that there seem to be important connections between the fact that *Rab* and the fact that *Rac*. Intuitively, both these facts are in some sense *about* relation *R* and individual *a*. Turner's view, though, begins with a set of unstructured, atomic facts $f, g, h \ldots$. Suppose that f expresses the state of affairs that we would normally describe by saying that "*Rab*" and g expresses the state of affairs that we'd normally describe by saying that "*Rac*". The problem is that if f and g are unstructured, then we seem to lack the resources to explain how they could describe states of affairs that are intuitively connected. Turner's solution is to develop a complex suite of quasi-geometric relations that can hold between facts like f and g. It turns out that there is a way of developing this network of relations that preserves the very same

connections that we observed among the structured facts without giving up the idea that f, g etc are unstructured.

To see how we might develop such a project, we begin by thinking of the possible R-relations things might stand in using a 2D plane, with possible occupants of the first argument place along the y-axis, and possible occupants of the second argument place along the x-axis. In this framework, any fact of the form $Ra\tau$ for some individual τ can be understood as a point on the horizontal line y = a. We recover the connections that seemed to hold between f and g then by saying that they are co-linear. More generally, two facts are colinear when (intuitively) they are about a given individual standing in a given relation to some other entities. It turns out that we can extend this program to define various other quasi-geometric structures in which facts might stand. I've explained colinearity in terms of the connections that intuitively obtain between structured facts, but in the official ideology of Turner's program things proceed in the opposite direction; our world is a world of unstructured facts standing in quasi-geometric relations. In virtue of these relations (colinearity etc.) we can equally well describe the unstructured facts by attributing an internal structure to them such that various facts count as being in some sense *about* the same kinds of things.

The general lesson is that we can carve up theoretical content in different ways: either with sophisticated structures and simple nodes, or more complex entities standing in simpler, external structures. Turner's program shows how to trade in theories comprising complex facts standing in simple relations for views that posit simple entities in complex structures, but the idea can be run in both directions. I propose to call information that would otherwise be conveyed structurally but is captured by the intrinsic monadic properties of individuals "internal structure". The contrast of the

atomist and monist theories is something like the limit case of the phenomenon we've just been observing. Mereological atoms are highly unstructured; the cosmos seems to have an extraordinarily rich structure. My suggestion is that we do not alter the demands a theory makes of the world by shifting the same informational content from external to internal structure. When moving house, you can choose whether to package your effects into multiple smaller boxes or fewer larger containers. Either way, though, the quantity of effects is unchanged. Analogously, the ontological demand imposed on the world by shifting from internal to external structure or vice versa is unchanged.

Turn now to a second account of complexity. I will call the view in question, the "configuration space view". Configuration space is a way of fully describing the state that a given system is in at a given time. The dimensions of configuration space correspond to the different degrees of freedom with regards to which an entity could vary. In a sentence, the configuration space view of complexity holds that an entity e's complexity is equal to the dimensionality of the configuration space required to describe e. Electrons, for instance, are less complex than persons because whereas electrons have only a few degrees of freedom—corresponding to their spin, charge, position and so forth—to describe a person we need to specify a location within a vast quality space, describing their conscious and physical states and the relationships between their various parts.

Again, it is straightforward to see under this view why monism is not vastly more parsimonious than atomism. The cosmos inhabits a configuration space of much higher-dimensionality than mereological atoms. Hence it constitutes a far more complex kind of thing. The configuration space view will often coincide with the struc-

turalist view. Two theories that describe the same system must convey the same quantity of information if they are to fully specify the state of the relevant system. This information can be conveyed either by positing many things inhabiting a lower dimensional space, or with fewer things inhabiting a higher-dimensional space, but in either case, the descriptions must fix the state of the system along an equal number of axes of possible variation.

The third view and the one that best coheres with the broader philosophical project to which this investigation belongs is a little different. On this account, an entity is complex to the extent that it is qualitatively heterogeneous. Being qualitatively heterogeneous is related to but not the same as being a composite entity. That is, while it is generally true that decomposing an entity into its parts will reduce qualitative heterogeneity it is not necessarily the case that mereological simples will be qualitatively homogeneous. Whether or not such simples are homogeneous in this way depends in part of whether or not they are extended. If they are unextended then they must be homogeneous (there is no extension along which they could vary in the ways heterogeneity requires). If they are extended, however, things are more complex. Some have argued that extended simples would have to be homogeneous.[39] If, though, we accept (as I am inclined to do) that extended simples could be heterogeneous,[40] say by instantiating distributional properties, then we might be tempted by the view that there is no limit to *how* heterogeneous they can be. Intuitively, if being heterogeneous doesn't imply being composite, then being massively heterogeneous shouldn't imply being composite either.

[39] E.g. Spencer 2010.

[40] See Parsons 2004 and McDaniel 2009. Parsons but not McDaniel favors appealing to distributional properties.

Nonetheless, it is very natural to assume that there is some connection between divisibility and qualitative variegation. Correspondingly, division tends to reduce heterogeneity: there are more than a hundred chemical elements, but these are made up of just three kinds of things—neutrons, protons, and electrons. These particles, and their particulate constituents in the case of neutrons and protons, seem in turn to be relatively homogeneous. Accordingly, it seems overwhelmingly plausible that we can say that simples will be far less heterogeneous than the cosmos, even in the absence of a full theory of mereology. (Since we have reasons to think simples will be relatively homogeneous and the cosmos is, quite literally, maximally heterogeneous.) As with our other criteria, then, the qualitative heterogeneity account of complexity supports the conclusion that monism should not count as significantly more parsimonious than atomism.[41]

As I indicated above, for present purposes any of these accounts would do equally well. Yet the third proposal, which ties complexity to qualitative heterogeneity, is the most relevant to the broader investigation of ontological innocence. The idea, which I have not defended in this dissertation but hope to develop elsewhere, is that there might be a connection between being sufficiently complex and being the value of an

[41] This isn't *necessarily* the case: it would for instance be false in a monist theory according to which the cosmos is composite but qualitatively homogeneous. Call this homogenized monism. If the homogenized cosmos is no more complex than a mereological simple, then perhaps homogenized monism is vastly more parsimonious than atomism. I don't find this possibility troubling: homogenized monism is a massively deficient empirical theory of our world. Insofar as the theory is more parsimonious than its atomist rivals, this is moot since it looks like it will utterly fail to explain the manifest world of appearances. (Compare: the theory that there is just a single, unextended mereological simple is much more parsimonious than almost every possible rival view. It's still clearly a bad theory.) This is not coincidental: a theory's capacity to explain our rich and complicated universe seems to be, at least in part, an artifact of the degree of qualitative variegation for which it allows. Atomist theories make space for such variegation by varying arrangements of homogeneous simples; monist theories, by contrast, are forced to posit a qualitatively variegated cosmos to do the same work. In dispensing with this variegation, the gains homogenized monism makes in terms of parsimony are outweighed by its loss of explanatory power.

ontologically committal quantifier.[42] Put differently, ontologically innocent quantification is quantification over things that fall below a certain complexity threshold.

This idea will work, though, only if there is a non-arbitrary way to specify the relevant threshold. In light of this, the configuration space view seems like a red herring: anything whatsoever over which we can quantify must inhabit a configuration space with a certain dimensionality. And there isn't obviously a non-arbitrary way to specify a number of dimensions n such that any entity with fewer than n degrees of freedom is somehow less than fully real. By contrast, the qualitative heterogeneity view does offer a natural cut-off point: most entities exist on a spectrum of increasing qualitative heterogeneity. But not everything has a qualitative nature; such things cannot even be aptly described as qualitatively homogeneous. Williamsonian bare possibilia (Williamson 1998), for example, or abstracta, the empty set say, are prime candidates for the kinds of things that might fail to be even minimally complex on this picture.

The structuralist picture falls somewhere between these extremes. On the one hand, the view could be connected to structuralist theories of mathematics.[43] The upshot of a view like this would be that mathematical objects, like the number 2, have no *internal* structure but stand in a rich *external* structure. Such things are maximally simple and therefore seem to be the kind of entity over which ontologically innocent quantifiers might range. At the same time, however, the structuralist view of complexity suggests that we might be able to trade in the rich external structure that obtains between the natural numbers for a single entity standing in no interesting relations, but with a rich and developed internal structure. Such things would seem to be highly complex and it is hard to imagine that quantification over entities like these could be

[42] Although see page 44 above.
[43] E.g. Shapiro 1997.

ontologically innocent. This is troubling insofar as it suggests a picture whereby we can trade off ontologically innocent and ontologically committal quantification without adopting meaningfully different theories. Intuitively, though, the difference between the values of ontologically innocent and ontologically committal quantifiers is deeper than this picture seems to indicate.

For these reasons, the general direction of my project favors accepting an account that ties complexity to qualitative heterogeneity. Yet, as noted above, for our immediate purposes, any of the accounts I have canvassed would suffice and the brief sketch of reasons to prefer the qualitative heterogeneity as a criterion of complexity falls far short of a fully-fledged defense of that account.

3.6 Conclusion

The foregoing argument was in the first instance an account of how best to understand the theoretical virtue of parsimony. Yet, as I suggested at various points, these also have a broader significance insofar as we are interested in vindicating ontological innocence. Recognizing that different entities make different contributions to the parsimoniousness of theories sets the stage for an account of quantification that does not treat all the entities over which a theory quantifies equally. This in turn can help us to explicate the difference between ontologically innocent and ontologically committal quantification. Put differently, our conclusion can help us to begin to make sense of the extra ingredient required for an entity to have being rather than merely existence (to use the terminology of chapter 1).

Yet we must be careful here: ontologically innocent quantification is not to be defined as the kind of quantification that doesn't contribute towards the parsimonious-

ness of a theory. Instead, the hope is that we can shed light on how ontologically innocent quantification might be possible and meaningful by showing what turns on whether or not a quantifier is innocent. If it can be shown that the innocence of a quantifier makes a difference to how we apply principles of parsimony that would be a step in the right direction. It would also cohere with one of the implications of the first two chapters: namely that whether or not a quantifier is ontologically innocent has a bearing on whether or not it could threaten the absolutely unrestricted character of other disjoint quantifier.[44]

[44] The ideas in this chapter were presented under the title "Metaphysical Parsimony" at the 2022 meeting of the Eastern Division of the American Philosophical Association in Maryland, Baltimore. I am grateful to Martina Botti for her comments on that occasion.

Appendix: Can we measure parsimony on an interval scale?

A concern looms: the argument I gave in chapter 3 assumed that we could make sense of the thought the gap between the parsimoniousness of *A* and *M* shouldn't be as great as the gap between 1 and a number larger than 10^{80}. But this assumes that we evaluate parsimony along what is called an interval scale.[1] Consider an analogy. Suppose that we are ranking our favorite movies. I rank *The Shining* above *Jurassic Park* and you rank *Rear Window* above *Citizen Kane*. We may disagree with one another about these judgments, but they should be intelligible to us. But we will have a much tougher time making sense of claims about the scale of differences in our preference-ranking. Imagine that we both agree that *Black Swan* and *American Beauty* are good but not outstanding movies and we both prefer the former to the latter, but I insist that you are wrong about the extent to which the former is preferable. I think that I think that the gap between them is smaller than you think and I think I'm right in my aesthetic judgments and you're wrong in yours. You can reasonably object that you have *no* views on the question I am raising. You were in the business of saying which films you preferred. But it would be a mug's game to try to assign some kind of transcendental

[1] For discussion see Tal 2020, §3.2.

goodness score to the films. You found *Black Swan* more compelling than *American Beauty*. How much more compelling you are unable to say.

Your protest is essentially that you were ranking movies on an ordinal scale—a scale that, as the name would suggest, encodes information about the ordering of various items. My insistence on thinking about how much better the various movies are wrongly assumed that we were using an interval scale—a scale that, as the name suggests, includes information about the gaps between the various items. Temperature is measured on an interval scale, favorite movies are measured on an ordinal scale. The former encodes information that the latter does not and so the intelligibility of ordinal rankings need not imply the intelligibility of interval judgments.[2]

A second analogy may help to make the point. While we can say that the difference between 1°C and 2°C is less than the difference between 3°C and 10°C, it is a mistake to think of 10°C as being twice as hot as 5°C. To see this notice what happens when we change units from °C to °F. (For simplicity I have listed the relevant conversions in table 1 below.) In Farenheit, the gap between 1°C and 2°C is 1.8°F. This is indeed

°C	°F
1	33.8
2	35.6
3	37.4
5	41
10	50

Table 1 | *Temperature Conversions*

smaller than the gap between 3°C and 10°C, which is 12.6°F. But while 10°C looks like

[2] Formally different scales are distinguished by the transformations under which they are invariant. The more information a scale encodes, the more limited the class of transformations that preserve invariance. Thus ratio scales are invariant under scalar transformations whereas interval scales are invariant under affine transformations—i.e. a function f such that $f(x) = ax + b$ for $a, b \in \mathbb{R}$ (an affine transformation is a scalar transformations iff $b = 0$). Ordinal scales are invariant under any monotonic transformation. See Tal 2020, §3.2 and Titelbaum 2022, ii: pp. 293–4.

it should be twice as hot as 5°C, 50°F is not twice as hot as 41°F. The point here is that temperature can be measured on an interval but not a ratio scale. Judgments about the ratios between temperatures are therefore incoherent. (By contrast, mass can be measured on a ratio scale. 10kg is twice as much as 5kg—in lbs, 22lb is twice as much as 11lb.)

Let's return to parsimony. The argument of the previous section assumed that interval judgments about parsimony were meaningful. But it's by no means obvious that this is true. Someone might not want to say that the extent to which Feng's theory is more parsimonious than Esther's is less than the extent to which M is more parsimonious than A. Parsimoniousness might be like movie rankings rather than temperature. In what follows, I will try to answer this objection by showing how we could recover information about the parsimony-interval between M and A from a set of ordinal judgments about parsimoniousness.

I suggested that the gap between $\mathcal{P}_L(M)$ and $\mathcal{P}_L(A)$ is implausibly large. We can think of the models of parsimony I've been using, according to which the parsimoniousness of a theory according to the **Laser** is equal to the reciprocal of the number of fundamental entities posited by that theory, as giving us a scale on which a maximally parsimonious theory is assigned 1 and a maximally unparsimonious theory is assigned a number arbitrarily close to 0. According to these models, the gap between $\mathcal{P}_L(M)$ and $\mathcal{P}_L(A)$ is given by $(1 - \frac{1}{10^{80}}) \approx 1$. In other words, these two theories differ about as much in terms of their parsimoniousness as it is possible for two theories to differ. This accounts for the suggestion that the gap between them is inplausibly large. But how can parsimony be measured on an interval scale?

The strategy is straightforward. The judgment that the gap between theory the parsimoniousness of A and B is larger than the gap between C and D can be understood as a way of encoding the fact that the number of theories T_i such that $\mathcal{P}(A) > \mathcal{P}(T_i) > \mathcal{P}(B)$ is larger than the number of theories T_j such that $\mathcal{P}(C) > \mathcal{P}(T_j) > \mathcal{P}(D)$. Suppose for simplicity that we are using the **Laser** to evaluate parsimoniousness and that $\mathcal{P}(A) = \frac{1}{1}, \mathcal{P}(B) = \frac{1}{10}$ and $\mathcal{P}(C) = \frac{1}{2}, \mathcal{P}(D) = \frac{1}{3}$. In other words, A is a form of monism, B posits two fundamental objects, C posits three, and D posits ten. The thought is that there are extensions of A positing $2, 3, \ldots, 9$ objects that should less parsimonious than A but more parsimonious than B. By contrast, there are no such extensions of C. that are less parsimonious than C itself, but more parsimonious than D. So the gap between the parsimoniousness of C and D is negligible, whereas the gap between A and B is more significant.

This style of reasoning relies on a number of non-trivial assumptions. First I've assumed that the relevant extensions of A/C can all be meaningfully compared for parsimoniousness with all the other theories. I'll return to this assumption below. I've also assumed that relative judgments about parsimoniousness are transitive. Again I'll return to this below. Most obviously, I've assumed that for comparable theories T, T' it is possible to get from $\mathcal{P}(T)$ to $\mathcal{P}(T')$ in a series of discrete steps. This assumption is perfectly appropriate given the models of parsimony I've favored: since there can only ever be natural-number many fundamentalia, it will be possible to get from $\mathcal{P}(T)$ to $\mathcal{P}(T')$ in $|m - n|$ steps, where $m, n \in \mathbb{N}$ are the number of fundamentalia according to T and T' respectively.

Nevertheless, different kinds of models might undercut this assumption. Suppose we use Schaffer's discounting proposal, discounting derivative entities at a rate of $1:$

10^{-2}. Consider then a theory T^* that posits 1 fundamental thing, and 25678 derivative things. Given Schaffer's proposal, T^*'s parsimoniousness is inversely proportional too 257.78. Clearly $257.78 \notin \mathbb{N}$. A simple solution is to express the relevant ratio as $100 : 1$. Given this $\mathcal{P}(T^*) = \frac{100}{25778}$ and, as before, it should be possible to get to the parsimony values for different theories using a series of discrete steps.

Return then to the other assumptions mentioned above. I do not want to claim that these assumptions—essentially the completeness and transitivity of parsimony orderings—hold universally. Happily the foregoing argument requires no such claim; I need only hold that if two theories $T1, T2$ are comparable then extensions of $T1$ are comparable both to the original theory (i.e. unextended versions of $T1$) and also to $T2$. Again this seems plausible. If monism and atomism are comparable parsimony-wise, then dualism, the theory that there are two fundamentalia corresponding to two halves of the cosmos, should also be a comparable view. Ultimately, then, the objection that I introduced at the start of this appendix is misplaced: the use of an interval scale here is appropriate because it can be understood to encode information that is meaningful within an ordinal scale.

Bibliography

Aaronson, Scott (2013). "Why Philosophers Should Care about Computational Complexity". In B. Jack Copeland, Carl J. Posy, and Oron Shagrir (eds.), *Computability: Turing, Gödel, Church, and Beyond*. Cambridge, MA: MIT Press, pp. 261–328.

Albert, David Z. (2023). *A Guess at the Riddle: Essays obn the Physical Underpinnings of Quantum Mechanics*. Cambridge, MA: Harvard UP.

Armstrong, David M. (1997). *A World of States of Affairs*. Cambridge: UP.

Azzouni, Jody (2004). *Deflating Existential Consequence: A Case for Nominalism*. Oxford: UP.

Bacon, Andrew (2019). "Is Reality Fundamentally Qualitative?" *Philosophical Studies* 176.1, pp. 259–295.

— (2024). *A Philosophical Introduction to Higher-order logics*. New York, NY: Routledge.

Barnes, E. C. (2000). "Ockham's Razor and the Anti-Superfluity Principle". *Erkenntnis* 53.3, pp. 353–374.

Baron, Sam and Tallant, Jonathan (2018). "Do Not Revise Ockham's Razor Without Necessity". *Philosophy and Phenomenological Research* 96.3, pp. 596–619.

Barrett, Thomas William and Halvorson, Hans (2016). "Glymour and Quine on Theoretical Equivalence". *Journal of Philosophical Logic* 45.5, pp. 467–483.

— (2017). "Quine's conjecture on many-sorted logic". *Synthese* 194.9, pp. 3563–3582.

Bennett, Karen (2017). *Making Things Up*. Oxford: UP.

Boolos, George (1984). "To Be is To Be a Value of a Variable (Or To Be Some Values of Some Variables)". *The Journal of Philosophy* 81.8, pp. 430–449.

— (1985). "Nominalist Platonism". *The Philosophical Review* 94.3, pp. 327–344.

Builes, David (2019). "Pluralism and the problem of purity". *Analysis* 79.3, pp. 394–402.

Burgess, John P. (2008). "Being Explained Away". In *Mathematics, Models, and Modality: Selected Philosophical Essays*. Cambridge: UP, pp. 85–103.

Burgess, John P. and Rosen, Gideon (1997). *A Subject with no Object: strategies for nominalistic interpretation of mathematics*. Oxford: UP.

Button, Tim and Trueman, Robert (2021). "Against Cumulative Type Theory". *Review of Symbolic Logic* 15.4, pp. 907–949.

Cameron, Ross P. (2010). "How to have a radically minimal ontology". *Philosophical Studies* 151.2, pp. 294–264.

— (2019). "Truthmaking, second-order quantification, and ontological commitment". *Analytic Philosophy* 60.4, pp. 336–360.

Caplan, Ben (2011). "Ontological Superpluralism". *Philosophical Perspectives* 25, pp. 79–114.

Cartwright, Richard L. (1994). "Speaking of Everything". *Noûs* 28.1, pp. 1–20.

Chalmers, David J. (2002). "Does Conceivability Entail Possibility". In T. Gendler and John Hawthorne (eds.), *Conceivability and Possibility*. Oxford: UP.

Colyvan, Mark (2024). "Indispensability Arguments in the Philosophy of Mathematics". In Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*. Summer 2024

edn. URL: https://plato.stanford.edu/archives/sum2024/entries/mathphil-indis/ (visited on 07/19/2024).

Cowling, Sam (2013). "Ideological Parsimony". *Synthese* 190.17, pp. 3889–3998.

Da Vee, Dean (2020). "Why Ockham's Razor should be preferred to the Laser". *Philosophical Studies* 177.12, pp. 3679–3694.

deRosset, Louis (2023). *Fundamental Things: Theory and Applications of Grounding*. Oxford: UP.

Dorr, Cian (2004). "Non-Symmetric Relations". In Dean Zimmerman (ed.), *Oxford Studies in Metaphysics*. Oxford: UP.

— (2016). "To be F is to be G". *Philosophical Perspectives* 30, pp. 39–134.

Dummett, Michael (1978 [1963]). "The Philosophical Significance of Gödel's Theorem". In *Truth and Other Enigmas*. Cambridge, MA: Harvard UP, pp. 186–201.

Fales, Evan (1990). *Causation and Universals*. London: Routledge.

Fine, Kit (1994). "Essence and Modality". *Philosophical Perspectives* 8, pp. 1–16.

— (2000). "Neutral Relations". *The Philosophical Review* 109.1, pp. 1–33.

— (2001). "The Question of Realism". *Philosophers Imprint* 1.1, pp. 1–31.

— (2005). "Tense and Reality". In *Modality and Tense*. Oxford: UP, pp. 261–320.

— (2006). "Relatively Unrestricted Quantification". In *Absolute Generality*. Oxford: Clarendon Press, pp. 20–44.

— (2009). "On the Question of Ontology". In David J. Chalmers, David Manley, and Ryan Wasserman (eds.), *Metametaphysics: new essays on the foundations of ontology*. Oxford: UP, pp. 157–177.

Florio, Salvatore and Jones, Nicholas K. (2021). "Unrestricted Quantification and the Structure of Type Theory". *Philosophy and Phenomenological Research* 102.1, pp. 44–64.

— (2023). "Two conceptions of absolute generality". *Philosophical Studies* 102.1.

Florio, Salvatore and Linnebo, Øystein (2021). *The Many and the One: A Philosophical Study of Plural Logic*. Oxford: UP.

Frege, Gottlob (1984a). "Function and Concept". In Brian McGuiness (ed.), *Gottlob Frege: Collected Papers on Mathematics, Logic, and Philosophy*. Oxford: Basil Blackwell.

— (1984b). "On Concept and Object". trans. Peter Geach. In McGuiness (ed.), *Collected Papers*, pp. 351–372.

Fritz, Peter and Jones, Nicholas K., eds. (2024). *Higher-order Metaphysics*. Oxford: UP.

Funkhouser, Eric (2006). "The determinable-determinate relation". *Noûs* 40.3, pp. 548–569.

Hazen, A. P. (1993). "Against Pluralism". *Australasian Journal of Philosophy* 71.2, pp. 132–144.

Horgan, Terence and Potrč, Matjaž (2008). *Austere Realism: Contextual Semantics Meets Minimal Ontology*. Cambridge, MA: MIT Press.

— (2012). "Existence monism trumps priority monism". In Philip Goff (ed.), *Spinoza on Monism*. Basingstoke: Palgrave-Macmillan.

Huemer, Michael (2009). "When is Parsimony a Virtue?" *The Philosophical Quarterly* 59.235, pp. 216–236.

Jacobs, Caspar (2022). "Jill North's *Physics, Structure, and Reality*". *British Journal for the Philosophy of Science Review of Books*. URL: `https://www.thebsps.org/reviewofbooks/jacobs-on-north/` (visited on 06/12/2024).

Jansson, Lina and Tallant, Jonathan (2017). "Quantitative Parsimony: Probably for the Better". *British Journal for the Philosophy of Science* 68.3, pp. 781–803.

Jones, Nicholas K. (2016). "A Higher-Order Solution to the Problem of the Concept *Horse*". *Ergo* 3, pp. 132–166.

Krämer, Stephan (2017). "Everything, and then Some". *Mind* 126.502, pp. 499–528.

Lederman, Harvey (2024). "Higher-order Metaphysics and propositional attitudes". In Peter Fritz and Nicholas K. Jones (eds.), *Higher-order Metaphysics*. Oxford: UP.

Lewis, David (1973). *Counterfactuals*. Oxford: Blackwell.

— (1983). "New Work for a Theory of Universals". *Australasian Journal of Philosophy* 61.4, pp. 343–377.

— (1990). "Noneism or Allism". *Mind* 99.393, pp. 23–31.

— (1991). *Parts of Classes*. Oxford: Blackwell.

Lewis, Peter J. (2016). *Quantum Ontology: A Guide to the Metaphysics of Quantum Mechanics*. Oxford: UP.

Linnebo, Øystein (2010). "Pluralities and Sets". *Journal of Philosophy* 107.3, pp. 144–164.

Linnebo, Øystein and Rayo, Agustín (2012). "Hierarchies Ideological and Ontological". *Mind* 121.482, pp. 269–308.

Lycan, William (1979). "The Trouble with Possible Worlds". In Michael J. Loux (ed.), *The Possible and The Actual*. Ithaca, NY: Cornell UP.

Maddy, Penelope (1997). *Naturalism in Mathematics*. Oxford: Clarendon.

— (1998). "Naturalizing Mathematical Methodology". In Matthias Schirn (ed.), *The Philosophy of Mathematics Today*. Oxford: Clarendon.

Magidor, Ofra (2009). "The last dogma of type confusions". *Proceedings of the Aristotelian Society* 109.1, pp. 1–29.

Magidor, Ofra (2013). *Category Mistakes*. Oxford: UP.

Maudlin, Tim (1994). *Quantum Non-Locality & Relativity: Metaphysical Intimations of Modern Physics*. Oxford: Basil Blackwell.

McDaniel, Kris (2009). "Extended Simples and Qualitative Heterogeneity". *The Philosophical Quarterly* 59.235, pp. 325–331.

— (2017). *The Fragmentation of Being*. Oxford: UP.

McGuiness, Brian (1984). *Gottlob Frege: Collected Papers on Mathematics, Logic, and Philosophy*. Oxford: Basil Blackwell.

Merricks, Trenton (2019). "The only way to be". *Noûs* 53.3, pp. 593–612.

Nolan, Daniel (1997). "Quantitative Parsimony". *British Journal for the Philosophy of Science* 48, pp. 329–343.

— (2011). "The Extent of Metaphysical Necessity". *Philosophical Perspectives* 25, pp. 313–339.

North, Jill (2021). *Physics, Structure, and Reality*. Oxford: UP.

Parsons, Charles (1990). "The Structuralist View of Mathematical Objects". *Synthese* 84.3, pp. 303–346.

Parsons, Joshua (2004). "Distributional Properties". In Frank Jackson and Graham Priest (eds.), *Lewisian Themes*. Oxford: UP.

Parsons, Terence (1979). "The Methodology of Nonexistence". *The Journal of Philosophy* 76.11, pp. 649–662.

Pederson, Nikolaj Jang (2010). "Stabilizing Alethic Pluralism". *The Philosophical Quarterly* 60.238, pp. 92–108.

— (2012). "Recent work on alethic pluralism". *Analysis* 72.3, pp. 588–607.

Prior, A. N. (1971). *Objects of Thought*. Geach, P. T. and Kenny, A. J. P. (eds.). Oxford: Clarendon Press.

Prokopenko, Mikhail, Boschetti, Fabio, and Ryan, Alex J. (2009). "An Information-theoretic primer on Complexity, Self-organization, and Emergence". *Complexity* 15.1, pp. 11–28.

Quine, W. V. O. (1948). "On What There Is". *The Review of Metaphysics* 2.5, pp. 21–38.

— (1951). "On Carnap's views on Ontology". *Philosophical Studies* 2.5, pp. 65–72.

— (1980 [1953]). "Two Dogmas of Empiricism". In *From a Logical Point of View: Nine Logico-Philosophical Essays*. 2nd edn, revised. New York, N. Y., pp. 20–46.

Rayo, Agustín (2006). "Beyond Plurals". In *Absolute Generality*. Oxford: Clarendon Press, pp. 220–254.

— (2021). "Absolute generality". In *The Routledge Handbook of Metametaphysics*. Abingdon: Routledge.

Rayo, Agustín and Uzquiano, Gabriel (1999). "Toward a theory of second-order consequence". *Notre Dame Journal of Formal Logic* 40.3, pp. 315–325.

— eds. (2006). *Absolute Generality*. Oxford: Clarendon Press.

Rayo, Agustín and Yablo, Stephen (2001). "Nominalism through Denominalization". *Noûs* 35.1, pp. 74–92.

Rettler, Bradley (2020). "Ways of thinking about ways of being". *Analysis* 80.4, pp. 712–722.

Rosen, Gideon (1999). "Review: Penelope Maddy, *Naturalism in Mathematics*". *British Journal for the Philosophy of Science* 50.3, pp. 467–474.

— (2015). "Real Definition". *Analytic Philosophy* 56.3, pp. 189–209.

Rosen, Gideon and Smith, Nicholas J. J. (2004). "Worldly Indeterminacy". *Australasian Journal of Philosophy* 82.1, pp. 185–198.

Schaffer, Jonathan (2008). "Truthmaker Commitments". *Philosophical Studies* 141.1, pp. 7–19.

— (2009). "On What Grounds What". In David J. Chalmers, David Manley, and Ryan Wasserman (eds.), *Metametaphysics: New essays on the foundations of ontology*. Oxford: UP, pp. 347–383.

— (2010). "Monism: The Priority of the Whole". *The Philosophical Review* 119.1, pp. 31–76.

— (2015). "What not to multiply without necessity". *Australasian Journal of Philosophy* 93 (4), pp. 644–664.

— (2017). "Laws for Metaphysical Explanation". *Philosophical Issues* 27, pp. 302–321.

Shapiro, Stewart (1991). *Foundations without Foundationalism: A Case for Second-order Logic*. Oxford: UP.

— (1997). *Philosophy of Mathematics: Structure and Ontology*. Oxford: UP.

Sider, Theodore (2001). *Four-Dimensionalism: An Ontology of Persistence and Time*. Oxford: UP.

— (2011). *Writing the Book of the World*. Oxford: UP.

— (2020). *The Tools of Metaphysics and the Metaphysics of Science*. Oxford: UP.

Sober, Elliott (1993). "Mathematics and Indispensability". *The Philosophical Review* 102.1, pp. 35–57.

— (2015). *Ockham's Razors: A User's Manual*. Cambridge: UP.

Spencer, Joshua (2010). "A tale of two simples". *Philosophical Studies* 148.2, pp. 167–181.

Studd, James P. (2019). *Everything More or Less: A Defence of Generality Relativism*. Oxford: UP.

Szekely, Rachel (2015). *Truth without Predication: The Role of Placing in the Existential There-Sentence*. New York: Palgrave Macmillan.

Takho, Tuomas E. (2018). "Fundamentality". In Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*. Fall 2018 edn. URL: `https://plato.stanford.edu/archives/fall2018/entries/fundamentality/` (visited on 01/05/2023).

Tal, Eran (2020). "Measurement in Science". In Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*. Fall 2020 edn. URL: `https://plato.stanford.edu/archives/fall2020/entries/measurement-science/` (visited on 06/13/2024).

Thunder, Simon (2021). "There is no reason to replace the Razor with the Laser". *Synthese* 199.3, pp. 7265–7282.

Titelbaum, Michael G. (2022). *Fundamentals of Bayesian Epistemology*. 2 vols. Oxford: UP.

Trogdon, Kelly (2013). "An Introduction to Grounding". In Miguel Hoeltje, Benjamin Schnieder, and Alex Steinberg (eds.), *Varieties of Dependence: Ontological Dependence, Grounding, Supervenience, Response-Dependence*. Munich: Philosophia.

Turner, Jason (2010). "Ontological Pluralism". *The Journal of Philosophy* 107.1, pp. 5–34.

— (2012). "Logic and Ontological Pluralism". *Journal of Philosophical Logic* 41.2, pp. 419–448.

— (2015). "What's So Bad About Second-Order Logic?" In Alessandro Torza (ed.), *Quantifiers, Quantifiers, and Quantifiers: Themes in Logic, Metaphysics, and Language*. Cham: Springer, pp. 463–488.

Turner, Jason (2016). *The Facts in Logical Space: A Tractarian Ontology*. Oxford: Oxford University Press.

— (2021). "Ontological Pluralism". In *The Routledge Handbook of Metametaphysics*. Abingdon: Routledge.

van Inwagen, Peter (1998). "Meta-Ontology". *Erkenntnis* 48.2/3, pp. 233–250.

Williamson, Timothy (1985). "Converse Relations". *The Philosophical Review* 94.2, pp. 249–262.

— (1998). "Bare Possibilia". *Erkenntnis* 48.2/3, pp. 257–273.

— (2003). "Everything". *Philosophical Perspectives* 17, pp. 415–465.

— (2013). *Modal Logic as Metaphysics*. Oxford: UP.

Wilson, Jessica M. (2013). "A Determinable-Based Account of Metaphysical Indeterminacy". *Inquiry* 56.4, pp. 359–385.

— (2014). "No Work for a Theory of Grounding". *Inquiry* 57.5–6, pp. 535–579.

— (2023). "Determinables and Determinates". In Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*. Spring 2023 edn. URL: `https://plato.stanford.edu/archives/spr2023/entries/determinate-determinables/` (visited on 01/21/2024).

Wright, Crispin (1992). *Truth and Objectivity*. Cambridge, MA: Harvard University Press.