

# Symmetry, Indiscernibility, and the Generalist Picture

Thomas Møller-Nielsen

Balliol College, University of Oxford



Submitted in partial fulfillment of the requirements for the degree  
of Doctor of Philosophy

Michaelmas Term, 2015

## Abstract

This thesis consists of four independent but thematically interrelated papers, plus an addendum to one of these papers.

Chapter 1 defends the view that Leibniz subscribed to *generalism*, or the view that the world is fundamentally purely qualitative in character. In particular, I respond to Cover & O’Leary-Hawthorne’s (1999) claim that Leibniz’s use of symmetry considerations in the correspondence with Clarke reveals that he was not a generalist. In addition, I claim that what reveals Leibniz’s generalism is not — as many scholars seem to believe — his mere commitment to his theory of truth, theory of complete individual substances and Principle of the Identity of Indiscernibles (PII). Rather, I argue, it is the fact that Leibniz claimed to derive the PII from his theories of truth and complete individual substances which ultimately demonstrates his commitment to generalism.

Chapter 2 examines the putative relationship between two notions: *symmetry* and *qualitativity*. I argue that, on the standard metaphysical construal of the notion of qualitativity, a widely-held view about the relationship between these two notions is mistaken. However, I also argue that on a nonstandard construal of the notion of qualitativity due to Ismael & van Fraassen (2003), the alleged relationship between the two notions holds much more promise. I finish by expanding upon Ismael & van Fraassen’s own account of the notion of qualitativity relevant to the notion of symmetry, and in doing so I attempt to elucidate the methodology of symmetry reasoning in physics more generally.

This chapter is followed by a brief addendum. Here, I discuss two recent independent proposals — due to Adam Caulton (forthcoming) and Shamik Dasgupta (forthcoming, *a*) — for understanding the methodology of symmetry reasoning that are *prima facie* very similar to the view that I defend at the end of Chapter 2. I argue that both proposals differ from my own, and each other, in subtle but nevertheless highly non-trivial respects.

Chapter 3 argues that the focus of much contemporary discussion concerning the proper role and philosophical significance of a modern-day version of the PII is misplaced. More specifically, I argue that metaphysicians’ criticism of the notion of weak discernibility on the basis of its alleged inability to ground objects’ numerical diversity is orthogonal to Saunders’ (2003a) main concern in his original paper, which is to use the notion of weak discernibility as part of a broader program for interpreting physical theories. I subsequently assess this interpretative program, and suggest some reasons for thinking it implausible.

Chapter 4 discusses a variety of recent objections to generalism. The objections are related by the fact that all of them appeal, at some stage in the relevant line of argument, to the possibility of worlds containing qualitatively indis-

cernible individuals. I argue that none of these objections ultimately succeed. Thus, I claim that a significant class of difficulties for the view is obviated.

Lightly-edited versions of some of these chapters have already been accepted for publication. At the time of writing, Chapter 1 is forthcoming in *Studia Leibnitiana*; Chapter 2 (without the addendum) has appeared in Tomasz Bigaj and Christian Wüthrich's edited collection *Metaphysics in Contemporary Physics* (Amsterdam/New York, NY: Rodopi, 2015); and Chapter 3 is forthcoming in *Ergo*. Chapter 4 and the addendum to Chapter 2 are currently unpublished.

This thesis is approximately 60,000 words long.

## Acknowledgements

First and foremost, I wish to thank my current supervisors, Oliver Pooley and Simon Saunders. I am very grateful to both of them for reading my work so carefully, for being so generous with their time, and for their support. It has been a great privilege to know, and learn from, both of them.

I am also grateful to several other Philosophy Faculty members at Oxford. Gonzalo Rodriguez-Pereyra provided the original inspiration and encouragement to work on Chapter 1, while Harvey Brown, Chris Timpson and David Wallace have each, in their own ways, extended their kindness and support over the years.

Of the philosophy graduate students here at Oxford, Neil Dewar, Joel Entwistle, Niels Martens, Joshua Melville, Tushar Menon, Carina Prunkl, James Read, Alex Robertson, Teru Thomas, Aron Vallinder, and Andy Yu all deserve special mention for providing helpful feedback on various aspects of this thesis.

There are many other philosophers outside of Oxford who deserve my thanks. In particular, I have greatly benefitted from discussions and comments relating to material in this thesis from Gordon Belot, Adam Caulton, Shamik Dasgupta, Sam Fletcher, James Fraser, Steven French, Nick Huggett, Dennis Lehmkuhl, Keizo Matsubura, Patrick McGovern, Ed Perez, Joe Rachiele, Jim Weatherall, and Alistair Wilson. Comments from anonymous referees for the journals *Studia Leibnitiana* and *Ergo* have also greatly helped improve Chapters 1 and 3 respectively. As this thesis' external examiner, James Ladyman deserves special mention here: together with David Wallace—this thesis' internal examiner, and a person to whom I am also especially grateful—he provided wonderfully detailed and insightful feedback on numerous parts of the thesis. Jeff Russell also deserves special mention, but for a different reason: he supervised me for my first two years as a DPhil student. Undoubtedly, much of what is good about this thesis I owe to him.

Finally, very special thanks to my family, and in particular to my parents. Needless to say, this thesis is dedicated to them.

# Contents

<b>Abstract</b>	<b>i</b>
<b>Acknowledgements</b>	<b>iii</b>
<b>Introduction</b>	<b>1</b>
<b>1 Was Leibniz a Generalist?</b>	<b>10</b>
1.1 Generalism vs Singularism . . . . .	12
1.2 Generalist and Singularist Pressures in Leibniz . . . . .	15
1.2.1 Generalist Pressures in Leibniz . . . . .	15
1.2.2 Singularist Pressures in Leibniz . . . . .	29
1.3 Weak Haecceitism to the Rescue? . . . . .	34
1.4 The Received View Defended . . . . .	46
1.4.1 The claim that Leibniz took <i>de re</i> modal claims at face value	47
1.4.2 The claim that Leibniz’s use of “switching” considerations in his correspondence with Clarke reveals that he was not a generalist . . . . .	52
1.5 Conclusion . . . . .	63
<b>2 Symmetry and Qualitativity</b>	<b>64</b>
2.1 More on Symmetry and Qualitativity . . . . .	67
2.1.1 Symmetry . . . . .	67
2.1.2 Qualitativity . . . . .	71
2.2 Symmetries Do Not Only Relate Qualitatively Indiscernible So- lutions . . . . .	77
2.3 Symmetry and the Nonqualitative . . . . .	87
2.3.1 The First Problem . . . . .	87
2.3.2 The Second Problem . . . . .	92
2.4 A New Notion of “Qualitative”? . . . . .	101

<b>Addendum to Chapter 2</b>	<b>110</b>
Caulton . . . . .	111
Dasgupta . . . . .	121
<b>3 Weak Discernibility, Again</b>	<b>129</b>
3.1 Methodology, Not Metaphysics . . . . .	132
3.2 Three Questions . . . . .	142
3.3 The Naïve Question . . . . .	143
3.4 The Epistemological Question . . . . .	144
3.5 The Metaphysical Question . . . . .	148
3.6 Conclusion . . . . .	155
<b>4 Some General Worries, Discharged</b>	<b>157</b>
4.1 The Standard Objection . . . . .	158
4.2 The Naïve Objection . . . . .	161
4.3 The Metaphysical Discontinuity Objection . . . . .	164
4.4 The Motivation Objection . . . . .	167
4.5 The Greavesian Objection . . . . .	171
4.6 Lewisian Determinism? . . . . .	176
4.7 The Kmentian Objection . . . . .	183
<b>References</b>	<b>198</b>

# Introduction

This thesis is not a research monograph. Instead, it is comprised of four chapters (plus an addendum to one of these chapters), each of which is intended to be readable independently of the others. The chapters are, however, united by a common theme — more precisely, by three interrelated sub-themes: *symmetry*, *indiscernibility*, and *the generalist picture*. Let me explain.

First, symmetry. We are all familiar with the notion of symmetry in its standard, geometrical sense. For instance, rotations by  $90^\circ$  are symmetries of a perfect square; rotations by  $45^\circ$  are not. This notion of symmetry will prove salient in Chapter 4, where I consider worlds which display a kind of geometrical symmetry up to a given moment in time, but which fail, as a matter of physical law, to exhibit the same symmetry thereafter. For the majority of this thesis, however, I will be interested in a quite different notion of symmetry: *dynamical symmetry*. Dynamical symmetries are slightly more abstract than geometrical symmetries. In brief, they are transformations which map solutions to solutions of a particular physical theory — they “preserve the laws”, in a (somewhat) similar manner to which geometrical symmetries preserve the structure of a given geometrical object. A large part of this thesis will focus on such symmetries’ “invariantist” construal, according to which only that structure which is left invariant under them is genuinely real. This invariantist construal of symmetry

plays a key role in Chapters 1 and 2 of this thesis; it plays a smaller but still significant role in Chapter 3 as well (see page 134, fn 4 below).

Second, indiscernibility. This notion is a notoriously slippery one in philosophy. A number of different kinds of thing can be indiscernible; and things can be indiscernible in a number of different ways. In this thesis, I will talk both of worlds and of various things within them as being indiscernible. My main focus, however, will be on the notion of *qualitative* indiscernibility: that is, indiscernibility by qualitative predicates alone. In particular, much of this thesis will focus on versions of a principle most famously advocated by Leibniz, namely the Principle of the Identity of Indiscernibles (PII). Thus, Chapter 1 is largely concerned with examining the philosophical and dialectical role the PII actually played for Leibniz in his considered metaphysics; Chapter 2 is primarily concerned with understanding to what extent it is the case (as many philosophers of physics appear to believe) that symmetries map solutions to “qualitatively indiscernible” solutions; Chapter 3 assesses the putative philosophical significance of a modern-day version of the PII which, it has been argued (notably by Simon Saunders), can be fruitfully applied to modern physics; and Chapter 4 examines to what extent generalism (more on which below) can be said to be refuted by the possibility of worlds which violate a version of the PII.

Third, the generalist picture. This is the view that the world is fundamentally purely qualitative in character; that the world’s fundamental facts do not make reference to particular individuals. Chapters 1 and 4 are the main parts of this thesis which examine this view: in the former, my primary goal is to defend the claim that Leibniz subscribed to generalism; in the latter (and to repeat slightly), my main objective is to defend generalism from a plethora of recent

objections, all of which appeal (at some stage in the relevant line of argument) to the possibility of worlds which (at some stage in their respective histories) violate some salient version of the PII. Generalism is also briefly mentioned, and plays some small part in, Chapter 2. It plays no substantive role in Chapter 3.

Before providing more detailed summaries of each chapter, I would like to pre-empt, and attempt to assuage, two potential concerns that might otherwise arise upon reading the main body of this thesis.

First, note that in the following chapters there is very little discussion of why we should believe in generalism in the first place. Given the central role that this view plays in several of the chapters below, such an omission might appear perplexing (to say the least). The reason for this omission, however, is straightforward: I do not believe that there is any dialectically effective — or particularly philosophically interesting — way of arguing for the view. I take it that the majority of philosophers who subscribe to generalism do so precisely because they are sceptical of the possibility of worlds which differ merely haecceitistically (i.e. nonqualitatively). I share the generalist's distaste for qualitatively indiscernible yet haecceitistically distinct possible worlds. However, many philosophers — including several philosophers of physics<sup>1</sup> — do not share such distaste. It is my belief that such philosophers' demurrals are primarily, if not exclusively, due to their endorsement of (one of) the many objections to generalism that I examine, and attempt to discharge, in Chapter 4 of this thesis. Thus, although nowhere in this thesis do I try to offer any detailed arguments *for* generalism, I do attempt to disarm what I take to be the most commonly-endorsed objections *against* it — and, in doing so, I hope to show that those theorists thus far inclined to reject

---

<sup>1</sup>See, e.g., Belot (1995), Skow (2005), and Brighouse (2008).

generalism do so in the absence of any genuinely compelling motivation.

Second, note that in the following chapters there is virtually no discussion of *subsystem symmetries*. (This worry requires a little bit of setting up.) In brief, a subsystem symmetry is a transformation that is performable on some suitably dynamically isolated subsystem of the universe such that, from the perspective of that subsystem, the universe in which the transformation has been carried out and the universe in which it has not are empirically indistinguishable. A famous example of a subsystem symmetry is, of course, Galileo's (1967, 186-8) ship. Thus, consider a ship on a calm day at sea, stationary relative to the shore. Now consider the scenario in which the ship is uniformly boosted relative to the shore. Relative to the cabin of the ship, the worlds in question are empirically indistinguishable.<sup>2</sup> However, the worlds themselves are not: there are possible measurements that would empirically discern them. (For instance, one could simply ascend to the ship's deck and look around.) Thus, boost symmetry in (e.g.) Newtonian Gravitation Theory is said to have *direct empirical significance*: given a ship, a shore, and a relatively calm ocean, one is able to observe the symmetry "in action", so to speak, by confining oneself to the cabin and performing experiments both when the ship is stationary and when it is moving relative to the shore.

Several authors have argued that it is *this* notion of symmetry — that is, as a transformation enactable upon some subsystem of the universe, rather than on the universe as a whole — which is of primary physical salience, and which is moreover the notion deserving of most interest among philosophers. As Brown

---

<sup>2</sup>Empirically indistinguishable, that is, subject to certain other conditions being met. For instance, the windows must be shut, and all GPS trackers confiscated (cf. Greaves & Wallace 2014, 64, fn 7).

& Sypel (1995, 250-1) write:

[I]n order to define active transformations — and hence symmetry principles — something directly observable, some physical marker, must be left unaffected by the carry-along operation in question, and not just the background spacetime structure.... *Dynamical symmetry principles, and in particular the relativity principle, are defined only for isolated subsystems of the universe.* This crucial point ... is not always adequately reflected in recent formulations of symmetry principles. (Emphasis in original)<sup>3</sup>

On such a construal of “dynamical symmetry”, then, it would appear that symmetry transformations never — essentially as a matter of pure definition — relate wholly empirically indistinguishable states of affairs. Rather, symmetry transformations relate empirically distinguishable scenarios that are only empirically indistinguishable from the perspective of the relevant subsystem.

One might initially be inclined to think that Brown & Sypel’s claim here poses a rather serious problem for some of the views defended in this thesis. For a central part of my own philosophical account of symmetry reasoning (defended at the end of Chapter 2 and in its addendum) involves the claim that symmetries — and in particular, those symmetries that are apt for an invariantist interpretation — are appropriately construed as acting on the universe as a whole, and moreover should be understood as relating worlds that are empirically indistinguishable *simpliciter*. But this conception of symmetry seems to be straightforwardly at odds with that endorsed by Brown & Sypel.

---

<sup>3</sup>Feynman (1967, 90) makes a similar point.

The contemporary philosophical literature on subsystem symmetries is large, and growing. I cannot hope to do justice to the intricacies of the debate here. But in the interest of resolving the apparent tension between Brown & Sypel and myself, I will limit myself to three brief remarks. First, it appears to be an open question whether all, or even most, of those dynamical transformations apt for an invariantist construal (e.g., gauge transformations in classical electromagnetism) may be afforded direct empirical significance in a manner analogous to Galileo's ship.<sup>4</sup> Second, (as far as I can gather) nothing that I say below on the methodology of symmetry reasoning hinges on what precisely the general relationship is between symmetries of subsystems and symmetries (or "symmetries") of the world. Third, and most importantly, however, "symmetry" is *just a word*, to be used in whatever way one might see fit for the purposes at hand. In particular, if it turns out that the notion of "symmetry" of interest to the subject of this thesis is different to, or perhaps even at odds with, the notion as used by and of interest to theorists such as Brown & Sypel, then so be it: so long as all

---

<sup>4</sup>To summarise (brutally) the recent literature: Kosso (2000) argues that such analogs exist for symmetries that are "global", or that do not vary in their application from spacetime point to spacetime point, but not for symmetries that are "local", which can vary in this way. Brading & Brown (2004) argue instead that only global *spacetime* symmetries have direct empirical significance, but not generic "internal" global symmetries (e.g., transformations of the absolute magnitude of the electric potential in electrostatics). Greaves & Wallace (2014) respond to both papers by pointing out, among other things, that theories possessing global symmetries have historically tended to give way to theories in which the relevant symmetries are "localised" (as is the case, for instance, in the transition from special relativity, with its associated group of global Poincaré transformations, to general relativity, with its associated group of local diffeomorphisms), and the *prima facie* mysteriousness of how it could be that a symmetry afforded operational significance in the former such theory could somehow lose such significance once one passes to the larger symmetry group of the successor theory. In their paper, Greaves & Wallace go on to sketch their own preferred abstract framework for thinking about subsystem symmetries according to which (as it turns out) both global and local symmetries *do* (sometimes) turn out to have Galileo's ship-type analogues. Their work has since been disputed by Friederich (2015), who defends the orthodoxy that there are no such analogues for local symmetries, and Teh (2016), who contests the claims of both Greaves & Wallace and Friederich.

relevant precautions against misunderstandings are undertaken, there should be no need to worry.

Let me now end this brief Introduction by providing more detailed summaries of the following chapters.

Chapter 1 is an extended defence of the claim that Leibniz subscribed to the generalist picture. Such an interpretation of Leibniz appears to have enjoyed a universal consensus in the literature until about 20 years ago, when it was challenged by Cover & O'Leary-Hawthorne (1999) in their much-lauded book *Substance and Individuation in Leibniz*.<sup>5</sup> An important, if not the most important, part of Cover & O'Leary-Hawthorne's argument against the Received View appeals to Leibniz's use of symmetry (or "switching") considerations in his correspondence with Clarke in 1715-16 — considerations which, Cover & O'Leary-Hawthorne argue, Leibniz could not plausibly have appealed to were he in fact a generalist. I argue to the contrary that Leibniz's use of symmetry considerations in the correspondence cannot credibly be taken to have the interpretational significance that Cover & O'Leary-Hawthorne take it to have. However, I also argue that the reasons usually given by defenders of the Received View for interpreting Leibniz as a generalist — which appeal to his mere commitment to his distinctive theory of truth, theory of complete individual substances and the PII — do not stand up to scrutiny. Rather, I contend that it is the fact that Leibniz used his theories of truth and complete individual substances to *derive* the PII which plausibly reveals his generalist sympathies.

Chapter 2, rather than seeking to defend a view of wide currency in the con-

---

<sup>5</sup>An earlier version of their challenge to the Received View appeared as O'Leary-Hawthorne & Cover (1996). The book received extremely positive reviews from, e.g., Futch & Rutherford (2001), Adams (2002), Detlefson (2002), Look (2002), Mugnai (2004), and Nelson (2004).

temporary literature, seeks to undermine one. Here, I argue against the claim that symmetries in physics — construed as those transformations which map solutions to “physically equivalent” solutions — only ever relate qualitatively indiscernible solutions, and thus only ever reveal the nonreality of structure that can only be construed as nonqualitative. I argue to the contrary (1) that symmetries, at least on some occasions, reveal the nonreality of qualitative structure, and (2) that there are compelling, though defeasible, reasons for thinking that symmetries can never, in fact, indicate the nonreality of nonqualitative structure. Following this discussion, I move on to discuss a recent — and slightly idiosyncratic — construal of the notion of “qualitativity” that has been claimed to be relevant to the notion of symmetry *qua* indicator of unreal structure, namely that defended by Ismael & van Fraassen (2003). I finish the chapter by sketching my own preferred account of the use of symmetries in physics, one which I think improves on but nevertheless owes much to Ismael & van Fraassen’s own account. An addendum immediately following this chapter then elaborates on this account, and draws comparisons and important distinctions between it and similar proposals that have recently been put forward in the literature by Shamik Dasgupta (forthcoming, *a*) and Adam Caulton (forthcoming).

Chapter 3 reconsiders a modern-day construal of the PII — one which, it has been argued, can be fruitfully applied to modern physics — due to Simon Saunders (2003a, 2006). I argue that Saunders’ philosophical project has thus far been seriously misunderstood in much of the contemporary literature. More specifically, I argue that Saunders is not correctly construed as attempting to *ground* objects’ numerical diversity by their satisfaction of his version of the PII. Rather, I claim that he is properly construed as proffering the notion of

“weak discernibility” only as an essential component of a broader method for *interpreting* physical theories in terms of objects. Moreover, I claim that a popular (“circularity”) objection frequently levelled against Saunders’ (alleged) proposal is in fact straightforwardly orthogonal to the interpretative scheme that he is correctly construed as proposing. This, of course, does not by itself vindicate Saunders’ interpretative scheme. Indeed, as I go on to argue in the latter half of the paper, Saunders’ interpretative scheme — properly assessed — faces several difficulties that are, at the very least, not straightforwardly discharged.

Chapter 4, the final chapter of this thesis, is a review of various arguments that have been put forward over the years against the generalist picture, all of which appeal to the possibility of worlds which violate some salient version of the PII. I argue that, in fact, even granting a highly controversial premise in many of the arguments — namely, that the generalist is committed to regarding various “symmetry-breaking” worlds as deterministic which, intuitively, are not deterministic — none of these arguments ultimately succeed. The chapter ends with a discussion of Kment’s (2012) recent anti-generalist arguments, which are intended to establish that the generalist who accepts the possibility of such PII-violating worlds is committed to *prima facie* absurd consequences regarding chance and counterfactual conditionals. I claim to the contrary that the generalist may plausibly respond to Kment’s arguments in a number of ways.

# Chapter 1

## Was Leibniz a Generalist?

According to the “Received View” of Leibniz’s considered philosophy, Leibniz was committed to the world’s being fundamentally *purely qualitative* in character: that is, he was committed to the view that the world can be completely and perspicuously described by general propositions.<sup>1</sup> Recently, however, Cover & O’Leary-Hawthorne (1999) have challenged this interpretative orthodoxy, claiming that it fails to make sense of crucial aspects of Leibniz’s modal metaphysics. Their challenge has, to my knowledge, received almost no response or discussion in the literature.<sup>2</sup> The goal of this chapter is to provide just such a

---

<sup>1</sup>For a classic, explicit statement of the Received View, see Adams (1979, §2). With varying degrees of explicitness, the view is also endorsed by, e.g., Hacking (1972, 148), Sleight (1990, 75), and Rescher (2003, 9). Di Bella (2005, 386-7) is an interesting exception to this interpretative orthodoxy, and is sceptical of the Received View for reasons largely similar to those adduced by Cover & O’Leary-Hawthorne. (Though Di Bella’s considered position with regard to the Received View is one of agnosticism, not outright rejection.)

<sup>2</sup>The only exceptions to this trend that I can find is Di Bella’s (2005, 376, fn 10) brief, positive, appraisal of Cover & O’Leary-Hawthorne’s thesis in a footnote to his book, and Adams’ (2002, 853-4) and Jauernig’s (2008, 194, fn 11) similarly brief, but more critical, assessments in their respective papers. All of these authors, however, only allude to the first, and by far the least convincing, of Cover & O’Leary-Hawthorne’s two arguments against the Received View: namely, the argument which involves the claim that Leibniz took *de re* (or “singular”) modal claims at face value.

response. It will be argued that, for a number of reasons both textual and philosophical, their challenge does not succeed. On the contrary, it will be argued that Leibniz *did* conceive of the world in purely qualitative terms, although the textual evidence for this is, I claim, somewhat different to that normally cited by Leibniz scholars. The conclusion will therefore be drawn that the Received View, or what Cover & O'Leary-Hawthorne label the "conservative reading" or "natural interpretation" of Leibniz, is in fact not only natural, but also correct.

The format of this chapter is as follows. In §1.1 I introduce the distinction between general and singular propositions, and the concomitant distinction between the "generalist" and "singularist" pictures. In §1.2 I provide, and subsequently refine, the standard reasons given in the literature for thinking that Leibniz was a generalist, before moving on to present the reasons Cover & O'Leary-Hawthorne provide for doubting whether the Received View of Leibniz construed as a generalist is correct. In §1.3 I discuss Cover & O'Leary-Hawthorne's proposed interpretative resolution of the claimed "deep tension" between the generalist and (allegedly) singularist strands in Leibniz's philosophy, and criticise it both for failing to constitute a plausible interpretation of Leibniz by their own lights (in virtue of its failure to resolve a claimed difficulty raised by Cover & O'Leary-Hawthorne against the Received View), as well as for failing to mesh satisfactorily with other important aspects of Leibniz's metaphysics. In §1.4 I move on to dispute Cover & O'Leary-Hawthorne's claim that there is in fact a "deep tension" inherent to Leibniz's philosophy in the first place: first, I take issue with their claim that one is "not well placed to deny" that Leibniz took singular or *de re* modal claims at face value; and second, I question their crucial assumption that Leibniz's use of symmetry (or "switching")

considerations in his correspondence with Clarke reveals something important about his philosophy. I conclude the discussion in §1.5.

## 1.1 Generalism vs Singularism

Take *generalism* to be the view that the world is fundamentally purely qualitative in character. In very rough terms, it is the view according to which there do not exist any individuating “primitive thisnesses”, or “haecceities”.<sup>3</sup> Thus, for instance, the “bundle theory” of individuals, according to which individuals are nothing but bundles of “compresent” universals, is a type of generalist metaphysics; but there are many more versions of generalism besides this.<sup>4</sup>

Conversely, take *singularism* to be the denial of generalism: again in very rough terms, it is the view according to which there *are* various nonqualitative (“haecceitistic”, or “singular”) aspects to fundamental reality. Thus, a “bare particularist” conception of individuals, according to which “thick” individuals are something over and above their instantiated properties, is a type of singularist metaphysics; but there are many more versions of singularism besides this.<sup>5</sup>

Although the gist of the view is, I think, reasonably transparent, generalism

---

<sup>3</sup>The view goes by a variety of names in the literature, although “generalism” seems to be the term favoured by most theorists. See, e.g., Cover & O’Leary-Hawthorne (1999), Saunders (2003a), Maunu (2005), Morganti (2008), Dasgupta (2009), Pooley (MS), and Russell (MS). Compare also van Fraassen’s (1991) “semantic universalism”, Fine’s (2005) “metaphysical anti-haecceitism”, Kment’s (2012) “anti-individualism”, and Dasgupta’s (2014) “qualitativism”.

<sup>4</sup>For instance, a nominalist suspicious of properties will not want to subscribe to the universal-reifying metaphysics just described, but might nevertheless still count as a generalist. In particular, the nominalist might still believe that the world is fully and perspicuously describable by general propositions.

<sup>5</sup>For instance, trope-bundle theory (cf. Dasgupta 2009, 47-8). Moreover, a nominalistically-inclined singularist will presumably be uncomfortable with the claim that a singularist is by nature committed to individuals’ being something “over and above” their various instantiated properties: for according to the nominalist, there aren’t any such properties!

is a doctrine that is notoriously difficult to state to any great degree of precision. Ever since Adams' (1979) classic discussion of the distinction between the qualitative and the nonqualitative, however, a "linguistic approach" to stating the view has become quite popular in the literature.<sup>6</sup> Thus, imagine a language devoid of proper names and "individual-involving" predicates such as *pegasizes*, but which is otherwise unconstrained in its expressive resources. Such a language will thus be able to express all *general* propositions like *There is at least one bald person*, but it will not be able to express any *singular* ("de re") propositions like *Nicolas Cage is a bald person*. The generalist will take such a language to be capable of offering a complete and perspicuous description of fundamental reality. The singularist will deny this.<sup>7</sup>

What is meant by a "perspicuous description" of fundamental reality?<sup>8</sup> The thought here is again difficult to make precise; nevertheless, it is plausibly elucidated by example. Thus, for instance, just as the statement "There is a table" could never amount to a perspicuous description of a world in which mereological composition never occurred (i.e., in which, strictly speaking, there *aren't* any tables), so "Nicolas Cage is bald" could not amount (given that it expresses a sin-

<sup>6</sup>Cf. Lewis (1986, 221), Cover & O'Leary-Hawthorne (1999, 146), and Kment (2012, 578-9).

<sup>7</sup>It is of course true that this "linguistic approach" to cashing out the distinction between the generalist and singularist world-views is unlikely to help those who entirely lacked any conceptual handle on the distinction between qualitative and nonqualitative predicates in the first place. For as Stalnaker (2012, 61) has recently emphasised, there is plainly no *syntactic* difference that could independently help one to distinguish between qualitative and nonqualitative predicates (for instance, *flies* and *Socratises*), while any putative *semantic* difference between such predicates is most likely not going to be independent of the very distinction at issue. My hope, however, is that most readers will have *some* initial grasp of the relevant distinction, and that at this point they find it reasonably clear, or at least clear enough to work with for the purposes of this paper.

<sup>8</sup>Cover & O'Leary-Hawthorne (1999, 147) frame the issue in terms of "truths" being "captured by" propositions. I take this locution to be essentially equivalent to the notion of descriptive perspicuity discussed here.

gular proposition) to a perspicuous description of a world which was ultimately purely qualitative in character. More physics-based examples of descriptive perspicuity might include claims regarding the “absolute” simultaneity of two distinct events in a relativistic world exhibiting a Minkowskian spatiotemporal structure, or claims regarding one’s “absolute” velocity in a world with Galilean spatiotemporal structure. Such claims might of course still be true in a “contextual” or “loose” or “non-literal” sense. But the thought is that such claims could not be true *simpliciter*. To put the point more figuratively, some propositions “correspond to” or “limn” the world’s structure. The disagreement between the generalist and the singularist ultimately boils down to which propositions, general or singular, are required in order to properly “limn” such structure in just this way. To repeat: the generalist will claim that only general propositions are required to carry out such “limning”. The singularist rejects this claim.<sup>9</sup>

I shall be assuming in what follows that the qualitative/nonqualitative distinction is at least clear enough for us to work with. (Those doubtful of its coherence might want to get off the boat now.)<sup>10</sup> With it and the associated distinction between the generalist and singularist pictures in hand, then, let us now move on to discuss the various aspects of Leibniz’s philosophy that might appear to incline him towards one or the other of these views.

---

<sup>9</sup>For more on the relevant notion of perspicuity, see O’Leary-Hawthorne & Cortens (1995, 154-7). See also Sider (2011, §2) for a defence of the view that talk of propositions’ “structure-limning” (or “joint-carving”) should be taken as primitive.

<sup>10</sup>But see Cowling (2015), who notes the seemingly ineliminable role played by the qualitative/nonqualitative distinction in the formulation of a wealth of philosophical doctrines (intrinsicity, physicalism, haecceitism, and others). He goes on to argue that the qualitative/nonqualitative distinction should be taken as metaphysically primitive.

## 1.2 Generalist and Singularist Pressures in Leibniz

### 1.2.1 Generalist Pressures in Leibniz

In presenting what they take to be the main generalist strands of Leibniz's considered philosophy, Cover & O'Leary-Hawthorne offer a similar exegetical analysis to that usually offered by defenders of the Received View.<sup>11</sup> In particular, they (1999, 150) claim that one prominent aspect of Leibniz's philosophy which looks to incline him towards generalism is his *predicate-in-subject theory of truth*, according to which

An affirmation is true if its predicate is in its subject; thus, in every true affirmative proposition, necessary or contingent, universal or singular, the concept of the predicate is somehow contained in the concept of the subject, in such a way that anyone who understood the two concepts as God understands them would *eo ipso* perceive that the predicate is in the subject.<sup>12</sup> [C: 16-17]

For Leibniz, then, it would appear that all true propositions are ultimately reducible to subject-predicate form, and are by nature analytic: for any given proposition  $p$ ,  $p$  is true only if it is ultimately analysable into a truth stating the identities of various predicates. Thus, statements such as "Eve was the first woman" or "Julius Caesar was assassinated" are analytically true statements: they are true by virtue of the fact that the singular concepts of "Eve" and "Julius

<sup>11</sup>Compare, for instance, their discussion with that of Adams (1979, 9-10; 2002, 854).

<sup>12</sup>For similar statements of this doctrine, see, e.g., *Discourse on Metaphysics*, DM: 12-13; *Correspondence with Arnauld*, LA: 58; *Primary Truths*, P: 87-88; *The Nature of Truth*, P: 94; *On Freedom*, P: 109; *On the Principle of Indiscernibles*, P: 135; *Metaphysical Consequences of the Principle of Reason*, P: 172.

Caesar" will, when fully and properly analysed, contain within themselves (in some appropriate sense) the notion of her being the first woman and of his being assassinated.<sup>13</sup> According to Cover & O'Leary-Hawthorne (1999, 149), it "follows straight away" from this doctrine concerning truth that singular propositions are "laden with generality" in the sense described earlier in §1.1. In other words, their claim is (or seems to be) that the predicate-in-subject doctrine of truth *ipso facto* entails that names are nothing but disguised definite descriptions, which can be cashed out in purely general terms: that for Leibniz, all singular propositions containing names ultimately reduce to and hence truly express general propositions.<sup>14</sup>

Closely related to this view of truth is Leibniz's *complete concept theory of substance*,<sup>15</sup> according to which

[T]he notion of an individual substance or of a complete being is to have a notion so complete that it is sufficient to comprise and to allow the deduction from it of all the predicates of the subject to which this notion is attributed.<sup>16</sup> [*Discourse on Metaphysics*, DM: 13]

<sup>13</sup>Leibniz was well aware of the fact that *our* notions of "Eve" and "Julius Caesar" might fail to contain all and only that which can be truly predicated of them (see, e.g., *Correspondence with Arnauld*, LA: 49-50). Singular concepts, then, should be construed as those notions that would be immediately perceptible and transparent to an infinitely discerning and all-knowing intellect, namely God.

<sup>14</sup>As far as I am aware, Leibniz never *argued* for the view that all truths are by nature analytic: in the *Correspondence with Arnauld* (LA: 63) he just bluntly declares that the view must be correct "else I do not know what truth is", while in the essay *On Freedom* (P: 107) he claims merely to have "seen", on reflection, what the nature of truth is.

<sup>15</sup>How closely related? My own view is that Leibniz construed his theory of complete individual substances as ultimately deriving from his view of truth: this is what texts such as *Primary Truths* and others would seem to suggest (cf. Jolley 2005, 48-9). However, and as Cover & O'Leary-Hawthorne (1999, 91, fn 5; 1999, 150) have pointed out, in the *Correspondence with Arnauld* (LA: 47) Leibniz seems to *equate* ("I mean nothing other than") his view of truth and his theory of complete individual substances. Precisely which of these two views is correct, however, has little bearing on the issues addressed in this chapter.

<sup>16</sup>Cover & O'Leary-Hawthorne (1999, 150) do not cite this passage as a canonical statement

Leibniz also frequently speaks of an individual substance “containing” everything true of it (or at least the “foundation” of everything true of it), or as “involving”, or containing vestiges or marks of, all of its past, present and future predicates. For Leibniz, then, each individual substance has an associated unique individual concept, which is sufficient to guarantee its numerical distinctness from all other individual substances. No two complete individual substances, or their associated concepts, are ever identical: for any individual substances *A* and *B*, it will be the case that *A* is numerically distinct from *B* if and only if their complete individual concepts are distinct. Cover & O’Leary-Hawthorne (1999, 150-1) take the relevant moral to draw from this Leibnizian doctrine to be precisely the same as the one drawn previously: namely, that proper names are at bottom nothing but disguised definite (general) descriptions; descriptions which, moreover, are each capable of uniquely referring to some particular individual (actual or possible), and which are sufficient in principle to derive all that is true of the individual so referred to.

Cover & O’Leary-Hawthorne’s basic exegetical claim, then, is that Leibniz’s predicate-in-subject doctrine of truth and his theory of complete individual substances, if they do not by themselves logically entail generalism, nevertheless very strongly suggest that Leibniz subscribed to some version thereof. As already noted (fn 11), they are not the only, or first, theorists to have made this claim: an essentially identical claim has also been made by Adams (2002, 854),<sup>17</sup>

---

of Leibniz’s theory of complete individual substances: rather, the passage they cite is S: 475-6. The passage quoted above, however, appears to me to be a slightly clearer, though essentially identical, statement of Leibniz’s view. For similar statements by Leibniz of this doctrine, see, e.g., *Correspondence with Arnauld*, LA: 48; *A Specimen of Discoveries About Marvellous Secrets*, P: 77-8; *Primary Truths*, P: 89; *The Nature of Truth*, P: 95.

<sup>17</sup>See also Adams (1979, 9-10).

who writes that Leibniz's "many ... statements about conceptual containment ... seem to imply that complete concepts of individual substances (which are supposed to express completely the individuality and identity of substances) are completely composed of general concepts."

Now, while I think it is true that *Leibniz's* construal and philosophical application of his theories of truth and complete individual substances indicate that he had something like the generalist picture in mind, it is nevertheless important to stress — something which the authors just cited fail to do — that the doctrines of truth and complete individual substances *purely in and of themselves* neither provide any support for nor any evidence against this generalist interpretation. This is because both the view that all truths are of subject-predicate form and are essentially analytic and the view that each individual substance has an associated complete concept sufficient to derive all that is true of it are each as at home in the singularist picture as they are in the generalist one. With regard to the predicate-in-subject theory of truth: the fact that all truths are essentially analytic and of subject-predicate form does nothing by itself to rule out, or rule in, statements expressing the identity of *nonqualitative* predicates as counting as genuine, primitive truths. Similarly, with regard to the theory of complete individual substances: the fact that each individual substance possesses an associated unique complete individual concept does nothing by itself to rule out, or rule in, some of the primitive predicates that might truly be ascribed to the substance in question being *nonqualitative*. Why, for instance, couldn't my complete individual concept include the primitive singular fact that *I am identical to Thomas Møller-Nielsen*, as well as other general facts about me? What feature of the aforementioned theories of truth and complete individual substances rules

this possibility out?

The claim that all truth is ultimately analytic, then, as well as the claim that each individual substance has a complete concept from which all of its associated truths can in principle be derived, are claims that would appear to be entirely *neutral* on the question of whether the world is ultimately qualitative or not. The mere fact that Leibniz subscribed to his theories of truth and complete individual substances does not provide any support *per se* for interpreting him as either a generalist nor as a singularist: the doctrines by themselves simply do not bear on the issue at all.<sup>18</sup>

Still, while it is true that neither the predicate-in-subject theory of truth nor the complete concept theory of substance in and of themselves logically compel one to adopt either generalism or singularism, the specific way in which Leibniz himself *made use* of such doctrines does, I think, provide some fairly persuasive evidence to suggest that he was a generalist. In particular, I think the fact that Leibniz, on various occasions,<sup>19</sup> stated explicitly that his theories of truth and/or complete concept theory of substance established the *Principle of the Identity of Indiscernibles* (PII), provides strong evidence for the claim that he was in fact a generalist.

This point requires elaboration. As Leibniz understood it, the PII states that there cannot exist intrinsically qualitatively indiscernible (“perfectly similar”) entities.<sup>20</sup> He believed that his theories of truth and complete individual sub-

---

<sup>18</sup>Many thanks to Gonzalo Rodriguez-Pereyra for help with this important point.

<sup>19</sup>See esp. the texts *Notationes Generales* and *Discourse on Metaphysics*, found in sources A and DM respectively. Compare also the texts *Primary Truths* and *On the Principle of Indiscernibles*, each of which can be found in source P.

<sup>20</sup>I take this construal of Leibniz’s version of the PII to be uncontroversial (cf. Rodriguez-Pereyra 2014, 37). What is more controversial is whether Leibniz thought the PII was even stronger than the principle just stated: in particular, whether he thought it ruled out intrinsically

stances were sufficient to establish the necessary truth of this principle. (The necessity of the PII follows directly from the necessity of the theories of truth and complete individual substances from which the principle purports to be derived.) Now, while it is to be admitted that the *precise* way in which Leibniz's arguments for the PII are supposed to function in the relevant texts (cited in fn 19 above) is far from completely transparent, nevertheless what is fairly transparent is that, in order for these arguments to have any chance at all of being successful, nonqualitative predicates must *not* be allowed to feature as irreducible constitutive elements of the world's fundamental truths, and nor should they be allowed to feature as primitive components of a given substance's complete individual concept.

Why should this be?<sup>21</sup> Consider all predicates that are "identity-involving" in the intuitive sense of involving the identity predicate: e.g., predicates such as *is identical to Nicolas Cage*, *is a human being* and *is numerically distinct from all red things*, etc. (Such predicates are therefore not necessarily nonqualitative as we have defined it.)<sup>22</sup> Now, either a given substance's satisfaction of such identity-involving predicates consists in its satisfaction of certain intrinsic qualitative predicates, or a given substance's satisfaction of such identity-involving predicates does not consist in its satisfaction of certain intrinsic qualitative predicates.

---

qualitatively indiscernible entities in distinct possible worlds as well, and whether he thought it allowed for entities differing merely in size alone. For further discussion, as well as an extensive list of references to the various primary sources in which Leibniz states the PII, see Rodriguez-Pereyra (2014, §2).

<sup>21</sup>The following argument is adapted from Rodriguez-Pereyra (2014, 58-9). Compare also Jauernig (2008, §3.1).

<sup>22</sup>In this respect I follow Cover & O'Leary-Hawthorne (1999, esp. 156-7), as well as, e.g., Dasgupta (2009, 48), Kment (2012, 580), and Pooley (MS, 101). For more on the distinction between identity- and individual- (or "object"-) involving predicates (e.g., *pegasizes*), see Ladyman, Linnebo and Pettigrew (2012, esp. 169).

Suppose it does not. Then substances *A* and *B* might have associated distinct complete concepts (from which, recall, all that can be truly said of each may be deduced) and yet nevertheless be intrinsically qualitatively indiscernible, in virtue of their respective concepts containing primitive, and distinct, identity-involving nonqualitative truths concerning each of them. Thus, for instance, *A* might satisfy the predicates *is identical to A* and *is numerically distinct from B*, and conversely *B* might satisfy the predicates *is identical to B* and *is numerically distinct from A*. (The letters “*A*” and “*B*” here are functioning as proper names.) Despite this, however, both *A* and *B* might still satisfy the *exact same* intrinsic qualitative predicates. Consequently, were Leibniz indeed thinking of his theory of truth and complete individual concepts in more than purely qualitative terms, then his arguments for the PII on the basis of such doctrines would not simply be less than fully straightforward, they would constitute almost trivial non-starters, incapable of getting off the ground from the get-go. That is, the theory of complete individual substances would perhaps be capable of establishing the principle that, necessarily, no two substances share all the same *predicates*; but it would be incapable of establishing the principle that, necessarily, no two substances are intrinsically qualitatively perfectly similar — which is to say, it would be incapable of establishing Leibniz’s version of the PII.

Conversely, if Leibniz *did* in fact regard a given substance’s satisfaction of identity-involving predicates as consisting in its satisfaction of some collection of intrinsic qualitative predicates, then it would seem to follow from the theory of complete individual substances that there cannot be any intrinsically qual-

itatively indiscernible but nevertheless numerically distinct substances:<sup>23</sup> two substances *A* and *B* could not be intrinsically qualitatively indiscernible but nevertheless numerically distinct, for facts stating or implying their numerical distinctness could *ex hypothesi* only be derived on the basis of their respective (perfectly similar) intrinsic qualitative characters.<sup>24</sup>

Two objections are worth considering, and discharging, at this point.

First Objection: Is it not consistent with the reconstruction offered above that Leibniz believed there to be primitive nonqualitative *non*-identity-involving facts — such as *loves Nicolas Cage*, or *pegasizes*, but not *is numerically distinct from Nicolas Cage*, or *is identical to that which pegasizes* — concerning substances? No. For if there were such primitive facts then there would inevitably also be facts about whether a given substance is identical to or numerically distinct from the substance which, for instance, loves Nicolas Cage or pegasizes. But for Leibniz's argument for the PII to work, such identity-involving facts must, as we just noted, in turn be derivable from such substances' satisfaction of intrinsic qualitative predicates — and this is only possible under the assumption that such facts are not primitively or unanalysably singular.

---

<sup>23</sup>As Rodriguez-Pereyra (2014, 59) notes, the thesis that identity facts are deducible from substances' intrinsic qualitative characters is actually sufficient *by itself* to establish Leibniz's version of the PII. However, the important thing to note for our purposes is that the complete concept theory of substance is sufficient to establish the PII *only when* conjoined with this thesis about identity. The fact that the thesis about identity by itself establishes the PII is immaterial to this claim.

<sup>24</sup>See also Rodriguez-Pereyra (2014, §4.5), who has argued persuasively that Leibniz's apparent commitment to the "strong necessity" of the PII (i.e., the principle which states that no two *possibilia*, even in distinct possible worlds, are intrinsically qualitatively indiscernible) — a thesis which his arguments in *On the Principle of Indiscernibles*, *Primary Truths*, *Notationes Generales* and the *Discourse* would in fact appear to commit him to — provides further support for the thesis that Leibniz thought of complete individual concepts as being intrinsically purely qualitative in character.

Second Objection: Is it not consistent with the reconstruction offered above that Leibniz believed there to be *further*, irreducibly singular aspects to fundamental reality which *don't* involve substances, or their associated complete concepts? Yes, it is consistent — however, this is not a view plausibly attributed to Leibniz. This is because, for Leibniz, all the actual world *is* is an infinite set of compossible substances (i.e., substances whose associated complete concepts do not contain mutually contradictory truths), and all possible (non-actual) worlds *are* are infinite sets of compossible complete concepts which a perfectly benevolent God, who decided to actualise only the best possible world, chose not to actualise.<sup>25</sup> There simply are no further truths over and above, or distinct from, those that may be derived on the basis of substances' associated complete concepts at any world, possible or actual. As Sleight (1990, 75) has pithily put it: “[W]hen you have said all that there is to say about the distribution of intrinsic denominations among individual substances in a given world, you have said it all.”

The singularist construal of Leibniz's theory of truth and complete individual substances, then, is one that, whilst perfectly logically coherent, is not a view that I believe Leibniz can plausibly be interpreted as being committed to. The basic reason for this is that the arguments for the PII that Leibniz offers in various places would be almost trivially unable to yield their conclusion were nonqualitative predicates to be admitted as primitive constitutive elements of individuals' complete concepts.

---

<sup>25</sup>Cf. Rescher (1967, 17), Mates (1986, 78), and Rodriguez-Pereyra (2014, 1-2). Such a reading of Leibniz is also strongly suggested in particular by what he writes in *A Resumé of Metaphysics* (P: 145-6).

While Cover & O'Leary-Hawthorne do not emphasise the *per se* neutrality of the predicate-in-subject doctrine of truth and the theory of complete individual substances, they do mention the fact that Leibniz's commitment to the PII strongly appears to incline him towards generalism. However, the main reason Cover & O'Leary-Hawthorne give for why Leibniz's commitment to the PII would appear to incline him in this direction is subtly different to the one just offered above. They argue (in a purported *reductio*) as follows:

Suppose one were to grant that PII is true, that *is Socrates* is a property had or lacked by every possible individual, and assume that this property is irreducible to qualitative properties. If no two possible individuals share the same set of qualitative properties, it would follow that some complex qualitative property (perhaps very long, perhaps very disjunctive) was necessary and sufficient for having the property *is Socrates*. But then it would seem that the property *is Socrates* would be constructable out of qualitative properties. So it would be reducible after all, contrary to our assumption. (Cover & O'Leary-Hawthorne 1999, 154)

This argument, however, is problematic for two reasons. First, the argument trades on an equivocation on the meaning of the word "irreducible". According to one understanding of the relevant notion of irreducibility (call it irreducibility<sub>1</sub>), the property *is Socrates* is irreducible in the sense of its being a metaphysically substantial or primitive property in its own right: when God created the world just of qualitative properties, so to speak, the property *is Socrates* didn't come along for free, but was something extra that needed to be added

separately. According to another way of understanding the relevant notion of irreducibility (call it irreducibility<sub>2</sub>), the property *is Socrates* is irreducible in the sense of its *not supervening* on some collection of other properties: the property *is Socrates*, in this second sense, would be reducible to some collection of qualitative properties if and only if fixing the full distribution of qualitative properties is sufficient to determine whether or not the property *is Socrates* is also instantiated. Now, while the property *is Socrates*' being irreducible<sub>2</sub> to some collection of qualitative properties necessarily implies its also being irreducible<sub>1</sub> to those properties, the converse need not be the case: the property *is Socrates* might well be a perfectly substantial, metaphysically self-sufficient entity in its own right whilst at the same time also (necessarily) supervening on some collection of qualitative properties.<sup>26</sup>

With the distinction between these two senses of irreducibility in hand, it is, I think, relatively easy to see that Cover & O'Leary-Hawthorne slip from speaking of the property *is Socrates* being irreducible<sub>1</sub> at the beginning of their proffered *reductio* to speaking of irreducibility<sub>2</sub> at the end. That is, Cover & O'Leary-Hawthorne's argument essentially amounts to the claim that the fact that the instantiation of a certain collection of qualitative properties constitutes a necessary and sufficient condition for the property *is Socrates* also being instantiated implies that *is Socrates* is not a metaphysically substantial property in its own right: they argue from a property's reducibility<sub>2</sub> to its reducibility<sub>1</sub>. But the inference is illegitimate: the fact that property *A* supervenes on property *B* (or some conjunction of such properties) does not entail that *A* is therefore

---

<sup>26</sup>The distinction between these two senses of irreducibility will become important in our discussion in §1.3 of Cover & O'Leary-Hawthorne's own proposal for reconciling Leibniz's (allegedly) dual generalist and singularist commitments.

“nothing over and above” *B*.<sup>27</sup>

There is also a second reason for why the above argument is problematic: namely, that the PII, much like the predicate-in-subject doctrine of truth and the theory of complete individual substances, is again entirely *neutral* on the issue of whether generalism or singularism is true.<sup>28</sup> That is, both the principle’s truth and its falsity are *equally* compatible with the singularist and generalist pictures. This is because the existence of irreducible<sub>1</sub> singular truths is perfectly compatible both with the PII’s not being true and its being true: the fact that there are not, or cannot be, two intrinsically qualitatively indiscernible individuals is perfectly compatible with individuals’ nevertheless bearing metaphysically primitive properties such as *pegasizes*. Similarly, generalism, understood as the denial that there are any irreducible<sub>1</sub> singular truths, is also perfectly compatible with the PII’s not being true, so long as either: (i) relational qualitative facts may be taken to ground facts concerning the numerical distinctness of individuals at each world, or (ii) more generally,<sup>29</sup> the numerical distinctness of such individuals is allowed to be taken as primitive. Thus, for instance, a world containing nothing but two numerically distinct qualitatively indiscernible individuals might be generalistically described as  $\exists x \exists y (Fx \ \& \ Fy \ \& \ x \neq y)$ , where *F* is the full conjunction of qualitative predicates satisfied by both individuals. This is a world that is perfectly at home in the generalist picture; or, at least, in

---

<sup>27</sup>Cf. Kim (1993, 167).

<sup>28</sup>*Pace* Adams (1979, 11) and Jauernig (2008, 194).

<sup>29</sup>Why “more generally”? This is because, whilst relational facts might (according to some) be sufficient to guarantee or even ground the numerical distinctness of individuals in many PII-violating worlds, they will be unable to do so when the individuals in question are relationally (or even “weakly”; see Saunders 2003a) indiscernible: in such cases, only the primitive relation of numerical distinctness may be appealed to by the generalist who does not wish to identify the putatively distinct individuals in question.

certain versions thereof (in which the relation of numerical distinctness is taken as metaphysically primitive).<sup>30</sup>

But while it is true that there are versions of generalism that are compatible with the failure of Leibniz's version of the PII, it is, I think, quite clear that the version of generalism that Leibniz himself was committed to *is* truly incompatible with such failure. This is because the versions of generalism that are capable of admitting such intrinsically qualitatively indiscernible entities are not versions of generalism that Leibniz himself, at least in his mature philosophy, would have felt comfortable with. With regard to option (i) above: Leibniz was repeatedly and emphatically clear that relations not founded upon the intrinsic natures of things — *external* relations, in Lewis' (1986, 62) sense — are never sufficient to individuate.<sup>31</sup> And with regard to option (ii) above: there is equally little doubt that he would have balked at the idea of individuals' being primitively numerically distinct, for the criterion of numerical distinctness is precisely what his version of the PII was originally intended to provide.<sup>32</sup>

---

<sup>30</sup>Cf. again Dasgupta (2009, 48), Kment (2012, 580), and Pooley (MS, 101). Cover & O'Leary-Hawthorne (1999, 156-7) actually go on to make an essentially identical point later in their paper in an extended response to Adams' (1979, 11) claim that "[t]he purely qualitative conception of individuality ... stands or falls with a certain doctrine of the Identity of Indiscernibles." It is thus unclear to me to what extent they originally intended to endorse their PII-to-generalism argument, particularly given that their seemingly unwitting equivocation over the two importantly distinct notions of "irreducibility" is so markedly at odds with the crucial role such a distinction plays in their own attempted interpretative resolution of Leibniz's metaphysics later on in their paper. But whatever the dialectical role played by the PII-to-generalism argument in their paper, what is important for our purposes is that the argument is a bad one, and does not establish its conclusion for the reasons given above.

<sup>31</sup>Indeed, in my view Leibniz is most plausibly read as advocating a view according to which all relational truths ultimately reduce<sub>1</sub> to intrinsic truths about monads. Rescher (1967, Ch 6) offers an especially useful treatment of the (vexed) topic of relations in Leibniz's philosophy.

<sup>32</sup>See, e.g., *Primary Truths*, P: 88-89; *On the Principle of Indiscernibles*, P: 133; *Monadology*, M: 222. For further exegetical support that Leibniz construed the PII primarily as a principle of numerical distinctness, see Rodriguez-Pereyra (2014, 67-9).

To sum up, then, it would seem that while Leibniz's doctrines of truth, complete concepts, and the PII themselves are entirely neutral on the generalism versus singularism question, the uses to which Leibniz put such doctrines, and the specific consequences he claimed to derive from them in conjunction with other aspects of his philosophy, nevertheless strongly suggest that he did indeed construe the world as being fundamentally purely qualitative in character.

I would like to close this subsection by remarking upon an interesting footnote found in Cover & O'Leary-Hawthorne (1999, 157, fn 17), where they appear to note almost exactly the point made in the preceding three paragraphs. Here, they are explicit that they do "not deny that generalism, together with the thesis that all relational facts are reducible to non-relational ones, may commit one to intra-world PII," and that because of this fact "Leibniz may have a resource for defending [the] PII even if he were a generalist." They are, however, hesitant about reading Leibniz in this way (i.e., as a generalist), for "a Leibnizian defense of the PII could not then proceed in the manner that it normally does." *Prima facie*, this is a slightly odd claim to make. As we have seen, it is primarily the fact *that* Leibniz, on several occasions, argued for the PII in the way that he did — in particular, that he claimed to derive the principle from his theories of truth and/or complete individual substances — which strongly suggests that he *did* subscribe to the generalist picture. But what, then, exactly is Leibniz's allegedly "normal" way of arguing for the PII? Moreover, is such a mode of argument truly incompatible with construing him as a generalist? And are there any other aspects of Leibniz's philosophy that are seemingly at odds with such an interpretation? These are the questions that we shall begin to tackle in the

next subsection.<sup>33</sup>

## 1.2.2 Singularist Pressures in Leibniz

Despite the fact that Leibniz's philosophical application and construal of his doctrines concerning truth, individual complete substances and the PII very plausibly suggest generalist tendencies, it is Cover & O'Leary-Hawthorne's chief contention that there are at least two other important aspects of Leibniz's philosophy which do not easily admit of such an interpretation. It is to these two aspects that we turn in this section.

(Note that the main purpose of this subsection is exegetical, insofar as I will for the most part merely be *presenting* the reasons that Cover & O'Leary-Hawthorne themselves give for thinking that there is a "deep tension" inherent to Leibniz's mature philosophy. Extended criticism of their various exegetical claims will be undertaken in §1.4.)

### Leibniz's construal of *de re* modal claims at face value

The first piece of evidence that O'Leary-Hawthorne & Cover cite in favour of the thesis that Leibniz was not a generalist is the fact that he often appeared to take *de re* modal ("transworld identity") claims at face value, claims which,

---

<sup>33</sup>Cover & O'Leary-Hawthorne (1999, 151-4) also claim that there is at least one other aspect of Leibniz's philosophy, distinct from his views on truth, complete individual substances, and the PII, that looks to incline Leibniz towards generalism: namely, his apparently "generalist" construal of the contents of the divine mind. To the extent that I find their claim plausible that such an aspect of Leibniz's considered metaphysics lends itself to a generalist interpretation, however, I think it is entirely derivative on his views of truth and complete individual substances. God, after all, can only be said to comprehend what is or is possibly the case: if the truths at all worlds are ultimately general, then the contents of his understanding will presumably be purely general as well.

straightforwardly understood, should have been unintelligible to Leibniz were he indeed thinking of worlds as being given purely in terms of general descriptions. For, on the generalist picture, singular modal claims — indeed, *all* singular claims, regardless of whether they are modal or not — can strictly speaking never be accorded anything more than a *de dicto* status. This follows from the fact that, for the generalist, proper names can never truly be anything other than mere shorthand for purely general definite descriptions. Thus, to repeat a point made earlier in §1.2.1, the reason why (e.g.) Julius Caesar could not have avoided assassination is because it is essentially part of the *definition* of the concept of *Julius Caesar* that he could not have avoided assassination. Hence, the fact that Julius Caesar could not have avoided assassination is one that, in a sense, is rather trivial: properly unpacked, it amounts to the mere assertion of the non-identity of some non-identical conjunction of general propositions. However, the apparent triviality would seem to derive from a Leibnizian thesis of much metaphysical profundity: namely, that individuals' complete concepts are fundamentally purely qualitative in character.<sup>34</sup>

Cover & O'Leary-Hawthorne (1999, 149), however, adduce two pieces of textual evidence in support of the opposite conclusion: namely, that Leibniz *did* in fact construe claims about *de re* modality at face value and, hence, cannot be

---

<sup>34</sup>Maunu (2005) points out that there is in fact a version of generalism that is capable of taking *de re* modal claims at face value. The trick involves allowing the generalist to quantify over *worlds* in her modal descriptions as well as the individuals within them — thus permitting locutions of the form, "There is a world  $w_1$  and a world  $w_2$  such that there is an individual  $x$  and an individual  $y$  where  $x$  is  $F$  and  $y$  is  $G$  in  $w_1$  and  $y$  is  $F$  and  $x$  is  $G$  in  $w_2$ ." (Both the worlds and the individuals within them are to be construed here as bound variables rather than as proper names.) On this version of "ultra-generalism", it is a straightforwardly meaningful question to ask whether or not the same individual that is actually  $F$  is possibly  $G$  (i.e., is  $G$  at some other possible world). Whilst this position is undoubtedly deserving of much discussion, I can find no textual evidence to suggest that Leibniz ever so much as considered such a view. I therefore set it aside here. (Compare also Sider's (2002) "ersatz pluriverse".)

plausibly interpreted as a generalist:

(i) You will object that it is possible for you to ask why God did not give you more strength than he has. I answer: if he had done that, you would not exist, for he would have produced not you but another creature. [Grua: 327]

(ii) But assuredly another will say, whence comes it that this man [Judas] will assuredly commit this sin? The reply is easy, it is that otherwise it would not be this man. [*Discourse on Metaphysics*, DM: 50]<sup>35</sup>

According to Cover & O’Leary-Hawthorne (1999, 149), such texts “offer what look to be unequivocally *de re* modal judgements,” and render one “not well placed to deny that Leibniz entertained a haecceitist metaphysical picture” — where a *haecceitist* is to be construed as someone who *is* capable of taking *de re* modal judgements at face value.<sup>36</sup> In the terminology developed previously, then, they are claiming that these texts support the view according to which Leibniz was actually a kind of singularist, who (to repeat) believed that singular propositions are required in order to perspicuously describe fundamental reality.

To what extent such a straightforward reading of these texts can be resisted will be examined in §1.4. For now, however, let us turn our attention towards the second — and, for Cover & O’Leary-Hawthorne, plainly much more decisive — reason for construing Leibniz as someone who did not subscribe to a generalist

<sup>35</sup>Cover & O’Leary-Hawthorne (1999, 149) quote Loemker’s (1969) translation of this passage, but to retain consistency with the rest of this chapter I have stuck with the translation found in Lucas & Grint (1953). As far as I can tell, there is no substantive difference between the two translations.

<sup>36</sup>In this respect Cover & O’Leary-Hawthorne follow Kaplan’s (1975) original use of the term.

world-view.

### Leibniz's use of "switching" considerations

The second piece of textual evidence that O'Leary-Hawthorne & Cover cite to support their claim that Leibniz was not a generalist is taken from Leibniz's correspondence with the theologian Samuel Clarke in 1715-16. Here, Leibniz famously made use of Principle of Sufficient Reason-type (PSR) arguments in order to establish the (actual) truth of the Principle of the Identity of Indiscernibles: which, as previously mentioned, in its Leibnizian form should be construed as the claim that there are no two things that are perfectly similar, in the sense of their being intrinsically, qualitatively indiscernible.<sup>37</sup> More specifically, the main way in which Leibniz tried to establish this conclusion in the correspondence was by arguing that, were there in actual fact two or more intrinsically indiscernible entities, then God would have had no "sufficient reason" to place them one way rather than another in which their roles are permuted or "switched" (LC: 61). Thus, there are no intrinsically indiscernible atoms. Similarly, there is no absolute space: for if there were, God wouldn't know where in absolute space to place the totality of the world's material content; he would have no "sufficient reason" to place the world's material content *here* rather than, say, three metres to the left of where it actually happens to be (or "chang[e] East into

---

<sup>37</sup>The fact that such "switching" considerations only purport to establish the actual (but not the necessary) truth of the PII should be uncontroversial: in order not to confront God with a problem regarding which world to actualise, it need only be the case that our actual world does not contain intrinsically qualitatively indiscernible ("switchable") entities. However, the argument does not, as it is sometimes mistakenly assumed (as it is by Cover & O'Leary-Hawthorne (1999, 212)), purport to establish the *contingency* of the PII: it simply does not bear on the question of whether there are distinct possible worlds containing intrinsically qualitatively indiscernible entities. See Rodriguez-Pereyra (2014, 115-6).

West"; LC: 26).

Whatever one makes of these arguments, it is — as Cover & O’Leary-Hawthorne emphasise — essential to their functioning that one be able to construe the world as being something over and above its full generalist description. This is because in order for them to work one needs to be able to make sense of, e.g., some particular atom *a* being *here* and another particular indiscernible atom *b* being 3 metres to the left of here, and also to be able to make sense of the putatively distinct “permuted” scenario in which atom *b* is here and atom *a* is 3 metres to the left of here. On the generalist picture, however, both the original world and its permuted cousin would receive the *same* full generalist description in virtue of their qualitative indiscernibility. That is, the only way in which such worlds *could* be said to meaningfully differ is with regard to *which particular* atoms are playing which qualitative roles, or *which particular* points of absolute space are occupied or unoccupied. And, according to Cover & O’Leary-Hawthorne, it is precisely this fact — namely, the fact that the coherence of such PSR-type arguments crucially depends on the full description of a world being more than purely general — which “convincingly betrays” (1999, 158, fn 19) Leibniz’s singularist metaphysical commitments, and which, they claim (1999, 167, fn 31), conclusively demonstrates that the “conservative, anti-haecceitist [generalist] Leibniz isn’t Leibniz.”<sup>38</sup>

---

<sup>38</sup>The overall dialectic of the correspondence is, of course, much more complicated than I have just suggested. For it is *Clarke*, rather than Leibniz, who first appeals to “switching” considerations in his second reply (LC: 20-1) in order to refute Leibniz’s construal of the PSR. (In doing so Clarke tries to establish that sometimes matters of fact obtain for no other reason than the “mere will of God”, a view that Leibniz (LC: 25, 27, 36) repeatedly and emphatically rejected throughout the correspondence.) In his third letter, Leibniz — apparently ignoring Clarke’s previously stated construal of the PSR — goes on to consider the famous Leibniz shift argument against absolute space, involving a global, time-independent repositioning of matter in space. In his third reply, Clarke (LC: 30-1) — echoing his comment in his previous letter

To conclude this section, it might seem as though we are left with no clear answer to the question of whether or not Leibniz was a generalist. For while Leibniz's own take on and use of the predicate-in-subject conception of truth, complete concept theory of substance, and the PII all appear strongly to incline him towards generalism, nevertheless his apparent construal of *de re* modal claims at face value, as well as his use of switching considerations in the correspondence with Clarke, seem quite strongly to count against such an interpretation.

In the next section, we shall examine whether Cover & O'Leary-Hawthorne's own attempt at salvaging a coherent interpretation of Leibniz's metaphysics from this seeming interpretative mess is successful; and in the section after that we will question whether there is in fact any such interpretative tension in the first place.

### 1.3 Weak Haecceitism to the Rescue?

Rather than construing him as renouncing the metaphysical propriety of general propositions *tout court*, Cover & O'Leary-Hawthorne construe Leibniz as instead singling out a special place for general propositions within his modal-metaphysical system. The view, which they label *weak haecceitism*, sees Leibniz as regarding singular facts as necessarily *supervening* on, but nevertheless not being *grounded* in, the totality of general facts: in the terminology developed

---

— points out that similar “switching” considerations would likewise rule out the existence of intrinsically indiscernible atoms as well. Leibniz (LC: 36) then agrees, and in his fourth letter (and to Clarke's utter astonishment) explicitly states that he takes atoms to be “confuted ... by the principles of true metaphysics.” For an excellent summary of the overall dialectic of this aspect of the correspondence, see Pooley (MS, §3.1).

in the previous section, Cover & O’Leary-Hawthorne thus regard Leibniz as being committed to the totality of singular facts as being reducible<sub>2</sub>, but not reducible<sub>1</sub>, to the totality of general facts at each possible world. (Recall that supervenience describes a relation of mere covariance between subject matters; it does not by itself entail any other metaphysical or explanatory commitments.) The weak haecceitist thus regards singular propositions as being required in order to fully and perspicuously describe fundamental reality, yet nevertheless sees such propositions as being (necessarily) a function of general propositions. Given our discussion above, it is clear, at the very least, that weak haecceitism does not by itself constitute an inconsistent position.

According to Cover & O’Leary-Hawthorne (1999, 161), this construal of Leibniz as a weak haecceitist not only “rescue[s] the intelligibility of his arguments concerning the PII and judgements of transworld identity and diversity,” but it also offers “independent motivation for (and so, partial explanation of) Leibniz’s commitment to strong essentialism.” This, they claim, is because weak haecceitism disallows the multiplication of possible worlds that the “strong haecceitist” — that is, someone who believes that there are genuine singular facts which do not necessarily supervene on the totality of all general facts — is forced to accept. This in turn is due to the fact that the strong haecceitist is unable to rule out worlds in which the same general facts obtain, but in which the singular facts are different. (E.g., a world in which atom *a* and atom *b* have simply “swapped roles”.) Thus, it would seem that the only sensible way for Leibniz to arrest the slide from weak to strong haecceitism is by adopting a form of essentialism sufficiently strong so as to be up to the task — an essentialism

that Leibniz did, in fact, uncontroversially sign up to.<sup>39</sup>

Let us grant that the weak haecceitist interpretation of Leibniz meshes rather well with his commitment to essentialism, while the strong haecceitist interpretation does not. A relevant question to ask at this juncture is: Does weak haecceitism similarly mesh well with *other* aspects of Leibniz's philosophy, *distinct* from his essentialism? That is, is weak haecceitism a plausible view to ascribe to Leibniz, *all* aspects of his philosophy considered?

A useful place to begin tackling this question, in my view, is with Cover & O'Leary-Hawthorne (1999, 160) own proffered reasons for not reading Leibniz as a strong haecceitist:

It is clear that strong haecceitism would be anathema to Leibniz. We could well imagine him saying: "There can be no difference between two things *solo numero*; that is, no real difference is merely *difference in thing alone*. To conceive two worlds exactly alike save only that one contains a certain object (Socrates, say) where the other contains a different but indistinguishable object (Schmocrates) is to conceive of mere fictions, between which God would have no sufficient reason for choosing."

The main thought seems to be this. Consider a world, qualitatively described, containing a variety of individual substances variously related to one another.

---

<sup>39</sup>While Leibniz's commitment to some kind of essentialism is uncontroversial, precisely what kind of essentialism Leibniz was committed to is a point of scholarly contention. Mondadori (1973, 1975) defends a (popular) "superessentialist" reading, according to which all of a given individual's properties are essential to it; Cover & O'Leary-Hawthorne (1999, Ch 3) defend a weaker, "strong essentialist" reading, whereby only all of an individual's intrinsic properties are essential to it; and Sleight (1990, Ch 4) defends an ostensibly distinct reading altogether, which he labels "superintrinsicism".

Now ask the question: in virtue of what, exactly, is the correct singular description of such a world determined? What determines, for instance, whether it is Socrates who is qualitatively such-and-such and is related to so-and-so in such a world, rather than, say, Schmocrates? What, to use more Leibnizian terminology, would be the “sufficient reason” for the correct singular description to be one involving Socrates, rather than Schmocrates?

The basic problem for strong haecceitism that Cover & O’Leary-Hawthorne seem to be pointing to here, then, is that if singular facts are ultimately to be construed as something over and above the general, then there will always be an infinity of distinct singular descriptions capable of supervening on any given general description. Put in more theological and Leibnizian terms, God would have no reason for choosing one such singular description over any other. They are all on a par, and hence God would never consider actualising any of them. Thus, strong haecceitism is not a view to which Leibniz himself would have subscribed, given his manifest commitment to the PSR.<sup>40</sup>

There is, however, something deeply puzzling about this argument. In particular, it is not immediately apparent whether weak haecceitism is any more compatible with Leibniz’s construal of the PSR than strong haecceitism is. For according to the weak haecceitist, at any given possible world there is a unique singular description truly compatible with that world’s general description; but, seemingly, the weak haecceitist has not provided us with any

---

<sup>40</sup>From what they write in the paragraph cited above, Cover & O’Leary-Hawthorne seem to think that this problem of PSR-compatibility for Leibniz only arises in the case of the assumed existence of two or more worlds that are qualitatively indiscernible but nevertheless singularly (“merely haecceitistically”) distinct. The problem they point to, however, appears to me to be much broader than this: namely, given *any* world (qualitatively described), in virtue of what — by what “sufficient reason” — is its correct singular description to be determined?

grounds for thinking that there is a genuine reason *why* any such singular description is in fact the correct one. Indeed, the weak haecceitist appears merely to have *stipulated that* one such singular description is truly compatible with any given world's complete qualitative description. But this, it seems, is hardly any more compatible with Leibniz's construal of the PSR than strong haecceitism is. The argument against strong haecceitism, recall, was that strong haecceitism's commitment to a plurality of worlds differing singularly but not qualitatively entails that God would never have a "sufficient reason" to actualise any of them. Weak haecceitism, it seems, is meant to solve this problem by claiming that, in fact, only one such singular description is compatible with any given general description: but we are not provided with any "sufficient reason" as to *why* the unique correct singular description is privileged relative to the others. In a sense, then, a version of the PSR has been saved: but it is a version of the principle that is in truth more Clarkian than Leibnizian (see fn 38 above).

A related problem for the weak haecceitist interpretation arises in considering how to square it with Leibniz's (construal of his) theories of truth and complete concepts. As we saw in §1.2.1 above, Leibniz's explicit assertions that his theories of truth and complete individual substances were sufficient to establish the truth of the PII makes it plausible to think that Leibniz believed that a given substance's satisfaction of various intrinsic, qualitative predicates constitutes a necessary and sufficient condition for its satisfaction of identity-involving predicates, such as *is identical to Nicolas Cage*, *is numerically distinct from all red things*, and so on. This in turn would appear to suggest that, while truths involving intrinsic qualitative predicates should be taken to feature as constitutive elements of an individual substance's complete concept, identity-involving

truths do not: for as Leibniz himself puts it, an individual substance's complete concept need only contain such truths that are in principle "sufficient to deduce" ("*suffisante à en deduire*"; G: 44) all that may be truly predicated of that substance. But now a dilemma arises: how can it be that *nonqualitative* identity-involving facts concerning such a substance (e.g., *is identical to Nicolas Cage*) are "deducible" from facts concerning its intrinsic qualitative character unless either: (a) such intrinsic qualitative facts are supplemented by brute entailment relations between intrinsic qualitative facts and nonqualitative identity-involving facts; or (b) such nonqualitative identity-involving facts are mere shorthand for statements concerning the identity or distinctness of intrinsic qualitative predicates (such that, for instance, a substance's being *identical to Nicolas Cage* ultimately amounts to an abbreviated identification of intrinsic qualitative predicates)? Unfortunately for Cover & O'Leary-Hawthorne's preferred interpretation, however, option (a) is one which would appear to lack any textual basis in Leibniz's writings (he certainly never explicitly spoke of any such brute entailment relations), while option (b) is of course just the straightforward generalist interpretation of Leibniz that Cover & O'Leary-Hawthorne are keen to deny.

To their credit, Cover & O'Leary-Hawthorne (1999, §3) are well aware of these kinds of difficulties as presenting a formidable obstacle to their interpretative account's overall plausibility: as they put it, weak haecceitism does seem to pose a threat to Leibniz's "rationalism", broadly construed. Indeed, they expend considerable philosophical energy trying to reconcile weak haecceitism with these other aspects of Leibniz's philosophy. Unfortunately, however, their proffered resolution is not one that is readily transparent.<sup>41</sup> It is therefore perhaps best if

---

<sup>41</sup>Indeed, in an earlier paper O'Leary-Hawthorne & Cover (1996, 25) admit that their account

we begin by stating their proposal in their own words (1999, 171-3):

[I]ndividual concepts must be acknowledged as having a dual aspect, of both singularity and generality.... [A] complete individual concept, one might say, has some content over and above the general properties that can (at least by God) be extracted from it. To say what this extra content is would be to express something that corresponds to the roles played by individuals or bare haecceities in singular propositions as nowadays conceived.... [I]ndividual concepts must be singular in themselves. They must in this respect be *like* Scotistic thisnesses, but unlike them in necessarily containing a particular set of qualitative properties.... [A]n individual concept contains the predicates of an individual in such a way that to understand the concept is to understand why the individual falling under it is the way it is.

As I understand it, the basic idea is as follows. An individual's complete singular concept is not to be construed as equivalent, or reducible<sub>1</sub>, to its complete general description. This would be tantamount to accepting the "natural" generalist interpretation of Leibniz that Cover & O'Leary-Hawthorne are keen to reject. Rather, singular concepts should be construed as being something over and above, and indeed as being in fact more basic than, general concepts: God, in seeing the complete concept of a particular individual, is able to grasp not only

---

remains "undeniably obscure" despite their own best efforts at its elucidation. However, they seem to regard such obscurity as a strength, rather than a weakness, of their proposal, for it "accommodates Leibniz's desire to say that ... God is in possession of individual concepts while we are not. What the finite mind cannot do, on Leibniz' (*sic*) behalf, is to explain what an individual concept is by putting one in your possession." Perhaps; or perhaps the account is just plain obscure!

all of the qualitative predicates that might be correctly ascribed to it, but is also able to grasp why, how, and in virtue of what that individual is, qualitatively, the way it is. In a sense, then, grasping the individual concept associated with a particular individual is tantamount to grasping the essence or nature of that very individual. Thus, on this view singular facts both supervene on general facts *and* indeed are more fundamental than them:<sup>42</sup> for the general truths pertaining to any given individual are *explained by* that individual's specific singular nature. The singular thus ultimately grounds the general, and not — as *per* the natural interpretation — the other way around.

It is, I think, at best debatable whether Cover & O'Leary-Hawthorne's response here suffices to explain how weak haecceitism can be made compatible with Leibniz's doctrine of complete individual substances and the PSR. Indeed, it is hard to shake the suspicion that they have not really answered the original objections at all: they appear merely to have stipulated *that* interpretative reconciliation of these apparently conflicting aspects of Leibniz's philosophy is possible, and that this interpretative reconciliation is (perhaps) intelligible to God, but not to us. In other words, we appear merely to have been *told* that the "rationalist" objection might be met: but how it is exactly that the individual substances and their associated concepts are to be construed, and how it is that God is able to see which general predicates are true of each of them, is not something that is ever fully explained; nor is it something we could ever be in a position to fully understand.

In an earlier paper,<sup>43</sup> O'Leary-Hawthorne & Cover (1996, 25) do not appear

---

<sup>42</sup>The supervenience is thus "two-way", and runs in both directions: at all possible worlds, the singular supervenes on the general, and the general also supervenes on the singular.

<sup>43</sup>A modified version of this paper appears as Chapter 4 of Cover & O'Leary-Hawthorne

to be entirely unsympathetic to the analysis offered in the previous paragraph:

Beyond saying about [individual concepts] what has already been offered, perhaps the best one can do is provide a *functional* characterisation of them: (1) they are such that from all the individual concepts realised at the actual world, God can *a priori* see all the general truths that obtain at the actual world; (2) they are the truthmakers for *de re* modal discourse; and so on. In connection with (2), individual concepts serve to ground strong essentialist strains in Leibniz's robust metaphysic of individual substances. In connection with (1), they serve to ground his rationalism.

But why, then, not simply do without (or reduce<sub>1</sub>) singular concepts entirely? After all, such concepts only seem to *get in the way* of our being able to understand, in a perfectly straightforward, transparent and intuitive way, the nature and meaning of Leibniz's complete concept theory of substances and the PSR. So what significant role, if any, are these singular concepts actually playing in Leibniz's metaphysics? To this concern Cover & O'Leary-Hawthorne (1999, 175) offer the following response, ostensibly "on Leibniz's behalf":

[W]ithout singular propositions, we cannot understand claims about Aristotelian essentialism and transworld identity at face value. But we do understand them at face value. They are perfectly intelligible, so understood. Isn't that reason enough to give singular propositions the status they deserve? Isn't that reason enough for denying the notion that "singular propositions ought to be eliminated because

---

(1999).

each of them stands or falls with a certain general proposition"?<sup>44</sup>

In the next section we shall examine whether the evidence that Cover & O'Leary-Hawthorne adduce in favour of the thesis that Leibniz construed *de re* modal claims at face value truly stands up to scrutiny. Before we get there, however, I would like to finish this section by remarking upon three points relating to Cover & O'Leary-Hawthorne's (1999, 161) claim that weak haecceitism "rescue[s] the intelligibility of [Leibniz's] arguments concerning the PII," points which will in many respects anticipate our discussion in the next section as well.

The first, and perhaps most obvious, point to make is that the claim that weak haecceitism "rescue[s] the intelligibility" of Leibniz's arguments for the PII must be construed as having restricted scope. As we saw above, interpreting Leibniz as a weak haecceitist if anything only poses an *obstacle* to understanding how it is that his arguments for the PII on the bases of his theory of truth and/or complete individual substances are supposed to proceed. To the extent that weak haecceitism indeed "rescue[s] the intelligibility" of Leibniz's arguments for the PII, then, the claim must (at the very least) be restricted to the arguments Leibniz adduces for the PII in the correspondence with Clarke. Weak haecceitism does not "rescue the intelligibility" of Leibniz's arguments for the PII in the general case.

The second point to make is that, if rescuing the intelligibility of Leibniz's arguments for the PII in the correspondence with Clarke is indeed a primary reason for thinking that one's own interpretation of Leibniz is correct, then it is

---

<sup>44</sup>The quote at the end of this paragraph is apparently drawn from work by Robert Adams. Cover & O'Leary-Hawthorne, however, provide no reference for this quotation in either their (1996) or their (1999); moreover, I have been unable to find the relevant quotation in any of the works by Adams that I have come across.

unclear why Cover & O’Leary-Hawthorne think that the weak haecceitist interpretation gains any extra exegetical plausibility in this regard. This is because in the Leibnizian switching arguments that Cover & O’Leary-Hawthorne consider, it is crucial to their functioning that one conceives of singular facts’ *not* supervening on the general. Take, for instance, the generic case of two worlds in which intrinsically indiscernible atoms have swapped qualitative roles: these are worlds that are *ex hypothesi* qualitatively indiscernible, but that nevertheless differ over which particular singular facts are true at each of them.<sup>45</sup> Similarly in the case of shifted worlds: these are supposed to be worlds that are qualitatively the same, but which are supposed to differ with regard to which particular points of space are occupied or unoccupied — facts which require the use of singular propositions if they are to be properly stated. If Leibniz was indeed thinking like a weak haecceitist, however, he presumably would not have regarded these arguments as resting on metaphysically coherent presumptions: worlds, for the weak haecceitist, *cannot* differ with respect to their singular facts if they are qualitatively the same. If rescuing the intelligibility of the switching or shift arguments was the main reason behind reading Leibniz as thinking of the world in less than fully general terms, then it is strong, not weak, haecceitism

---

<sup>45</sup>As Rodriguez-Pereyra (1999) has pointed out, it is not the case that in *all* possible worlds “switching” intrinsically indiscernible atoms will inevitably yield a world that can coherently be regarded as distinct from the original, even if singular propositions are allowed to be used in the original world-description. (A world containing nothing except two perfectly similar simples some distance away from another in a relationalist universe, for instance, is not a world that can coherently be “switched”.) Moreover, Rodriguez-Pereyra notes that Leibniz’s inability to rule out our actual world’s being one of these “unswitchable” worlds is what ultimately renders his argument for the PII in the correspondence with Clarke invalid. For the purposes of the discussion above, however, I am simply granting that our world is one which can be coherently switched: we are not, after all, interested in the validity of such arguments *per se*; rather, our concern is with what these arguments allegedly indicate regarding Leibniz’s metaphysical presuppositions.

that would seem to be called for.<sup>46</sup>

The third and final point that I wish to make in closing this section is that it is, on reflection, not at all clear why the mere fact that certain metaphysical assumptions must be presupposed in order to make sense of a particular author's arguments should constitute a sufficient evidential basis for interpreting the author in question as being committed to these assumptions. In other words, the inference from the fact that a certain metaphysical theory or framework "rescue[s] the intelligibility" of a certain form of argument used by an author to thereby interpreting the author in question as being committed to the view so presupposed is not, I think, an inference that is straightforwardly legitimate. To take the case at hand: it is, of course, essential to Leibniz's shift and switching arguments' functioning that one be able to take singular or *de re* modal claims at face value, and moreover that one be able to take singular facts as being neither reducible<sub>1</sub> nor reducible<sub>2</sub> to any collection of general facts. But it is similarly true that in order for these arguments to work one should be able to make coherent sense of worlds containing intrinsically indiscernible atoms, or absolute space, as well: entities that Leibniz was adamantly and uncontroversially not committed to. Should one thereby conclude that Leibniz was committed to the existence of atoms and absolute space? No. The obviously correct conclusion to draw is that both the "switching" and "shift" arguments are designed to yield conclusions *against* such entities' (actual) fundamental existence. By parity of reasoning, then, should we not construe Leibniz's arguments as similarly directed against the fundamental existence of singular features of the world as well? Why not, in other words, see him as arguing *from* such worlds' putative

---

<sup>46</sup>Pooley (MS, 72, fn 11) makes a similar point.

commitment to the existence of irreducible (in both senses) singular facts *to* the conclusion that singular facts are not truly fundamental features of the actual world? Given the structural parity of these arguments, such an inference appears almost inescapable; it is surprising, therefore, that it is one which Cover & O'Leary-Hawthorne apparently fail to ever consider.

## **1.4 The Received View Defended**

In the previous section I questioned whether Cover & O'Leary-Hawthorne's attempt to reconcile weak haecceitism with other aspects of Leibniz's philosophy is ultimately successful. In particular, I argued that not only does their favoured "weak haecceitist" interpretative resolution fail to satisfactorily resolve one of the (alleged) core difficulties in interpretation that they originally raised — namely, that of "rescu[ing] the intelligibility" of Leibniz's arguments for the PII in the correspondence with Clarke — but I also claimed that such an interpretative resolution plausibly fails to mesh in a satisfactory way with other important aspects of Leibniz's philosophy, in particular his views on truth, complete individual substances and the PSR. Given the apparent failure of Cover & O'Leary-Hawthorne's attempted resolution of the ostensibly warring generalist and singularist aspects of Leibniz's philosophy, then, one might be forgiven for concluding that perhaps there is no coherent interpretation of Leibniz's metaphysics available. I think, however, that this conclusion would be premature. Indeed, I believe that the "natural" or "conservative" generalist interpretation of Leibniz that Cover & O'Leary-Hawthorne reject is in fact the correct one, and that the textual evidence we previously saw them cite in favour of their view

that Leibniz was not a generalist is not, all things considered, very compelling.

I begin this section by disputing Cover & O’Leary-Hawthorne’s claim that Leibniz in fact construed *de re* modal claims “at face value”, in the sense that he took the truth-makers for *de re* modal discourse to be the irreducible<sub>1</sub> (but not irreducible<sub>2</sub>) general propositions that obtain at distinct possible worlds. After that I will move on to question Cover & Hawthorne’s claim that Leibniz’s use of switching considerations in the correspondence with Clarke reveals something profound about the nature of his considered metaphysics.

### 1.4.1 The claim that Leibniz took *de re* modal claims at face value

One of Cover & O’Leary-Hawthorne’s stated reasons for not interpreting Leibniz to be a full-blooded generalist is (to reiterate) that Leibniz appeared to construe claims about *de re* modality at face value, something which should be impossible were he thinking of worlds in purely general terms. Cover & O’Leary-Hawthorne’s resolution to this problem is to claim that Leibniz’s (alleged) weak haecceitistic *metaphysics* provided him with a uniform *semantics* for interpreting *de re* modal discourse: a semantics which he did, they claim, in fact fully subscribe to and make use of.

The implication here — namely, that Leibniz *had* a considered modal semantics, and that he had a considered and developed position on what grounds the truth of various *de re* modal claims — is, exegetically, a bold one to make.<sup>47</sup> Moreover, it is one that is especially curious to see being advocated by Cover

---

<sup>47</sup>For conflicting views, see, e.g., Mondadori (1973, 1975), Adams (1977), and Wilson (1979).

& O’Leary-Hawthorne, given their explicit and repeated avowals elsewhere in their book that Leibniz did *not* in actual fact have a developed modal semantics (1999, 89, 115): “[S]o far as we are aware, Leibniz *had* no well-developed modal semantics.... Leibniz to our knowledge had no developed, considered semantic theory of *de re* modality of the sort one confronts in contemporary discussions....” Furthermore, Cover & O’Leary-Hawthorne’s proffered thesis is also difficult to square with their various other claims in their book to the effect that, *to the extent that Leibniz had any kind of quasi-developed modal semantics at all*, Leibniz did *not* in fact construe modal claims at face value in the relevant sense. Thus, they (1999, 118, 121) claim that certain texts (LA: 15-6; T: 371)<sup>48</sup> suggest “[n]o other interpretation” other than one ultimately cashed out in terms of *counterparts*: “When Leibniz has the grounds of *de re* counterfactuals in mind, a counterpart picture, we have argued, is at work.... Leibniz may be viewed as proposing a sort of rescue operation: modal claims are not to be taken at face value but are rather to be understood in terms of counterparts.” Barring an interpretation according to which Leibniz was some kind of a philosophical schizophrenic,

---

<sup>48</sup>The relevant passages read:

[B]y the individual concept of Adam I mean, to be sure, a perfect representation of a particular Adam who has particular individual conditions and who is thereby distinguished from an infinite number of other possible persons who are very similar but yet different from him.... There is a possible Adam whose posterity is thus, and an infinite number of other Adams whose posterity would be different.... [Correspondence with Arnauld, LA: 15-6]

I will now show you some [worlds], wherein shall be found, not absolutely the same Sextus as you have seen (that is not possible, he carries with him always that which he shall be) but several Sextuses resembling him, possessing all that you know already of the true Sextus, but not all that is already in him imperceptibly, nor in consequence all that shall yet happen to him. You will find in one world a very happy and noble Sextus, in another a Sextus content with a mediocre state, a Sextus, indeed, of every kind and endless diversity of forms. [Theodicy, T: 371]

not all of these claims can be unequivocally true together: Leibniz cannot be said to have had no developed modal semantics, and yet simultaneously be said to have subscribed to both a non-straightforward and modestly sophisticated counterpart-theoretic semantics as well as a straightforward and (arguably) less sophisticated “singularist” semantic construal of *de re* modal discourse.

Such an *ad hominem* reply to Cover & O’Leary-Hawthorne, however, does nothing by itself to remove the claimed interpretative tension in passages (i) and (ii) cited in §1.2.2. But can it be removed? I think it can. For, on reflection, it is clear that neither of these passages are at all incompatible with reading them as stating mere *de dicto* truths.<sup>49</sup> Read *de dicto*, what Leibniz is saying in these passages is that, given that the complete concept of a particular individual is essentially equivalent to some (infinite) conjunction of general propositions, and given that this complete concept is both necessary and sufficient to individuate this individual in the sense of guaranteeing its numerical distinctness from all other possible individuals, then altering any one or more of the general propositions that are true of that particular individual is (to the extent that the complete concept thus yielded is consistent) to yield a concept of a possible individual numerically distinct to the one before. Thus, for instance, if it is part of the concept of Judas Iscariot that he betrayed the Son of God, and we go

---

<sup>49</sup>It is also perhaps worth mentioning that Cover & O’Leary-Hawthorne themselves acknowledge that passages (i) and (ii) are perfectly compatible with a *de dicto* reading, although their apparent confidence in the claim that Leibniz did indeed construe *de re* modal claims at face value seems to be greater in their (1999) book than in their (1996) article. Thus, after noting the possibility of a *de dicto* reading of such passages in their (1999, 155), they go on to assert that “We remain convinced that Leibniz does endorse the *de re* modality crucial to his essentialism and the intelligibility of transworld identity judgements.” In their (1996, 11), however, an identical passage is prefixed with the word “suppose” (thus yielding the sentence “Suppose we remain convinced that Leibniz....”). I take it that the absence of the hypothetical in their (1999) suggests that they regard Leibniz’s apparent construal of *de re* modal claims at face value as being more exegetically decisive than they did in their (1996).

on to alter this particular individual concept in a consistent way so as to yield an individual that did *not* in fact betray the Son of God, then we have yielded a distinct individual concept, not identical to the original concept of Judas Iscariot. On this *de dicto* reading, then, what passages (i) and (ii) amount to are simply trivial assertions of the non-identity of some non-identical conjunction of general propositions: however (and to repeat a point made earlier in §1.2.2), the triviality of these claims in turn naturally follows from specific Leibnizian doctrines (more precisely, his own construal of such doctrines) — in particular, his theories of truth and complete individual substances — that are themselves anything but.<sup>50</sup>

Now, while I believe these passages may indeed plausibly be read as offering mere *de dicto* rather than irreducibly *de re* truths, I do not believe that *all* of the modal claims Leibniz makes throughout his works may be read in the same way; nor do I wish to claim that there is in fact a uniformly applicable approach to interpreting Leibniz's conception of *de re* modality. Indeed, my own suspicion is that Leibniz himself never had, nor even saw any need to develop, any particular

---

<sup>50</sup>Such a reading might also be taken to be partially supported by the fact that passage (ii) in the *Discourse* is immediately followed by a sentence which explicitly links the discussion with the theory of complete individual substances:

But assuredly another will say, whence comes it that this man [Judas] will assuredly commit this sin? The reply is easy, it is that otherwise it would not be this man. For God sees from all time that there will be a certain Judas, of whom the notion or idea that God has of him contains this future free action. (Emphasis added)

This passage bears a striking resemblance to another, in which a similar claim is again succeeded by a reference to the theory of complete individual substances:

... [W]hen I ask what would have happened if Peter had not denied Christ, it is asked what would have happened if Peter had not been Peter, for denying is contained in the complete notion of Peter. (Emphasis added) [Grua 358; cited by Mates (1986, 140)]

“modal semantics”, at least in any modern-day sense of the term.<sup>51</sup> Moreover, while passages like (i) and (ii) may, I think, be offered a somewhat plausible *de dicto* interpretation, there do exist other passages (such as those quoted by Cover & O’Leary-Hawthorne (1999, 120-1), and cited in fn 48 above) in which Leibniz, in attempting to fend off accusations of Spinozistic necessitarianism, seems to appeal to something very close to a counterpart-theoretic (and arguably inconsistently non-essentialist) analysis of modal statements. And what is more, in much of his other work Leibniz appealed to an entirely separate doctrine — the doctrine of *infinite analysis*, according to which, in brief, necessary truths are those which can be analysed into identity truths in a finite number of steps, while contingent truths are those that can be analysed into identity truths only in an infinite number of steps — which does not clearly connect to counterpart theory in any way, nor indeed to any other contemporary modal semantic doctrine.<sup>52</sup> Thus, in claiming that irreducible<sub>1</sub> singular propositions are required for Leibniz’s semantics to work, Cover & O’Leary-Hawthorne are arguably guilty of a double mistake: first, they fail to take into adequate consideration the complexity and apparent multiplicity of Leibniz’s general analyses of contingency and necessity; and second, they fail to provide adequate justification for their crucial implicit assumption, one which they in fact explicitly disavow elsewhere in their book — namely, that Leibniz in actual fact had anything like a developed modal semantics to begin with.<sup>53</sup>

<sup>51</sup>A view apparently shared by the majority of contemporary Leibniz scholars. See, e.g., Adams (1977), Wilson (1979), Sleight (1990, 51-52), and Look (2013, §2).

<sup>52</sup>For a sample of Leibniz’s statements of his infinite analysis doctrine of contingency, see, e.g., *A Specimen of Discoveries About Marvellous Secrets of a General Nature*, P: 75; *Necessary and Contingent Truths*, P: 97; *On Freedom*, P: 108-9.

<sup>53</sup>Cf. Sleight (1990, 52):

In our time period [i.e., the time of the *Correspondence with Arnauld*] Leibniz did

## 1.4.2 The claim that Leibniz's use of "switching" considerations in his correspondence with Clarke reveals that he was not a generalist

We come now to the main piece of evidence that Cover & O'Leary-Hawthorne cite in support of their interpretation of Leibniz as someone who, in actual fact, did not believe that the world is fundamentally purely qualitative in character. Recall the main claim: Leibniz's use of switching considerations in the correspondence with Clarke "convincingly betrays" the fact that, in reality, he did not conceive of the world in purely general terms. This is because the arguments in question depend for their very coherence on the presumed existence of particular singular facts (e.g., facts concerning whether spatial point  $p$  is materially occupied or unoccupied in shifted worlds) that are not even supervenient upon,

---

not use the structure of possible worlds to account for *de re* modalities. He used it primarily as a vehicle to discuss creation and attendant theological matters.... So it would be a mistake to look for an explanation of Leibniz's metaphysical commitments in his *de re* modal semantics based on possible worlds, since he did not have such a *de re* modal semantics.

A further question which is perhaps worth considering before closing this section is: Why do Cover & O'Leary-Hawthorne argue both for (in Ch 4) and against (in Ch 3) the view according to which Leibniz construed *de re* modal claims at face value in their (1999) book? One possible reason — the only reason I can charitably think of — is the following: given that they endorse merely a "strong" rather than a "superessentialist" reading of Leibniz — that is, a reading of Leibniz according to which only an individual's intrinsic, rather than relational, properties are essential to it (see fn 39 above) — it follows that, on their view, certain *de re* modal ascriptions (in which the merely possible individual shares all its intrinsic properties with the individual whose modal profile is under consideration) might come out to be "straightforwardly" true after all, while other *de re* modal ascriptions (in which the merely possible individual does *not* share all its intrinsic properties with the individual whose modal profile is under consideration) might turn out to be "straightforwardly" false. However, even if one accepts this strong essentialist interpretation of Leibniz (which I do not), it is nevertheless difficult to see how exactly this ability to read some *de re* modal claims in such straightforward terms relates to the generalism/singularism exegetical debate: for the strong essentialist's ability to read such *de re* modal claims "straightforwardly" would seem to be uncomplicatedly amenable to a generalist (re-)interpretation — namely, that the complete individual concept of the individual in question, which is describable in purely intrinsic, general terms, is instantiated at each world.

let alone grounded in, the totality of general facts.

We have already seen in §1.3 above that Cover & O’Leary-Hawthorne’s own attempted interpretative resolution of Leibniz on the basis of his use of switching considerations not only fails to resolve the original tension that they raise (insofar as weak haecceitism is similarly unable to make sense of merely haecceitistically distinct possible worlds), but also raises further interpretative difficulties of its own (insofar as weak haecceitism as an interpretative thesis is difficult, if not impossible, to square with Leibniz’s “rationalism”, broadly construed). Thus, to the extent that the generalist interpretation is incapable of making sense of Leibniz’s use of switching considerations — and to the extent that one thinks that Leibniz’s use of such switching considerations reveals something significant concerning his considered metaphysics — a draw is all that could, at best, be yielded between weak haecceitism and generalism *qua* (failed) interpretative theses of Leibniz. If Leibniz’s use of switching considerations is to be taken as indicative of something important regarding his metaphysics, then, it is as yet unclear what exactly this important fact is supposed to be. (It isn’t that he was a weak haecceitist.)

As a preliminary remark before resolving the apparent interpretative tension that they raise, it should be noted that Cover & O’Leary-Hawthorne rather exaggerate the role that switching arguments play in Leibniz’s considered philosophy. Thus, they claim (1999, 158) that such switching arguments appear “in the Clarke correspondence and elsewhere,” and in a footnote (fn 19) they go on to cite two (more-or-less) relevant parts of the correspondence,<sup>54</sup> as well

---

<sup>54</sup>The passages they cite correspond roughly to §1-§9 of Leibniz’s fourth letter, and §24-§31 of his fifth letter. To my mind, however, the relevant “switching” argument occurs at §21 in the fifth letter.

as a page just over two paragraphs into the main text of *Primary Truths*.<sup>55</sup> The “elsewhere”, I take it, can therefore only be plausibly construed as referring to the relevant part of *Primary Truths*. But, if one actually looks at this text — and, somewhat bafflingly, as Cover & O’Leary-Hawthorne (1999, Ch 5) themselves are very careful to stress elsewhere in their book — one sees that the argument for the PII broached there proceeds on *completely different* grounds to the argument presented in the correspondence. For in *Primary Truths*, Leibniz argues for the truth of the PII on the basis of his predicate-in-subject theory of truth, rather than by appeal to any switching considerations or God’s choice. Indeed, there is no reference to God or switching considerations at all in the putative derivation. Furthermore, the arguments in *Primary Truths* and the correspondence are importantly different insofar as each of them supports conclusions of distinct modal scope: for while in the correspondence, appeal to God’s freedom to choose which “switched” world to actualise only establishes the *actual truth* of the PII — in order not to give God a problem concerning which world to actualise, all that needs to be the case is that our actual world is not “switchable” in the relevant way — in *Primary Truths* the relevant argument purports to establish the *necessity* of the principle, following as it does from Leibniz’s conception of truth, which is itself presumably necessary.

Thus, the evidence that Cover & O’Leary-Hawthorne actually cite in support of the claim that Leibniz appealed to switching considerations “elsewhere” than in the correspondence is precisely nil. Moreover, and as far as I am aware, Leibniz never in fact appealed to such switching considerations in his arguments for the PII anywhere else in his writings. Thus, Cover & O’Leary-Hawthorne’s

---

<sup>55</sup>The citations are repeated in O’Leary-Hawthorne & Cover (1996, 28, fn 22).

repeated claims that switching considerations constitute Leibniz's "most familiar" way of arguing for the lack of actually distinct indiscernible entities (1999, 159); that Leibniz's defence of the PII "normally" proceeds by appeal to switching considerations (1999, 157, fn 17); and that Leibniz "is not merely willing to use, but indeed generally favours the style of PSR-to-PII argument" wielded in the correspondence (1999, 159, fn 21): all of these claims are, at best, seriously unsubstantiated.<sup>56</sup>

Nevertheless, Cover & O'Leary-Hawthorne's original charge still holds true: Leibniz *did* make use of switching considerations in arguing against absolute space and atoms in the correspondence with Clarke. Cover & O'Leary-Hawthorne infer from this that Leibniz was not, after all, a generalist. But is this the only inference that may be legitimately drawn? Moreover, is the inference itself legitimate?

In my opinion, the answer to both questions is *no*. For consider the following argument (repeated in §1.3 earlier): The presumed existence of intrinsically indiscernible entities (e.g., atoms, or points of space) is required in order for Leibniz's switching arguments to be coherent. Leibniz's arguments are, in fact, coherent. Therefore, Leibniz was committed to the existence of intrinsically indiscernible entities. The argument is clearly invalid: the mere fact that certain features or entities are presumed to exist or obtain for the purposes of some particular argument does not entail that the person formulating the relevant argument is thereby committed to those features or entities. Similarly, then, the mere fact that Leibniz's arguments presume that the singular facts

---

<sup>56</sup>For an in-depth analysis of the multitude of ways in which Leibniz in fact argues for the PII, see Rodriguez-Pereyra (2014).

that obtain at any possible world are not grounded in or even supervene on the general does not entail that he was in actual fact committed to singular facts not being grounded in or supervening on the general. Inferring Leibniz's commitment to the existence of irreducible<sub>1</sub> singular propositions on the basis of his use of arguments which depend for their coherence on the irreducibility<sub>1</sub> (and irreducibility<sub>2</sub>) of such propositions is therefore an illegitimate inference to make: analogous considerations would see Leibniz as committed to the existence of a plethora of entities and substances (atoms, absolute space) which he was emphatically and explicitly not committed to.

Some of what Cover & O'Leary-Hawthorne write, however, would seem to suggest the following (partial) counter-response to the argument just mentioned. As they (1999, 156-7) point out (and as we saw in §1.2.1 above), generalism (or at least certain versions thereof) is in fact perfectly compatible with the existence of intrinsically qualitatively indiscernible entities, including intrinsically qualitatively indiscernible points of space and intrinsically qualitatively indiscernible atoms. (For instance, a Max Black (1952)-type world containing nothing but two perfectly similar individuals existing two miles apart from one another, could be generalistically described as  $\exists x \exists y (x \text{ is a sphere, } y \text{ is a sphere, } x \text{ is two miles apart from } y, \text{ and } x \neq y)$ .) Furthermore, given that on the generalist picture it is simply incoherent to speak of distinct possible worlds which differ solely with regard to a "switch" of atoms or a "shift" of the material universe in space, adopting the generalist picture would have allowed one to "score an easy victory for the enemy in this pivotal battle of the war with Clarke and the Newtonians" (Cover & O'Leary-Hawthorne 1999, 159). That is, Clarke could simply have responded to Leibnizian switching considerations by claiming that

his metaphysics is a thoroughly generalist one — one which crucially allows for the fundamental existence of intrinsically qualitatively indiscernible individuals — and that therefore he is not committed to regarding such “switched” worlds as representing genuinely distinct possibilities. Thus, the question arises: If Leibniz was in fact a generalist, why did he seem to think that the mere commitment to the existence of intrinsically qualitatively indiscernible individuals at a given possible world entails that there is a distinct possible world in which the individuals in question have swapped roles? Why, *unless* Leibniz was thinking like a singularist, did he appear to assume that, purely in virtue of a possible world’s containing such entities, there is a corresponding distinct possible world in which the entities in question are “switched”?

One might legitimately question whether there is a genuine onus on the defender of the Received View to try to answer this question. For at worst, all that a failure to answer it would indicate is that we currently have no good explanation for why Leibniz appeared, mistakenly, to believe that a failure to subscribe to his version of the PII entailed a commitment to singularism — not that Leibniz himself was in actual fact a singularist.

Nevertheless, I think that there are at least two possible, and variously plausible, answers to this question that the defender of the Received View could attempt to provide. The first is that, as already mentioned in footnote 38 above, it was originally *Clarke* who — while arguing for the view that God’s “mere will” can sometimes constitute a sufficient reason for why things are as they are — raises the possibility of “switched” worlds, in his second reply to Leibniz in the correspondence (LC: 20-1). It is, to my mind, not implausible to think that in his later letters Leibniz, seeing that Clarke himself was committed to the

possibility of such “switched” worlds and (hence) a singularist metaphysics, tried to capitalise on this fact in order to demonstrate the truth of the PII, at least in the actual world.

The second — and, perhaps, even more plausible — reason for why Leibniz might well have assumed that singular propositions are required in order to properly describe worlds containing intrinsically indiscernible individuals is that *not* assuming this would entail that he must either assume (i) that external relations (i.e., relations which do not supervene on the intrinsic natures of their *relata*) are capable of grounding the numerical diversity of their *relata*, or (ii) that there obtain relations of primitive numerical distinctness. As previously mentioned in §1.2.1, however, neither assumption is one that Leibniz — nor, as it would seem from what he writes in the correspondence, Clarke — would have seriously considered. Instead, the assumption common to both of them would have been that the numerical diversity of intrinsically qualitatively indiscernible individuals must be ultimately grounded in some primitive, *nonqualitative* feature — some primitive thisness, or haecceity — that the individuals themselves each possess. (And, once this *intra*world criterion for numerical distinctness is in play, it is — conceptually, albeit not philosophically — a short step towards regarding these very features as likewise grounding the primitive *trans*world identification of such individuals.)<sup>57</sup> On this view, then, the fact that Leibniz seemed to think that a commitment to the existence of intrinsically qualitatively indiscernible individuals implies a commitment to a form of singularism is not due to his own construal of the world as being more than purely qualitative in

---

<sup>57</sup>See Adams (1979) for an influential argument to the effect that commitment to the *intra*-world violation of the PII implies commitment to primitive *trans*world identification.

character. It is that, given the kind of additional metaphysical presuppositions that both he and Clarke were inclined to accept (external relations not being sufficient to individuate; non-acceptance of primitive numerical distinctness), a commitment to intrinsically qualitatively indiscernible entities would have necessitated the acceptance of nonqualitative aspects to fundamental reality.

To sum up: Leibniz's use of switching considerations in the correspondence with Clarke does not provide any legitimate grounds for thinking that he conceived of the world in more than purely general terms. The arguments themselves, as they stand, merely purport to demonstrate that the PII, as applied specifically to points of space and atoms, is actually true. That Leibniz himself appeared to think that the existence of intrinsically qualitatively indiscernible individuals implied a commitment to irreducible<sub>1</sub> singular facts is plausibly a result of the fact that the additional philosophical assumptions required to render the existence of such individuals compatible with a generalist metaphysics are ones he would never have accepted.

We should finish this essay by taking a brief step back. For we have yet to mention a much broader point in our discussion so far: a point which, in fact, Cover & O'Leary-Hawthorne appear to completely miss in their analysis of Leibniz's arguments against Clarke in the correspondence. This is that the correspondence with Clarke, in actual fact, occupies a rather special place in the broader body of Leibniz's works.<sup>58</sup> For as many scholars have noted, it is a work of a distinctly *ad hominem* nature, in which Leibniz provides little, if any, insight, into what his considered metaphysics actually is, or indeed into what the actual

---

<sup>58</sup>If only for reasons of degree, if not of kind. As Whipple (2013) and others have noted, throughout his life Leibniz had a significant tendency to employ various "exoteric" or polemical techniques, both in his correspondences and in his published works.

arguments he accepts for adopting such a metaphysics truly are. To quote only from one of the most recent such authors:

[Leibniz's] argument [for the PII in the correspondence with Clarke], at least in the way I interpret it, presupposes that individuals can exist in more than one possible world, that there are, or at least that we can make sense of, indiscernible possible worlds, and that being a certain individual is not being intrinsically thus and so. Has Leibniz changed his mind? Is he contradicting himself? Neither. It has long been recognized that much of Leibniz's argumentation in the correspondence with Clarke has an *ad hominem* character, since Leibniz is granting things he does not believe in order for his arguments to be effective (Broad 1946/1981). Thus that his argument for the Identity of Indiscernibles makes those non-Leibnizian presuppositions is simply another manifestation of the *ad hominem* character of his exchange with Clarke.<sup>59</sup> (Rodriguez-Pereyra 2014, 105)

Not only is it the case, however, that the arguments that Leibniz appeals to in the correspondence are of a distinctively *ad hominem* character, but a number of scholars have provided several (to my mind) plausible, and variously related, explanations for why the arguments in the correspondence took the theological, PSR-driven turn that they in fact did take. To briefly, and brutally, summarise:

---

<sup>59</sup>As far as I am aware, this fact has been (almost!) universally appreciated since Russell's (1900, 57-8) classic study. See also, e.g., Broad (1946/1981) and Parkinson (1965, 132-4), both of whom argue that Leibniz's apparent ambiguousness in the correspondence on the issue of whether the PII is necessarily or merely actually true reveals the distinctively *ad hominem* nature of the dialectic. Brown (2004, 283) has also recently noted that in a letter written to Johann Bernouilli during the correspondence Leibniz refers to his "encounter" with the Newtonians as being a "game and amusement" to him, a statement which (Brown writes) "could nag a suspicious mind into doubts about Leibniz's sincerity [in the correspondence]."

Brown (2004) has provided a compelling case, constructed mainly from letters sent by Leibniz to Princess Caroline (the correspondence's intermediary), that Leibniz was led to place extreme emphasis on theological considerations in the correspondence because he thought that it was the best way of arresting Caroline's conversion to the Newtonian world-view during her residency in England (not an implausible thought, given the Princess' professed love for his *Theodicy*). Similarly, Rodriguez-Pereyra (2014, 167-8) has hypothesised that the reason why Leibniz chose not to employ what he calls the "Direct Argument" against absolute space — *viz.*, if space were absolute, then some of its parts would be perfectly similar; but there cannot be perfectly similar things; therefore, space is not absolute<sup>60</sup> — is due to its inability to imply a reduced conception of God, something which (Rodriguez-Pereyra claims) was one of Leibniz's primary philosophical goals in the correspondence. And Vailati (1997, 122-3) has argued that Leibniz was compelled to make substantial use of the PSR against Clarke, and was furthermore initially confident that it would wield considerable dialectical force against him, in large part because Clarke had himself made substantial use of a (causal) version of the principle himself in his work *A Demonstration*, published some years earlier in 1705.

I do not wish to endorse here any specific thesis concerning why Leibniz chose to appeal to switching considerations in the correspondence with Clarke. Nevertheless, I think it is reasonably clear why Leibniz did *not* attempt to argue for the PII in the correspondence as he had done in some of his previous work, in which (as we have seen) he attempted to derive the principle from his views

---

<sup>60</sup>As Rodriguez-Pereyra (2014, 160) notes, this is an argument which appears explicitly in the *New Essays* against — among other putative entities — atoms and absolute space (NE: 57-8).

of truth and complete individual substances: namely, because these arguments depend on premises that Clarke, arch-empiricist, would have had no inclination to accept. At best, then, their dialectical effectiveness would have been nil; at worst, they would have led to a protracted discussion of the general plausibility of *Leibniz's* metaphysics, and would have heavily side-tracked (and most probably would have had a severely detrimental effect upon) Leibniz's primary goal of refuting the *Newtonian* world-view.<sup>61</sup>

It is high-time we concluded our lengthy discussion. The main point that I wish to close on, then, is this: there is, in actual fact, an almost universal scholarly consensus to the effect that Leibniz's use of switching considerations in the correspondence with Clarke is a means to a specific dialectical end. That is, such considerations are primarily tools utilised so as to try to *refute* Clarke's and the Newtonians' position regarding atoms and absolute space; they do not, *pace* Cover & O'Leary-Hawthorne, in and of themselves demonstrate anything of deep or fundamental significance regarding Leibniz's considered metaphysics, or the reasons that he himself had for believing in it.

---

<sup>61</sup>It is also worth noting that Leibniz had other arguments against the existence of absolute space and atoms that he could have wielded (but apparently opted not to wield) in the correspondence with Clarke. Thus, for instance, in the *New Essays* (NE: 158) he had previously argued against the existence of absolute space on the basis of its being "an infinite whole made up of parts," something which, he claimed, "implies a contradiction" (Vailati 1997, 122). And in *Primary Truths* (P: 91), Leibniz appears to argue against the existence of atoms on the basis of their incapability of expressing or "mirroring" the state of the entire universe, something which his monads were supposedly capable of doing. Why he chose not to employ these arguments (in addition to those which appeal to the PSR) against Clarke is an interesting question, though answering it is unfortunately beyond our remit here.

## 1.5 Conclusion

In this chapter, I have presented and criticised Cover & O’Leary-Hawthorne’s (1999) attempt to overturn the Received View, according to which Leibniz is said to conceive of the world in purely qualitative terms (“generalism”). They argue that Leibniz’s apparent willingness to construe *de re* modal claims at face value, coupled with his use of “switching” considerations in the correspondence with Clarke, strongly indicate that Leibniz was not, in fact, a generalist, but rather conceived of worlds (actual and possible) as being something over and above their respective qualitative characters. I have argued (i) that Cover & O’Leary-Hawthorne’s own attempted interpretative reconciliation of Leibniz’s philosophy is problematic, and (ii) that the evidence they draw upon in support of their claim that Leibniz was not a generalist is not philosophically or textually convincing, and fails in particular to take into account the distinctively *ad hominem* character of the correspondence. I therefore conclude that, in fact, there is little evidence to show that the Received View of Leibniz construed as a generalist is not correct, and that the interpretative orthodoxy thus survives Cover & O’Leary-Hawthorne’s attack very much unscathed.

## Chapter 2

# Symmetry and Qualitativity

In this chapter I wish to examine the relationship between two notions whose close connection has often been remarked upon in the philosophy of physics literature but which as yet has escaped any detailed analysis. The first notion — one of central concern to many areas of physical enquiry — is *symmetry*, where (to a first approximation) a symmetry is a transformation which preserves the space of solutions of a given theory. The second notion — one of comparably central concern to many areas of *metaphysical* enquiry — is *qualitativity*, where (again to a first approximation) a qualitative property is one which is not essentially “individual-involving” in any respect. Defined in this rough-and-ready manner, the existence of any kind of interesting relationship between these notions might appear surprising. After all, if a symmetry is just a transformation which maps solutions of a theory to solutions, then little would seem to stand in the way of our labelling “symmetries” transformations which map solutions to solutions whose qualitative characters are distinct, and similarly not much would seem to stand in the way of our also labelling “symmetries” other transformations

which map solutions to solutions whose qualitative characters are identical.<sup>1</sup> Whence, then, the relevant connection between these two notions?

The connection — at least according to one influential school of thought — begins to come into much clearer focus when one attempts to construe symmetries in a more specific sort of way: namely, as *tools of metaphysical inference*. On this view, symmetries are no longer construed as arbitrary mappings of solutions to solutions of a given theory. Rather, symmetries are taken to map only those solutions to each other that are in agreement with regard to the genuine metaphysical structure they ascribe to the concrete world. In other words — and assuming for argument's sake that the theory in question is in fact true, or at least approximately true — whatever is left *invariant* under a given theory's symmetries is taken to represent structure that is genuinely real; conversely, whatever is *variant*, or varies, under a given theory's symmetries is not taken to represent anything genuinely real: rather, such variant structure is construed as being “surplus”, “redundant”, not real — a mere mathematical artefact of our theory, nothing more.

It will be useful for our purposes to give this particular view concerning symmetries — *viz.*, that only the structure left invariant under them is genuinely real — a name: adopting Saunders' (2007) useful coinage, then, let us label it the *Invariance Principle* (IP).<sup>2</sup> Now, one immediate difficulty with understanding symmetries in this way is that of specifying *which* particular transformations among all possible solution space-preserving ones in fact relate solutions which

---

<sup>1</sup>Here and below, when I speak of solutions' “qualitative characters” what I mean more specifically are the qualitative characters solutions *represent* the (concrete) world as possibly having: we do not want to claim that two solutions are “qualitatively distinct” purely in virtue of the fact that they are distinct *mathematical* entities. Cf. Lewis (1986, 224-5).

<sup>2</sup>Cf. Dasgupta's (forthcoming, a) “symmetry-to-reality”-type reasoning.

agree on all such “genuine structure”.<sup>3</sup> It is here that the notion of symmetry (putatively) makes contact with the notion of qualitativity. Symmetries, it is claimed, should be taken to relate — and only relate — those solutions that are *qualitatively indiscernible*, the corollary being that any such solutions’ variant *nonqualitative* structure is thereby *not real*. So understood, then, symmetries become razors which may be wielded in order to excise the variant nonqualitative structure that our theories are reputed to ascribe to the concrete world.

Such an assessment of the metaphysical implications of symmetries — viz., that they only ever demonstrate the superfluousness of nonqualitative structure, and that we are justified in excising such structure from our general metaphysical framework — is seemingly so widely accepted in the contemporary philosophical literature that it would not, in my view, be much of an overstatement to label it the *Received View*.<sup>4</sup> As I shall argue in this paper, however, the Received View is not only straightforwardly false, but it is also seriously naïve. It is straightforwardly false because as a matter of plain fact symmetries do sometimes reveal the superfluousness of specific kinds of variant *qualitative* structure (see §2.2); and it is seriously naïve because there are plausible reasons to suspect that symmetries can *never*, in fact, reveal the superfluousness of nonqualitative structure (see §2.3). In addition to this discussion, I shall also consider (in §2.4) a recent alternative (and slightly idiosyncratic) construal of the notion of “qualitativity” that is — it is claimed — relevant to drawing IP-based metaphysical inferences,

---

<sup>3</sup>Neither Saunders nor Dasgupta, of course, sign up to the view according to which symmetries are properly construed as arbitrary mappings among the space of solutions of a given theory. Rather, they each have a clearly restricted notion of “symmetry” in mind in their respective papers. (They appear to differ, however, on the issue of how it is exactly that the notion is to be properly restricted.)

<sup>4</sup>I should mention that one notable recent dissenter from the Received View is Pooley (2013), whose overall line of argument is very much in tune with this paper’s.

namely that due to Ismael & van Fraassen (2003). In particular, I shall argue that although their account is far from perfect, it is nevertheless much closer to the truth than the Received View is, and moreover does a far better job in providing an adequate philosophical and justificatory account of the IP. The paper will end, then, with my attempt to sketch an outline of what I take such a correct account to be, one which I think improves on but nevertheless owes much to Ismael & van Fraassen's own account.

Before we begin all this, however, we would do well to examine the relevant notions of symmetry and qualitativity, as well as their supposed connection, in a bit more detail. This we do in the next section.

## 2.1 More on Symmetry and Qualitativity

### 2.1.1 Symmetry

In what follows it will be helpful — if only for illustrative purposes — to construe theories “semantically”: that is, as specifying from a set  $\mathcal{K}$  of kinematically or “metaphysically” possible models a subset  $\mathcal{S}$  of dynamically or “physically” possible models.  $\mathcal{K}$  should thus be taken to represent the set of all possibilities (or “possible worlds”) consistent with the posited basic ontology of the theory, while  $\mathcal{S}$  should be construed as a particular subset of these possibilities, namely those also consistent with the theory's laws.

One quite natural way of construing what a symmetry is on this view is to take it to be any map  $T$  from  $\mathcal{K}$  to itself that preserves  $\mathcal{S}$ : more formally and precisely, and where  $\mathbf{m}$  is an arbitrary model in  $\mathcal{K}$ , a *symmetry* is said to be any

one-to-one and onto map  $T : \mathbf{m} \in \mathcal{K} \mapsto T(\mathbf{m}) \in \mathcal{K}$  such that  $\mathbf{m} \in \mathcal{S}$  iff  $T(\mathbf{m}) \in \mathcal{S}$ . (This is basically a precisified version of the notion of “symmetry” as approximately defined at the beginning of the previous section.) As several authors have noted, however, such a notion of symmetry, at least when combined with the IP, quickly leads to theoretical disaster.<sup>5</sup> For, on this view, *arbitrary* permutations of  $\mathcal{S}$  will count as a symmetry of any given theory. Thus, for instance, in Newtonian Gravitation Theory (“NGT”) — i.e., the theory comprising Newton’s three laws, plus his inverse gravitational square law, governing the behaviour of point-particles in Newtonian spacetime — models putatively representing  $n$  number of particles moving in such-and-such a dynamically possible fashion will be mapped under some symmetry transformation to dynamically possible models of arbitrary particle cardinality. Particle cardinality, then, would not appear to constitute a genuinely real feature of a solution of NGT according to this notion of symmetry: a seemingly unacceptable conclusion, given that particle number is arguably a canonical example of a “genuine feature” of solutions of NGT. (Indeed, it is a conclusion particularly unpalatable for those Newtonians who would have liked to believe that some material entities actually exist!)

The question naturally arises, then, as to whether we can devise a *restricted* notion of “symmetry” such that arbitrary models in  $\mathcal{S}$  are not mapped onto one another: a notion which, in addition, yields correct metaphysical inferences regarding the reality or otherwise of various putative physical quantities. As Belot (2013) has recently emphasised, however, attempting to work out what the correct, and suitably restricted, formal definition of “symmetry” is to combine with the IP is no easy task. As he points out, many of the formal notions of

---

<sup>5</sup>See, e.g., Belot (2003, 402; 2013, §3); Ismael & van Fraassen (2003, 378-9).

“symmetry” used by practising physicists are quite unsuited to the general task of providing IP-based metaphysical inferences. For instance, identifying the relevant symmetries of one’s theory as its Hamiltonian symmetries<sup>6</sup> plausibly does not give us what we want: among other things, it would seem to rule out Galilean boosts as counting as genuine symmetries of NGT (boosting a system will normally fail to preserve the Hamiltonian in virtue of its altering the system’s kinetic energy).<sup>7</sup> Our focus here, however, is on quite a different issue, one that arguably remains largely implicit in Belot’s paper: namely, in virtue of what, precisely, do we decide that a particular formal notion of symmetry is, or is not, yielding “correct” metaphysical inferences? What, in other words, are the relevant *criteria* by which we decide whether or not a particular set of transformations on  $\mathcal{S}$  is giving us reasonable IP-based metaphysical conclusions? Why, for example, do we think that the *purely formal* notion of a theory’s Hamiltonian symmetries are giving us *philosophically* the wrong result, when combined with the IP? Why do we *want* to include boosts, as well as translations and rotations, among the relevant  $\mathcal{S}$ -preserving transformations on NGT’s space of kinematically possible models?<sup>8</sup>

---

<sup>6</sup>Very briefly, one determines the Hamiltonian symmetries of a given theory by (i) giving the theory the canonical Hamiltonian (“phase space”) treatment and (ii) identifying the relevant symmetries as those diffeomorphisms on phase space that preserve both the symplectic form (which encodes the geometric structure of the phase space) and the Hamiltonian function (which assigns to each point of phase space the total energy of the relevant physical state).

<sup>7</sup>See Belot (2013, §6) for further discussion of the pitfalls of defining the relevant symmetries in this way, and for reasons why other such formal notions are similarly problematic.

<sup>8</sup>From what Belot (2013, 329-33) writes in his paper it might seem that he would answer the question of how it is that we determine whether a particular formal definition of symmetry is philosophically satisfactory or not by appealing to something like *intuition*, or perhaps the working intuition of the mathematician, physicist, or philosopher of physics: after all, it certainly does seem intuitive that in the case of NGT the relevant symmetries should include boosts, but not mappings of solutions of (say) different particle cardinality! However, unless we have good reason to believe that all theorists’ intuitions will cohere in all cases, and unless we think that we are somehow justified in taking such intuitive judgements as being epistemically privileged

For many theorists, it would seem that the answer to this question is straightforward: a given formal definition of symmetry is adequate if it relates all and only those solutions to one another that are *qualitatively indiscernible*. Thus, for instance, Rickles (2008, 4) writes that “It is a certain class of symmetry that I am interested in: those that preserve all *qualitative* structure of a model or world....” Similarly, Thébault (2012, 814) affirms that the antihaecceitist — that is, someone who “denies the possibility of non-qualitative determinants of cross-identification” of objects in different worlds, and who denies that there are any worlds that differ “only with regard to which objects play which role[s]” — is invariably able to deny that “models related by symmetry transformations are [representationally] distinct”: the obvious implication being that the relevant symmetry transformations therefore relate models that differ at most nonqualitatively. And Belot (2003, 394) himself claims that “[o]bjects [including possible worlds] related by a symmetry occupy identical roles in the pattern of relations described by their structure ... so objects related by symmetries will be qualitatively indistinguishable.”<sup>9</sup> According to these theorists, then, it would seem that it is the metaphysical notion of qualitative indiscernibility (or discernibility) which provides the basis or grounds for any claims to the effect that a particular

---

or beyond suspicion, this would only appear to push the core issue one step further back: for on what grounds do these theorists make the intuitive judgements that they do — assuming, of course, that they are generally correct judgements?

<sup>9</sup>For further seeming endorsements of this view, see, e.g., Belot (2001), Pniower (2005, 134), and Saunders (2003a; 2013, 356). (Baker (2010) seems to express some sympathy for the view, though officially his own view is different.) It should perhaps be noted that in more recent work (Belot 2013) Belot has refrained from using the word “qualitative” at all in connection with the notion of symmetry. However, given that in this later paper Belot does not provide any *other* kind of principled basis on which to determine when and on what basis symmetries can be guides to superfluous structure (see fn 8 above), this leaves us pretty much back where we started — after all, if it isn’t the qualitative indiscernibility of solutions which allows us to identify them as representationally equivalent, then what is the relevant feature that does?

formal notion of symmetry is yielding correct (or incorrect) IP-based inferences. That is, to the extent that any given formal notion of symmetry yields correct IP-based metaphysical inferences, then it relates and only relates qualitatively indiscernible solutions; but to the extent that any such formal notion does not yield correct IP-based metaphysical inferences, then it fails to relate and only relate qualitatively indiscernible solutions.

But are things really so simple? Is it really the case that the restriction to qualitative character-preserving transformations gives us the principled basis that we want, insofar as it would invariably allow us to draw legitimate IP-based inferences — specifically, inferences to the effect that only the variant nonqualitative structure of our (best) theories is not real? Before we turn to examine this question more closely, it will be helpful if we first attempt to explicate the notion of “qualitativity” in some more detail, if only to aid ourselves in getting a better handle on what the view just expressed properly amounts to.

### 2.1.2 Qualitativity

The idea that the world is in some sense fundamentally “qualitative” in character has fairly deep historical roots.<sup>10</sup> It is also a doctrine that is notoriously difficult to state to any great degree of precision. Ever since Adams’ (1979) classic discussion of the distinction between the qualitative and the nonqualitative, however, a “linguistic approach” to stating the view has become quite popular in the literature. Imagining for simplicity that standard predicate logic (with the

---

<sup>10</sup>Adams (1979, §2) has argued that the view itself dates back at least as far as Leibniz, although Cover & O’Leary-Hawthorne (1999) have more recently disputed whether Leibniz was in fact a “generalist” in any straightforward sense. I reply to Cover & O’Leary-Hawthorne’s claims in Chapter 1 of this thesis.

identity predicate taken as primitive)<sup>11</sup> contains all of the expressive resources required in order for us to clearly and perspicuously describe the actual world (thus we are ignoring, e.g., plural, higher-order, and other more complicated linguistic resources), we can define a *general sentence* of PL to be any sentence that neither (i) contains any proper names nor (ii) is constructed using any essentially “individual-involving” predicates, such as *pegasizes*. Correspondingly, a *singular sentence* of PL is simply any sentence that is not general, i.e., which does contain proper names and/or individual-involving predicates. Thus, “ $\exists xFx$ ” is a general sentence of PL — provided that *F* is a predicate that isn’t in any way “individual-involving” — for it doesn’t explicitly refer to any particular individual, while “*Fa*” is a singular sentence of PL, for it does explicitly refer to some particular individual, namely *a*.

The *generalist* will then go on to claim that only general (or “qualitative”) sentences (expressing general propositions) are required in order to correctly and perspicuously describe the fundamental structure of the world.<sup>12</sup> The *singularist*, on the other hand, will disagree: for him, singular sentences (expressing singular propositions) are also required. The notion of “perspicuous description” is again one that is notoriously difficult to make precise, but nevertheless it can plausibly be elucidated by example. Thus, for instance, just as the statement “There is a table” could never amount to a perspicuous description of a world in

---

<sup>11</sup>Although note that on Saunders’ (2003a) version of generalism the identity predicate is taken to be a defined rather than an ideologically primitive entity.

<sup>12</sup>The view goes by a variety of names in the literature, although “generalism” seems to be the term favoured by most theorists. See, e.g., Cover & O’Leary-Hawthorne (1999), Saunders (2003a), Maunu (2005), Dasgupta (2009), Pooley (MS), and Russell (MS). Compare also van Fraassen’s (1991) “semantic universalism”, Fine’s (2005) “metaphysical anti-haecceitism”, Kment’s (2012) “anti-individualism”, and Dasgupta’s (2014) “qualitativism”. The view also bears obvious resemblances to various “ontic” forms of structural realism (see, e.g., Ladyman & Ross 2007), although I shall not pursue the connection between these views here.

which mereological composition never occurred (i.e., in which, strictly speaking, there *aren't* any tables), so “Nicolas Cage is balding” would not amount — indeed, could not amount, given that it expresses a singular proposition — to a perspicuous description of a world which was ultimately purely qualitative in character.<sup>13</sup> Some propositions, to put the point more figuratively, “correspond to” or “limn” the world’s structure. The disagreement between the generalist and the singularist ultimately boils down to which propositions, general or singular, are required in order to properly “limn” such structure in just this way. The generalist will claim that only general sentences or propositions are required in order to carry out such “limning”. The singularist will deny this.<sup>14</sup>

I shall be assuming in what follows that the qualitative/nonqualitative distinction is at least clear enough for us to work with. With the distinction at our disposal, we can now state the two propositions whose viability we shall be scrutinising for the majority of this chapter, and whose conjunction I take to comprise the core of the Received View on symmetries:

- (P1) Symmetries — insofar as they are guides to “surplus” theoretical structure — only ever relate models which differ at most nonqualitatively.
- (P2) The world is fundamentally purely qualitative, or “general”, in char-

<sup>13</sup>For more on the relevant notion of perspicuity, see O’Leary-Hawthorne & Cortens (1995, 154-7). For a related discussion, see Sider (2011, §2). More physics-based examples of descriptive perspicuity might include claims regarding the “absolute” simultaneity of two distinct events in a relativistic world exhibiting a Minkowskian spatiotemporal structure, or claims regarding one’s “absolute” velocity in a world with (say) a Galilean spatiotemporal structure. Such claims might be true in a “contextual” or “loose” or “non-literal” sense; but, the thought is, they could not be true *simpliciter*.

<sup>14</sup>Of course, it almost goes without saying that no generalist or singularist would want to sign up to the view just stated in this precise form. After all, no one seriously believes that PL is a language anywhere near expressive enough to capture facts about our actual world! The hope, however, is that presenting the views in this “linguistic” way captures *something* about the views, if only their broad philosophical “flavour”.

acter.<sup>15</sup>

According to the Received View, then, symmetries are an ontological razor: a razor which, more specifically, may be wielded to excise the variant nonqualitative structure that our theories are (at least putatively) committed to.

Though much of our discussion in what follows will primarily focus on (P1), it might be useful at the outset to say just a few words on (P2), and its apparent relationship to (P1). For, on reflection, there would appear to be a rather curious tension that arises for those who subscribe to both propositions. That is, if we already believed that the world is fundamentally purely qualitative in character, of what precise *use* are symmetries in telling us about the nonreality of various nonqualitative structure that our theories purport to ascribe to the concrete world, given that — *ex hypothesi* — we would *already know* that the world fundamentally contains no nonqualitative elements? In other words, for the theorist who subscribes to both (P1) and (P2), it would seem that symmetries could at best only confirm the nonreality of structures that she antecedently took not to be real. (Cf. Dasgupta forthcoming, *a*, §5.3.)

There is also a more obvious question to consider, namely: What is our justification for believing (P2) in the first place? What, in other words, are our grounds for believing that the world is fundamentally general rather than singular — grounds which, in turn, are meant to legitimise our excision of the nonqualitative structure that our theories are putatively committed to?

As far as I am aware, there have been three explicit, and variously inter-

---

<sup>15</sup>It is worth noting the logical distinctness of the two propositions: one could coherently believe in both, neither, or one or the other of them. However, as the view expressed — albeit largely implicitly — by philosophers who work on symmetries generally seems to include a commitment to the truth of both, it is their conjunction that we shall focus on here.

related, arguments in the literature which actually purport to argue independently for (P2). Thus, Saunders (2003a) has attempted to derive<sup>16</sup> generalism from modern-day versions of “Leibniz’s Principles”, namely the Principle of Sufficient Reason and the Principle of the Identity of Indiscernibles; Morganti (2008) has claimed that “the best (if not the only)” way of arguing for generalism, one which he does not in fact ultimately endorse, is to appeal to a modern-day version of Bertrand Russell’s Principle of Acquaintance; and Dasgupta (2009) has adopted a generalist metaphysics on the basis of a generalised symmetry argument involving the permutation of individuals: an argument which, at its core, was explicitly formulated by Clarke and whose conclusion was likewise explicitly endorsed by Leibniz in their correspondence.<sup>17</sup> I shall not comment on the individual merits of each such argument here (although we shall touch on a version of Dasgupta’s in §2.3.1 below). All I wish to emphasise here is the rather trivial point that the claim that the world fundamentally possesses no nonqualitative structure cannot go without saying, but must be explicitly *argued for*. And if the response to this (and the previous) challenge is to claim that it is the (canonical) *symmetries* of our best physical theories that (in conjunction with the IP) ultimately justify our belief in the purely qualitative structure of the world (i.e., that (P1) justifies our belief in (P2)), then the reason *why* symmetries might be legitimately wielded in this fashion — in other words, why it is that we should think that (P1) and the IP are true in the first place — will stand in similar, non-trivial need of justification.

Before we close this subsection one further comment regarding (P2) should

---

<sup>16</sup>The turn of phrase Saunders actually uses is “put in place”.

<sup>17</sup>See Alexander (1956, ed., esp. 20-1, 30-1, 36). The argument found in Wilson (1959) is also plausibly made in much the same spirit.

be made. For while it is indeed true that there exists a body of related arguments in the literature which aims to establish the generalist picture, it is also important to note that there exists an important body of work (e.g., Adams 1979; Kment 2012) which pulls in the opposite direction — that is, which attempts to *refute* the generalist picture. The rough idea behind these arguments is that, should the generalist accept the possibility of worlds which exhibit a certain “symmetry-breaking” property — e.g., a Blackian world (cf. Black 1952) containing nothing except two spheres, two miles apart from one another, with one of them destined to be annihilated at time  $t = 13$  minutes — then she will struggle to make sense of certain *de re* possibility claims (as in Adams’ 1979 argument) and/or will struggle to provide a decent account of counterfactual and probabilistic discourse in such worlds (as in Kment’s 2012 argument). I draw attention to these arguments not because I believe that they refute the generalist picture. (I don’t believe they do.) Rather, I mention them because I believe it is crucial to stress the perhaps obvious point that, insofar as we believe that the world is qualitative and that symmetries drawn from physics are supposed to provide some support for this belief, we would also do well to examine just what exactly the belief in the fundamentally qualitative nature of the world ultimately commits us to, and indeed whether in fact such commitments are ones that, all things considered, we might prefer to do without.

## 2.2 Symmetries Do Not Only Relate Qualitatively Indiscernible Solutions

The view that surplus structure-indicating symmetries only relate solutions that are qualitatively indiscernible (*viz.*, (P1) above) is not a *prima facie* implausible one. Below are three examples of symmetries — one drawn from quantum mechanics, one drawn from classical Newtonian physics, and the other from general relativity — all of which seem to provide support for this proposition.

**Ex. 1: Permutation symmetry in quantum mechanics.** In the labelled tensor product Hilbert space formalism of ordinary quantum theory, if we apply the unitary permutation operator  $\mathbf{P}$  corresponding to the “exchange” of indistinguishable particles 1 and 2 in a two-particle quantum mechanical state  $\Psi_{12}$  such that  $\mathbf{P}\Psi_{12} = (-)\Psi_{21}$  and  $\mathbf{P}\Psi_{21} = (-)\Psi_{12}$ , then it is true that the quantum state will be left qualitatively invariant under the action of the permutation group. Indeed, the fact that the quantum state is left qualitatively invariant under such a permutation follows trivially, for according to the quantum formalism itself,  $\Psi_{12} = (-)\Psi_{21}$ : the states themselves are equivalent according to the very formalism of the theory (up to a phase), and hence cannot be used to represent states that differ *in any way at all*, qualitatively or otherwise.<sup>18</sup>

**Ex. 2: Translational/rotational symmetry in Newtonian gravity.** Taking a generic (“coordinate-free”) model of Newtonian gravitation theory set in New-

---

<sup>18</sup>Another way to put this point is to note that permutations of the particle labels in quantum mechanics correspond not just to symmetries of the *theory* — in the sense that they map solutions of the theory to solutions — but are also symmetries of the *solutions themselves* — in the sense that they act as the *identity* on solutions of the theory (up to a phase factor of -1 in the case of fermions, if the number of permutations is odd). For further discussion of this point, see Pooley (2006, §4.7).

tonian spacetime to be of the form  $\mathcal{M} = \langle M, t_a, h^{ab}, \sigma^a, \Phi_i \rangle$  (where  $M$  is a differentiable 4-dimensional manifold, and  $t_a, h^{ab}, \sigma^a$ , and  $\Phi_i$  encode the temporal structure, the Euclidean structure of instantaneous 3-space, the inertial “rigging” structure of absolute space, and the mass content of the spacetime respectively)<sup>19</sup> and applying the appropriate diffeomorphism<sup>20</sup>  $d$  to yield a new model  $\mathcal{M}_{static} = \langle M, t_a, h^{ab}, \sigma^a, d^*\Phi_i \rangle$ , then (on the stipulation that each point of  $M$  represents the very same point of physical spacetime as it occurs in both  $\mathcal{M}$  and  $\mathcal{M}_{static}$ )<sup>21</sup> the two worlds represented will differ at most nonqualitatively. More specifically, they will differ at most with regard to which *particular* points of the spacetime manifold are underlying various parts of the matter fields.

**Ex. 3: Diffeomorphism invariance in general relativity.** Taking a generic model of general relativity (GR) to be of the form  $\mathcal{M} = \langle M, g_{ab}, T_{ab} \rangle$  and applying an arbitrary diffeomorphism  $d$  to yield a new model  $\mathcal{M}_{diff} = \langle M, d^*g_{ab}, d^*T_{ab} \rangle$  (where  $M$  is again a differentiable 4-dimensional manifold,  $g_{ab}$  is the metric tensor, and  $T_{ab}$  is the stress-energy tensor which, roughly speaking, represents the model’s matter content<sup>22</sup>), then the two worlds represented will again (under the exact same representational stipulation as before) differ at most nonqualita-

<sup>19</sup>See Pooley (2013) and references therein for further details on the model-theoretic treatment of NGT and other spacetime theories.

<sup>20</sup>That is, a diffeomorphism which is both a dynamical and a spacetime symmetry of the theory, as explicated by Earman (1989, §3.4). To see what this means exactly, begin by taking a model of some generic spacetime theory  $T$  to be  $\mathcal{M} = \langle M, A_1, A_2, \dots, P_1, P_2, \dots \rangle$ , where  $M$  is a four-dimensional differentiable manifold,  $A_i$  are the geometric-object fields on  $M$  characterising the structure of spacetime, and  $P_j$  are geometric-object fields on  $M$  characterising the model’s matter contents. A *spacetime symmetry*,  $\Psi$ , is defined as a diffeomorphism (i.e., a bijection from  $M$  to  $M$  such that both it and its inverse are differentiable) such that  $\Psi^*A_i = A_i$  for all  $i$ . A *dynamical symmetry*,  $\Upsilon$ , of a given spacetime theory  $T$ , on the other hand, is defined as a diffeomorphism such that, if  $\mathcal{M} = \langle M, A_1, A_2, \dots, P_1, P_2, \dots \rangle$  is a model of  $T$ , then  $\mathcal{M}_\Upsilon = \langle M, A_1, A_2, \dots, \Upsilon^*P_1, \Upsilon^*P_2, \dots \rangle$  is also a model of  $T$ .

<sup>21</sup>This qualification cannot go without saying; see Pooley (2006, 102-3).

<sup>22</sup>Strictly speaking, the matter content of GR is represented by other fields in terms of which  $T_{ab}$  is defined.

tively. That is, they will differ at most with regard to which *particular* points of the spacetime manifold are underlying various parts of the metric and matter fields.

In addition to all of these examples of symmetries relating qualitatively indiscernible states of affairs and thus putatively revealing the “superfluosness” of various nonqualitative structures — “labelled” quantum particles; points of space; and points of spacetime respectively<sup>23</sup> — there also appear to be several important cases of symmetries relating models that are *not* qualitatively indiscernible, thereby revealing the “superfluosness” of various *qualitative* structures as well. I list three such counter-examples here.

**C-Ex. 1: “Galilean boost” symmetry in Newtonian gravity.** Taking again our model of Newtonian gravitation theory set in Newtonian spacetime to be  $\mathcal{M} = \langle M, t_a, h^{ab}, \sigma^a, \Phi_i \rangle$ , then applying an appropriate diffeomorphism<sup>24</sup>  $d$  (corresponding to a “velocity boost”) to  $\mathcal{M}$  will invariably yield a new model  $\mathcal{M}_{kin} = \langle M, t_a, h^{ab}, \sigma^a, d^*\Phi_i \rangle$  such that the two models will straightforwardly represent worlds that *do* differ qualitatively.  $\mathcal{M}$  might, for instance, represent a world in which the matter content is entirely stationary with respect to absolute space, while  $\mathcal{M}_{kin}$  might represent a world in which the matter content is moving uniformly with a constant absolute velocity. These are, I take it, worlds which in no conceivable way differ merely nonqualitatively, given how the qualitative-nonqualitative distinction is normally understood: the worlds differ more than merely with regard to which particular objects are “playing which qualitative

<sup>23</sup>For recent endorsements of the stated metaphysical implications of each of these symmetries, see, e.g., French (2014), Russell (2014), and Dasgupta (2011) respectively.

<sup>24</sup>That is, a dynamical symmetry which is *not* also a spacetime symmetry; see fn 20 above.

roles”.<sup>25</sup> Furthermore, this boost symmetry is arguably a canonical example of a symmetry that is “surplus structure-revealing” in the sense defined above, insofar as the existence of this symmetry is what is commonly taken to justify the reconceptualisation of Newtonian absolute space so as to excise the “absolute” rigging field  $\sigma^a$  and its replacement with an affine (“straightness”) connection  $\nabla$  as a primitive piece of ideology, in the move from a Newtonian gravitation theory set in Newtonian spacetime to an ideologically sparser but empirically equivalent theory set in Galilean spacetime.

**C-Ex. 2: “Dynamical boost”<sup>26</sup> symmetry in Newtonian gravity.** Taking  $\mathcal{M} = \langle M, t_a, h^{ab}, \nabla, \rho, \phi \rangle$  to be a solution of Newtonian gravitation theory now set in Galilean spacetime (with  $\rho$  and  $\phi$  representing the matter density and the gravitational potential field respectively), then for a diffeomorphism  $d$  applied to  $\mathcal{M}$  corresponding to an element of the so-called “Maxwell group” of transformations and for which the gravitational potential field is appropriately transformed, one is able to yield a new dynamically possible model of Newtonian gravitation theory set in Galilean spacetime,  $\mathcal{M}_{dyn} = \langle M, t_a, h^{ab}, \nabla, d^* \rho, \phi' \rangle$ . By wide agreement, what this symmetry is supposed to indicate is that (at least in a materially infinite universe) the laws of the theory plus the global matter distribution  $\rho$  underdetermine the exact combination of the inertial structure and the gravitational force, rendering a given observer incapable in principle of

---

<sup>25</sup>Dasgupta (2015, 608) also makes this point. Sklar (1974, 178-9), Baker (2010, 1160), and Saunders (2013, 356) all appear to think that it is plausible to regard both shifted and boosted solutions in NGT as qualitatively indiscernible. (Although Sklar admits that he is also suspicious whether there is a coherent qualitative-nonqualitative distinction to be drawn to begin with.) My own suspicion is that Sklar, Baker, and Saunders are all led to think this because they fail to realise that transtemporal facts (specifically involving the persistence of points of space through time) might serve to ground boosted worlds’ qualitative discernibility.

<sup>26</sup>I draw the term “dynamical boost” loosely from Huggett (1999, 166-7), who labels it the “dynamic shift”.

determining whether she is moving in a force-free inertial manner or whether she is being accelerated under a gravitational force. This difference is also qualitatively specifiable: the difference between a model which represents an observer moving in a force-free inertial manner and a model which represents an observer being accelerated under a gravitational force is one that can be perfectly well articulated in general or non-individual-involving terms. Moreover, it is this so-called “gauge-redundancy” of Newtonian gravitation theory which is widely thought to motivate the “geometrisation” of Newtonian gravity and consequent reconceptualisation of the theory’s posited qualitative structure by moving to a theory of Newtonian gravity set in *Newton-Cartan* spacetime. Here, in brief, the flat inertial connection  $\nabla$  is replaced by a new kind of *dynamical* inertial structure  $\nabla^{NC}$ , with models of the form  $\mathcal{M}_{NC} = \langle M, t_a, h^{ab}, \nabla^{NC}, \rho \rangle$ . Up to isomorphism, the two models  $\mathcal{M}$  and  $\mathcal{M}_{dyn}$  described in this paragraph correspond to a unique model of a Newtonian gravity geometrised in this way, thus (it is said) removing the undesirable “gauge-redundancy” inherent in all non-geometrised versions of Newtonian gravitation theory.<sup>27</sup>

**C-Ex. 3: “Gauge” symmetry in electromagnetism.** For our final counter-example, consider the (source-free) theory of electromagnetism set in a fixed Minkowskian spatiotemporal background, but with the matter fields, rather than being represented in terms of the Faraday tensor  $F_{ab}$  (satisfying the equations  $\nabla_{[a}F_{bc]} = 0$  and  $\nabla_a F^{ab} = 0$ ), instead being represented in terms of the electromagnetic 4-potential  $A^a$  (i.e., such that  $F_{ab} = \nabla_{[a}A_{b]}$ , and with the corresponding field equation of the theory being written as  $\nabla_a \nabla^a A^b = \nabla^b \nabla_a A^a$ ). Picking a model of this theory to be given by the triple  $\mathcal{M} = \langle M, \eta_{ab}, A^a \rangle$ , and

---

<sup>27</sup>For further details and discussion, see, e.g., Knox (2014).

considering the transformation which maps models of this form to models of the form  $\mathcal{M}_{gauge} = \langle M, \eta_{ab}, A'^a \rangle$  where  $A'^a = A^a + \nabla^a \psi$  (and where  $\psi$  is some smooth scalar field), it will be the case that, if  $\mathcal{M}$  is a dynamically possible model of this theory, then  $\mathcal{M}_{gauge}$  will also be a dynamically possible model of this theory. Read literally,  $\mathcal{M}$  and  $\mathcal{M}_{gauge}$  once again assign qualitatively *distinct* material distributions over the manifold: the distributions do not differ merely with regard to which particular manifold points are underlying the various parts of the vector potential field. *Modulo* various concerns which arise as a result of the Aharonov-Bohm effect, the conclusion invariably drawn is that this “gauge invariance” indicates the superfluousness of the vector potential *qua* physical quantity: that it is merely a mathematically convenient “shorthanded” way of describing and determining the values of the Faraday tensor, which *is* taken to represent the genuine material content of the theory (cf. Weatherall forthcoming, *a*).<sup>28</sup>

The moral in each of the above three examples is the same: the fact that the theory allows for these putatively unpalatable qualitative distinctions between solutions is supposed to motivate the search for and adoption of an alternative physical theory which collapses the qualitative distinctions between the symmetry-related models in question. (Why these distinctions *are* considered unpalatable is a question that we shall address in §2.4.) Symmetries, therefore,

---

<sup>28</sup>Furthermore, trying to defuse this argument by regarding “absolute” values of the vector potential as nonqualitative will not be of much use here, for in general  $\mathcal{M}$  and  $\mathcal{M}_{gauge}$  will differ “comparatively” as well; that is, the *variation* in field values from spacetime point to spacetime point will nearly always be different across gauge-related models. This is due to such gauge symmetry’s being suitably “local”, or being variable from spacetime point to spacetime point: a gauge transformation will thus generically yield a model which differs from the original in more than purely “absolute” terms. For more on the issue of “absolutism” versus “comparativism” about quantity, see Dasgupta (2013).

sometimes *do* relate qualitatively discernible models; moreover, symmetries do not (merely) motivate us to excise nonqualitative structure, but rather can and do relate qualitatively distinct solutions, and are capable of motivating the excision or reconstrual of the structure which “varies” in the appropriate way.

All of this might seem utterly transparent. Why, then, do so many theorists appear to insist on symmetries relating — and only relating — qualitatively indiscernible solutions? Unfortunately, I do not have any particularly convincing answer to this question. To close this section, then, I will consider what I take to be three of the most promising candidate responses.

The first, very natural response would be that all of these theorists are using the word “qualitative” in a somewhat different manner to the way in which I have used it here. I do not think this is at all the case,<sup>29</sup> though I admit that this claim is difficult to substantiate without engaging in a rather long (and tiresome!) exegetical discussion. I shall therefore leave it as an exercise for the committed reader to try and work out what such authors *could* possibly mean by “qualitative”, at least on any charitable interpretation, unless they mean what I am taking them to mean here.<sup>30</sup>

A second possible answer to the above question might be that these authors are implicitly claiming that symmetries only motivate the excision of nonqual-

---

<sup>29</sup>One important exception to this are Ismael & van Fraassen (2003), who are explicit in their distinct construal of the notion of “qualitative”. I discuss their paper in §2.4.

<sup>30</sup>Perhaps I should add in my initial defence that the fact that (i) these authors usually contrast their notion of “qualitative” with the doctrine of haecceitism (i.e., the view according to which worlds might differ solely with regard to which objects are playing which qualitative roles), (ii) they often cite metaphysicians (e.g., Lewis 1983b) who use the word in the same (standard) sense used here, and (iii) the canonical examples of symmetries that they consider (e.g., Leibniz shifts) *are* transformations that relate qualitatively indiscernible solutions, very strongly suggests to my mind the interpretation according to which they are simply mistaken.

itative structure in “correctly formulated” theories.<sup>31</sup> Thus, for instance, they might follow Earman (1989, §3.4) in claiming that counter-examples 1 and 2 above are not in fact counterexamples to proposition (P1) — which, to recall, was the claim that symmetries, insofar they are guides to “surplus” theoretical structure, only ever relate models which differ at most nonqualitatively — because the relevant theories violate a “condition of adequacy” on theories that the symmetries of the dynamics should not outstrip the symmetries of the relevant spacetime (Earman’s criterion “SP1”), and are therefore not “correctly formulated” in the appropriate sense.

Although I find this line of reasoning intriguing, I do not, for various reasons, ultimately find it convincing. One immediate worry is that it is difficult to see how one might flesh out what is meant by a “correctly formulated” theory in such a way that the notion is both (a) broadly applicable to all theories and (b) non-*ad hoc*, insofar as the notion of a theory’s being “correctly formulated” isn’t merely concocted on a case-by-case basis to preserve the truth of proposition (P1). (Why, for instance, isn’t ordinary quantum mechanics as formulated in the labelled tensor product Hilbert space formalism also not a “correctly formulated” theory, given the availability of the Fock space formalism?) A second worry is that often what we *mean* by a particular theory’s being “correctly formulated” is only evident in retrospect, *after* the relevant surplus structure has been identified and theoretically dispensed with. After all, it took quite some mathematical ingenuity and innovation (invention of the notion of an affine connection, etc.) before we could even meaningfully speak of there being such things as “Galilean” and “Newton-Cartan” spacetimes: indeed, it is plausible

---

<sup>31</sup>Thanks to Nick Huggett for suggesting this response to me.

to think that for Newton, who had no alternative, “Newtonian” spacetime *was* the “correct” spacetime setting for his theory at the time he wrote the *Principia*! And a third and final worry about this kind of response is that, even assuming that a plausible, broadly applicable and non-*ad hoc* notion of a theory’s being “correctly formulated” can be found, it is at best not obvious why we should expect such a notion to line up with the qualitative/nonqualitative distinction in any uniform way. *A priori*, what reason do we have to think that such a notion will invariably affirm that those theories involving “excess” qualitative structure are not “correctly formulated”, but that those theories that involve “excess” nonqualitative structure are?<sup>32</sup>

Finally, a third, slightly more speculative answer to the question of why the view that symmetries only ever relate qualitatively indiscernible solutions is so prevalent is that since the explosion of literature on structural realism over the last two decades or so, discussion of theories’ symmetries has (perhaps overly-) focussed on two specific symmetries — namely, the permutation invariance of quantum mechanics, and the diffeomorphism invariance of general relativity — and their connection to various structuralist theses concerning the metaphysics of individuality, objecthood, and relations.<sup>33</sup> These symmetries, of course, and as we noted above, *do* invariably relate solutions to qualitatively indiscernible solutions. It is, perhaps, not entirely implausible to think that many theorists have on this basis illegitimately extrapolated to the general conclusion that

---

<sup>32</sup>Recall that Earman (1989, §9) himself was led to conclude that the diffeomorphism invariance of GR motivated the search for a theory of gravitation that did not quantify over manifold points. For Earman, then, a theory’s violation of SP1 plausibly constitutes only a sufficient condition of its being “incorrectly formulated”, but not a necessary one.

<sup>33</sup>For broadly “structuralist” discussions of these specific symmetries, see e.g., Stachel (2002), Ladyman & Ross (2007), and Rickles (2008).

symmetries thereby *always* relate qualitatively indiscernible solutions, when in fact a bit of reflection would have revealed that, in truth, this is not so.

But whatever the reasons might be for the predominance of the view that symmetries only ever relate qualitatively indiscernible solutions, the important point to note is that, as a matter of plain fact, it is false. Symmetries do in fact map solutions to qualitatively distinct solutions, and moreover can on occasion motivate the excision of theoretical structure that can only be plausibly construed as qualitative.

In the next section, we change tack. Having shown in this section that proposition (P1) is straightforwardly false, in the next section I will attempt to push the claim that (P1) is also, quite plausibly, philosophically naïve. More specifically, I first want to press a “*reductio*” point originally made by Clarke in the correspondence with Leibniz — namely, that if one is motivated to excise points of space on the basis of “shift-style” arguments, then one should likewise be motivated to excise intrinsically indiscernible atoms as well — and secondly, I wish to develop and offer some defence of a separate line of argument according to which symmetries can, in fact, *never* serve as guides to the nonreality of the nonqualitative.<sup>34</sup>

---

<sup>34</sup>To fix ideas in what follows I shall primarily be focussing on spacetime symmetries in the classical Newtonian case, although I see no reason why the points made in this setting will not generalise to the general relativistic or indeed other theoretical contexts as well. (Though it should also be noted that interpreting the permutation invariance of quantum mechanics as indicating the non-individuality of quantum particles faces various other, more specific difficulties, one of which is the fact that *classical* statistical mechanics is plausibly interpreted as being a permutation invariant theory as well; see Saunders (2013).)

## 2.3 Symmetry and the Nonqualitative

In this section I shall (to repeat) consider two distinct problems associated with the view that symmetries can act as guides to redundant nonqualitative structure. The first problem begins to emerge when one considers what proposition (P1) above might plausibly commit one to: namely, the possible non-existence of individuals *tout court*. The second problem is that there appears to be a plausible way of *resisting* symmetry arguments directed against the reality of nonqualitative structure: a way of resisting which, it seems, is not available in the case of symmetry arguments directed against the nonreality of qualitative structure. We consider each of these problems in turn below.

### 2.3.1 The First Problem

In getting to grips with this problem it might be helpful to quickly remind ourselves of (one of) the core issues discussed in the well-known correspondence between Clarke and Leibniz, which took place almost exactly three centuries ago in 1715-1716. Here, recall, Leibniz argued against the existence of substantial space on the grounds that, were space a real substance, then "'tis impossible there should be a reason" why God would choose to place the world's total material content in one region of space rather than another, the two scenarios' being "absolutely indiscernible" from one another (LC: 26). Clarke, on the other hand, admonished Leibniz for such a principle's entailing that God "has neither created, nor can possibly create any matter at all" (LC: 45), for a structurally identical argument could easily be levelled against the existence of atoms as well (LC: 20-1; 30-1) — there would equally be no reason for God to have arranged two

(intrinsically qualitatively indiscernible) bodies one way rather than another, “permuted” way — and on this basis Clarke concluded that Leibniz’s principles (namely, the Principle of Sufficient Reason and the Principle of the Identity of Indiscernibles) were illegitimate tools of metaphysical theorising. Leibniz, however, was perfectly happy to bite the bullet both when it came to arguments against the existence of substantival space *and* when it came to arguments against the existence of intrinsically indiscernible atoms: thus for him (and to Clarke’s astonishment) the actual existence of “atoms ... are confuted ... by the principles of true metaphysics” (LC: 36).

It is important to note two things about this very brief summary of the Leibniz-Clarke correspondence. First, one should note that both Clarke and Leibniz were in dialectical agreement insofar as they both believed that, should our actual — or indeed any possible — world contain such intrinsically indiscernible entities, then there is also a possible world in which such entities have swapped roles (i.e., a merely “haecceitistically distinct” possible world). Second — and more importantly — both Leibniz and Clarke agreed (in modern terminology) that *if* symmetry considerations ultimately motivated the excision of absolute space from our fundamental metaphysics, then it *also* motivated, to a precisely equal extent, the excision of atoms well.

“Anti-haecceitist” or “sophisticated” substantivalists, of one form or another, deny the possibility of such haecceitistically distinct worlds. I take it as given that one cannot simply declare by *fiat* that one denies the possibility of such worlds. One wants to know *why* — on what metaphysical grounds, or basis — one is in fact able to deny the possibility of such individuals’ switching roles. (Similarly, in a Newtonian context one cannot simply deny that there are no pos-

sible worlds differing solely over an absolute velocity boost: one must, I take it, demonstrate the metaphysical coherence of such a denial by developing an alternative metaphysical framework — e.g., the notion of a Galilean spacetime — on which basis one is able to make such claims; cf. Dasgupta 2011.) One fairly straightforward way to accomplish this would be to claim that only one world — namely, our actual one — is metaphysically possible. Such a rigidly essentialist response, however, seems blatantly *ad hoc*. We would not, after all, have accepted an analogous response to the boost invariance of NGT: merely stipulating that whatever absolute velocity I have I have essentially does nothing to rid us of our unease about accepting absolute velocity as a genuine physical quantity. A second, better way of responding to the possibility of such shifted worlds, then, would be to try to develop a particular metaphysical framework from which the supposed representational equivalence of shifted models might follow in a more-or-less natural way. One *prima facie* plausible way to do this might therefore be to construe individuals (and in particular, spacetime points) in “modestly structural” terms:<sup>35</sup> that is, as constituting nothing more — or less — than “nodes” in the relational (geometrical) physical structures in which they are embedded, or as “contextually individuated” entities (cf. Ladyman 2007). This is not meant to be equivalent to an eliminativism about individuals: one is perfectly free to take facts about the numerical identity and distinctness of such individuals as metaphysically primitive. Nor is it necessarily equivalent to the bundle theory, where individuals are ontologically secondary to properties and various “compresent” collections of them. Rather, the claim is that concrete, metaphysically robust individuals fundamentally exist, but that nev-

---

<sup>35</sup>I draw the term from Pooley (2006, 102).

ertheless there is no substratum or haecceity underlying them which grounds facts about their identity and distinctness. A shifted scenario, then, in virtue of the exact same relational, geometrical structures being instantiated as in the original, amounts to a mere redescription of the original scenario. Crucially, this spatiotemporal metaphysics still purports to be a version of substantivalism — construed as the view that spacetime points constitute fundamental, basic elements of the actual world — very much worthy of the name.<sup>36</sup>

Recently, however, theorists have questioned the viability of this kind of “modestly structuralist” metaphysics (Dasgupta 2011, §5; Russell 2014, §4-5). In particular, these theorists have claimed not to be able to understand what kind of metaphysics “modest structuralism” truly amounts to without it collapsing into either (a) a version of strong essentialism about which spacetime points are occupied or (b) a version of “straightforward” or non-structural substantivalism about spacetime points. At the very least, these authors have argued, the “modestly structuralist” or sophisticated substantivalist position needs to be more fully worked out before it can truly be said to constitute a response to the “Leibniz shift” argument against absolute space (or, in the context of GR, the “hole” argument against the existence of spacetime points) at least roughly on a par with the way in which the move to Galilean spacetime is usually taken to constitute an adequate response to the boost invariance of NGT.

I do not wish to take a position here on whether these authors are right on the issue of whether more needs to be said if one wants successfully to respond to these sorts of shift arguments. (However, see the addendum to this chapter

---

<sup>36</sup>For further defence of this sort of view, see e.g., Brighouse (1994), Hofer (1996), Saunders (2003a), Esfeld & Lam (2008), and Pooley (2006; 2013, §7).

below.) What I wish to point out, however, is merely the Clarkian point that *should* these theorists be right that some kind of modest structuralism about spacetime points is not a coherent position, then *exactly* parallel considerations should militate against the existence of atoms as well. That is to say, *if* one thinks that the static shift argument is a good argument against absolute space, *then* — and as Clarke pointed out to Leibniz in the correspondence — one should also believe that “permutation” arguments against intrinsically indiscernible particles should mitigate against the existence of them as well: what goes for one should go for the other. Thus, if symmetry considerations can plausibly be used as a guide to the nonreality of nonqualitative variant structure (and assuming that modest structuralism is not in fact a tenable or coherent position), then they should be taken to indicate not just the nonreality of points of space(time), but the nonreality of all individuals.

One might, of course (*à la* Leibniz), still find shift and permutation-style arguments appealing: indeed, it is for precisely these reasons that Dasgupta (2009) has attempted to formulate a metaphysical position (“algebraic generalism”) that purports to contain no fundamental individuals at all. Whether such an individual-less metaphysics is truly viable is an open question.<sup>37</sup> But regardless of whether it is, what I want to emphasise here is that, to the extent that one finds the notion of a modest structuralism about spacetime points or individuals in general untenable, then attempting to draw IP-based metaphysi-

---

<sup>37</sup>Though I must say that the early signs of developing such an “individual-less” metaphysics do not look promising. See, e.g., Rynasiewicz (1992) for a famous criticism of the the “Einstein Algebra” approach to GR advocated by (among others) Earman (1989, §9); Lam & Wüthrich (forthcoming) for some trenchant criticisms of the “category-theoretic” approach advocated by Bain (2013); and Turner (forthcoming) for a critical discussion of Dasgupta’s “algebraic” approach to cashing out an individual-less metaphysics.

cal consequences on the basis of symmetries' relating qualitatively indiscernible solutions is a far from straightforward task: a wholly revisionary, fundamentally "individual-less" metaphysics will be necessitated; a metaphysics which tells against not just the existence of points of space (as in Leibniz's "shift" argument) or points of spacetime (as in the "hole" argument), but against the existence of all individuals: a consequence that many theorists believe, not entirely implausibly, constitutes a *reductio* of Leibniz-style arguments directed against the nonreality of the nonqualitative.<sup>38</sup>

### 2.3.2 The Second Problem

Having emphasised in the previous subsection that the claim that symmetries motivate the excision of nonqualitative structure from our theoretical framework might excise too much (i.e., not just spacetime points, but individuals *tout court*), in this subsection I wish to focus on a possible way of *resisting* any symmetry argument to the effect that some particular nonqualitative aspect of the world is not real — a way of resisting which is apparently not available in the case of symmetries' being putative guides to the nonreality of variant qualitative structure, and which arguably must be responded to by those who think that symmetries can indeed be taken to reveal the redundancy of the variant nonqualitative aspects of our theories.<sup>39</sup>

---

<sup>38</sup>See, e.g., Horwich (1978, 409), who calls the consequence "unacceptable"; Field (1984, 77, fn 15), who claims it renders Leibniz's argument "obvious[ly] unsound"; and most recently Arntzenius (2012, 178), who thinks it entails that Leibniz's shift argument "is just not a good argument".

<sup>39</sup>Although in what follows I shall be focussing almost exclusively on Maudlin's (1993) discussion, it should be noted that Maudlin's point was independently anticipated in more-or-less identical form earlier in the literature by Horwich (1978) and Teller (1987). (As Pooley (MS, 81) notes, however, Horwich and Maudlin draw very different conclusions from their shared

It is helpful to start by considering a pertinent difference between what Maudlin (1993) labels the *static shift* — which involves a global, time-independent repositioning of all matter fields in space (Ex. 2 above) — and the *kinematic shift* — which involves “boosting” the absolute velocity of all the matter content by a constant value against the background of Newtonian spacetime (C-Ex. 1 above). As Maudlin makes explicit, NGT’s commitment to kinematically-shifted possibilities would appear to indicate the existence of a genuine, contentful *question* that an observer living in such a Newtonian world would be in principle unable to answer: namely, the question as to what her absolute velocity actually *is*. In the case of the static shift, on the other hand, no such analogous question expressing our ignorance appears to be available. Indeed, as Maudlin (1993, 190) writes, the only question one might sensibly ask oneself — namely, “Where am I in absolute space?” — deserves the pithy answer: “I am *here*, not three meters north or anywhere else.” In other words, no *directly analogous* epistemological problem to the kinematic shift appears to be generated in the case of the static shift: for the only way in which one can even state the static shift argument is in such a way as to antecedently determine that all such statically shifted scenarios are in fact counterfactual, requiring as they do essential reference to the actual world. Another way to put essentially the same point is to note that in a Newtonian world you will always be certain that you are *not* (say) 5 metres to the left of where you *actually are* right now; but you will be never certain whether you are moving at 0 m/s, or 5/ms, or 10 m/s with respect to absolute space.

---

observation.) What sets Maudlin’s discussion apart, however, is the fact that he was the first person to note the important *difference* between the two sorts (static and kinematic) of symmetry argument, and it is for this reason that I shall focus primarily on his discussion here. (See also Weatherall (forthcoming, *b*) for a more recent, related discussion — one which, I believe, in all essential respects reiterates Maudlin’s.)

Now, it is crucial to see that the disanalogy Maudlin notes between the static and kinematic shift arguments is not one that arises due to some inherent difference in the nature of the static and the kinematic shifts *per se*.<sup>40</sup> Rather, it arises as a consequence of the fact that, while the kinematic shift involves *qualitative* differences between distinct models, the *static* shift involves differences that are purely haecceitistic: that is, which involve differences only with regard to the scenarios' respective (putative) *singular* facts. Indeed, depending on the nature of the spacetime in question, a "static" shift can be used to generate qualitatively distinct models, while a "kinematic" shift can correspondingly be employed in certain spacetimes to generate distinctions between models that are merely haecceitistic. Thus, in an "Aristotelian" spacetime<sup>41</sup> setting — where models are of the form  $\mathcal{M} = \langle M, t_a, h^{ab}, \sigma^a, \gamma, \Phi_i \rangle$ , where  $\gamma$  is a privileged integral curve of  $\sigma^a$ , intuitively representing a "special" persisting point of absolute space (perhaps the "centre of the universe") — a "static" shift can be wielded to yield a distinction between models that *do* differ qualitatively: for such shifted models would typically represent all physical systems' differing with respect to their (qualitatively specifiable) spatial distance relative to the privileged point. Conversely, in a Galilean spacetime setting, a "kinematic" shift would introduce merely haecceitistic distinctions between models — for the boosted models would differ only with regard to *which particular* spacetime points were successively (un)occupied by the relevant physical systems — without differing qualitatively in any way

---

<sup>40</sup>Though Maudlin does not explicitly mention in his paper that his point generalises in this way, he has confirmed (in personal correspondence) that it does. Dasgupta (forthcoming, *b*) also notes that Maudlin's objection similarly applies to cases of property-switching as well, so long as properties are construed "quiddistically", such that it makes sense to speak of (say) mass and charge "swapping roles" in some possible world.

<sup>41</sup>I draw the term from Earman (1989, §2.6).

at all (cf. Pooley 2002, 59).<sup>42</sup> Maudlin's point can thus be put more broadly as follows: empirically indistinguishable worlds which differ qualitatively present an epistemological problem which is not present in the case of worlds which differ merely haecceitistically, for such qualitative distinctions entail that a *relevant question* can be asked regarding what it is that I, *qua* agent "embedded" in the model, am ignorant of, and which cannot be asked in the case of models or worlds that differ purely haecceitistically.

Maudlin (1993, 191) is entirely explicit concerning the upshot of all of this:

[T]he static shift does not result in an indistinguishable state of affairs, nor does it imply that there are any real but empirically undeterminable spatiotemporal facts about the world. The world described by the shift may be *qualitatively* indistinguishable from the actual world in the sense that no purely qualitative predicate is true of the one which is false of the other. But we have more than purely qualitative vocabulary to describe the actual world; we have, for example, the indexicals, without which the Leibniz shift cannot be described.

Thus, for Maudlin, statically shifted solutions are distinguishable from one another: we distinguish between them *indexically*, i.e., by noting that *I* am *here*. Conversely, kinematically shifted solutions are not distinguishable from one another: for merely stating that my absolute velocity is in fact *whatever it actually is right now* does not get one anywhere nearer to answering the pertinent question of *how fast* I am actually moving with respect to absolute space. (Is it 5 m/s? 10 m/s? 20 m/s?) And, Maudlin argues, it is in virtue of this very distinguishability

---

<sup>42</sup>This is because in Newtonian gravitation theory as set in Galilean spacetime "Galilean boosts" now count both as a spacetime and as a dynamical symmetry.

that one might legitimately resist drawing any relevant metaphysical inferences when the symmetry-related models in question are qualitatively indiscernible: that is, when they represent at most haecceitistically distinct possible worlds.<sup>43</sup>

Theorists who are resistant to Maudlin's point, and who believe that symmetries can in fact motivate the excision of nonqualitative facts from our fundamental metaphysics, have attempted to counter Maudlin's point in a multitude of ways. Thus, Dasgupta (2011, 146) argues that Maudlin's construal of the relevant notion of distinguishability is flawed because it "implies that whether something is detectable depends on factors that are, intuitively, entirely irrelevant to the matter [of detectability]": for in the case of the static shift in NGT, he writes, the issue as to whether we are able to "detect" our absolute position would then seemingly depend on whether or not we lived in (say) a Newtonian or Aristotelian spacetime setting — or whether "God had a favourite point of space," as Dasgupta puts it — something which (Dasgupta claims) seems intuitively wrong: the thought being that in either such spacetime our absolute position would presumably be as "undetectable" as in the other.

I am, however, inclined to think that Dasgupta simply misses the point of Maudlin's objection here: indeed, he appears to me to simply be *re-stating* Maudlin's core claim that merely haecceitistically distinct scenarios are never indistinguishable. For as we saw above, in an Aristotelian spacetime setting

---

<sup>43</sup>It is perhaps worth noting that uniformly altering the absolute velocities of each material system in a given world will not *always* give rise to a qualitatively distinct world. For instance, changing the absolute velocity of each such material system by merely changing the *direction* of their absolute motion will give rise to a world that differs merely haecceitistically from the original, for such a transformation will only alter which particular points of spacetime are materially occupied. It would therefore be more correct to say that only alterations in absolute *speeds* invariably give rise to qualitatively distinct solutions. (Many thanks to Nick Huggett for help with this point.)

“shifting” the entire material universe *will* invariably yield a world qualitatively distinct (but nevertheless empirically indistinguishable) from the original. But now, in virtue of the qualitative distinctness of the two worlds, there is a fact which I can perfectly coherently express in qualitative vocabulary but which I am in principle unable to know, namely my distance relative to the privileged point of space. Moreover, it is for this very reason — *viz.*, that there is a stateable fact that I am in principle unable to determine or detect — that (Maudlin would presumably argue) one should excise the privileged point of space from one’s fundamental metaphysics, and move to (e.g.) Newtonian spacetime. Rather than posing a problem for Maudlin, then, Dasgupta appears merely to have provided a classic instance of Maudlin-type reasoning at work.

A second way of responding to Maudlin’s point — one which Dasgupta also appears to endorse in the same paper — might be to claim that, just because one cannot *state* what it is that one is ignorant of in the case of merely haecceitistically distinct scenarios, it doesn’t automatically follow that there is therefore *no fact* of which one is ignorant. There might, in other words, be genuine facts that I am in principle unable to detect, and which, moreover, I am in principle unable to express; all Maudlin has in fact shown us is that any such fact is inexpressible — not that there isn’t any such fact. Indeed, as Dasgupta (2011, 146) notes, if this is all that Maudlin has actually shown us, then “[O]ur epistemic situation *vis à vis* our location in space is much *worse* than our epistemic position *vis à vis* our velocity through space, since in the former case we cannot even formulate questions about what it is we cannot detect!”

But Maudlin might respond: whoever said anything about facts being inexpressible? I *can* in fact detect what my position in absolute space actually is:

specifically, I am right *here*, and/or the object before me is *that one*. In other words, Maudlin might respond by claiming that *singular facts are in fact detectable after all*, and that they are stateable using spatial indexicals and/or demonstrative terms; the fact that symmetries cannot be used as legitimate guides to nonqualitative structure is because this very nonqualitative structure is structure that we can in fact unequivocally detect: (P1) above is undermined because we know (“indexically”) that proposition (P2), or the claim that the world is fundamentally purely qualitative, is false. There is no genuine fact of which I am ignorant, and there is nothing of which I am ignorant that I cannot express: our metaphysics, our epistemology, and our semantics — they all line up.

If this interpretation of Maudlin is correct (and I think it is), how might the anti-Maudlinian — that is, someone who believes that symmetries can in fact be used as a guide to the nonreality of nonqualitative structure — respond? One very natural way to do it would be to offer a *positive* account of what detection “truly” is such that merely haecceitistically distinct scenarios turn out to be indistinguishable (i.e., “undetectably distinct”) after all: indeed, this is the strategy that both Dasgupta (forthcoming, *b*) and Russell (2011, §3.5) have independently tried to pursue in recent work. I am, however, quite sceptical of the general viability of this kind of anti-Maudlinian strategy. This is because I suspect that our pre-theoretical notion of “detection” is much too ambiguous for any account of what it “truly” or “genuinely” amounts to not to simply beg the question against Maudlin: that is, by simply ruling out by *fiat* cases of “indexical detection” at some point in one’s positive account of detection.<sup>44</sup> Rather, what

---

<sup>44</sup>This is especially evident in Russell’s (2011) account, in which he first distinguishes between the “semantic” and the “functional” contents of belief (in which the former, but not the latter, such contents are sensitive to the absolute position of a given subject), and then simply asserts (149)

I think the generalist must do if his response to Maudlin is to be dialectically effective is to try to *account for* Maudlin's (apparently false) intuition that he is in fact able to "indexically detect" which singular facts are true at his world; that is, the generalist must try to provide a way of accommodating Maudlin's intuition that the possibility of such indexical or "singular" detection decisively (indeed, almost trivially) refutes generalism *from within* the generalist framework.

I do not wish to advocate any particular way of doing this, but here is at least one plausible way that the generalist might try.<sup>45</sup> The trick that the generalist might be able to exploit here is that not all "knowledge" is plausibly *propositional* knowledge; that, in other words, some knowledge which one acquires is merely "locational", in the sense that by acquiring it one does not thereby acquire another *objective fact*, but rather merely (to paraphrase Pooley MS, 97) "locates oneself in the objective order." Thus, this account will run, by "detecting where he is" at any given time Maudlin is not thereby acquiring knowledge of a genuine singular fact, but is instead only acquiring knowledge of "where he is" in the full and complete generalist description of the world in which he is located: in Lewis' (1979) terminology, it is knowledge *de se*, not *de dicto*, which Maudlin lacks and subsequently acquires when he "detects" where he is in absolute space or which individual is in front of him at any given time. The subject knows *de se* that he is *here* and not *there*; but he does not "detect" this difference, for only propositional knowledge is knowledge that one can properly-speaking "detect".

To repeat, this is only one plausible way for this line of argument to go. All

---

that "what should count as *observed* is an observational belief's functional content," and that this conjecture is permissible because "I'm not worried about capturing intuitive judgements about the ordinary use of the word 'observe'." This, I take it, will hardly go very far in convincing the committed Maudlinian.

<sup>45</sup>This approach is advocated by Pooley (MS, 96-7); see also van Fraassen (1991, 465-6).

I wish to emphasise here is that Maudlin's intuition that he can directly detect specific singular facts (i.e., his position in absolute space and, more broadly, facts concerning particular individuals) must one way or another be accounted for by any generalist metaphysics which attempts to collapse any number of putatively haecceitistically distinct possibilities to one. Maudlin's objection would thus appear to constitute yet another significant, though perhaps on this occasion not insurmountable, problem for the Received View.

Let us now take stock. Recall the two propositions first stated in §2.1.2 above:

- (P1) Symmetries — insofar as they are guides to “surplus” theoretical structure — only ever relate models which differ at most nonqualitatively.
- (P2) The world is fundamentally purely qualitative, or “general”, in character.

In §2.2 we showed that proposition (P1) is straightforwardly false, for the reason that symmetries do, on occasion, reveal the superfluousness of variant qualitative structure. In this section (§2.3), we have seen that (P1) is not only straightforwardly false, but also plausibly naïve as well: for not only is it arguably the case (assuming that modest structuralism is not in fact a tenable position) that granting that symmetries are a guide to the nonqualitative grants too much (insofar as such a view might also indicate the fundamental nonreality of, e.g., electrons and other material entities besides), but there also seems to be a generic (Maudlinian) way of resisting arguments to the effect that some variant nonqualitative aspect of our theoretical formalism is not real (which would seem to appeal to the fact that (P2) can be proved trivially false by cases of “indexical” detection) — a way of resisting which, it seems, is not available in the case of

symmetries' revealing the superfluousness of qualitative structure.

In the next and final section, our philosophical focus will shift slightly. Rather than addressing problems specific to the Received View *per se*, our attention will now be directed toward the issue of how and why symmetries can be used as legitimate guides to superfluous structure in the first place (to the extent, that is, that they can in fact be so used). It is in addressing this question that Ismael & van Fraassen (2003) have recently proposed a notion of "qualitativity" distinct from its usual metaphysical connotation. Thus, they claim, it is the fact that symmetries relate "qualitatively indiscernible" solutions *in their sense* which, they argue, ultimately justifies the use of symmetries in picking out "surplus" theoretical structure. The next section will assess their proposal, and examine to what extent they are correct.

## 2.4 A New Notion of "Qualitative"?

Ismael & van Fraassen's (2003) main concern in their paper is (to repeat) to provide a philosophical account of how and why (and when) it is the case that drawing symmetry-based metaphysical inferences is justifiable. Similarly to those theorists who would appear to subscribe to the Received View, they too argue that the "qualitative indiscernibility" of a theory's solutions plays a crucial role in such an account. However, their construal of what it means for two solutions to be "qualitatively indiscernible" is crucially different from the notion as construed by adherents of the Received View. Rather than construing qualitative properties as being those which are not "individual-involving" in some intuitive sense, Ismael & van Fraassen (2003, 376) instead construe qualitative properties

as being those that are “directly observationally accessible to the observer,” and are “distinguishable by ... a gross discrimination of colour, texture, smell, and so on.” Furthermore, they are careful in their paper to contrast qualitative properties with those that are merely “measurable” in their sense: such measurable properties are those that are able to “make some discernible impact on gross discrimination of colour, texture, smell and so on ... [no] matter how attenuated the connection is, how esoteric the impact, or how special the conditions under which it can be discerned.”

Ismael & van Fraassen (2003, 380) go on to summarise their proposal as follows:

*... [W]e submit that it is precisely the qualitative-structure-preserving symmetries of the laws that are indicative of the presence of superfluous theoretical structure and should always be interpreted as trivial. (Emphasis in original)*

The thought would seem to be this. Take the space of solutions  $\mathcal{S}$  of your favourite theory; determine which solutions are qualitatively indiscernible in the sense of their involving the exact same distribution of “directly observable” quantities; take the relevant set of  $\mathcal{S}$ -preserving transformations on the space of kinematically possible models  $\mathcal{K}$  to be just those that map qualitatively indiscernible solutions to one another; and then go on to take solutions related to one another in this way to represent the same physical state of affairs.

In assessing this account, the first obvious question to ask is: Why are Ismael & van Fraassen so careful in their paper to stress the distinction between quantities or properties that are “qualitative” or directly observable, and those

that are merely “measurable”? Moreover, why do they think that this is a distinction that is *relevant* to the IP? Unfortunately, the answer to both of these questions is rather difficult to discern from what they write: indeed, they often write as if it *is* the measurable/unmeasurable distinction, rather than the qualitative/nonqualitative distinction, that is relevant to the IP. (E.g., “... [O]ur main topic: superfluous structure will align with the presence of *unmeasurable* quantities in the theory’s world picture. [...] To sum up: we are going to connect superfluous structure with the presence of *unmeasurable* quantities.” (376, 378; my emphasis).) They do on occasion (e.g., 378; see also 376), however, suggest that the distinction is crucial because “what is measurable/unmeasurable cannot be read off directly from the theory,” and that “[w]e need to make use of what is observable [i.e., qualitatively discernible] in order to make this distinction.” But, even assuming the correctness of this claim (a big assumption), the question arises: Why should the fact that certain quantities can be “read off” more-or-less “directly”, rather than “indirectly”, from a given theory’s formalism, have any bearing on what kind of quantities superfluous structure-indicating transformations should necessarily preserve? To be sure, which quantities are directly observable might be *easier to determine* than which quantities are merely measurable. But why should this have any particular bearing on the IP, or the issue of which symmetry transformations on  $\mathcal{K}$  among the  $\mathcal{S}$ -preserving ones are the *right* ones? Ismael & van Fraassen do not appear to provide an answer to this question in their paper; nor is it easy to imagine what a satisfactory answer to it could even be.

A second, related, problem also confronts their account. Given Ismael & van Fraassen’s (2003, 376) explicit admission that “many measurable quantities

will be non-qualitative,” their account would seem to imply that  $\mathcal{S}$ -preserving on  $\mathcal{K}$  transformations which map *measurably distinct*, but nevertheless “qualitatively indiscernible”, solutions to one another could plausibly be interpreted as revealing the presence of superfluous structure. But such a consequence is patently absurd. Now, of course, the obvious way to remedy this defect in their account would be to connect the presence of superfluous structure to the existence of unmeasurable quantities — and, indeed, we noted in the previous paragraph that this is how Ismael & van Fraassen often seem to frame their view — but then it would seem that the qualitative/nonqualitative distinction has lost its primary relevance for their account: it would be symmetries that preserve all *measurable* structure that would be indicative of surplus structure, with the qualitative/nonqualitative distinction, quite simply, having no substantive role to play in their account at all (other than, perhaps, insofar as qualitative quantities help us “get a fix on” what is measurable according to a given theory).<sup>46</sup>

A third and final problem facing Ismael & van Fraassen’s account is also perhaps the most interesting.<sup>47</sup> That is, if we agree with Ismael & van Fraassen’s proposal that one can “directly observe” certain quantities, it would appear to follow almost ineluctably that *once* identified as “directly observable”, such

---

<sup>46</sup>One might, of course, also be suspicious of the the very coherence of the distinction between the qualitative and the measurable that Ismael & van Fraassen draw in their paper: a distinction which, I suspect, ultimately derives from the importance that van Fraassen (1980) places upon this distinction in his constructive empiricist philosophy of science. (Where, roughly speaking, one should believe what one’s theory says about the directly observable or “qualitative” to be true, but remain agnostic about what it says about the unobservable or merely “measurable”.) Indeed, it is interesting to note that in earlier work Ismael (2001, 131-2) herself appears to raise just such a concern about the relevance and indeed even coherence of such a distinction: “[T]here is not an epistemologically interesting difference between unimplemented sight and sight augmented by imaging instruments.... Seeing, whether with our bare eyes or through a microscope, is just measuring....”

<sup>47</sup>This point has been explicitly noted by Dasgupta (forthcoming, *a*, 31), while Saunders (2003a, 300) also appears to make an essentially identical observation.

quantities *must* be left invariant under the symmetries of any future theory. The history of physics, however, would seem to suggest a very different lesson. Thus, for instance, in the transition from Newtonian to relativity theory, it is plausible to think that symmetry considerations ultimately led us to accept that what we “directly observe” in terms of the distance between two physical events is not their spatial distance *simpliciter*, but rather only their “spatial distance” *relative to a particular frame of reference*, the latter quantity being left invariant under relativistic (Lorentz) symmetry transformations. In other words, what we take ourselves to “directly observe” is *itself* seemingly a function or product of our theory: even supposedly “directly” observable quantities might turn out not to be real in the end. The move to a new theory, and a new associated set of transformations, might well yield the conclusion that quantities that we previously thought we could straightforwardly and unproblematically detect are, in fact, not detectable or indeed even real after all. The notion that we “directly observe” certain quantities *simpliciter*, then, *pace* Ismael & van Fraassen, is one that is extremely difficult, if not impossible, to square with the history of physics.

Despite these faults, however, I think that Ismael & van Fraassen are latching onto something relatively important in their attempted explanation and justification of IP-based inferences. For I, too, think that issues to do with observation and, more specifically, detection and measurability are essential in justifying IP-based metaphysical inferences.<sup>48</sup> I will end, then, by sketching the barest

---

<sup>48</sup>Indeed, in recent work Caulton (forthcoming) and Dasgupta (forthcoming, *a*) have — apparently independently — pressed this same point, albeit in different ways. Given the already quite significant length of this chapter, I have relegated the task of working out the relevant differences and similarities between these and my accounts to an addendum, which can be found at the end of this chapter.

outlines of how I think the correct account of the IP goes. (One which I take to be broadly van Fraassen-esque in spirit.)

To keep things relatively concrete, consider again the case of NGT and absolute velocity. This theory has an associated set of  $\mathcal{S}$  of dynamically possible models. Now, we seem to have very good reason to think that, for instance, models related by a boost transformation are empirically indistinguishable: there is no possible measurement that any observer “embedded” in such a Newtonian world could perform in order to determine what her absolute velocity actually is. Absolute velocity, in other words, is a quantity that no Newtonian observer could ever empirically measure. But why do we think this? It is often claimed (by, e.g., Friedman 1983) that it is a simple consequence of Newton’s *laws*: that it follows merely from the fact that accelerations are left invariant under the Galilean transformations that absolute velocity is an undetectable physical quantity. As several theorists have pointed out (e.g., Barbour 1989; Brown 1993), however, this is simply incorrect: in order to derive the conclusion that absolute velocity is a truly undetectable dynamical quantity one needs an additional assumption — one which in fact Newton implicitly made in his derivation of Corollary V in the *Principia* — namely, that both the inertial and gravitational mass of bodies (and the corresponding forces which act upon them) are *independent* of the bodies’ absolute state of motion.<sup>49</sup> But this assumption is plausibly still only a necessary, and not a sufficient, condition to guarantee absolute velocity’s undetectability. One additional (extremely obvious, but nevertheless non-trivial) assumption that needs to be made in order to ensure such undetectability concerns subjects’

---

<sup>49</sup>Indeed, this assumption is required in order to ensure that NGT counts as a Galilean (or “boost”) invariant theory in the first place.

internal mental states: namely, that they do not covary in a systematic way with their absolute motion. That is (and Wittgensteinian “private language” arguments to the contrary notwithstanding), one must assume that subjects do not (e.g.) possess “absolute velocity recorders” in the corners of their visual fields, or indeed any other devices which would allow them to have direct knowledge of what their own particular absolute velocity is at any given time.<sup>50</sup> And, finally, one plausible assumption that would appear to be necessary to guarantee absolute velocity’s undetectability is that no concurrent or future theory (e.g., classical electrodynamical theory) will eventually render one’s absolute velocity detectable according to some other method.<sup>51</sup>

All of these assumptions are, of course, very natural ones to make. Indeed, most theorists writing on these topics appear to make them implicitly. What I wish to emphasise here, however, is simply their *non-triviality*; none of them, after all, follow purely from “the truth” of Newtonian theory alone; if one of them were not to obtain, absolute velocity might well have turned out not to have been an undetectable quantity after all.

But why do these assumptions feel natural? Well, one thing worth pointing

---

<sup>50</sup>This point in particular is emphasised by Roberts (2008, §6). As he notes, if such knowledge did exist it would be a very strange kind of knowledge indeed: in particular, it would not be communicable through standard physical channels. For instance, writing a letter to someone to inform him or her of what your absolute velocity actually is wouldn’t work, as the relative positions of the ink particles on the written paper would be preserved in the boosted scenario as well.

<sup>51</sup>It could of course be debated whether such a proper subclass of frames in classical electrodynamical theory should be identified with the absolute space rest frame in NGT (for why can’t the “ether” be moving with respect to absolute space?). I follow Friedman (1983, 105) (and, apparently, Earman (1989, 51-55)), however, in believing that such an identification is a perfectly natural and obvious one to make. (Perhaps Occamist reasoning could also be mobilised here: for Occam’s razor would plausibly dictate that we should, other things being equal, posit only one privileged subclass of inertial frames common to both such theories (NGT and classical electrodynamics), rather than two.)

out is that all of them seem to fit in with our “scientific knowledge” in the broadest sense. We are, indeed, reasonably confident that forces do not systematically covary with systems’ “absolute” velocities (as our experiences on trains and aeroplanes, along with more detailed experiments, will testify); that humans do not, for instance, possess “absolute velocity recorders” in the corners of their visual field; and that no other theory will eventually allow one to determine such a thing as one’s “true” motion with respect to some privileged inertial frame. It would thus seem to be the case that it is to *science itself*, in some very general sense, that we must appeal to if we are to determine whether or not some specific quantity is detectable or not, and to whom we must ultimately defer if we are to justify IP-based metaphysical inferences. In a certain sense, then, we should embrace van Fraassen’s (1980, §3.7) *hermeneutic circle*: we should let science itself be our guide to what we (think we) can detect.

This general proposal that I am suggesting (or, rather, sketching) here might therefore be summarised as follows. First, one determines what the superfluous structure-indicating transformations on the solution space of one’s theory actually are by engaging in hermeneutic circle-type reasoning: this allows us to determine which models of our theory represent empirically indistinguishable, but nevertheless putatively physically distinct, solutions. Second, one justifies the excision of the superfluous “variant” structure in question by appealing to a mixture of both scientific realism (i.e., the view that the models of our theory should be construed more-or-less literally) and to Occam’s razor (i.e., the assumption that, other things equal, it is better for one’s theory not to allow for physically distinct but empirically indistinguishable solutions).

Now, of course, in the Newtonian case things are relatively straightforward:

working out which solutions are empirically indistinguishable is relatively easy once we have determined (by hermeneutic circle-type reasoning) that all that we in fact have empirical access to are the relative positions and velocities that material systems instantiate with respect to one another. In more complex or heavily mathematicised theories, working out the empirical content of one's theory will almost certainly constitute a far less straightforward task. Nevertheless, I think that the general process of reasoning that applies in these more complex cases will be the same in all essential respects to the one I am here suggesting applies in the simpler case of NGT. That is, in the first instance we engage in hermeneutic circle-type reasoning to work out which solutions represent empirically indistinguishable but physically distinct ways for the world to be: in the Newtonian case, this will involve identifying those solutions in which the same histories of relative distances and relative velocities of material systems are instantiated. And in the second instance, we seek a novel, superior theory — or a superior interpretation or “characterisation” of the original theory — according to which these two solutions are not only *empirically* indistinguishable, but represent *physically* identical states of affairs: in the case of the “variant” quantity of absolute velocity in NGT, for instance, this will involve moving to Galilean (or, better, Newton-Cartan<sup>52</sup>) spacetime.

In closing: I would of course admit that much of what I have just claimed could be disputed. Nevertheless, I believe that, at the very least, this account provides a modest improvement over Ismael & van Fraassen's own proposal, insofar as it appears to solve what are arguably its most pressing difficulties. For in my account, there is no dubious distinction between the “qualitative” and the

---

<sup>52</sup>But see page 116, fn 6 in this chapter's addendum below.

“measurable” drawn; my account does not have the (unwanted) consequence that measurably distinct solutions can sometimes be indicative of the presence of superfluous structure; and furthermore my account would seem to be fully capable of accommodating the transition from classical Newtonian physics — in which we “directly observe” spatial distances *simpliciter* — to relativity theory, in which we only “directly observe” spacetime intervals. This is because on my account the hermeneutic circle serves (as it appears to have served for van Fraassen (1980, §3.7)) an essential *dual* role: it is not only the means by which we determine *whether* two solutions are empirically indistinguishable; but it also, in a crucial and important sense, informs us of *what* it is that we think we can empirically detect. Of course, there is no guarantee that what we take ourselves to detect now will be preserved under theory change. But this is just the Humean predicament that we, as scientists and philosophers, find ourselves in: a predicament which, as Quine (1969, 72) noted several decades ago, needn’t trouble us, for it would appear to be essentially equivalent to the human one.

## Addendum to Chapter 2

In this addendum, I distinguish the view of symmetries and Invariance Principle-style reasoning sketched at the end of chapter 2 from similar proposals that have recently been put forth in the literature by Adam Caulton (forthcoming) and Shamik Dasgupta (forthcoming, *a*). I argue that, though similar, each proposal differs from the other, and my own, in significant respects.

### Caulton

The basic structure of Caulton's proposal is as follows. One begins with the space of solutions of one's theory, with each mathematical model being initially endowed with what we might call a "minimal" interpretation: this is Caulton's "first phase of the interpretative process". This minimal interpretation fixes a determinate representation relation between each model and the empirical phenomena. Thus, given this representation relation, one is able to determine which models of one's theory represent empirically distinguishable, or empirically indistinguishable, solutions, and by the same token one is able to determine which solutions are compatible with the empirical phenomena so far observed.

This minimal interpretation is, however, entirely non-committal on the issue

of which models represent the same *physical* state of affairs. Fixing this relation of physical equivalence among models is precisely the job of Caulton's "second phase of the interpretative process". To this end Caulton distinguishes between what he calls "analytic symmetries" and "synthetic symmetries":<sup>1</sup> the crucial point is that while synthetic symmetries relate physically *distinct* solutions of the theory, analytic symmetries instead relate solutions that represent the *same* physical state of affairs.<sup>2</sup> But how is one to determine which solutions are related by an analytic symmetry, without having antecedently determined which quantities of the theory are genuinely real?<sup>3</sup> The answer Caulton proposes is straightforward: one should regard as many transformations among the theory's solution space as analytic as is possible without compromising the theory's (assumed) empirical adequacy. As Caulton (forthcoming, 8) concisely puts it, one should "*maximise the analytic symmetries, subject to empirical adequacy.*"

An initial worry one might have with Caulton's proposal, one which he himself raises, is whether it is tantamount to "a form of verificationism" (Caulton forthcoming, 9): more specifically, whether it is compatible with a form of realism that is not *forcibly* committed to regarding observationally equivalent models as representing the same physical state of affairs. Such a problem would seem to arise because, if it is indeed a constitutive feature of the suggested inter-

<sup>1</sup>The obvious analogy here, as Caulton notes, is between analytic and synthetic propositions.

<sup>2</sup>Caulton's (forthcoming, §2.3) definition of a "synthetic symmetry" is in fact more nuanced than this: he distinguishes between "synthetic symmetries of the first kind" (which fail to leave all genuine physical quantities invariant) and "synthetic symmetries of the second kind" (which preserve all genuine physical quantities only for some proper subset of a theory's solutions). This subtlety, however, need not concern us here.

<sup>3</sup>Essentially this same problem of "inferential circularity" — to use Dasgupta's useful label — in determining a given theory's relevant solution-preserving transformations has been independently noted by Nozick (2001, 79), Ismael & van Fraassen (2003, 386), Debs & Redhead (2007, 66), Swanson & Halvorson (MS, 7), and, most perspicaciously, by Dasgupta (forthcoming, a, §5.3).

pretative process that one is to “maximise” the number of analytic symmetries, subject to the (apparently, sole) condition of empirical adequacy, then it would appear to follow — by interpretative *fiat* — that one is unable to make sense of empirically indistinguishable but nevertheless physically distinct solutions: for the second phase of the interpretative process would appear to dictate that we *must* regard all empirically indistinguishable solutions as representing the same physical state of affairs.

Caulton (forthcoming, 9) responds to this objection as follows:

Finally, it might be objected that my proposal amounts to nothing more than a form of verificationism, the doctrine that only observable differences make for meaningful differences. But that would not be an accurate characterisation of my proposal. Like Leibniz, I do not claim that non-observable physical differences do not make *sense*; my claim is that the maximisation of analytic symmetries serves as a methodological principle, or a “super-empirical virtue” — a means to overcome theoretical underdetermination, to get at an account of what the world is like.

I find it difficult to shake the suspicion that Caulton is equivocating on the proper construal of his own account here. According to one such construal (my own construal, offered above), Caulton is claiming that empirically equivalent solutions must always be taken to represent the same physical state of affairs. As noted, this seems to be a straightforward consequence of Caulton’s “second phase of the interpretative process”: namely, that one should maximise the analytic symmetries of one’s theory whilst respecting the theory’s assumed

empirical adequacy. According to a second construal of Caulton's proposal, however — which is suggested in the paragraph quoted above — one is merely inherently *motivated* to regard empirically equivalent solutions as representing the same physical state of affairs, but one is not *necessarily* committed to — or, indeed, warranted in — regarding such solutions as physically equivalent.

On the first construal of Caulton's account, it appears inescapable that the proposed method will leave one unable to regard solutions that are empirically indistinguishable as representing distinct physical possibilities. On the second construal, on the other hand, it seems easily escapable — but then the second phase of the interpretative process is *not* correctly described as a process in which one maximises the “analytic symmetries” of one's theory: rather, what one is doing during this process is working out which (empirically equivalent) solutions one is *motivated* to regard as representing the same physical situation. *Analytic* symmetry would therefore not be the apt term according to this second construal of Caulton's position: for on this construal interpretative practice would not in general require that the solutions in question be regarded as physically equivalent.

To further illustrate this (to my mind, crucial) distinction between these two possible construals of Caulton's proposal, consider again the case of the (field-theoretic) formulation of Newtonian Gravitation Theory (NGT), set in Newtonian spacetime. Recall that this theory has associated models of the form  $\mathcal{M} = \langle M, t_a, h^{ab}, \sigma^a, \rho, \phi \rangle$ , where  $M$  is a differentiable 4-dimensional manifold,  $t_a$  is the temporal metric,  $h^{ab}$  is the spatial metric,  $\sigma^a$  is the timelike vector field whose integral curves represent the persisting points of absolute space, and  $\rho$  and  $\phi$

represent the matter density and the gravitational potential field respectively.<sup>4</sup>

As noted in chapter 2, the symmetry group of this theory not only includes transformations corresponding to time-independent velocity “boosts” of solutions’ total matter content, but it also includes transformations corresponding to time-dependent translational accelerations of such content (so long as the gravitational potential field is also appropriately transformed). Thus, read “naïvely”, the symmetries of this theory include transformations that map solutions to solutions that represent physically distinct, but nevertheless empirically indistinguishable, states of affairs in which a given material system is:

1. Force-free and stationary with respect to absolute space.
2. Force-free and moving at constant absolute velocity.
3. Absolutely accelerating under a gravitational force-field.

According to the first construal of Caulton’s proposal (which I take to be his actual position),<sup>5</sup> we should take all of these mathematically distinct models as *in fact* representing the same physical state of affairs. On the second construal of Caulton’s proposal, however, we are merely *motivated* to regard all such models as representing the same physical state of affairs — the motivation presumably arising from the general Occamist principle that, other things being equal, our preferred scientific theories should not allow for physically distinct but nevertheless empirically equivalent solutions. According to this second construal

---

<sup>4</sup>Note that the canonical presentations of Newtonian spacetime (see, e.g., Friedman 1983, III, §1; Earman 1989, §2.5) take the affine connection as ideologically primitive. I find such presentations unsatisfactory for historical rather than for philosophical reasons: in particular, it threatens to make the move to Galilean spacetime seem almost trivial, and the associated timelike vector field trivially superfluous. For more on this point, see Pooley (MS, §4.4-4.5).

<sup>5</sup>Caulton has privately confirmed to me that he subscribes to this view.

of Caulton's position, then, absent a metaphysically perspicuous characterisation of the reality underlying the symmetry-related models in question (as is achieved by moving to, e.g., Newton-Cartan theory<sup>6</sup>), we have no choice but to regard such solutions as representing physically distinct states of affairs.

Thus, there are two distinct ways of understanding Caulton's "analytic symmetries": as possessing *interpretational*, or merely inherently *motivational*, force. Setting Caulton's own views to one side, which of these views is the more plausible one? I myself incline towards the second. There is nothing, after all, obviously absurd about admitting in principle undetectable facts into one's ontology; nor is there any obvious reason why we should always be capable of discovering a theory (or a perspicuous characterisation of a theory) which explains such solutions' empirical equivalence in terms of their fundamental physical equivalence; nor is there even any obvious way of guaranteeing that there will always be such a theory waiting in logical space to be discovered.

There are also important explanatory matters to consider. For although the Newtonian who adopts the merely motivational construal of symmetries might indeed be committed to there being facts beyond one's epistemic grasp, he nevertheless has a perfectly good explanation as to *why* such facts are epistemically inaccessible: they are inaccessible precisely because the world is in fact truly and accurately represented by tuples of the form  $\langle M, t_a, h^{ab}, \sigma^a, \rho, \phi \rangle$ , and because all

---

<sup>6</sup>Strictly speaking, moving to Newton-Cartan theory is not *by itself* sufficient for one to be able to transparently understand as physically equivalent symmetry-related models of Newtonian theory set in flat spacetime. This is because (as noted in Chapter 2) such symmetry-related models will generically correspond to a single model of Newton-Cartan theory *only up to isomorphism*. Thus, in order to have a fully transparent understanding of how it is that symmetry-related models of Newtonian theory set in flat spacetime can correspond to a single model of Newton-Cartan theory, a modestly structuralist (see below) conception of spacetime ontology is also required. (A similar moral applies in the case of moving to Galilean spacetime as a response to NGT's boost invariance. Many thanks to David Wallace for pushing me on this point.)

any Newtonian observer ultimately has direct empirical access to are the instantaneous relative distances and relative velocities between material entities.<sup>7</sup> For such a Newtonian, then, the empirical phenomena underdetermine the genuine physical facts; however, the theory itself is able to provide a perfectly transparent explanation of the reality behind the phenomena in terms of which the underdetermination can be straightforwardly understood.

The Newtonian who adopts the interpretational construal of symmetries, however, would appear to lose this explanatory transparency: he knows *that* he must regard the symmetry-related solutions in question *as* “physically equivalent”, but the reality in terms of which this physical equivalence is to be understood may remain opaque to him; he is offered no explanation as to *how* such physical equivalence is to be construed, or how it could even be said to arise.<sup>8</sup>

<sup>7</sup>As Stein (1991, 156-7) notes, this is a simplification: instantaneous quantities are never *directly* empirically accessible. Rather, determining their values is an indirect process, and occurs over a finite period of time.

<sup>8</sup>In a recent paper, Neil Dewar — who defends the interpretational construal of symmetries — considers the possibility that “surplus” (symmetry-variant) structure might be playing an “explanatorily indispensable” role in one’s theory. He responds:

[I]ndispensable for what? If they were indispensable for explanation, then it does seem that they ought to be retained. But if they’re only indispensable for the purposes of characterising or representing the structures that are (explanatorily, hence physically) significant, then it seems that we have much greater leeway to regard them as mere mathematical scaffolding, without being honour-bound to accord them physical significance. (Dewar forthcoming, 6)

The claim made in this paragraph crucially hinges on the precise meaning of the term “significant”. As I understand it, Dewar is basically stipulating that the only “significant” physical quantities of a given theory are those that remain invariant under symmetry transformations. This claim is, I think, dubious at best: Why, for instance, should absolute velocity be considered an “insignificant” quantity in NGT, given that ostensibly “significant” quantities (e.g., relative velocity; absolute acceleration) are naturally understood as being metaphysically and/or conceptually derivative upon it? But even ignoring the dubiousness of this claim, Dewar fails to offer a response to the central claim made above: namely, that the Newtonian who subscribes to the motivational construal of symmetries has a perfectly transparent conception of the reality underlying symmetry-related solutions, a conception which is lost by the Newtonian who subscribes to the interpretational construal of symmetries. In a slogan: only the former such Newtonian, but not the latter, is able to give us a satisfactorily realistic picture of *what the world*

My view also differs from Caulton's in another important respect. To illustrate this difference, it will prove helpful to quote his own summary of his account, which comes near the end of his paper:

We proceed[ed] in two phases. During the first phase we set up representational links between the theory and the observable portion of the physical world, under the assumption that the theory is empirically adequate (or similar). In the second phase, we maximise the theory's analytic symmetries, taking advantage of the representational links forged in the first phase so as not to compromise empirical adequacy. *The result is an interpretation for the theory that prompts appropriate reform towards a new formalism, in which the physical properties and relations, including the unobservable ones, are transparently represented without redundancy.* (Caulton forthcoming, 9. My emphasis)

It is the final, italicised claim that I disagree with. More specifically, I disagree with the suggestion that *all* (IP-relevant) symmetry transformations should be construed as “prompt[ing]” a *reformulation* of the relevant physical theory.

It is important to distinguish this claim — *viz.*, that symmetry transformations invariably prompt a mathematical reformulation of the relevant theory — from the “motivational” view of symmetries just discussed (and endorsed) above. According to the motivational view of symmetries, one is invariably only motivated to regard symmetry-related models as physically equivalent; moreover, one is justified in regarding such models as physically equivalent only insofar as one is in possession of a metaphysically perspicuous characterisation

---

*is really like.* (For related discussion, see Friedman (1983, 112-3), Maudlin (1993, 192), and Pooley (2013, 531, 551; MS, 118).)

of the reality underlying such models. However — and this is the crucial point to note — I believe that one *can*, in fact, be in possession of such a metaphysically perspicuous characterisation of the reality underlying symmetry-related models *even in the absence* of a mathematical formulation of the theory which removes the relevant representational redundancy.

This, of course, prompts the question: When, exactly, is it possible to be in possession of a metaphysically perspicuous characterisation of the reality underlying symmetry-related models whilst simultaneously not being in possession of any mathematical formulation of the theory which excises such representational redundancy? I think the answer is straightforward: when such models are *isomorphic*, or — which amounts to the same thing — when they are naturally understood as representing at most haecceitistically distinct possible worlds. Thus, I claim, neither the time-independent translational (“Leibniz shift”) symmetry of NGT, nor the diffeomorphism invariance of general relativity, *by themselves* motivate any mathematical reconstrual of the respective theories. This is because *I* believe there is a perfectly transparent, anti-haecceitist, “modestly structuralist” — but nevertheless fully substantivalist — way of understanding such models’ representational equivalence even in the absence of such a mathematical reformulation. On this view (and recalling our discussion from chapter 2 above), spacetime points are construed as genuinely real, fundamental entities. However, they are “contextually individuated”: they are not to be understood as being anything more — or less — than “nodes” in the relational, geometrical structures in which they are embedded. Shifted models in NGT, and diffeomorphically-related models in GR, then, are understood as representing the same physical state of affairs precisely because the exact same pattern

of relational, geometrical structures is represented as obtaining in each case. In particular, this view denies that there are any primitive, singular (haecceitistic) facts about spacetime points which would even allow for a distinction between shifted or diffeomorphically-related scenarios to be coherently drawn.<sup>9</sup>

In discussing the diffeomorphism invariance of GR, Pooley (2002, 106) has made this point particularly perspicaciously:

It is sometimes suggested that the sophisticated [anti-haecceitist] substantialist is obliged to provide a mathematical description of his conception of reality that does not include any redundancy. While the construction of such a description might hold the prospect of proving *technically* productive in the context of the search for a quantum theory of gravity, I wish to stress here that there is no *philosophical* pressure to find such a description. The sophisticated substantialist has a *perfectly clear* conception of the single reality which he believes underlies an equivalence class of isomorphic models: it is a 4-dimensional spacetime of a particular geometric structure, the numerical diversity of the points of which is grounded in nothing more than the geometric properties they have, and the relations in which they stand. There is also absolutely no obscurity in how the mathematical models represent such a reality and in why the model-reality representation relation (under a certain interpretation of the manifold points) is many to one.

---

<sup>9</sup>Modest structuralism, as I am construing it, is thus a type of generalist metaphysics. For further defence of this sort of view, see e.g., Brighouse (1994), Hoefer (1996), Saunders (2003a), Ladyman (2007), Esfeld & Lam (2008), and Pooley (2006; 2013, §7).

Two remarks that are worth making about this passage. First, note that the sophisticated substantivalist *need* not (as Pooley seems to suggest here) adopt the view according to which the numerical diversity of the points of spacetime is *grounded* in their geometric properties and relations: it is perfectly legitimate for the sophisticated substantivalist to take such points' numerical diversity to be a metaphysically primitive, *ungrounded* fact.<sup>10</sup> Second, note Pooley's central claim: it is *because* the sophisticated substantivalist has a "perfectly clear" conception of the reality underlying diffeomorphically-related models which *explains why* there is no philosophical motivation ("pressure") to reformulate the theory so as to remove such representational redundancy — the obvious corollary being that if *no* such "perfectly clear" conception were in hand, then such a reformulation of the theory *would* be philosophically motivated.

As indicated above, I think the moral drawn by Pooley here is quite general: when two symmetry-related models are isomorphic, they can be understood as representing at most haecceitistically distinct possible worlds; hence, adopting modest structuralism (which entails anti-haecceitism) about the relevant theory's ontology transparently collapses the number of represented physical possibilities to one — with no mathematical reformulation of the theory ever being philosophically necessitated. In short, then, my claim is this: symmetry-related, isomorphic models invariably do *not* motivate a mathematical reformulation of the relevant theory (modest structuralism invariably suffices); but symmetry-related, *non*-isomorphic models invariably *do*.<sup>11</sup>

<sup>10</sup>In more recent work Pooley (MS, 100) appears sympathetic to taking numerical diversity as primitive.

<sup>11</sup>As briefly noted in Chapter 2, in recent work Dasgupta (2011) and Russell (2014) have claimed not to understand the view propounded by "sophisticated substantivalists" like Brig-house (1994), Hoefer (1996), Saunders (2003a), Pooley (2006), Ladyman (2007), Esfeld & Lam

## Dasgupta

Dasgupta, like Caulton and myself, subscribes to the view that a theory's IP-relevant symmetry transformations must invariably map solutions to empirically indistinguishable solutions. Unlike Caulton, but like myself, however, he quite explicitly subscribes to the motivational, rather than the interpretational, construal of symmetries. As he writes:

... [W]e can draw the conclusion of the inference [*viz.* that a given theory's relevantly "variant" quantities are not real] only when we have the alternative theory in hand and have shown that all else is equal. This explains why it was rational for Newton to believe in absolute velocity even though he knew that it was variant in NG[T] and undetectable. The reason this was rational for him was that he had no good alternative theory to hand. He had good reason (his bucket argument) to think that relationalism was not empirically adequate. And relationalism was the only alternative view he knew of (he was not aware of Galilean space-time structures in which there is a well-defined feature of absolute acceleration, as required by his bucket argument, but no[t] absolute velocity). So for Newton, all else was not equal and he was rational to believe in absolute velocity.

---

(2008), and others. I am sceptical as to whether there is anything that can be said that is dialectically effective against such confessions, other than to note that sophisticated substantivalism is *not* (to use Dasgupta's terminology, and as I am construing it) a "bare modal" thesis: that is, it is not the *mere assertion* that worlds cannot differ purely over which spacetime points are playing which qualitative roles. Rather, such anti-haecceitism is supposed to be a consequence of — "flow" from — modest structuralism (at least about spacetime ontology). Plausibly, Dasgupta and Russell would claim not to understand even this modest structuralist view, to which all I can say in response is that the view expressed by Pooley in the paragraph cited, and elaborated on above, seems perfectly clear to *me* — and, it would seem, to majority of other philosophers who have written on these issues as well.

(Dasgupta forthcoming, *a*, 17-18)

However, Dasgupta's view differs from my own — and concurs with Caulton's — insofar as he, too, believes that symmetries also invariably motivate a novel mathematical reformulation of the relevant theory which lacks such representational redundancy. As he goes on to write in a footnote:

There is a lesson here for contemporary structuralists, such as Ladyman and Ross (2007, 130), who take the fact that diffeomorphisms are symmetries of general relativity to suggest that “[t]here are no things. Structure is all there is”. For it is not enough to note that individual points of the manifold are variant features and declare that they are therefore not real. That would be analogous to Newton declaring that there is no such thing as absolute velocity without a genuine alternative theory in hand, a move that we would rightly have regarded with suspicion. To motivate structuralism, one must present a clear theory of the fundamental structure of the material world without making reference to regions of the manifold, a theory that does well on other theoretical virtues such as simplicity, elegance, and so on. But contemporary structuralists tend not to present such a theory. (Dasgupta forthcoming, *a*, 18, fn 23)

I agree with Dasgupta's suggestion that structuralists — *insofar as they wish to dispense with spacetime points qua primitive entities* – should be motivated to seek a mathematical alternative to GR which does not explicitly quantify over points of the spacetime manifold.<sup>12</sup> I also agree with his suggestion that the diffeomor-

---

<sup>12</sup>For discussion of some of the problems surrounding this program, see the papers cited in fn 37 in Ch 2 above.

phism invariance of GR should motivate us to articulate a clear conception of the reality supposedly underlying diffeomorphically-related models. However, I do not agree with his suggestion that the mere existence of this symmetry should motivate us to develop an “alternative theory” — if by “alternative theory” one means a *formally* distinct theory.<sup>13</sup> The reason for my disagreement is (to repeat) because diffeomorphically-related models are *isomorphic*: they represent at most haecceitistically distinct possible worlds. And as mentioned above, there is (in my view) a perfectly transparent way of understanding such isomorphic models’ physical equivalence which by no means requires any mathematical modification or reformulation of GR: namely, by adopting a modestly structuralist, substantivalist conception of spacetime points. In other words, there is an important *disanalogy*, elided by Dasgupta, between the case of GR’s diffeomorphism invariance and the case of NGT’s boost invariance: in the former, the models are isomorphic, hence adopting a modestly structuralist (anti-haecceitist) conception of space-time ontology suffices to understand how such mathematically distinct models can legitimately be taken to represent the same physical state of affairs; but in the latter, the models are *not* isomorphic — naturally understood, they represent *qualitatively* distinct possible worlds — hence adopting a modestly structuralist conception of spacetime ontology is not by itself sufficient to understand how their (alleged) physical equivalence could be said to arise.<sup>14</sup>

Dasgupta also differs from Caulton and myself in another important respect: he, but not Caulton or myself, thinks that more needs to be done to properly

---

<sup>13</sup>This suggestion is also elaborated on in Dasgupta (2011).

<sup>14</sup>Cf. Pooley (2013, 576-7).

explicate the notion of “empirical indistinguishability” in play. Thus, he reasons:

Rather than being a primitive relation, it is natural to think that the relation of observational equivalence holds between two structures in virtue of their intrinsic properties. So a better definition [of “symmetry”] would identify the intrinsic properties that make for observational equivalence, and then define a symmetry to be a transformation that (in addition to preserving the laws) preserves them. (Dasgupta forthcoming, *a*, 30-1)

What could these “intrinsic properties that make for observational equivalence” be? Dasgupta argues that they must not be *physical* features, for to do so would be to fall into the trap of “inferential circularity”: that is, of merely stipulating, without any apparent antecedent justification, that some particular physical feature must be preserved under a theory’s IP-relevant transformations and then concluding on that very basis that this (*ex hypothesi* invariant) feature is genuinely real. (Cf. fn 3 and the immediately surrounding discussion above.) Rather, he argues, any satisfactory “definition” of the relevant notion of symmetry, and the associated notion of observational equivalence, must be spelled out in what he calls “epistemic” terms: that is, in terms which ultimately

do not depend on the underlying metaphysics, such that we can be in a position to know whether two structures are observationally equivalent prior to knowing anything (via symmetry-to-reality reasoning) about the metaphysics of our world. (Dasgupta forthcoming, *a*, 31)

Dasgupta then goes on to sketch two ways in which a proper “analysis” of the notion of observational equivalence could go: one in terms of a (suitably

rigourised) notion of “how things look”; the other in terms of the notion of “observation sentence” drawn from the work of Quine (cf. Quine 1970, 1975).<sup>15</sup> Moreover, although Dasgupta (forthcoming, 35) is careful to stress that he does not fully endorse either such “analysis” of observational equivalence (they “are merely approximation[s] of what a completed epistemic definition of symmetry might look like”), he is nevertheless explicit that “the legitimacy of symmetry-to-reality reasoning depends on our making out some notion of observational equivalence along these lines.”

It is precisely this latter conclusion of Dasgupta’s that I disagree with. In particular, I do not believe that the notion of “observational equivalence” *must* necessarily be amenable to an analysis of the kind Dasgupta considers in order for Invariance Principle-style (“symmetry-to-reality”) reasoning to be justified. For to fail to offer an “analysis” of observational equivalence in terms of (e.g.) Quinian observation sentences does not seem to me to be equivalent to taking the notion of observational equivalence as a “primitive” or unanalysable relation. For instance, in the Newtonian context, we *know* what accounts for the apparent “observational equivalence” of the relevant symmetry-related states of affairs: it is the fact that they all share the same pattern of instantaneous relative distances and relative velocities instantiated by their respective material systems. It is

---

<sup>15</sup>Without wanting to go into too much detail, the basic idea behind Quine’s analysis of empirical equivalence is that two worlds are empirically equivalent iff they assign (“peg”) the same observation sentences — roughly speaking, those sentences to which a generic subject will be immediately disposed to assent to or dissent from when appropriately neurally stimulated — to each spacetime point throughout the worlds’ respective histories. The proposal is, of course, variously problematic. (For instance: Does it make sense to assign observation sentences to *points* in spacetime, given that human beings are not zero-dimensional objects? Does it make sense to speak of “pegging” observation sentences to “place-times” in which human observers could not survive? And which particular’s subject assent/dissent to neural stimuli should we take as canonical anyway? Etc.)

these “intrinsic properties”, coupled with the (as it turns out, false) assumption that all we can (more-or-less) “directly observe” are such instantaneous material distances, from which the noted observational equivalence may be inferred. One never needs resort to any notion of “observation sentences”, nor to any rigorised notion of “how things look”, in this inferential process: these notions play, at best, a superfluous role.<sup>16</sup>

But is this not equivalent to committing Dasgupta’s noted fallacy of inferential circularity? I do not believe that it is. The assumption that relative spatial distances are all that we can truly detect is, after all, defeasible: its implicit assumption of a unique global foliation of the spacetime manifold is dispensed with in relativity theory. The fact that what we take ourselves to “directly detect” is itself open to revision on the basis of theoretical innovation is just one aspect of the practice of science, and of what van Fraassen (1980, §3.7) has dubbed the “hermeneutic circle” more generally. In other words, theories *themselves* are ultimately the best guides we have to what we take ourselves to observe, and what we take ourselves to observe in turn provides epistemic support (or refutation) of those same theories. Taking the relevant solution-preserving transformations to invariably map solutions to observationally equivalent solutions, and by (defeasibly) taking such observational equivalence to consist in such solutions’ representing the same instantiation of relative material distances and velocities,

---

<sup>16</sup>Of course, it may be *nice* to have some such “formal”, or suitably rigourised or “mathematicised”, definition of observation equivalence. My point here (in analogy with Pooley’s suggestion, quoted above) is simply that there is no *philosophical* pressure to provide such an account. (For a related discussion of this point see Belanger (2013), who points out the extensional inadequacy of two other “mathematical” definitions of observational equivalence (one based on the notion of a “manifest isomorphism”, the other based on the notion of “ $\epsilon$ -congruence”), and who goes on to argue (76) that any such apparently rigorous or mathematicised notion of observational equivalence, while perhaps serving a useful heuristic or formal role, will never be able to escape its ultimate basis in “context-sensitive judgements based on physical hypotheses”.)

is not simply equivalent to committing the fallacy of inferential circularity, for the core *physical* criterion of observational equivalence itself is open to revision: a new theory (e.g., special relativity) might well end up suggesting a different criterion for observational equivalence (e.g., the same pattern of instantiation of material spatiotemporal *intervals*) from the theory that preceded it. There is nothing wrong with each theory suggesting its own criterion for observational equivalence; what is wrong is to assume that this criterion is not itself open to revision on the basis of theory.

## Chapter 3

### Weak Discernibility, Again

Do quantum particles violate the Principle of the Identity of Indiscernibles (PII)? The answer, of course, depends on how the PII is to be understood. According to one recent influential construal of the principle proposed by Simon Saunders (2003a, 2006) (drawing on the work of Quine 1976), elementary bosons do violate the PII, but fermions (and composite bosons with fermionic constituents) do not: for the latter, but not the former, are always at least *weakly discernible* — they invariably satisfy some two-place irreflexive physical relation. The conclusion drawn by Saunders is that fermions' weak discernibility in turn guarantees their status as "objects" in some appropriate sense, whereas elementary bosons' failure to stand in such irreflexive relations reveals that they are not to be construed as "objects", but rather as "mode[s] of the corresponding quantum field" (Saunders 2006, 60).

As an illustrative example, consider the spherically-symmetric singlet state of two intrinsically identical fermions. Despite this state's very high degree of symmetry, the fermions in question nevertheless still stand in the irreflexive

relation “ ... has opposite direction of each component of spin to ... ”. It is the fact that fermions *always* stand in at least *some* such irreflexive relation to others that is said to ensure their status as objects. The same cannot be said, however, of the elementary bosons: it is possible for them all to exist in exactly the same quantum state such that none of them stands in even an irreflexive physical relation to any other. This, according to Saunders, should lead us to question their status as genuine objects.

We might summarise the general argument as follows:

(P1) Being invariably at least weakly discernible is a necessary and sufficient condition for objecthood.

(P2) Fermions are invariably at least weakly discernible; the elementary bosons are not.

(C) Fermions are objects; the elementary bosons are not.

Let us grant the second premise: fermions, but not the elementary bosons, are always at least weakly discernible.<sup>1</sup> But what about the first premise? Why think that weak discernibility is a necessary and sufficient condition for objecthood?

The precise content of this question, unfortunately, is somewhat obscured by the fact that there is no real consensus among philosophers as to what the appropriate criteria for objecthood are: indeed, one might be tempted to read (P1) as having the status of a mere *definition* of the word “object” (i.e., as being, in some minimal sense, an “entity” which is always at least weakly discernible). Most theorists, however, take (P1) to have an implied content which is much more

---

<sup>1</sup>But see Muller & Seevinck (2009), who argue that the elementary bosons are invariably at least weakly discernible as well.

substantive: more specifically, they view Saunders as claiming that fermions' weak discernibility *grounds*, or "metaphysically explains", their status as numerically distinct entities, and, hence, as objects. (In this context theorists usually implicitly assume that numerical distinctness is a necessary and sufficient condition for objecthood.) Moreover, many of these theorists have criticised this proposal for its implicitly involving a dubious circularity: weak discernibility, they claim, rather than grounding the numerical distinctness of the relevant relata, illicitly presupposes it.

Steven French (2014, 40) has recently put this worry as follows:

... [A] circularity threatens: in order to appeal to such [irreflexive] relations, one has had to already individuate the particles which are so related and the numerical diversity of the particles has been presupposed by the relation which hence cannot account for it.

As French notes, this worry basically reprises a dispute which is very old — centuries, if not more — in the history of philosophy: that of whether relations *in general* are capable of accounting for the numerical diversity of their relata.<sup>2</sup> Many philosophers who take a position on this issue today are liable to claim that those who disagree are in thrall to mere metaphysical prejudice; indeed, some philosophers have even gone as far as to call into question the very intelligibility of the debate itself.<sup>3</sup> At best, then, it is a dispute — and hardly a novel one at

---

<sup>2</sup>Compare, for instance, French's remark with Bertrand Russell's (1903, 458), made in a similar context: "[T]wo terms cannot be distinguished in the first instance by difference of relation to other terms; for difference of relation presupposes two distinct terms, and cannot therefore be the ground of their distinctness."

<sup>3</sup>Those who endorse or at least appear to express some sympathy with this "circularity objection" to Saunders' (alleged) proposal include French & Rickles (2003, 228), French & Krause (2006, 170-1), Wüthrich (2009, 1048), Hawley (2009, 109-11), Esfeld & Lam (2011, 148), Caulton

that — which is unlikely to be resolved any time soon.

What many of these contemporary philosophers seem to have missed — or, perhaps, ignored — is that the notion of weak discernibility was never originally intended to have any specific impact on this debate. As I shall argue, Saunders' main motivation in (re-)introducing the term to philosophy was *methodological*, rather than metaphysical. In other words, weak discernibility's intended role was to serve as an essential aspect of a broader "logical aid" (Saunders 2003a, 291) for *interpreting* physical theories; it was not intended to have any particular bearing on the more robustly metaphysical question of whether relations, irreflexive or otherwise, are capable of grounding numerical diversity.

The remainder of this chapter thus serves a dual purpose: first, as an attempt to rehabilitate discussion of this methodological construal of weak discernibility; and second, as an attempt to make some tentative inroads into assessing this construal's overall tenability.

### 3.1 Methodology, Not Metaphysics

That Saunders sees weak discernibility as primarily serving a methodological as opposed to metaphysical function, and that he does not construe objects' weak

---

& Butterfield (2012, 50, fn 29), Arenhart (2013, 471), Dorato & Morganti (2013, 596), and French (2014, 40). Those apparently somewhat less sympathetic include Ladyman & Ross (2007, 137-8), Dieks & Versteegh (2008, 927), Frigg & Votsis (2011, 248), French & Ladyman (2011, 29), and Muller (2015, §3). For expressions of suspicion at the very intelligibility of the debate, see Ladyman, Linnebo, Pettigrew (2012, 164) and Pooley (MS, 100). To my knowledge, Pniower (2005, §2.4), Pooley (MS, 98) and Ladyman (2016, 201) are the *only* explicit exceptions to the current interpretative orthodoxy, according to which Saunders is understood as claiming that weak discernibility serves to ground objects' numerical diversity. Neither Pniower nor Pooley nor Ladyman, however, provide significant exegetical support for their preferred reading of Saunders; nor do they subject Saunders' methodological project to anything like the scrutiny it receives here.

discernibility as being that which serves to ground their numerical diversity, is, I take it, reasonably clear from what he writes in his original paper:

I do not suppose there is anything wrong with identity, taken in an irreducible sense.... The proposal, rather, is that in a situation in which we *do not know* what physical objects there are, but only, in the first instance, predicates and terms, and connections between them, then we should tailor our ontology to fit; we should admit no more as entities than are required that can be made out by their means. (Saunders 2003a, 292)

Similarly, elsewhere in the paper Saunders explicitly states that (“in the first instance”) he takes his version of the PII to be a “*methodological principle*” (289); a few pages later, he refers to his “method of *interpreting* [physical theory] in terms of objects” (293); and, perhaps most decisively, in the conclusion to the paper he claims to have “spoken throughout of the *interpretation* of theories in terms of objects” (304). (My emphasis in each case.)

How should we understand this proposal? As I read it, weak discernibility’s primary function for Saunders is to serve as part of a general method of “extracting” or “reading off” objects — and, in particular, *talk* of objects, using declarative sentences and standard first-order predicate logic (2003a, 290) — from physical theory. The obtaining of the relevant irreflexive physical relations is therefore meant to serve as the minimum (and sufficient) condition for when one may permissibly speak of there *being* objects in the appropriate sense; moreover, one should refrain from granting objecthood to those (putative) entities whose (alleged) numerical distinctness cannot be specified using the predicates

and terms drawn from physical theory alone.

The interpretative recipe that Saunders seems to be suggesting might therefore be usefully summarised as follows:

1. Begin with an “initially interpreted” **physical theory**  $T$ . This will include various interpreted physical predicates, terms, and *putative* objects.
2. See which putative objects are at least **weakly discernible** according to the various physical predicates that appear in  $T$  (stripped of identity).<sup>4</sup>
3. Take the putative objects that can be suitably discerned in this way to constitute (what we might call)  $T$ 's **genuine ontology**; putative objects which cannot be so discerned are thus not part of  $T$ 's genuine ontology.

I think it is not difficult to see, on reflection, that the methodological and metaphysical construals of weak discernibility are orthogonal to one another. For accepting — or rejecting — this interpretative package simply *does not bear* on the separate metaphysical question of what, precisely, grounds facts about numerical diversity. On Saunders' scheme, one begins with a collection of putative objects in the initially interpreted theory: “theories are born interpreted,” as he writes, hence “we have a rough and ready idea of the objects they are predicates of” (2003a, 290-1). The goal of Saunders' interpretative project is precisely to sharpen this “rough and ready” objectual interpretation to yield what is for him the theory's “genuine” physical ontology (2003a, 295). But it is a *further* question, separate from the interpretative scheme that I take Saunders

---

<sup>4</sup>Strictly speaking, for Saunders the relevant predicates must also satisfy the requirement of being *invariant under the symmetries* of the theory at hand (2006, 53). I will mostly slide over this (not insignificant) subtlety here.

to be proposing, *how it is* that the numerical diversity of these objects, putative or otherwise, is ultimately grounded — if indeed it is so grounded.

This point is critical to the argument of this chapter; and, because it is so apt to be misunderstood, it is worth repeating. On Saunders' interpretative scheme, one starts with the (plausible) assumption that from their very inception physical theories are construed as quantifying over a putative ontology. The central purpose of Saunders' methodology is to distil from this initial or naïve ontological interpretation the objects that the theory in question should be taken genuinely to quantify over. But — and this is the crucial point to note — the viability of Saunders' scheme is strictly compatible both with relations' being able to ground the numerical diversity of their relata, and with their not being so able. This is because, however the numerical diversity of a theory's putative or genuine ontology is established or grounded metaphysically, the fact remains that physical theories are invariably initially interpreted in terms of a putative ontology — which is all that needs to be the case in order for Saunders' methodological project to get up and running.

Let me illustrate this point with a simple example. Consider, again, the spherically-symmetric singlet state of two intrinsically identical fermions. As previously noted, the fermions in this state stand in the irreflexive relation “... has opposite direction of each component of spin to ...”. They are thus weakly discernible; hence, according to Saunders' proposed methodology, they are numerically distinct objects. Now, let us assume for the sake of argument that relations — and, as a corollary, merely weakly discerning relations — are incapable of grounding numerical diversity. Does this in any way impugn Saunders' suggestion that this state consists of two numerically distinct objects?

No. Granted, it follows from Saunders' proposed methodology *that* the fermions in the singlet state should be regarded as numerically distinct objects. But *what* exactly metaphysically explains, or grounds, such objects' numerical diversity is a question on which Saunders' interpretative scheme is entirely neutral. Attempting to ground the fermions' numerical diversity by appealing to their weakly discerning relations is just *one* way in which such distinctness might be established. For instance, one could — in principle — attempt to ground such objects' numerical diversity by appealing to each object's possession of some intrinsic, empirically transcendent property (e.g., a "primitive thisness" or "haecceity") — though this option would of course be anathema to the many theorists suspicious of the existence of such properties.<sup>5</sup> But there is another, arguably much more attractive, option available. This option would simply be to take the fermions' numerical distinctness as *ungrounded*: that is, as metaphysically *primitive*, and not standing in need of any further metaphysical explanation. The fermions in the singlet state may, in full accordance with Saunders' methodological program, simply be taken to be brutally distinct.<sup>6</sup>

In summary, then, the interpretative program that Saunders is plausibly advocating is logically distinct from the debate which appears to have consumed the majority of philosophers when it comes to discussions of weak discernibility.<sup>7</sup>

---

<sup>5</sup>Cf. French & Rickles (2003a, 223). Ladyman (2016, 201) also notes that it is not, in fact, entirely obvious why appealing to intrinsic properties to ground objects' numerical diversity is any less problematic than appealing to relations, "since one might just as well insist that in order for a property to be instantiated there must be a metaphysically prior individual, or there would be nothing to bear the property."

<sup>6</sup>This is the option that I take Saunders himself to endorse ("I do not suppose there is anything wrong with identity, taken in an irreducible sense...." (2003a, 292)). For further defence of the legitimacy of taking objects' numerical diversity as primitive, see Pooley (2006).

<sup>7</sup>Without wanting to be unpleasantly accusatory: I take *all* of the authors cited in footnote 3 above (apart from the exceptions noted therein) to be guilty of this misinterpretation of Saunders.

The viability of Saunders' interpretative scheme does not stand or fall with the thesis that relations may legitimately be taken to ground the numerical diversity of their relata — on the contrary, it stands quite apart from it.

Before moving on to assess the general viability of Saunders' interpretative program, I wish to make two important clarifications about the claims made thus far. Then I would like briefly to respond to some exegetical criticism.

First clarification: in claiming that weak discernibility should not be construed "metaphysically", *all* I mean is that weak discernibility should not be construed as being that which serves to ground objects' numerical diversity. The claim is not in any way intended to be understood as "anti-metaphysical", or as being in some sense directed "against" mainstream analytic metaphysics. Nor is the claim intended to be directed against the coherence of the grounding idiom in general. (For what it's worth, I have no gripe with either analytic metaphysics or the grounding idiom.) The point is simply that weak discernibility is not correctly construed as being in any way *related* to issues about ground, and in particular to issues about what grounds objects' numerical diversity: weak discernibility, I claim, is not to be understood "metaphysically" solely and precisely in *this* sense.

Second clarification: I similarly do not wish to be understood as claiming that, by construing weak discernibility methodologically as opposed to metaphysically, Saunders' project is thereby in some sense metaphysically sanitised, or "metaphysics-free". Indeed, as I shall argue in the sequel, Saunders' own methodological construal of weak discernibility very plausibly relies upon his having antecedently adopted certain quite substantive metaphysical assumptions. In other words, it is plausibly the case that weak discernibility, construed

methodologically, is dependent upon a significantly non-trivial — and by no means straightforwardly obvious, or ineluctable — metaphysical conception of what the world is like, and in particular of objectual structure's place within it. None of this, however, should be taken to contradict the central claim made above, namely that the “methodological” and “metaphysical” construals of weak discernibility are orthogonal to one another. For as I emphasised in the previous paragraph, *all* I mean by the “metaphysical” construal of weak discernibility in this context is the construal according to which weak discernibility is understood as being that which serves to ground, or metaphysically explain, objects' numerical diversity. *These* two construals of weak discernibility *are* orthogonal to one another, in the sense that adopting either such construal has no straightforward logical bearing on one's ability to adopt the other. But this claim of orthogonality is clearly very different from the claim that, methodologically construed, weak discernibility is orthogonal to metaphysics *tout court*.

With these two points of clarification out of the way, let me now discharge one important worry. In particular, I would like to pre-empt the criticism<sup>8</sup> that my quotations drawn from Saunders' (2003a) paper were selective by briefly examining other passages in the paper which, at least *prima facie*, appear to lend themselves to a more thoroughly metaphysical construal of the role weak discernibility is supposed to play in Saunders' system. (Those uninterested in such exegetical concerns should feel free to skip to the next section.)

There are two such passages:

- (i) What is wrong with identity taken as primitive? In the most general context, I see nothing wrong with identity. But in physics —

---

<sup>8</sup>Thanks to an anonymous referee for encouraging me to address this criticism.

specifically identity as it figures in physical *theory* — there are special reasons to view it as derivative. (290)

(ii) ... [C]onsider Black's two iron spheres, one mile apart, in an otherwise empty space (Black 1952). The irreflexive relation  $A$  is '... one mile apart from ...'. It is *because* this relationship holds that we may say that there are two — that it is intuitively evident that there are two. (294)

The arguably quite natural thought is that by “derivative” in (i) Saunders means *metaphysically* derivative: that he is saying that in physics we have “special reasons” to think that facts about objects’ identity and numerical diversity cannot be taken as metaphysically primitive, but must instead be grounded in weak (or stronger) forms of discernibility. Similarly, the thought is that Saunders’ use, and emphasis of, the word “because” in (ii) suggests the metaphysical construal of weak discernibility: that he is saying that it is the spheres’ weak discernibility which *metaphysically* explains, or grounds, their numerical diversity.

I think, however, that such a reading of both passages can be legitimately resisted. With regard to (i): there is no compelling reason, I think, to take “derivative” here to mean metaphysically as opposed to methodologically derivative (in a sense to be explained presently). For if one looks at the actual context in which this quote occurs, one sees that Saunders is making two claims about the identity relation in physics: first, that as it features in physical theory the identity relation is not a straightforwardly measurable physical quantity (in the same way that, e.g., mass and charge are); and second, that the identity relation as it features in physical theory invariably only represents the equality of

mathematical expressions — not of objects. Saunders is, I think, thus plausibly construed in this passage as claiming that the identity relations which hold between a given physical theory's genuine objects are "derivative" merely in the sense that *they are not part of the theory's initial interpretation*; that such identity claims must be (metaphorically-speaking) teased out, extracted — *derived* — using the *interpretative* method that he is proposing.<sup>9</sup>

What about (ii)? Admittedly, this passage does not lend itself as naturally to a methodological (re-)construal: the issue of how to correctly interpret physical theories in terms of objects would appear to be orthogonal to the question of whether, and in virtue of what, Black's two spheres are genuinely numerically distinct. Nevertheless, it is implausible to think that the *only* way of reading this passage is by understanding it as making a metaphysically robust claim about how the numerical diversity of Black's two spheres is ultimately grounded. For a much more deflationary reading of this passage is also possible: on this deflationary reading, Saunders is simply making the rather more mundane claim that the two spheres' satisfying the relevant irreflexive relation *makes it plausible to say* ("intuitively evident") that there are two. In other words, Saunders may be plausibly read as claiming that, at least in the Blackian case, the interpretative scheme he is proposing *lines up with our intuitions* about how many objects there are. (Conversely, were such spheres *not* even weakly discernible — but were instead, e.g., co-located for all eternity — then presumably it would be "intuitively evident" that the scenario is most accurately described as containing only *one*

---

<sup>9</sup>Moreover, that a mere two sentences after passage (i) Saunders explicitly claims that his desideratum is for a "*clear interpretation of ... theories and experiments in terms of physical objects*" (my emphasis) further suggests to my mind a reading according to which Saunders intends "derivative" here to be construed only in the methodological, not metaphysical, sense.

such sphere.) Saunders may therefore be construed in this passage as arguing more straightforwardly for the intuitive plausibility of weak discernibility *qua* criterion in adjudicating, and perhaps also in explaining our intuitions, about how many objects there are in a given situation; he need not be understood as claiming that weak discernibility is itself metaphysically explanatory of numerical diversity.

Now, I should perhaps say that I do not take the comments made in the previous two paragraphs to be fully conclusive, or irrefutable. (Irrefutability is, after all, an impossible standard to achieve in any work of exegesis.) Certainly, there is a *possible* reading of Saunders which views him as being committed *both* to regarding weak discernibility as an aid in interpreting the ontology of physical theories *and* as being that which serves to ground objects' numerical distinctness; indeed, I strongly suspect that the majority of theorists (including those referenced in footnote 3 above) who read Saunders as attempting to ground objects' numerical diversity do so precisely because passages (i) and (ii) above are, at least superficially, quite suggestive of such a reading. My point is simply that such a reading is not at all compellingly supported by anything he writes — at the very least, it is one that is far from ineluctable.<sup>10</sup>

In any case, my central contention remains the same: the *clearly intended* methodological construal of weak discernibility has thus far been neglected in the philosophical literature. Moreover, given both the proposal's patent philosophical novelty, as well as its orthogonality to the much more widely

---

<sup>10</sup>For what it's worth, Saunders (personal communication) has confirmed to me that it was always his intention for weak discernibility to be construed methodologically, rather than metaphysically — though, of course, this does nothing by itself to alleviate the (alleged) textual difficulties which arise in interpreting his (2003a) paper as suggesting such a purely methodological construal (i.e., passages (i) and (ii) above).

discussed (and historically rather jaded) metaphysical construal, it is one which, in my view, finally deserves our scrutiny.

## 3.2 Three Questions

Several questions naturally arise in considering this interpretative account. To keep our discussion manageable, I shall mention only what I consider to be three of the more interesting ones here:

- **The Naïve Question.** What, exactly, is wrong with the “naïve” approach to reading off ontology from one’s theory? That is, why not simply take the *putative* objects — e.g., fermions *and* bosons — of one’s theory as representative of the world’s actual ontology?
- **The Epistemological Question.** On what legitimate basis do we decide that Saunders’ interpretative proposal is in fact the correct, or best, way of interpreting physical theories in terms of objects?
- **The Metaphysical Question.** What kind of metaphysics — of objecthood, and the world more generally — motivates the interpretational proposal Saunders is suggesting? And how plausible is this metaphysics?

In the following three sections we shall consider each of these questions in turn. We begin with the Naïve Question.

### 3.3 The Naïve Question

What is wrong with the “naïve” approach to theory interpretation? To take the specific case of quantum theory once again: why not “naïvely” read off the ontology of this theory as being comprised of both fermions *and* bosons?

Saunders does not provide any extensive criticism of interpretative naïvety in his original paper (nor, to my knowledge, elsewhere). He merely states that there would be “nothing systematic” to such an interpretative approach, and that it is therefore “plausible” that we should look for a way (namely, his own) of reading off a theory’s ontology which does have some such claim to systematicity (2003a, 291).

This criticism is, I take it, far from compelling. Indeed, the claim itself doesn’t even appear to be correct: plausibly, interpretative naïvety *is* systematic — it is systematically naïve! However, to criticise Saunders on the basis of his failure to discredit interpretative naïvety would, I think, be unfair. For he is in my view best read not as attempting to offer a detailed *criticism* of interpretative naïvety. Rather, I think he is best read as simply proposing an interpretative method which purports to offer an *improvement* over — is, in some sense, “better” than — interpretative naïvety. The relevant question then becomes: Why, systematicity aside, should we think his method is better?

For Saunders, it would seem that the primary reason for thinking that his method is better is straightforward: namely, that it rules *out* the elementary bosons *qua* genuine objects, but nevertheless rules *in* all fermions (and particles with fermionic constituents). As he writes:

The stable constituents of ordinary matter are all fermions. Apart

from the Higgs particle ... all elementary bosons are gauge quanta; they all mediate forces between fermions.... [T]he PII treats fermions quite differently. Given the contrast between [bosons and fermions], as gauge fields and sources respectively, *it is a merit of the principle that it does*. (Saunders 2003a, 295. My emphasis)

This claim of merit, however, can be legitimately questioned. To be sure, the elementary bosons are generally fermionic force-mediators. But why should this fact give us any reason to question their status as genuine objects? Indeed, unless adequate justification is provided in support of the claim that Saunders' interpretative scheme (rather than some other) is *in fact* the correct, or best, way of interpreting physical theories in terms of objects, *neutrality* on the issue of whether the elementary bosons are genuine objects would seem to be the stance that is most naturally called for. Absent any such justification, however, Saunders' claim of merit seems, at best, premature.

This, of course, brings us to our second question.

### 3.4 The Epistemological Question

This is precisely the question (just alluded to) of what the relevant criteria for a successful objectual interpretation of a theory are supposed to be. More precisely, it is the question of how we are to legitimately decide when and whether any such proffered interpretation is actually *correct*.

I think we can gain some insight into how Saunders is thinking about this question by examining a particularly revealing footnote:

... [I]f I am concerned with metaphysics at all, it is *descriptive* metaphysics, in Strawson's sense, as an aid to the interpretation of physics, and to that end I aim to preserve a good part of established practice. Ordinary objects had better turn out to be objects, on any account, and so they do on mine; it is as an extension from this that their stable constituents had better turn out to be objects as well. With the rest there is more latitude. (Saunders 2003a, 295, fn 7)

Recall that, for Strawson (1959, 9), "Descriptive metaphysics is content to describe the actual structure of our thought about the world." I thus take Saunders' claim here to be that *what ultimately justifies the use of his methodological scheme is the fact that "ordinary" objects, such as tables, chairs, etc., are invariably at least weakly discernible*. In other words, I take his suggestion to be that the proposed minimum and sufficient objectual condition of weak discernibility is sufficient to *describe* our ordinary ontological scheme (in that every object of our ordinary ontological scheme is invariably at least weakly discernible by some physically salient predicate); moreover, it is *because* the objects of our everyday ontological scheme are invariably at least weakly discernible which ultimately justifies our taking weak discernibility as a minimum and sufficient criterion for objecthood, and which in particular justifies our using weak discernibility as part of a broader logical aid for determining what objects there are according to a given physical theory.

Arguably, Saunders' claim to be engaging in descriptive metaphysics has some initial plausibility: for tables, chairs, and many other ordinary objects besides, very plausibly *are* invariably (at least) weakly discernible by some

physically salient relation — for instance, by some spatiotemporal relation — from all other such objects.<sup>11</sup> Nevertheless, the claim is not without its problems. I shall mention two such difficulties here, one of which I think is defeasible, the other less so.

The first, defeasible, objection is that our everyday ontological scheme would *prima facie* appear to contain some examples of objects that are not even weakly discernible by any physically salient predicate. As Hofstadter & Dennett (1981, 6-7) have pointed out, voices, languages, haircuts, symphonies and the game of bridge would all appear to be straightforward examples of “things that are neither mysterious [nor] ghostly” which populate our everyday ontological scheme, but which nevertheless are not obviously identifiable with nor reducible to anything describable in the language of fundamental physics. In particular, it is difficult to see what *physically* salient irreflexive relation any two (e.g.) symphonies will invariably satisfy: after all, which physically salient irreflexive relation weakly discerns, say, Tchaikovsky’s 5th and 6th Symphonies? (Not any two particular *performances* of the symphonies, mind — for these could presumably be discerned spatiotemporally — but rather the symphonies *simpliciter*?) Moreover, is it not true that such entities constitute part of our everyday ontological scheme, one which any attempted “descriptive metaphysics” should be expected to recover? Of course, Saunders might respond: so much the worse for our ordinary ontological scheme. To which the obvious reply is: so much the worse for Saunders’ claim to be doing *purely* “descriptive” metaphysics.

I think, however, that Saunders might well have a plausible response to

---

<sup>11</sup>In general, of course, such objects will be “absolutely” discernible, in the sense that they differ with respect to some physically salient intrinsic or relational property.

this objection: in particular, I think he could argue that his claim to be doing descriptive metaphysics was always intended to have restricted scope. For given that his manifest interest in his paper is with *physics*, and in particular with interpreting physical theories in terms of objects, I think he might have legitimate grounds for claiming that his descriptive-metaphysical enterprise was solely intended to recover commonsense *physical* ontology, not commonsense ontology *simpliciter*. And, indeed, although it is true that the kinds of objects mentioned above are not naturally thought of as being *purely* abstract objects (on a par with, say, numbers), it is nevertheless similarly natural *not* to think of them as constituting *physical* objects (as Hofstader & Dennett (1981, 7) themselves point out). Saunders, then, might justifiably claim that it is no mark against his physically-oriented descriptive-metaphysical enterprise that it might fail to recover them.

There is, however, a second — and, I think, less defeasible — problem with Saunders' claim to be doing purely descriptive metaphysics: namely, that it would appear to do nothing to justify his claim (noted in the previous section) that his scheme's ruling out the elementary bosons *qua* physical objects constitutes a virtue of it. For while Saunders is plausibly right in saying that the stable constituents of ordinary physical entities such as tables and chairs should count as objects according to any attempted objectual (and descriptive-metaphysical) interpretation of a given physical theory, and while he is also plausibly correct in claiming that there is more "latitude" in determining the elementary bosons' status as genuine objects given the fact that they are generally gauge quanta (2003a, 295, fn 7), it is nevertheless difficult to fathom why the fact that the elementary bosons *aren't* counted as objects according to one such scheme should

be seen as beneficial. (Consider: if the elementary bosons *were* granted objectual status according to this or some similar interpretative scheme, would this fact thereby count against it?) Neutrality is what appears to me to be what is most naturally called for; Saunders' claim of merit seems unwarranted.<sup>12</sup>

### 3.5 The Metaphysical Question

Let us now ask the question concerning what kind of metaphysical framework — and, in particular what kind of metaphysical conception of objecthood — is underwriting the interpretational program that Saunders is advocating.

There are several different ways of approaching this question. Here is one. Assume, for the sake of argument, that we have a theory which we have good reason to believe is true, or at least approximately true. Assume, further, that the world comes equipped with a mind-independent, conceptual-scheme independent fundamental ontology. (That is, assume that the world fundamentally contains *things*.) And now assume that we're in the process of trying to interpret this theory objectually, to figure out what the ontology of the world actually is. The relevant question is: *Does the fundamental ontology of the actual world and the ontology yielded by adopting Saunders' interpretative method line up?* To take

---

<sup>12</sup>Saunders (personal communication) has suggested that his interpretative scheme's ruling out the elementary bosons as genuine objects might plausibly be regarded as a virtue because it demonstrates that the minimal objectual requirement of weak discernibility is not trivially satisfied by every putative physical entity. Perhaps he is right that this is a positive feature of his method. (Though note that, according to Muller & Seevinck (2009), the minimal objectual requirement of weak discernibility plausibly *is* trivially satisfied by every putative physical entity — for they argue that the elementary bosons can in fact be weakly discerned as well.) Nevertheless, why this apparent non-triviality should be taken to constitute an advantage of Saunders' interpretative scheme *in comparison to other such schemes* (e.g., interpretative naïvety) remains opaque, at least to me.

quantum theory once again as an illustrative example: Does the world fundamentally only contain fermions, but not bosons, as genuine entities; or does it contain both?

Absent any antecedently justified reason for thinking that Saunders' proposed methodological scheme is in fact the correct way of interpreting physical theories in terms of objects, it is (to say the least) extremely difficult to see how one might attempt to answer this question. Fortunately for Saunders, however, it would seem that he need not respond to it — for his conception of objecthood and, more generally, his metaphysics, allows him to avoid it. As he has written in a recent paper:

It may even be that the world is at bottom a mathematical structure, or 'has' a mathematical structure.... *Objectual structure ... I see as a coarse-graining of the mathematical structure of the world: the pegs and poles that gather its materials and most reliably tie them together.*<sup>13</sup>

(Saunders 2016, 166. My emphasis)

For Saunders, then, what his interpretative scheme is supposed to yield is not the "true" ontology of the world in any metaphysically robust sense: strictly speaking, there is no such thing. Rather, what it yields is a description of the world *in terms of* objects and their properties; it is an approximate or "blurry"

---

<sup>13</sup>Compare also:

The world is a structure, and it is thought of as such in exact physical, interpreted mathematical terms, but how it is to be broken down into parts, to be spoken of predicatively, can be a more rough and ready affair, sufficient only in the sense of FAPP, to use Bell's acronym; sufficient linguistically, but only for all practical purposes. (Saunders 2003b, 132)

My reading of this and the above passage — according to which Saunders does not believe that the world has any fundamental ontology — has also been confirmed to me privately by Saunders. (Thanks to an anonymous referee for pressing me on this point.)

description of a world whose structure is by nature mathematical, or more precisely of a world whose structure is most perspicuously described in the language of mathematical physics, rather than in objectual terms (i.e., in terms of objects and their properties). Crudely put, then, the job of the theoretical physicist is to delineate this mathematical structure as perspicuously as she can, using physical equations; the job of the (descriptive) metaphysician is to break this structure down, as perspicuously as *she* can, into objectual terms.

There are many interesting issues and questions that arise in considering this metaphysical proposal. Again, to keep our discussion manageable, I shall only mention three of what I take to be the most compelling ones here.

First, one might wonder whether there are alternative objectual interpretative proposals, distinct from Saunders' own, that are similarly capable of offering a decent enough, or better, coarse-grained description of fundamental reality. That is, granting the fact that the world is most perspicuously described in the language of mathematics and that it can only be described in a more-or-less coarse-grained fashion in terms of objects and their properties, one might ask what reason we might have for thinking that describing the world in terms of objects that are at a minimum weakly discernible will offer a better, or more faithful description of this underlying reality than a description of the world in terms of objects (or "objects") that aren't. To take the case of quantum theory again: why think that describing the world as presented by quantum theory in terms of fermions, but not bosons, *qua* genuine objects offers us a more faithful coarse-grained description of the world than a ("naïve") objectual description in terms of both?

The answer is not, I think, immediately obvious. One might initially be

inclined to think that Daniel Dennett's (1991) influential criterion of what a suitably "coarse-grained" description of fundamental reality should be — one which has since been eloquently elaborated on and defended by David Wallace (2012, Ch 2) — might be of help here.<sup>14</sup> On Dennett and Wallace's view, an emergent ontology should be regarded as "real", or constitutes a "real pattern", to the extent that it in some way contributes to the explanatory or predictive power of various scientific theories which admit such entities as part of their ontology. So, for instance (to use Wallace's example), tigers are real, albeit emergent, objects in virtue of the fact that by positing them as genuine entities we greatly increase the explanatory and predictive success of a number of theories across the biological sciences. And it is for this reason that, according to Dennett and Wallace, a suitably coarse-grained description of the world which construes tigers as genuine objects should be taken to provide us with a more faithful coarse-grained description of fundamental physical reality than one which does not.

Unfortunately, however, Dennett's criterion would appear to fail to have any substantive application in our case of interest (namely, quantum mechanics). For here, recall, we have two candidate coarse-grained ontologies, one of which includes both fermions and bosons, the other of which includes only fermions. Now, is there any extant theory, either in the special sciences or elsewhere, which admits only fermions as elements of its ontology, but not bosons? Or is there any such extant theory in which removing any commitment to bosons, but not fermions, as objects would nevertheless allow the theory to retain all of its explanatory or predictive power? As far as I can tell, the answer to

---

<sup>14</sup>Compare also the discussion in Ladyman & Ross (2007, Ch 4).

both questions is *no*. Rather, it would seem, scientific theories either fail to quantify over *both* fermions and bosons in the first place (as in, for instance, theories in the biological sciences), *or* they indispensably quantify over both, to an essentially equal extent (as in, for instance, the Standard Model of particle physics).<sup>15</sup> Thus, I submit, Dennett's criterion plausibly fails to provide any support for the view that an ontology composed exclusively of fermions offers a better coarse-grained description of the structure of the world than an ontology composed of both fermions and bosons.<sup>16</sup>

Second (and relatedly), given that on Saunders' proposal the world is most perspicuously described in the language of mathematics, it is at the very least not immediately obvious what the "robust" metaphysical significance is of his denial of the status of objecthood to the elementary bosons. Would we really be flat-out "wrong" — as Saunders (2006, 60) suggests we would be — if we thought of bosons as genuine objects, rather than merely as modes of the corresponding quantum field? And, if so, what exactly would be wrong about it? Presumably, the answer to this question is that thinking of bosons in objectual terms would lead to a *worse* coarse-grained description of the world than it would were we not thinking of bosons objectually. But, again, it is at best unclear why we should think this is true. Indeed, absent an adequate — and presumably non-Dennettian — account of what a satisfactory "coarse-grained" objectual interpretation of

---

<sup>15</sup>As an anonymous referee for this paper has usefully pointed out to me, quantum field theory in general makes no distinction between fermions and bosons with respect to their being "excitation number[s] of a certain mode of a quantum field" (Saunders 2003a, 295).

<sup>16</sup>Of course, it is perhaps conceivable that an objectual description of the world in terms of fermions just *is*, in some metaphysically brute and unknowable sense, a much more faithful coarse-grained description of reality than an objectual description in terms of both fermions and bosons. The pertinent question is: What reason do we have to think that such a possibility — even if conceivable — is actually true?

the world is meant to be, the worry inevitably arises as to whether *anything* of genuine significance hinges on Saunders' claim that fermions, but not the elementary bosons, are "genuine" objects (other than, perhaps, our refrainment from *calling* the latter "objects").

Third and finally, one might ask how Saunders' proposed methodology fits into the broader philosophical context in which it is frequently discussed, namely ontic structural realism (OSR).<sup>17</sup> So: What is the connection between Saunders' method and OSR? The answer, somewhat unsurprisingly, depends on how exactly we construe OSR. Here are three versions of the view:

- **Eliminativist OSR:** There are no objects, fundamental or otherwise.<sup>18</sup>
- **Emergentist OSR:** There are no fundamental objects; however, objects do exist emergently, at a suitably coarse-grained level of description.<sup>19</sup>
- **Grounding OSR:** Whatever objects there are, their numerical diversity is grounded in the relations they bear to one another.<sup>20</sup>

It is easy to see that Saunders' proposed methodology is essentially incompatible with Eliminativist OSR: namely, for the reason that (e.g.) fermions turn out to be genuine objects according to his method. Moreover — and as previously discussed — Saunders' method is entirely neutral on the question of whether relations in fact ground objects' numerical diversity: that is, it is neutral on the issue of whether Grounding OSR is true.

<sup>17</sup>See, e.g., Ladyman & Ross (2007, 137-8), Rickles (2008, §5.3.3), and French (2014, §2.8). (Thanks to an anonymous referee for pressing me on this question.)

<sup>18</sup>This is the view that I take, e.g., French (2014; see esp. Ch 7) to endorse.

<sup>19</sup>This is the view that I take, e.g., Ladyman & Ross (2007; see esp. 131) to endorse.

<sup>20</sup>For further discussion of this and other versions of OSR, as well as further references to the relevant literature, see Ladyman (2014, §4.)

Saunders' method, however, arguably finds a quite natural home in Emergentist OSR: indeed, we have seen that Saunders himself appears to subscribe to precisely this version of OSR. Thus, for the Emergentist OSR-ist, Saunders' scheme could be said to yield precisely those objects which exist only at a suitably emergent level of description. Nevertheless, it should be noted that the Emergentist OSR-ist is — at the very least — by no means logically *compelled* to adopt Saunders' proposed methodology for yielding such an emergent objectual description of the world: for there are other *prima facie* plausible (e.g., Dennettian) ways of cashing out what the relevant criteria are for such an emergent objectual description. Emergentist OSR thus does not necessarily stand or fall with the success or otherwise of Saunders' interpretative method.

There is, then, no straightforward answer to the question of whether Saunders' methodology ultimately speaks for, or against, OSR: the answer depends too heavily on precisely which version of OSR one is considering. Nevertheless, two things can be said with confidence: first, not every version of OSR has its fate inextricably tied to that of Saunders' interpretative method; and second, not every version of OSR has its fate inextricably tied to that of every other version of OSR. In particular — and to note a point of particular relevance to the arguments of this paper — the (alleged) failure of Grounding OSR does nothing to impugn Saunders' methodological project; and nor does it in any way call into question those versions of OSR which are happy to accept the numerical diversity of objects, fundamental or otherwise, as primitive.<sup>21</sup>

---

<sup>21</sup>For an example of just such a version of (what the authors label "moderate") OSR, see Esfeld & Lam (2008, esp. 33-4).

### 3.6 Conclusion

In summary, I have done two things in this chapter.

First, I have argued that the originally intended philosophical significance of weak discernibility has, thus far, been seriously misunderstood in the literature. Moreover, I have argued that, construed as originally intended, the notion of weak discernibility avoids — indeed, is orthogonal to — a common (“circularity”) objection often levelled against it. Thus, I have argued weak discernibility is properly understood only “methodologically”: that is, as playing an essential role in a broader scheme of *interpreting* physical theories in terms of objects; it is not correctly (i.e., “metaphysically”) construed as being that which serves to ground objects’ numerical distinctness.

Second, I have tried to make some tentative inroads into assessing this methodological proposal’s overall tenability. More specifically, I have argued that: (i) Saunders has yet to provide any genuinely compelling grounds for thinking that his proposal offers a significant improvement over “interpretative naïvety”, according to which one regards a given physical theory’s *putative* ontology (i.e., the ontology as conceived in the theory’s initial or “naïve” interpretation) as its true or “genuine” ontology; and (ii) Saunders’ proposal plausibly relies upon his having adopted several highly non-trivial or non-transparent assumptions about what the world is like, and in particular about objectual structure’s place within it.

As should be clear from the preceding discussion, I do not claim that any of my questions or criticisms are unanswerable. But what I do claim is that such questions and criticisms — rather than common concerns about the ability of

relations to ground the numerical diversity of their relata — are at least *genuinely relevant* to the notion of weak discernibility, at least as originally construed by Saunders. Moreover, they are in my opinion much more worthy of discussion than the questions which usually revolve around discussions of weak discernibility in the contemporary literature — questions which, arguably, were already discussed to death in the metaphysics literature over a century or so ago.

## Chapter 4

# Some General Worries, Discharged

Take *generalism* to be the view according to which the world is fundamentally purely qualitative in character. That is, generalism claims that a language devoid of proper names and predicates that make implicit or explicit reference to particular individuals (e.g., *pegasizes*) could, at least in principle, suffice to give a complete and perspicuous description of fundamental reality.<sup>1</sup> It is a popular view;<sup>2</sup> but it also faces a popular set of objections. In brief, these objections involve (at some point in the relevant line of argument) an appeal to the possibility of worlds which (at some stage in their respective histories) violate a version of the Principle of the Identity of Indiscernibles (PII). In this chapter, I aim to provide a systematic analysis and appraisal of each of these “PII-involving” objections to generalism. My conclusion will be that none of them provide anything like a compelling refutation of the view. Thus, whatever problems generalism may face, I claim they are independent of any consideration of the PII-violating worlds in question.

---

<sup>1</sup>See, e.g., Adams (1979, §1) for a similar statement and elaboration of the view.

<sup>2</sup>See, e.g., Saunders (2003a), Dasgupta (2009), and Pooley (MS) for recent endorsements.

The structure of this chapter is as follows. In section §4.1 I discuss, and dismiss, what is perhaps the most common and straightforward objection to generalism: namely, that it rules out the possibility of worlds containing qualitatively indiscernible individuals. Building on this discussion, sections §4.2, §4.3, §4.4, and §4.5 will examine objections of varying degrees of sophistication which rely, at least to some extent, on generalism's alleged treatment of certain "symmetry-breaking" (and PII-violating) worlds as deterministic which, intuitively, are not deterministic. I claim that none of these objections ultimately succeed. Moreover, in section §4.6 I argue that there is in fact at least one plausible conception of determinism according to which such worlds are *not* treated as deterministic by the generalist after all. Finally, section §4.7 will examine, and reject, a recent argument put forth by Boris Kment (2012) which suggests that generalism (or at least PII-violating versions thereof), when combined with other *prima facie* plausible assumptions about chance and counterfactual conditionals, yields absurd consequences. I claim to the contrary that the generalist may plausibly avoid the conclusion of Kment's argument in a number of ways.

## 4.1 The Standard Objection

We begin with the most well-known and commonly-cited objection to generalism.<sup>3</sup> It runs as follows:

(P1) Generalism entails that possible worlds containing individuals sharing all of their intrinsic and relational qualitative properties are metaphysically impossible.

---

<sup>3</sup>This objection is endorsed by Adams (1979).

(P2) Worlds containing individuals sharing all of their intrinsic and relational qualitative properties are, in fact, metaphysically possible.

(C) Generalism is false.

The generalist, I take it, would not want to deny (P2). (Such worlds are, perhaps, *physically* impossible. But that's irrelevant in this context.) To avoid the argument's conclusion, then, the generalist must deny (P1). May she legitimately do so? The answer is that she can, for at least two reasons.

First, note that the Standard Objection only applies to generalism construed as a thesis which purports to hold of metaphysical necessity; it is utterly toothless as a refutation of generalism construed merely as a *contingent* thesis about the metaphysics of our actual world. An analogy here might prove helpful. Suppose, for the purposes of argument, that our actual world is general relativistic. Does the fact that there plausibly are metaphysically possible worlds at which Newtonian gravitation theory is true stand in contradiction to this supposition? No. The fact that general relativity is the true and complete theory of our *actual* world does nothing to curtail Newtonian theory's being a *metaphysically* possible way for the world to be. (At best, general relativity's actual truth might be taken to rule out Newtonian theory as a genuine *physical* possibility.) Similarly: generalism's actual truth by itself has no bearing on the question of whether there are metaphysically possible worlds where it is (allegedly) false.<sup>4</sup>

---

<sup>4</sup>One might object to the above analogy. For given that generalism, unlike general relativity, is a *metaphysical* (rather than merely "physical") thesis, does it not *trivially* follow that it is true at all metaphysically possible worlds? In one sense, perhaps, the answer is "yes": the only possible worlds "compatible with the metaphysics" of our actual world will be worlds where generalism is true. But in another sense, the answer is clearly "no": if generalism is construed as a metaphysically *contingent* thesis, then there will be (by definition) possible worlds at which it is not true — *this* is the crucial point to note. (Whether such worlds are properly regarded as "genuinely" metaphysically possible or not is largely a semantic issue: either way, the intuitive

Second, the allegation is false anyhow. For it is simply not true that generalism — *even construed as a metaphysically necessary thesis* — rules out the possibility of worlds containing qualitatively indiscernible individuals. Take, for instance, Max Black's (1952) imagined world of two qualitatively indiscernible iron spheres, which remain two miles apart from one another forever in a relationalist universe. As Simon Saunders (2003a) has pointed out, such spheres are still *weakly discernible*, in the sense that they each satisfy an irreflexive formula in two free variables (namely, the relation "... is two miles apart from ..."). Each of Black's spheres stands in this relation to the other sphere, but not to itself. But these facts together imply that there are at least two spheres. Moreover, such facts do not require proper names or nonqualitative predicates to be stated: they are purely qualitative in character.<sup>5</sup>

But what of worlds in which the individuals in question are not only qualitatively indiscernible, but also not even weakly discernible — for instance, worlds in which the spheres in question are co-located for all eternity? Does the generalist have a problem characterising such worlds? A quick response here might be to claim that such worlds do not after all contain distinct individuals. (Thus, in the "co-located" version of the Max Black world, one might plausibly argue that such a world is most accurately described as containing only one sphere.) A more concessionary response, however, is also available: namely, that nothing precludes the generalist from taking the *identity predicate* as primitive. Qualitatively indiscernible individuals may simply be taken by the generalist to be brutally distinct. (Thus, in first-order predicate logic, a world containing two

---

force behind the argument is blocked.)

<sup>5</sup>Cf. Pooley (MS, 97).

qualitatively indiscernible individuals that are not even weakly discernible may be described as  $\exists x \exists y (Fx \& Fy \& x \neq y)$ , where  $F$  is short for some conjunction of qualitative predicates.)<sup>6</sup>

In short: nothing in fact prohibits the generalist from making sense of worlds containing qualitatively indiscernible individuals. This, of course, does not entail that worlds containing qualitatively indiscernible individuals cannot pose a *prima facie* problem for generalism. Indeed, as we shall see over the next few sections, there are many arguments that may plausibly be wielded against the generalist which make essential use of, and in fact even *exploit*, generalism's compatibility with worlds containing such indiscernible individuals.

## 4.2 The Naïve Objection

Let us now imagine a variant of the Max Black world (cf. Adams 1979, §5). This world initially (i.e., at  $t = 0$ ) consists of nothing except two qualitatively indiscernible iron spheres, two miles apart from one another in a relationalist universe. The sole law of this universe is that, at  $t = 13$  minutes, one of the spheres will be annihilated, leaving the remaining sphere to persist for all eternity. Nothing else ever happens. Call this world  $W_1$ .

Intuitively,  $W_1$  is an indeterministic world: more specifically, it is indeterministic with regard to *which* one of the two spheres will be destroyed at  $t = 13$  minutes. On the generalist picture, however,  $W_1$  is (*prima facie*) fully deterministic.<sup>7</sup> To see this, consider how one might *specify* the way in which  $W_1$  is

<sup>6</sup>Cf. Cover & O'Leary-Hawthorne (1999, 156-7), Dasgupta (2009, 48), and Kment (2012, 579-80).

<sup>7</sup>As we shall see in §4.6, not all generalists subscribe to a conception of determinism according

indeterministic. As a little reflection shows, the only way to do this would be to say that  $W_1$  is indeterministic with regard to which *particular* sphere will be annihilated at  $t = 13$  minutes. But, of course, any such apparent fact would evidently be singular — it would concern some particular sphere — and would therefore not be a fact that the generalist will countenance as genuine. Thus, it seems, for the generalist the initial state of  $W_1$ , plus its sole law, fixes the complete (qualitative) evolution of  $W_1$ 's history. Hence,  $W_1$  is naturally classified by the generalist as an example of a fully deterministic universe.

We are now in a position to state our first argument against generalism which exploits this counter-intuitive consequence of the view. Somewhat tentatively, let us call it the *Naïve Objection*. It runs as follows:

(P1) Generalism's commitment to regarding  $W_1$ -type worlds as deterministic is deeply counter-intuitive.<sup>8</sup>

(P2) Such counter-intuitiveness is unpalatable; it gives us a strong reason to think that generalism is false.

(C) We have a strong reason to think that generalism is false.

(P2) is, of course, by far the most controversial of the two premises. Why should we think that the mere *counter-intuitiveness* of a given metaphysical view gives us any reason to reject it? After all, theories with counter-intuitive consequences abound in physics. For what reason should we not legitimately expect them to abound in metaphysics as well?<sup>9</sup>

---

to which  $W_1$  is properly construed as deterministic. Pooley (MS) is one such example.

<sup>8</sup>For the sake of argument over the next few sections, we will simply grant the assumption that generalism is committed to regarding  $W_1$ -type worlds as deterministic. (See fn 7 above.) We will subject this assumption to scrutiny in §4.6.

<sup>9</sup>Indeed, according to one influential (albeit controversial) way of arguing for generalism

(P1) is, perhaps, a more difficult premise to question. Surely generalism's commitment to regarding worlds like  $W_1$  as deterministic *is* counter-intuitive? As Carolyn Brighthouse (1997, 475) has emphasised, however, a significant amount of caution should be exercised in making this judgement. As she notes, it is not implausible to suspect that our intuitions about whether worlds like  $W_1$  are deterministic "are influenced quite significantly by considering its less symmetric relatives" — that is, by considering worlds which contain some minor *asymmetry* as part of their initial conditions and which are therefore straightforwardly regarded *even by the generalist* as being genuinely indeterministic.<sup>10</sup> Thus, for instance, in imagining  $W_1$  it is possible that we invariably unwittingly introduce — in our "mind's eye" — some asymmetry into the world's initial state: for instance, we might have a tendency to illicitly imagine a human observer much like ourselves "in"  $W_1$ , perhaps some distance away from the two

---

due to Shamik Dasgupta (2009), generalism is (or purports to be) argued for on almost identical grounds to which in physics one usually argues for Galilean spacetime, as opposed to Newtonian spacetime, as constituting the more appropriate spacetime setting for Newtonian gravitation theory: namely, on the basis that by adopting the relevantly "superior" theory one eliminates undetectable and/or redundant structure — *singular facts* in the move to a generalist metaphysics, *absolute velocity* in the move to Galilean spacetime — that appears in the "inferior" theory. Arguably, the move to Galilean spacetime requires a deeply counter-intuitive reconceptualisation of the nature of spatiotemporal structure (e.g., in Galilean spacetime it is no longer meaningful to speak of the same spatial point existing at two distinct moments in time, as it is in Newtonian spacetime). Nevertheless, it is a move widely acknowledged as legitimate by philosophers of physics. If Dasgupta's analogy holds, then, for what reason should we think that generalism's *mere* counter-intuitive commitment to the determinism of  $W_1$ -type worlds gives us any more of a legitimate reason to reject it?

<sup>10</sup>Rickles (2008, 97, fn 158) makes a related, though distinct, point that regarding  $W_1$ -type worlds as indeterministic is a "mistake" for the reason that "we can[not] ... refer uniquely and determinately to the indiscernible parts of symmetric worlds." It is, however, unclear how such referential indeterminacy is supposed to bear on the issue of whether  $W_1$ -type worlds are correctly construed as deterministic or not. For even granting the assumption that reference to one of Max Black's spheres rather than the other is impossible, this does not in any way appear to preclude our ability either (i) to state that  $W_1$ 's initial condition plus its sole law fixes  $W_1$ 's complete history, nor (ii) to quantify over *both* spheres and claim that, for each, it is undetermined what will happen to *it* at  $t \geq 13$  minutes. Cf. Dewar (2013, 55-6).

spheres, focussing her attention on one of the spheres in question, thus breaking the symmetry of the initial state. (Indeed, given that human beings are not symmetrical objects, the *mere* introduction of any single *normal* human observer into the world in question will invariably break the symmetry of the initial state.)

These considerations are, of course, extremely far from being conclusive.<sup>11</sup> But the point remains that the generalist is not *obviously* incapable of *explaining away* our intuitions concerning the alleged indeterminism of  $W_1$ -type worlds. Absent any argument to the contrary, it is not inconceivable that our intuitions concerning  $W_1$ -type worlds might simply be arising on the basis of our failure to appreciate the artificially high degree of initial symmetry of the worlds in question, rather than for reasons of any genuine metaphysical salience.

### 4.3 The Metaphysical Discontinuity Objection

This objection to generalism is one that runs via an analogy with an argument previously developed by Robert Adams (1979, §4). In this paper, Adams attempts to establish the metaphysical possibility of qualitatively indiscernible individuals on the grounds that (1) *Almost*-indiscernible individuals are meta-

---

<sup>11</sup>Thus, Melia (1999, 650) responds that Brighouse's objection "does not do justice to our intuitions", for (he claims) we are capable of imagining  $W_1$ -type worlds in which the relevant laws "depend ... upon the symmetry of the situation." Thus, for instance, in  $W_1$ -type worlds we might, according to Melia, imagine that *unless* a perfect initial symmetry is instantiated the two iron spheres both turn green and remain so for the entirety of that world's history. "Even so," Melia writes, "the intuition persists" that  $W_1$ -type worlds (instantiating the requisite perfect initial symmetry) could have evolved in more than one way. But, granting Melia's claim concerning his own intuitions here, none of this would appear to have any bearing upon Brighouse's main point, which I take to be the claim that drawing *any* intuitive conclusions from such cases is plausibly doomed to failure: for whatever intuitions we tend to draw on the basis of considering such worlds will inevitably be tainted by our illegitimate introduction of some kind of asymmetry into the initial imagined state. (Note, however, that in more recent work Brighouse (Brighouse 2008, 157) no longer appears sympathetic to this claim.)

physically possible and (2) Incremental changes in the qualitative properties of any individual should not be taken to have any drastic or “discontinuous” metaphysical consequences: in particular, such incremental changes should not take one from a metaphysically possible world to a metaphysically impossible world. Adams’ argument then runs that, because an *almost* Blackian world is surely possible — a world in which, say, one of the spheres has a tin atom at its centre rather than an iron atom — a “fully” Blackian world *sans* blemish is also possible. Thus the necessary version of the PII, construed as the claim that no qualitatively indiscernible entities exist at any possible world, is refuted.<sup>12</sup>

*Prima facie*, an analogous argument against generalism might now be run as follows. Imagine a world,  $W_2$ , almost exactly like  $W_1$  except that one of the spheres in the initial state is similarly “blemished” (as in Adams’ argument), and where the sole governing law is the same as in  $W_1$ . Now, this world, *even according to the generalist*, is straightforwardly indeterministic — for the world’s possible future states can now be distinguished qualitatively, for instance by specifying whether it is the blemished or the unblemished sphere that continues to persist at  $t \geq 13$  minutes. Now “unblemish” the sphere in question, to yield world  $W_1$ . *According to the generalist, this world is now deterministic*. But now we seem to have derived a conclusion similar to the one derived in Adams’ argument. That is, we seem to have derived a rather unpleasant “metaphysical discontinuity” which we apparently have no good reason to accept. Thus — so this argument runs — if we are drawn to accept Adams’ argument, we should

---

<sup>12</sup>See Rodriguez-Pereyra (2004, 74) for a more recent development and endorsement of this argument. Note also that in his original paper Adams claimed that his argument, given that it (allegedly) establishes the non-necessity of the PII, refutes generalism. As we saw in §4.1 however, this additional inference is illegitimate.

be compelled to accept this one as well, and conclude that, *contra* generalism,  $W_1$  and other symmetry-breaking worlds like it are genuinely *indeterministic*, and hence, by a simple application of *modus tollens*, that generalism is false.

How might the generalist respond to this argument? As I see it, in two ways. The first response is simply to bite the bullet (i.e., accept the metaphysical discontinuity), but to add in (partial) mitigation that the difference between symmetrical and asymmetrical situations *is* a “metaphysically discontinuous” distinction, and that it is therefore unsurprising if the transition from an asymmetrical to a symmetrical initial state has metaphysically discontinuous consequences. However, such a response faces the problem that it *also* would seem to rule out Adams’ argument as illegitimate; for in that case, too, we pass from an asymmetrical to a symmetrical state. Perhaps such a move is ultimately defensible. I will not, however, try to defend it here. For I believe that the generalist may avail herself of a better response to the argument.

Thus, a second, better response to the argument is to point that there exists an important *disanalogy* between Adams’ original argument and the one levelled against generalism here. For while Adams was attempting to refute the thesis that certain possible worlds (namely, those that violate the PII) cannot obtain as a matter of metaphysical necessity, the argument discussed here is not similarly directed against any such claim. This is because the generalist does not *rule out* any world as metaphysically impossible, as the believer in the necessitated version of the PII does. Instead, the generalist simply *identifies* worlds that one might have pre-theoretically thought were distinct. Thus, while there is indeed a certain discontinuity with regard to the *laws* in the transition from  $W_2$  to  $W_1$  — insofar as a previously indeterministic law becomes deterministic as a result

of the unblemishing of the relevant sphere — there has been no discontinuity with regard to what is deemed *metaphysically possible* according to the generalist: a small change in the constitution of one of the spheres does *not* take one from a world deemed metaphysically possible to one deemed metaphysically impossible according to the generalist. The analogy fails.

## 4.4 The Motivation Objection

This objection is related to the objection discussed in the previous section. It is also one that, admittedly, is slightly *ad hominem* to Dasgupta. But I think it is an interesting argument nonetheless, one whose failure proves quite instructive.

First, let us recall Dasgupta's (2009, §2) stated reason for rejecting bundle theory as a response to his argument against singular facts on the basis of their (claimed) "undetectability" and "redundancy": namely, that due to bundle theory's apparent commitment to the PII, bundle theory would appear to *constrain* the sorts of general facts that can obtain, and that therefore bundle theory constitutes (in Dasgupta's words) "at best an inelegant response" to the argument against singular facts developed in his paper. Another way to put Dasgupta's point is that, given that Dasgupta's argument is directed *solely* against the fundamental existence of singular facts (on the basis of their undetectability/redundancy), the optimum response to such an argument is to adopt a metaphysics which dispenses *solely* with such facts.<sup>13</sup> Dasgupta suggests that

---

<sup>13</sup>It should perhaps be remarked that whether *all* versions of bundle theory are incompatible with the PII is a contentious issue in the contemporary metaphysics literature. (See, e.g., Hawthorne & Sider (2002), Rodriguez-Pereyra (2004), and Demirli (2010) for discussion.) Note, however, that even if it is true that not all versions of bundle theory are committed to the PII, Dasgupta's core point would still appear to have some application: for he may then simply be

there is a relevant analogy here with the case of absolute velocity in Newtonian mechanics: here, the adoption of a Galilean spacetime structure places no further constraints on what *relative* positions and velocities might also obtain in different worlds. And this is a good thing, according to Dasgupta (2009, 49): for “we only wished to dispense with absolute velocities; we did not in addition want to rule out certain patterns of relative velocities as impossible.”

To have further implications beyond what is strictly necessary to excise such “dangler” facts (to use Dasgupta’s useful coinage) from our metaphysics, then, is, other things being equal, a Bad Thing, and in particular counts as a mark against adopting bundle theory *qua* response to the undetectability/redundancy of singular facts. Similarly — or so the *Motivation Objection* runs — generalism’s commitment to treating symmetry-breaking worlds like  $W_1$  as deterministic is *likewise* unmotivated. For, granting the soundness of Dasgupta’s arguments against facts fundamentally involving particular individuals, one might nevertheless ask the question: Why should these arguments thereby motivate us to accept the one-way dynamical evolution of certain sufficiently symmetrical universes, when such an acceptance would appear to be utterly unmotivated by Dasgupta’s original arguments? Generalism, it seems, in addition to ridding us of “surplus” metaphysical structure, goes further than it should in constraining the *number of possible ways* in which  $W_1$ -type worlds can dynamically evolve.

As I see it, there are at least three independently plausible replies to this objection. The first is to bluntly claim that regarding worlds such as  $W_1$  as deterministic plausibly *is* motivated by the sort of considerations that Dasgupta

---

understood as claiming that we should be hesitant about adopting those versions of the bundle theory which *do* imply the PII.

mentions. That is, given the fact that the putatively distinct futures in  $W_1$ -type worlds all differ *solely* with regard to which singular facts are true at them, and given that (if Dasgupta is right) we are legitimately motivated to try to dispense with such facts, we *plausibly are* motivated to regard  $W_1$  as genuinely deterministic after all. For insofar as the generalist is not committed to the fundamental existence of singular facts, the differences between the allegedly distinct futures in  $W_1$ -type worlds are not differences that the generalist should be in any way inclined to regard as genuine.

My own view is that this first response is sufficient to reply to the Motivation Objection. For those who remain unconvinced, however, two other responses might also be considered. Thus, a second response is to point out that, whenever one excises structure from one's metaphysics — whether on the basis of the type of the (“symmetry”) considerations that Dasgupta considers, or for some other reason — the adoption of the appropriately motivated or “superior” theory will *almost invariably* have modal implications. Thus, in the case of the Max Black world (even set against the background of substantival space), adopting the generalist picture will naturally entail that one is unable to distinguish between such a world and one in which the spheres are “permuted”: but, of course, the generalist is *only* unable to distinguish between such worlds for the simple reason that he doesn't think that the putative distinctions between such worlds are in fact genuinely real. Indeed, it is worth noting that the modal implications that result from adopting a Galilean spacetime structure are somewhat similar to those being considered here. Given an initial state consisting of the positions and the relative velocities of various particles and Newton's laws of force and gravitation, one might ask the question: Is the future

evolution of the system fixed? The answer, as it turns out, depends on what our underlying spatiotemporal metaphysics is. If we are thinking of spacetime in Newtonian terms, then the answer is “no”: there are many possible future evolutions consistent with Newton’s laws and the inverse gravitational square-law for the initial state so described, for the simple reason that there are many (indeed, infinite) initial states consistent with such an initial state description that differ only in a uniform boost. However, if we are thinking of spacetime as possessing only a Galilean structure, then the answer is “yes”: there is only one possible future evolution consistent with that initial state and Newton’s laws. Thus, in both cases — the case of absolute velocity, and the case of singular facts — it turns out that certain facts that initially did *not* fix the future evolution of the world (positions and relative velocities alone; the qualitative state of the world at a time) now do fix its future evolution.<sup>14</sup>

Finally, a third reply to this objection reiterates a point made in the previous section: namely, that there is an important disanalogy between the case of bundle theory “restricting” the space of metaphysical possibility and generalism “restricting” the number of ways in which the initial state of  $W_1$  might evolve. The disanalogy is this: in the case of bundle theory, certain possible worlds are deemed impossible, or are ruled out of the space of metaphysical possibility.<sup>15</sup> In the generalist’s case, on the other hand, *no* possible state of affairs is in fact ruled out: it is just that, as a matter of fact, worlds that we might pre-theoretically have

---

<sup>14</sup>A small caveat: in the case of velocity, Newton’s laws do not go from being indeterministic to being deterministic when we go from Newtonian to Galilean spacetime. But still, the basic point remains that certain facts about the initial state and the laws do not fix the future evolution of the system in Newtonian spacetime but do imply the future evolution of the system in Galilean spacetime. (Many thanks to Shamik Dasgupta for help with this point.)

<sup>15</sup>But see footnote 4 and the surrounding discussion above.

thought differed in some (singular, or purely “haecceitistic”) way turn out *not* to differ at all. Thus, generalism’s commitment to regarding certain symmetry-breaking worlds as deterministic is more properly described as a collapsing of the space of genuine possibilities, rather than as a restriction imposed on that space. Yet again, the analogy fails.

## 4.5 The Greavesian Objection

This objection to generalism is slightly more elaborate than the ones discussed previously. In particular, it requires a modification of our original example so as to include human observers. (Cf. Adams 1979, §5.)<sup>16</sup>

Imagine a world in almost all core respects exactly identical to  $W_1$  — call it  $W_3$  — but with a slight difference. In particular, instead of a world containing merely two qualitatively indiscernible spheres, in  $W_3$  imagine that we have two spheres on top of each of which is a human being qualitatively indiscernible from the other. Imagine, further, that the sole law of  $W_3$  is the same as in  $W_1$ , and that each human inhabitant’s fate is tied inextricably to the fate of her respective sphere. Now assume — just to make the case particularly vivid — that *you*, the reader, are in fact one of the inhabitants of  $W_3$  (with your clone being on the other sphere), and that you are privy to the full generalist description of your world at  $t < 13$  minutes. The *Greavesian Objection* to generalism then runs as follows: despite *ex hypothesi* being privy to the full generalist description of your world, and despite the world being fully deterministic, nevertheless there still would appear to remain a *further fact* of which you are ignorant. More specifically,

<sup>16</sup>Similar objections are also suggested by Melia (1999, 653) and Arntzenius (2012, 179).

you would still appear to be ignorant of whether *you yourself* will continue to survive at  $t \geq 13$  minutes. Thus — the argument runs — general statements cannot invariably capture all genuine truths at a given world: they crucially *leave something out*.  $W_3$  is therefore not a world which may be correctly described as being *purely* qualitative in character: for there are facts (e.g., whether you in particular will survive) over and above those that can be captured in purely qualitative terms.

It is important to tease out the tacit assumption on which this argument rests. Hilary Greaves (2004, 441) is one person who has stated it explicitly (in a related context). In homage, then, let us label it “Greaves’ Dictum”:

**Greaves’ Dictum.** I can feel uncertain over  $P$  only if I think there is a genuine fact of the matter regarding  $P$  of which I am ignorant.

The argument’s reliance on this assumption is plain. Given the apparent uncertainty that an inhabitant of  $W_3$  would feel with regard to his continued (non)existence — even when he is privy to the full general description of his world — the conclusion this argument tells us to draw is that this uncertainty must be due to the individual’s lack of knowledge of some *other* facts. Note also that this is a problem which is apparently peculiar to the generalist: the problem is that he seems unable to avail himself of the singularist’s answer to the question of what it is that each individual inhabitant in  $W_3$  is ignorant of.

How might the generalist respond to this argument? Perhaps the most obvious way would be simply to reject Greaves’ Dictum out of hand. After all, it does not appear to have the status of an *a priori* or necessary truth. On what basis should we feel compelled to accept it?

A slightly more conciliatory approach to understanding Greaves' Dictum, however, is also possible. Such a conciliatory approach would first draw attention to an important distinction between two different kinds of knowledge, knowledge which (following Lewis 1979) is often called knowledge *de dicto* and knowledge *de se*. Knowledge *de dicto* is propositional knowledge: that which we are able to learn from science textbooks, from maps, and so on. Knowledge *de se* is (a special case of) nonpropositional knowledge, which invariably involves the use of *indexicals*: it concerns knowledge relating to where, when or even who one is. In a sense, it is a form of "locational" knowledge which, when possessed, allows one (as Pooley (MS, 97) has recently put it) to "locate [one]self in the objective order" of things. In the case at hand, it is knowledge *de se* that, according to this response, our inhabitants in  $W_3$  lack — they do not know whether *they themselves* will continue to exist at  $t \geq 13$  minutes — rather than any knowledge *de dicto*. Moreover, it is precisely the sphere-dwellers' lack of such locational knowledge which, according to this response, explains their uncertainty concerning their respective futures.

There is, of course, much to be said about the *de dicto* and *de se* distinction. I will therefore content myself with only three brief, related comments here.

First, it is not immediately obvious that the singularist in fact has a story to tell about what accounts for a given sphere-dweller's initial uncertainty — *unless*, that is, he too appeals to the notion of *de se* uncertainty. For assume that singularism is true, and that each sphere-dweller is privy to the full singularist description of his world. Such a description would therefore presumably include primitive facts such as *sphere A will be destroyed at  $t = 13$  minutes*, and *sphere B will continue to persist at  $t \geq 13$  minutes*. (Note that the letters "A" and "B" here are

functioning as proper names.) *Prima facie*, however, it is difficult to fathom how *mere* knowledge of such singular facts would help our sphere-dweller alleviate his uncertainty about the future. For in what way would knowledge of these facts allow our sphere-dweller to determine, *de se*, whether it is *his* sphere that will be destroyed at  $t = 13$  minutes?

Second, it is worth remarking that the appeal to *de se* knowledge made here is not meant to carry with it any further commitments about the “specialness” of the *de se*. In particular, it is not meant to carry with it any claim to the effect that the recognition of such attitudes necessarily entails a revisionary account of propositional attitudes (as Lewis 1979 believed). Indeed, I am happy to grant (as Cappelen & Dever (2013) and Magidor (forthcoming) have recently argued) that puzzles about the *de se* are just special instances of Frege puzzles. (Indeed, such a view is, I think, plausibly suggested by our considerations in the previous paragraph.) All that is important for our purposes is simply that, for an agent embedded within a given world, knowledge of all the world’s fundamental facts can still generically allow for *de se* ignorance and uncertainty to persist.

Third, there is a notable parallel between our discussion here and discussions of indexical uncertainty which arise in discussions of Everettian quantum mechanics. (Indeed, the latter is Greaves’ concern in her original paper.) In brief, in Everettian quantum mechanics one “goes it alone” with the universal wavefunction, which evolves deterministically in accordance with the Schrödinger equation (or its relativistic analogue). Nevertheless, a similar kind of uncertainty — similar, that is, to the uncertainty of our sphere-dweller — would appear to arise for the Everettian at the “subjective” level: that is, for a given observer prior to (certain types of) measurement. Whilst fleshing out the paral-

lels between Everettianism and our symmetry-breaking worlds would take us far beyond the scope of this paper, I feel it is nevertheless worth remarking that there, too, *de se* uncertainty is appealed to as an explanans for an apparently similar explanandum.<sup>17</sup>

In summary, then, the generalist should deny that uncertainty requires that one be ignorant of some objective qualitative fact, and maintain that in  $W_3$  a given sphere-inhabitant's ignorance of whether *he* will be destroyed at  $t = 13$  minutes is explained by that inhabitant's inability (given the perfect symmetry of the initial situation) to locate himself in the full qualitative order of things. Crucially, a given sphere-inhabitant's uncertainty is not explained by his failure to know any singular fact which he *ex hypothesi* fails to possess.

What, then, is the proper, or best, way for the generalist to understand Greaves' Dictum? I believe that the generalist should read it as having restricted scope, such that it only be understood as applying to *de dicto* (or propositional) knowledge:

**Greaves' Dictum Redux.** I can feel uncertain *with regard to de dicto knowledge*  $P$  only if there is a genuine *de dicto* fact of the matter regarding  $P$  of which I am ignorant.

<sup>17</sup>I should add, though, that I am much more confident about the legitimacy of the appeal to indexical uncertainty in  $W_3$ -type worlds than I am in the Everettian context. This is because on what I take to be the natural understanding of the theory's formalism, Everettian worlds *branch* (i.e., share initial segments) rather than *diverge* (i.e., possess qualitatively identical but numerically distinct initial segments). (This terminology is due to Lewis (1983a, 359).) It is thus unclear to me how in the Everettian picture an experimenter cognisant of all the relevant third-person facts could be said to be indexically uncertain of *anything* prior to measurement: *prima facie*, such an inhabitant will be structurally-dynamically ("R")-related to all his "future selves" to precisely the same extent; hence, any indexical used by such an inhabitant prior to measurement will fail to pick out any particular post-measurement inhabitant over any others. (For conflicting views, however, see Saunders & Wallace (2008), Saunders (2010), and Wilson (2013).)

This latter construal of Greaves's Dictum is credibly the one that Greaves herself originally intended. For she is quite explicit in her paper that she is sceptical about *de se* knowledge being utilised to ground a subject's uncertainty prior to measurement in an Everettian world (2004, 442). (She argues — plausibly — that the subject's *splitting* into two distinct subjects post-measurement renders such an appeal problematic; see fn 17.) Moreover, given that Greaves never explicitly defines what "*P*" is meant to stand for in her paper, I believe that we should take it to stand for what it would commonly be taken to signify in the philosophical literature: namely as representing *propositional* or *de dicto* knowledge. I do not wish to take a position here on whether this modified (or precisified) version of Greaves' Dictum is correct. (Though I strongly suspect it is.) All I wish to claim is that generalism's commitment to regarding worlds such as  $W_3$  as deterministic gives us no obvious reason to doubt its truth.

## 4.6 Lewisian Determinism?

Given our discussion so far, it would seem that the treatment of  $W_1$ -type (and  $W_3$ -type) worlds as deterministic poses no insuperable difficulties for the generalist. Indeed, the *only* difficulty that the generalist appears to face by regarding such worlds as deterministic would seem to be the view's counter-intuitiveness (but see §4.2). In this section, we will discuss whether the generalist may legitimately discharge even this worry. More specifically, we will examine a conception of determinism (and criticisms of that conception) to the effect that *even the generalist* may legitimately regard  $W_1$ -type worlds as genuinely indeterministic.

How can this be done? The trick, in brief, is to adopt a broadly Lewisian

analysis of determinism, and in particular to make use of the notion of a *duplication mapping*. This is defined to be a mapping between (parts of) worlds which preserves all objects' (and their parts') properties and relations that are, in Lewis' terminology, "perfectly natural" (Lewis 1983a, 356). (All such properties are, for Lewis, purely qualitative in character.) Using this notion, Lewis defines the doctrine of determinism as follows:

**Def 1:** A world  $w$  is deterministic if, whenever  $w'$  is physically possible with respect to  $w$  and  $t, t'$ , and  $f: w_t \rightarrow w'_t$  are such that  $f$  is a duplication, there is some duplication  $g: w \rightarrow w'$ .<sup>18</sup>

As the reader will have noticed, however, Def 1 turns out to classify  $W_1$ -type worlds as deterministic: for any such worlds that are initial duplicates, there will always be *some* global duplication between them. It is for this reason that Belot (1995, 191) proposes the following modification of Lewis' original definition:

**Def 2:** A world  $w$  is deterministic if, whenever  $w'$  is physically possible with respect to  $w$ , and  $t, t'$ , and  $f: w_t \rightarrow w'_t$  are such that  $f$  is a duplication, there is some duplication  $g: w \rightarrow w'$  whose restriction to  $w_t$  is  $f$ .<sup>19</sup>

According to *this* conception of determinism,  $W_1$ -type worlds are *not* classified as deterministic. For instance, if the duplication mapping on the initial (i.e.,  $t < 13$  minutes) segment of  $W_1$  maps each iron sphere onto its sister sphere, then there will be no way of extending this initial duplication into a global duplication mapping in virtue of the fact that, at  $t \geq 13$  minutes, one of the spheres will no

<sup>18</sup>This phrasing of Lewis' proposal is not Lewis' own, but Belot's (1995, 190).

<sup>19</sup>Note that, though Belot was (to my knowledge) the first to explicitly formulate this definition of determinism, he does not endorse it.

longer exist. (Switching which sphere gets mapped to which at  $t = 13$  minutes would violate the rule that spatial distance relations must be preserved under a duplication mapping.)<sup>20</sup>

So far, so good: Def 2 appears to accord with our intuitive judgements about determinism in  $W_1$ -type (and, by extension,  $W_3$ -type) cases. But does it also accord with our intuitions in other, more fanciful cases?

The answer is that it does — at least in many such cases. Thus, consider a world,  $W_4$ , much like  $W_1$ , but in which, at  $t = 13$  minutes, *both* spheres are spontaneously destroyed and instantaneously replaced by two *new* spheres (located precisely where the previous two spheres were), one of which is blemished.<sup>21</sup> Yet again, this world is indeterministic according to Def 2 for (more-or-less) the same reason as  $W_1$  is.<sup>22</sup> Or consider (even more fancifully) a relationalist world,  $W_5$ , temporally infinite in both past and future directions, initially containing a single material particle which, at some unspecified time, disappears.<sup>23</sup> Yet again, this world is classified as indeterministic according to Def 2: given an initial duplication mapping in which, for instance, the particle's time-slice one minute prior to the disappearance of the particle is mapped onto the particle's time-slice two minutes prior the disappearance of the particle, there will be no way of extending this initial duplication mapping to a global one whilst also preserving the universe's temporal metric structure.

*Prima facie*, then, Def 2 is a conception of determinism which seems to accord with our intuitions in a large number of cases. For our purposes, there are two

---

<sup>20</sup>Cf. Arntzenius (2012, 181).

<sup>21</sup>Thus, in  $W_4$  we are assuming that spatial distance relations may obtain between non-simultaneous events. (Recall that  $W_1$  and  $W_4$  are relationalist universes.)

<sup>22</sup>Cf. Arntzenius (2012, 180-1).

<sup>23</sup>This example is loosely drawn from Rynasiewicz (1994, 418). Cf. Dewar (2013, 58-9).

important questions to answer. First: Is Def 2, all things considered, the *correct* analysis of determinism? Second: Assuming that it is, may the generalist make *legitimate* use of it?

With regard to the first question: I am not overly optimistic that there *is* in fact any such thing as the “correct” analysis of determinism. Instead, I think there are simply various (and variously related) notions of “determinism” which respectively accord, or fail to accord, with our pre-theoretic intuitions in various ways. We have seen that, in several cases, Def 2 accords with our pre-theoretic intuitions about whether certain worlds are (in)deterministic. Indeed, the current consensus in the contemporary literature on determinism would appear to be that Def 2 accords with our intuitions in *all* conceivable cases.<sup>24</sup>

Brighouse (2008), however, is one notable recent dissenter from this consensus.<sup>25</sup> In her paper, she provides two examples of pairs of worlds each of which, she claims, is treated differently by Def 2 (i.e., is deemed either deterministic or indeterministic) but which nevertheless “should be judged similarly with respect to determinism” (165). She concludes that all four of the worlds that she considers are properly construed as indeterministic.

We should examine each of her examples in turn:

- **First Example**

- World PAIR-red-blue ( $W_p$ ): This is a relationalist world in which a single alpha particle decays after 13 years into two beta particles, one red and one blue, which go on to move apart from one another. The

---

<sup>24</sup>See Melia (1999, 659-60), Pooley (2002, 108-10), Skow (2005, 50), and Arntzenius (2012, 179-81) for apparent endorsements of this view.

<sup>25</sup>To my knowledge, Belot (1995) is the only other notable dissenter from this consensus, for reasons similar to — but much elaborated upon — by Brighouse.

laws of this world fix the time of decay, the products of the decay, and the angle between the products of the decay. The laws do not fix which particular beta particle is red and which is blue.

- World PAIR-red-blue-no-decay ( $W_{PN}$ ): This is a relationalist world in which two alpha particles are co-located for exactly 13 years, at which point one will turn blue and the other red, and which go on to move apart from another. The laws of this world fix the time of separation, the colour-products of the separation, and the angle of separation. The laws do not fix which particular alpha particle will turn red or blue.

- **Second Example**

- CONTINUUM-no-decay ( $W_C$ ). This is a Newtonian substantialist world, in which continuum many alpha particles are co-located for exactly 13 years, after which they begin to move away from one another, forming a spherical shell around the point of co-location as they travel. The laws of this world fix which trajectories will be occupied by alpha particles after 13 years. The laws do not fix which trajectory any particular alpha particle will take.
- CONTINUUM-no-decay-red ( $W_{CR}$ ). This world is exactly like  $W_C$ , except that the laws also decree that one of the alpha particles after 13 years will be red.

According to Brighouse, Def 2 classifies  $W_P$  and  $W_C$  as deterministic, but  $W_{PN}$  and  $W_{CR}$  as indeterministic. Her claim is that this is an unacceptable conse-

quence of the definition:  $W_P$  and  $W_C$  are not “relevantly dissimilar” enough from  $W_{PN}$  and  $W_{CR}$  so as to be judged differently with respect to determinism. Thus, for Brighouse, all such worlds are properly classified as indeterministic.

How compelling is Brighouse’s argument? Not very, I think, in the case of the First Example: my intuitions, for what they’re worth, strongly push me towards thinking that Def 2 *correctly* classifies  $W_P$  and  $W_{PN}$  as deterministic and indeterministic respectively. I will not, however, attempt to justify this intuition here: so let me just assert that I have it, and hope that the reader has it as well. (Indeed, as far as I can gather Brighouse fails to provide any substantive reasons for accepting her intuitive judgement in this case — though of course the same charge may also be levelled against myself here.)

What about the Second Example? Here, I think, Brighouse’s intuitive judgement that  $W_C$  is indeterministic is much more plausible. However, I think that Brighouse is mistaken in her technical judgement: Def 2 *does* count  $W_C$  as indeterministic. To see this, take an initial ( $t < 13$  years) duplication map  $d$  which maps each spacetime point to itself, but which does not map every alpha particle to itself. Now take  $\alpha$  to be the temporal stage of some alpha particle some time after separation, and let  $p$  be the point of space that it occupies at that time. The spatial distance between  $\alpha$  and  $p$  is therefore zero. But spatial distance is a perfectly natural relation, and duplications preserve such relations. So, under any global duplication mapping  $f$  the spatial distance between  $f(\alpha)$  and  $f(p)$  must also be zero. However,  $d$  will not in general be extendable into  $f$ : according to such an extended duplication  $d'$  of  $d$ , the distance between  $d'(\alpha)$  and  $d'(p)$  will not always be zero. But this implies that  $W_C$  is indeterministic.<sup>26</sup>

<sup>26</sup>What explains Brighouse’s mistake here? Perhaps she thinks that duplications need not

So much for Brighouse's objections. Now let us now turn to the second question raised previously, namely: Assuming that Def 2 does offer the correct analysis of determinism, may the generalist make legitimate use of it? In a purely formal sense of "may", the answer is that she can. After all, nothing in our definition of Def 2 precludes  $w$  or  $w'$  being the same world, or  $w_t$  and  $w'_t$  being the same initial world-segment. Thus, should the generalist wish to make use of Def 2 as her preferred analysis of determinism, the question as to whether a given world is deterministic or not will (sometimes) come down to whether there are certain kinds of duplication mappings from that world onto itself.<sup>27</sup>

A more interesting question to ask, however, is whether the generalist can actually *motivate* the adoption of Def 2 over a more "straightforward" analysis of determinism according to which a world is deterministic iff, given a specification of all facts that obtain at a given temporal stage of the world's history, its complete evolution is fixed. Arguably, the answer is that she can: in particular, the generalist may, I think, motivate the adoption of Def 2 as the correct analysis of determinism by simply claiming that in many (indeed, perhaps all) cases, *it accords with our pre-theoretic intuitions* about whether worlds are deterministic or not. Given that, plausibly, *all* we mean by a suitably "adequate" conception of determinism is that such a conception accords with our pre-theoretic intuitions, this very plausibly (though not irrefutably) constitutes by itself a sufficient philosophical motivation to adopt Def 2.<sup>28</sup>

---

preserve the the spatial locations of material particles. But, if so, I am not sure why she thinks this. Certainly, this is not how other theorists construe the notion of a duplication (e.g., Belot 1995, 194). (Many thanks to Teru Thomas for helpful discussion here.)

<sup>27</sup>Cf. Pooley (2002, 109), Skow (2005, 51, fn 30), and Arntzenius (2012, 180-1).

<sup>28</sup>Melia (1999) and Skow (2005, Ch 2) have argued that Def 2's agreement with our intuitions about particular cases flows from a more general intuitive conception of determinism according to which, for a world to count as deterministic, the qualitative state of the world at a time plus

There is, of course, one evident snag in the argument just offered: namely, that Def 2's accordance with our pre-theoretic intuitions arguably clashes with *another* important pre-theoretic intuition we have about determinism, specifically the one just mentioned: that a world is deterministic iff, given a specification of all facts that obtain at a given temporal stage in the world's history, its complete evolution is fixed.<sup>29</sup> Trying to determine which violation of our pre-theoretic intuitions is the more undesirable is, plainly, a far from straightforward task. Nevertheless, I think enough has been said in this section to suggest at least that the generalist is not *manifestly* committed to an incorrect analysis of determinism — something which, I think, is sufficient at this stage of the dialectic.

## 4.7 The Kmentian Objection

In a recent paper, Boris Kment (2012) claims to demonstrate that various unpalatable consequences arise for the PII-violating generalist who subscribes to the following two assumptions about chance and counterfactual conditionals:

- **Chance.** The chance that  $P$  is  $x$  just in case the chance measure of the set of possible worlds where it's true that  $P$  is defined and equals  $x$ .
- **Counterfactual.** A counterfactual ' $P \square \rightarrow Q$ ' is true at possible world  $w$  just in case  $Q$  is true at all the possible  $P$ -worlds that are closest (most similar overall) to  $w$ .

---

the laws must fix: (i) the qualitative state of the world at all other times, and (ii) the qualitative distribution of properties and relations for all past existents at all other times. As both Brighouse (2008, 167, fn 14) and Arntzenius (2012, 180-1) have independently pointed out, however, if this were true then we would seemingly be at a loss to explain why  $W_4$ -type worlds are intuitively just as indeterministic as  $W_1$ -type worlds — as Def 2 says they are.

<sup>29</sup>See, e.g., Earman (1986) for a defence of this conception of determinism.

It is important to note that the alleged problems that Kment raises for the generalist do not arise when  $P$  is a purely qualitative proposition. Rather, they arise only when  $P$  is a proposition that is peculiarly *de re*. For Kment, it is generalism's inability to make sense of such *de re* propositions without entailing *prima facie* absurd consequences which should ultimately force us to reject it.

One might well be puzzled at this point. After all, given that the generalist ultimately *denies* the propriety of *de re* propositions in providing a full and complete perspicuous description of fundamental reality, to what extent can she make genuine sense of *de re* propositions *at all* — let alone *modal de re* propositions? Moreover — and insofar as the generalist can in fact make sense of such propositions — there is the broader dialectical issue to consider, namely: To what extent should the generalist feel genuinely troubled by the worries Kment raises, if the putatively absurd consequences arise for her view only on the basis of her inability to make sense of propositions whose metaphysical propriety she (presumably) has antecedent reason to reject?

Responding in part to this worry, Pooley (MS, 101) writes:

It would be disappointing if the generalist anti-haecceitist has to deny that there are such *de re* possibilities.... Note that giving up *de re* modality would not merely do violence to our everyday modal intuitions. The application of science looks to be threatened. Physics doesn't merely provide us with purely qualitative models of phenomena. It tells us, for example, what *this* spacecraft would have done had its rockets been set to fire according to these alternative parameters. Consider just how ubiquitous and central to the appli-

cation of theory such *de re*, counterfactual reasoning is.

Though I would dispute Pooley's somewhat hyperbolic suggestion that "the application of science" would be threatened were it truly impossible for the generalist to salvage the intelligibility of *de re* modal discourse,<sup>30</sup> I think we should at the very least grant that it would indeed be "disappointing" for the generalist were no such semantic rescue operation in the offing. The question at hand, then, essentially becomes: Can the generalist avoid such disappointment?

According to Pooley, and many other theorists besides, she can. The essential ingredient is *counterpart theory*, a philosophical theory of modality which owes much to the writings of David Lewis. (See Lewis 1968; 1973; 1986, Ch 4.) Here, in brief, an individual *a* is *F* at world *w* (or *w* "represents" *a* as *F*) just in case there is an individual in *w* who stands in the appropriate similarity relation to (is a *counterpart* of) *a*, and is *F*. Thus, for instance, the claim that I could have been a footballer is cashed out by the counterpart theorist as the claim that, at some possible world, I have a counterpart (i.e., someone sufficiently qualitatively similar to me) who is a footballer.<sup>31</sup>

For our purposes, the important thing to note about counterpart theory is this: a given individual may have *more than one* counterpart at a given possible

---

<sup>30</sup>Indeed, Pooley himself doesn't appear to truly endorse the suggested claim that physics deals with irreducibly *de re* models of phenomena. As he writes in the very next paragraph: "[W]hen we look to physics to tell us how *this* rocket would have behaved if its engines had been fired in such-and-such a way, it is to purely qualitative models [that] we turn." (102)

<sup>31</sup>For the counterpart theorist, precisely which individuals at distinct possible worlds count as *a*'s admissible counterparts is a context-sensitive affair. That is, depending on the appropriate context, it may be true or false whether *w* contains any counterparts of *a* which represent the possibility of *a*'s being *F*. Arguably, this is an attractive feature of the theory: for intuitively we *do* feel that whether a given modal statement is true or not varies depending on the relevant context. For instance, in some contexts it may be true to say that I could have been a fish (albeit under some "extraordinarily generous" counterpart relation, to use Lewis' phrase), but that in others, I couldn't (under a less generous counterpart relation).

world (or, for that matter, in the actual world.)<sup>32</sup> It is in virtue of this fact that a given possible world may, for the generalist, represent *more than one* possibility *de re* for a given individual. Furthermore, though this theoretical severance between possible worlds and the possibilities they represent may perhaps seem counter-intuitive (or unpalatable) to some, many generalists regard this aspect of counterpart theory as a positive feature of the theory: in particular, they believe that it provides a neat way of accounting for the intuition that  $W_3$ -type (and, indeed,  $W_1$ -type worlds more generally) *could have evolved* in more than one way. As Pooley (MS, 102) writes:

For each duplicate of the original [i.e., non-destructive Blackian world] world there *are* two possibilities. But a single generalist possible world,  $W$ , containing one sphere that survives and one sphere that is destroyed, can represent both. Imagine, again, that you are an inhabitant of the original world. One possibility is given by your taking as your counterpart in  $W$  the relevantly similar inhabitant who does not get destroyed. The other is given by your taking as your counterpart the relevantly similar inhabitant of  $W$  who does get destroyed. We need no primitive fact of the matter about which of these two individuals you really are transworld identical to. Both represent (different) possibilities for you.<sup>33</sup>

---

<sup>32</sup>Lewis originally denied that an individual can have more than one counterpart in his or her own world in his (1968), but later reneged on this claim in his (1983b) and (1986, Ch 4).

<sup>33</sup>Note, however, that the fact that a given world may evolve in more than one way in this counterpart-theoretic sense does not *necessarily* entail that the world is (intuitively) indeterministic. Consider, for instance, a world containing two indiscernible alpha particles,  $a$  and  $b$ , initially one metre apart from one another, which (at  $t = 13$  minutes) respectively decay into two indiscernible beta particles,  $c$  and  $d$ , which remain one metre apart for all eternity. The laws of this world only decree *that* the two alpha particles will decay into two beta particles; they don't

A generalist construal of *de re* modality along the lines developed here, then, is tantamount to a commitment to *cheap haecceitism*: the doctrine that a single possible world may correspond to (represent *de re*) more than one possibility. It is precisely this cheap haecceitist, PII-violating generalist modal-metaphysical package that is Kment's (2012) target in his paper.<sup>34</sup>

With the preliminaries out of the way, let us begin with the argument which appeals to Chance. What, then, is Chance's correct generalist counterpart-theoretic (re-)construal for *de re* propositions? According to Kment (2012, 586), "it seems that the anti-haecceitist [generalist] has to" interpret it as follows:

- **Chance De Re.** The chance that  $\Phi(a)$  (where  $a$  is some particular individual) equals the chance measure of the set of possible worlds that contain a counterpart of  $a$  that satisfies  $\Phi(x)$ .

Kment's argument then proceeds as follows. Consider a world like  $W_1$  with spheres  $A$  and  $B$ , but with the added stipulations that (where  $t = 13$  minutes):

- (i)  $\text{Ch}_t(A \text{ decays}) = \text{Ch}_t(B \text{ decays}) = 50\%$ .
- (ii)  $A$ 's decay and  $B$ 's decay are probabilistically independent.

This entails that:

- (iii)  $\text{Ch}_t(\text{no spheres decay}) = \text{Ch}_t(\text{two spheres decay}) = 25\%$

---

specify *which* particular beta particle each alpha particle will decay into. By the choice of the appropriate counterpart relations, however, counterpart theory allows us to say that, despite  $a$ 's decaying into  $c$  and  $b$ 's decaying into  $d$ , nevertheless  $a$  *could have* decayed into  $d$  rather than  $c$ , and similarly  $b$  *could have* decayed into  $c$  rather than  $d$ . Nevertheless, I submit, this world is intuitively deterministic. Cf. Melia (1999, 647) and Skow (2005, 44).

<sup>34</sup>Skow (2008) and Fara (2009) have also recently offered (related) arguments against cheap haecceitism. Their arguments are, however, importantly independent of any consideration of the sorts of PII-violating worlds that are our concern here. See Pooley (MS, 103-4) and Russell (2013) for what I take to be effective replies to Skow and Fara respectively.

(iv)  $\text{Ch}_t$  (one sphere decays) = 50%.

The “straightforward” haecceitist will assume that there are four distinct possible worlds consistent with the above stipulations, each with a 25% chance measure: these are the possible worlds in which (1) only *A* decays, (2) only *B* decays, (3) *A* and *B* both decay, and (4) neither *A* nor *B* decay. For the generalist, on the other hand, there are only three such worlds.<sup>35</sup> These are the possible worlds in which (1′) no sphere decays, (2′) only one sphere decays, and (3′) both spheres decay: as stipulated by (iii) and (iv), these worlds have a 25%, 50%, and 25% per cent chance of obtaining respectively. Now, given the plausible assumption that each sphere in each generalist possible world is a counterpart of both *A* and *B*, it would seem that the open possibilities (i.e., the epistemically possible ways in which the world could evolve prior to  $t = 13$  minutes) where a counterpart of *A* decays include just those worlds where at least one sphere decays, and the open possibilities where a counterpart of *A* continues to exist are just those where at least one sphere continues to exist. (And similarly for *B*.) So:

(v)  $\text{Ch}_t(A \text{ decays}) = \text{Ch}_t(A \text{ continues to exist}) = 75\%$

But this, as Kment notes, is absurd. For not only does it contradict the initial stipulation (i), but it also conflicts with the extremely compelling claim that:

(vi)  $\text{Ch}_t(A \text{ decays}) + \text{Ch}_t(A \text{ continues to exist}) = 100\%$ .

---

<sup>35</sup>Strictly speaking, this is not true: if our generalist were also a modal realist then he could straightforwardly allow for the existence of *more than one* world satisfying each complete qualitative description (cf. Lewis 1986, 224). It would be straightforward, albeit somewhat tiresome, to modify our discussion above to accommodate this fact.

How persuasive is this argument? Certainly, if the generalist (*qua* cheap haecceitist) accepts Chance De Re then the absurdity seems ineluctable. But consider the following alternative assumption, suggested by Thomas (MS):

- **Chance De Re Redux.** The chance that  $\Phi(a)$  (where  $a$  is some particular individual) equals the chance measure of the set of possible worlds that contain a counterpart of  $a$  that satisfies  $\Phi(x)$  multiplied by the proportion of  $a$ 's counterparts in each world which satisfy  $\Phi(x)$ .

To see how this recipe is supposed to work, recall again Kment's example. For generalist, there are three worlds consistent with Kment's stipulations (i) and (ii). We want to be able to say that the chance that sphere  $A$  decays is 50%. Now consider the two worlds where a sphere decays. In the first world, only one sphere decays. By stipulation, this world has a 50% chance of obtaining. But in this world  $A$  has *two* counterparts, of which only *one* decays: the proportion of  $A$ 's counterparts which decay in this world is therefore 0.5. Chance De Re Redux thus recommends that we multiply 50% by 0.5, yielding a 25% chance that  $A$  decays. In the second world, both spheres decay. By stipulation, this world has a 25% chance of obtaining. In this world  $A$  again has two counterparts. However, *both* of these counterparts decay: the proportion of  $A$ 's counterparts which decay in this world is therefore 1. Chance De Re Redux thus recommends that we multiply 25% by 1, yielding a 25% chance that  $A$  decays. Adding these two calculated chances together, we get 25% + 25% = 50%, as desired.

As Thomas notes, it is relatively straightforward to show that this alternative recipe will always work whenever it makes sense.<sup>36</sup> But as he points out, it does

<sup>36</sup>In the generic two-particle case, where  $p$  is the probability that some particle will decay at some specified time, the probability of a particular particle decaying is given by  $(1 - p)^2 \times 0 +$

not always make sense. Thus, imagine a world,  $W_6$ , initially containing *countably infinitely* many qualitatively indiscernible spheres. The sole law of  $W_6$  is that each sphere has an independent probability  $p$  of being annihilated at  $t = 13$  minutes. The following qualitative future ( $t \geq 13$  minutes) state of  $W_6$  therefore plausibly occurs with probability 1: infinitely many spheres are annihilated, and infinitely many spheres remain. A given sphere  $A$  therefore has infinitely many counterparts which remain in  $W_6$ , and infinitely many which are annihilated: there is thus no mathematically meaningful sense in which one can speak of the “proportion” of such spheres which remain, or are annihilated. In short: Chance De Re Redux cannot help us recover  $p$  in  $W_6$ .

Before offering a response to this new problem, it will prove dialectically useful to discuss Kment’s second objection to generalism, which appeals to the assumption Counterfactual. Thus, consider a world,  $W_7$ , initially containing two qualitatively indiscernible, spatially separated persisting spheres on top of each of which is a qualitatively indiscernible sphere-dweller. Nothing happens until  $t = 13$  minutes, when sphere-dweller  $A$  decides to have a cup of coffee, while sphere-dweller  $B$  opts for tea. The two decisions are causally independent. Hence, at  $W_7$  the following counterfactual claim is surely true:

- **Plausible Claim.** Had  $A$  decided to opt for tea rather than coffee at  $t = 13$  minutes, there would have been two tea-drinking sphere-dwellers.

Nevertheless, given the plausible assumptions that (i) Sphere-dweller  $B$  counts as a counterpart for  $A$  and (ii) Every world is at least as close to itself as every other world (“weak centering”), it would seem that  $W_7$  *itself* is one of the worlds

---


$$2p(1 - p) \times 0.5 + p^2 \times 1 = p.$$

closest to  $W_7$  which satisfies the antecedent of the relevant counterfactual. However,  $W_7$  is a world which contains only *one* tea-drinking sphere-dweller: thus the consequent of the relevant counterfactual is false. Thus (by Counterfactual), Plausible Claim is false. But this, as Kment notes, is (very!) implausible.<sup>37</sup>

What to do? Kment himself suggests that the best way for the generalist to respond to the considerations he mentions is to appeal to “possible-world descriptions” as the bearers of the probability measure and the counterfactual closeness relation.<sup>38</sup> A possible-world description is, for Kment (2012, 599), an ordered pair composed of a possible world  $w$  and a (not necessarily total) function from individuals in  $w$  to their counterparts. Thus, a qualitative proposition is generically said to be true at a world description  $\langle w; f \rangle$  just in case the proposition is true at  $w$ ; a singular proposition of the form “ $a$  is  $F$ ” is true at a world description  $\langle w; f \rangle$  just in case, where  $f$  assigns  $a$  to  $b$  in  $w$ ,  $b$  is  $F$ .

How does this machinery apply in the problem cases discussed so far? In short, the generalist can define both the closeness relation and chance measure over sets of *possible-world descriptions*, rather possible worlds. Thus, in the case of the decaying spheres, the world containing only one decaying sphere will correspond to *two* relevantly distinct possible-world descriptions. One of these possible-world descriptions will map the decaying particle onto  $A$ , and will map the persisting one onto  $B$ ; the other possible-world description will do the

---

<sup>37</sup>Note that the haecceitist avoids this problem because he denies that the closest worlds where  $A$  chooses tea include  $W_7$ . For the haecceitist, there is a (distinct) world qualitatively indiscernible to  $W_7$  where  $A$  has tea, but it is not among the worlds *closest* to  $W_7$  where  $A$  has tea. This shows that, for the haecceitist, closeness between worlds is not solely a matter of qualitative similarity. Cf. Kment (2012, 595).

<sup>38</sup>Kment (2012, §5.2) also suggests second possible response for the generalist, which involves appealing to *proper parts* of worlds as bearers of the probability measure and the closeness relation. However, he is sceptical of the viability of this response for precisely the same reason he is sceptical of the viability of appealing to possible-world descriptions, discussed below.

opposite. The chance measure over these possible-world descriptions can then be stipulated to be 25% (and the rest of the solution follows straightforwardly).

Similarly, in world  $W_6$ , which initially contains countably infinitely many particles, there will be two relevantly distinct (countably infinitely big) sets of possible-world descriptions. In one such set,  $A$  will be mapped onto a non-decaying sphere; in the other,  $A$  will be mapped onto a decaying sphere. The probability measure over the two sets can then (again) be distributed as desired.

Likewise, in the case of counterfactual conditionals: the relevant possible-world descriptions which have  $W_7$  as their first member are  $\langle W_7, I \rangle$  (in which each individual inhabitant is mapped onto himself) and  $\langle W_7, D \rangle$  (in which each individual inhabitant is mapped onto his twin). On the assumption that each possible-world description is as close to itself as it is to any other,  $\langle W_7, I \rangle$  is as close to  $\langle W_7, I \rangle$  as any other possible-world description is to  $\langle W_7, I \rangle$ .  $\langle W_7, D \rangle$  is, however, farther away from  $\langle W_7, I \rangle$  than a possible-world description where both inhabitants have tea. Thus, Plausible Claim can come out as true.

So is all well and good? Not according to Kment (2012, 601), who suggests that this proposal faces one major, and quite possibly insuperable, difficulty:

[A]ccording to world-description theorists, neither [Chance] nor [Counterfactuals] is true of the entities they call 'worlds'; both claims hold of *world-descriptions* instead. That means that the entities they call 'worlds' don't play the role we associate with worlds pre-philosophically. That role is taken over by the world descriptions. But it seems plausible that what entities our thoughts and utterances about worlds (or ways the world could have been) are about is determined in large part

by the (folk-)theoretical role associated with the concept of a world. If the entities that best fit this role are the world descriptions, then, other things being equal, these entities are better candidates for being the referents of 'world' than the entities that the world-description theorists call 'worlds'. So, world-description theorists seem simply to be misdescribing their own account. If their view is correct, then the world descriptions are really the possible worlds (they are the things we are talking about when we speak of ways things could be). But world descriptions satisfy the haecceitist's conception of worlds, not the antihaecceitist's. For clearly, there can be world descriptions where the same qualitative claims are true but where different claims about specific individuals hold. World-description theory, when correctly described, turns out to be a version of haecceitism.

Russell (2015, 395) has recently framed the objection rather more pithily:

We see [on Kment's suggested response to his own argument, world-descriptions] taking over more and more of the jobs standardly done by possible worlds. What could justify us in continuing to withhold the term 'world' from these objects? What work is left for worlds to do that isn't already done by mere possibilities?<sup>39</sup>

How might the generalist (*qua* cheap haecceitist) attempt to respond to these worries? To be frank, I am not sure what the *best* generalist response to them is. In closing, then, I will suggest three attempts to resolve the difficulties Kment raises which seem to me to be, at least *prima facie*, not totally implausible.

---

<sup>39</sup>Cf. Belot (1995, 193, fn 10).

One line of response would be to follow a suggestion made by Maunu (2005),<sup>40</sup> who points out that there *is* in fact a version of generalism which is capable of regarding purely “haecceitistically” distinct worlds (i.e., worlds which differ solely with respect to which individuals are playing which qualitative roles) as genuinely distinct. The trick, in brief, involves allowing the generalist to quantify over *worlds* in her full description of modal reality, as well as over the individuals within them. This permits locutions of the form, “There is a world  $w_1$  and a world  $w_2$  such that there is an individual  $x$  and an individual  $y$  where  $x$  is  $F$  and  $y$  is  $G$  in  $w_1$  and  $y$  is  $F$  and  $x$  is  $G$  in  $w_2$ .” (Both the worlds and the individuals within them are to be construed here as bound variables rather than as proper names.) On this version of what we might call “ultra-generalism”, it is a straightforwardly meaningful question to ask whether or not the same individual that is actually  $F$  is possibly  $G$  (i.e., is  $G$  at some other possible world). Furthermore, by explicitly allowing for such (primitive) “transworld identity”, ultra-generalism has the resources to be able to distinguish between the haecceitistically distinct possibilities that feature in Kment’s arguments not as “mere” differences between various world-descriptions, but rather as genuine distinctions between distinct possible worlds.

I have no knock-down argument against this proposal. However, I do not believe that it is one that many generalists will be much inclined endorse. The reason for this is that such a version of ultra-generalism (i.e., one which allows for genuinely haecceitistically distinct worlds) plausibly underwrites a lot of the putatively distasteful metaphysical commitments which, I think, motivates the adoption of the generalist picture in the first place. Indeed, many generalists

---

<sup>40</sup>Cf. Sider (2002).

(e.g., those cited in fn 2 above) are explicitly drawn towards generalism *precisely because* they are sceptical of the possibility of purely haecceitistically distinct possible worlds. It is thus difficult to imagine someone drawn towards generalism for precisely the reason just mentioned, but who nevertheless unconcernedly adopts a conception of modality which allows for precisely the undesirable (haecceitistic) distinctions which encouraged him to adopt such a generalist metaphysics in the first place.

A second — and, I think, somewhat more plausible — line of response to Kment's worry would be to follow Russell (2015), who explicitly takes up Kment's challenge by trying to articulate distinct theoretical roles for possibilities and possible worlds to play in modal and metaphysical discourse. Russell's proposal, in brief, draws on a "distinction between metaphysical FACTS and mere 'thin' facts" (406), or between "factual" matters and those matters that are "merely 'perspectival', 'representational', or 'conventional'" (400). Possible *worlds*, on this proposal, play the role of representing metaphysically substantive ways which the world itself could have been: any two distinct possible worlds will differ with regard to which FACTS they represent as being true. Not so *possibilities*: these merely "represent", in some "perspectival" or "conventional" way, some genuinely metaphysically substantive (i.e., "FACTUAL") way for things to be. Thus, two distinct possibilities may be said to represent the *same* "genuinely factual" state of affairs on account of the fact that they "differ just in the 'perspective' one takes on the facts, not on the facts themselves" (404). On Russell's proposal, it is possibilities (not possible worlds) that turn out to be proper bearers of the chance measure, the closeness relation, and so forth (at least in the case of *singular* probabilistic or counterfactual statements); but

possible worlds still have an important part to play in this modal-metaphysical system, representing as they do the substantively (qualitatively) distinct ways the world — or “WORLD” — could have been.

I think this is reply to Kment’s arguments is fine as far as it goes. Call it the “defensive” response: it offers an explicit response to the challenge posed. Let us finish by considering a somewhat more “offensive” response to Kment.<sup>41</sup>

This (third) response to Kment naturally suggests itself, I think, once we remind ourselves again of the overall dialectic. The original problem for generalism, recall, is that the generalist is apparently incapable of making straightforward sense of *de re* modal claims. In a semantic rescue operation, the generalist appeals to counterpart theory: claims about particular individuals are not true or false at (or “represented” as being true or false at) possible worlds *simpliciter*, but are only (represented as) true or false at a world *relative* to a particular choice of counterpart relation. Which is to say: the generalist, by endorsing such a semantic rescue operation, is *already committed to rejecting* the thesis that modal truths are cashed out in terms of truths holding at various possible worlds *simpliciter*. Kment’s arguments may, then, perhaps force such a “two-tier” conception of modal reality to become rather more manifest than it is normally taken to be in the standard literature on counterpart theory. But, plausibly, his arguments do not pose any *special* problem for the counterpart-theoretic generalist: if the problem is that the generalist, by endorsing counterpart theory, is endorsing an unacceptable two-tiered conception of metaphysical modality, then the problem arose at the very outset. The central problem Kment raises would thus appear to be the generalist’s *mere* adoption of a counterpart-theoretic semantics — cru-

---

<sup>41</sup>Thanks to Teru Thomas for extensive discussions of this point.

cially for our purposes, his worries have nothing *in particular* to do with the PII-violating worlds which have been our concern throughout this chapter.

## References

### Primary Sources (Leibniz)

A: *Gottfried Wilhelm Leibniz. Sämtliche Schriften und Briefe*. Berlin: Akademie Verlag. 2006.

C: *Opuscles et fragments inédits de Leibniz*. Couturat (ed. & trans.) Paris: Ancienne Librairie Germer Baillière et Cie. 1903.

DM: *Discourse on Metaphysics*. Lucas, P. G. & Grint, L. (trans. & ed.). Manchester: Manchester University Press. 1953.

G: *Die Philosophischen Schriften von Gottfried Wilhelm Leibniz*. C. I. Gerhardt (ed.). Berlin: Weidmann. 1879.

Grua, G. (ed.), *Textes inédits d'après les manuscrits de la Bibliothèque provinciale de Hanovre*. London: Garland. 1948.

LA: *The Leibniz-Arnauld Correspondence*. Mason, H. T. (trans. & ed.). Manchester: Manchester University Press. 1967.

LC: *The Leibniz-Clarke Correspondence*. Alexander, H.G. (ed.). Manchester: University of Manchester Press. 1956.

Loemker (1969): *Philosophical Papers and Letters*. Loemker, L. E. (trans. & ed.). 2nd Edition. Dordrecht: Reidel. 1969.

M: *Monadology*. Latta, R (ed. & trans.). London: Oxford University Press. 1898.

NE: *New Essays on Human Understanding*. Remnant, P. & Bennett, J. (trans. & eds.). Cambridge: Cambridge University Press. 1981.

P: *Leibniz: Philosophical Writings*. Parkinson, G. H. R. (trans. & ed.), Morris, M. (ed.). London: Dent & Sons. 1973.

S: *Fragmente zur Logik*. Schmidt, F. (ed. & trans.). Berlin: Akademie-Verlag. 1960.

T: *Theodicy*. Farrar, A. (ed.), Huggard, E. M. (trans.). London: Routledge. 1951.

## All Other Sources

Adams, R. M. (1977), "Leibniz's Theories of Contingency." In Kulstad, M. (ed.), *Essays on the Philosophy of Leibniz*. Rice University Studies, Vol. 63, pp. 1-41.

Adams, R.M. (1979), "Primitive Thisness and Primitive Identity." *The Journal of Philosophy*, Vol. 76, No.1, pp. 5-26.

Adams, R. M. (2002), "Review: *Substance and Individuation in Leibniz*." *Mind*, Vol. 111, No. 444, pp. 851-855.

Arenhart, J. (2013), "Weak Discernibility in Quantum Mechanics: Does It Save PII?" *Axiomathes*, Vol. 23, No. 3, pp. 461-484.

Arntzenius, F. (2012), *Space, Time, and Stuff*. Oxford: Oxford University Press.

Bain, J. (2013), "Category-Theoretic Structure and Radical Ontic Structural Realism." *Synthese*, Vol. 190, No. 9, pp. 1621-1635.

Baker, D. (2010), "Symmetry and the Metaphysics of Physics." *Philosophy Compass*, No. 5, Vol. 12, pp. 1157-1166.

Barbour, J. (1989), *Absolute or Relative Motion? Vol. 1: The Discovery of Dynamics*. Cambridge: Cambridge University Press.

Belanger, C. (2013), "On two mathematical definitions of observational equivalence: Manifest isomorphism and  $\epsilon$ -congruence reconsidered." *Studies in History and Philosophy of Modern Physics*, Vol. 44, pp. 69-76.

Belot (1995), "New Work for Counterpart Theorists: Determinism." *The British Journal for the Philosophy of Science*, Vol. 46, No. 2, pp. 185-195.

Belot, G. (2001), "The Principle of Sufficient Reason." *The Journal of Philosophy*. Vol. 98, No. 2, pp. 55-74.

Belot, G. (2003), "Notes on symmetries." In K. Brading & E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press.

Belot, G. (2013), "Symmetry and Equivalence." In R. Batterman (ed.), *The Oxford Handbook of Philosophy of Physics*. Oxford: Oxford University Press.

Black, M. (1952), "The Identity of Indiscernibles." *Mind*, Vol. 61, No. 242, pp. 153-164.

Brading, K. & Brown, H. (2004), "Are Gauge Symmetry Transformations Observable?" *The British Journal for the Philosophy of Science*. Vol. 55, pp. 645-665.

Brighouse, C. (1994), "Spacetime and Holes." *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1994*, pp. 117-125.

Brighouse, C. (1997), "Determinism and Modality." *The British Journal for the Philosophy of Science*, Vol. 48, No. 4, pp. 465-481.

Brighouse, C. (2008), "Understanding Indeterminism." In Dieks, D. (ed.) *The Ontology of Spacetime II*. Amsterdam: Elsevier Press.

Broad, C. D. (1946), "Leibniz's Last Controversy with the Newtonians." *Theoria*, Vol. 12, pp. 143-168. Reprinted in Woolhouse, R. S. (ed.) *Leibniz: Metaphysics and Philosophy of Science*. Oxford: Oxford University Press. 1981.

Brown, G. (2004), "[...] et je serai toujours la même pour vous': Personal, Political, and Philosophical Dimensions of the Leibniz-Caroline Correspondence." In Lodge, P. (ed.), *Leibniz and His Correspondents*. Cambridge: Cambridge University Press.

Brown, H. R. (1993), "Correspondence, Invariance, and Heuristics in the Emergence of Special Relativity." In S. French & H. Kamminga (eds.), *Correspondence, Invariance and Heuristics: Essays in Honour of Heinz Post*. Dordrecht: Springer Press.

Brown, H. & Sypel, R. (1995), "On the meaning of the relativity principle and other symmetries." *International Studies in the Philosophy of Science*, No. 9, Vol. 3, pp. 235-253.

Cappelen, H. & Dever, J. (2013), *The Inessential Indexical: On the Philosophical Insignificance of Perspective and the First Person*. Oxford: Oxford University Press.

Caulton, A. (forthcoming), "The Role of Symmetry in the Interpretation of Physical Theories." *Studies in the History and Philosophy of Modern Science*.

Caulton, A. & Butterfield, J. (2012), "On Kinds of Indiscernibility in Logic and Metaphysics." *British Journal for the Philosophy of Science*, Vol. 63, No. 1, pp. 27-84.

Chalmers, D., Manley, D. & Wasserman, R. (eds.) (2009), *Metametaphysics: New Essays on the Foundations of Ontology*. Oxford: Oxford University Press.

Cover, J. & O'Leary-Hawthorne, J. (1999), *Substance and Individuation in Leibniz*. Cambridge: Cambridge University Press.

Cowling, S. (2015), "Non-Qualitative Properties." *Erkenntnis*, Vol. 80, No. 2, pp. 275-301.

Dasgupta, S. (2009), "Individuals: An Essay in Revisionary Metaphysics." *Philosophical Studies*, Vol. 145, No. 1, pp. 35-67.

- Dasgupta, S. (2011), "The Bare Necessities." *Philosophical Perspectives*, Vol. 25, No. 1, pp. 115-160.
- Dasgupta, S. (2013), "Absolutism vs Comparativism about Quantity." In K. Bennett & D. Zimmerman (eds.) *Oxford Studies in Metaphysics: Volume 8*. Oxford: Oxford University Press.
- Dasgupta, S. (2014), "On the Plurality of Grounds." *Philosophers' Imprint*, Vol. 14, No. 20, pp. 1-28.
- Dasgupta, S. (2015), "Substantivalism vs Relationalism About Space in Classical Physics." *Philosophical Compass*, Vol. 10, No. 9, pp. 601-624.
- Dasgupta, S. (forthcoming, a), "Symmetry as an Epistemic Notion (Twice Over)." *The British Journal for the Philosophy of Science*.
- Dasgupta, S. (forthcoming, b), "Inexpressible Ignorance." *Philosophical Review*.
- Debs, T. & Redhead, M. (2007), *Objectivity, Invariance, and Convention*. Cambridge, MA: Harvard University Press.
- Demirli, S. (2010), "Indiscernibility and Bundles in a Structure." *Philosophical Studies*, Vol. 7, No. 1, pp. 1-18.
- Dennett, D. (1991), "Real Patterns." *Journal of Philosophy*, Vol. 88, No. 1, pp. 27-51.
- Detlefsen, K. (2002), "Review: *Substance and Individuation in Leibniz*." *Philosophy in Review*, Vol. 22, No. 1, pp. 19-21.
- Dewar, N. (2013), *Symmetries in Physics: A Linguistic Approach*. B.Phil Thesis, University of Oxford.
- Dewar, N. (forthcoming), "Symmetries and the Philosophy of Language." *Studies in the History and Philosophy of Modern Science*.
- Di Bella, S. (2005), *The Science of the Individual: Leibniz's Ontology of Individual Substance*. Dordrecht: Springer.
- Dieks, D. & Versteegh, M. (2008), "Identical Quantum Particles and Weak Discernibility." *Foundations of Physics*. Vol. 38, No. 10, pp. 923-934.
- Dorato, M. & Morganti, M. (2013), "Grades of Individuality: A Pluralistic View of Identity in Quantum Mechanics and in the Sciences." *Philosophical Studies*. Vol. 163, No. 3, pp. 591-610.
- Earman, J. (1986), *A Primer On Determinism*. Dordrecht: Kluwer.
- Earman, J. (1989), *World-Enough and Space-Time*. Cambridge, MA: MIT Press.

- Esfeld, M., & Lam, V. (2008), "Moderate Structural Realism About Space-Time." *Synthese*, Vol. 160, No. 1, pp. 27-46.
- Esfeld, M. & Lam, V. (2011), "Ontic Structural Realism as a Metaphysics of Objects." In Bokulich, A. & Bokulich, P. (eds.), *Scientific Structuralism*. Dordrecht: Springer Press.
- Fara, D. (2009), "Dear Haecceitism." *Erkenntnis*, Vol. 7, No. 3, pp. 285-297.
- Feynman, R. (1967), *The Character of Physical Law*. Cambridge, MA: MIT Press.
- Field, H. (1984), "Can We Dispense with Space-Time?" *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, pp. 38-90.
- Fine, K. (2005), *Modality and Tense*. Oxford: Oxford University Press.
- French, S. (2014), *The Structure of the World: Metaphysics and Representation*. Oxford: Oxford University Press.
- French, S. & Krause, D. (2006), *Identity in Physics: A Historical, Philosophical, and Formal Analysis*. Oxford: Oxford University Press.
- French, S. & Ladyman, J. (2011), "In Defence of Ontic Structural Realism." In Bokulich, A. & Bokulich, P. (eds.), *Scientific Structuralism*. Dordrecht: Springer Press.
- French, S. & Rickles, D. (2003), "Understanding Permutation Symmetry." In Brading, K. & Castellani, E. (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press.
- Friederich, S. (2015), "Symmetry, Empirical Equivalence, and Identity." *The British Journal for the Philosophy of Science*, Vol. 66, No. 3, pp. 537-559.
- Friedman, M. (1983), *Foundations of Space-Time Theories*. Princeton, NJ: Princeton University Press.
- Frigg, R. & Votsis, I. (2011), "Everything You Always Wanted to Know About Structural Realism But Were Afraid to Ask." *European Journal for Philosophy of Science*, Vol. 1, No. 2, pp. 227-276.
- Futch, M. & Rutherford, D. (2001), "Review: *Substance and Individuation in Leibniz*." *Journal of the History of Philosophy*, Volume 39, Number 4, pp. 591-592.
- Galilei, G. (1967), *Dialogue Concerning the Two Chief World Systems*. Drake, S. (trans.), University of California Press. (2nd Edition.)
- Greaves, H. (2004), "Understanding Deutsch's Probability in a Deterministic Universe." *Studies in the History and Philosophy of Modern Physics*, Vol. 35, No. 3,

pp. 423-456.

Greaves, H. & Wallace, D. (2014), "Empirical Consequences of Symmetries." *The British Journal for the Philosophy of Science*, Vol. 65, No. 1, pp. 59-89.

Hacking, I. (1972), "Individual Substance." In Frankfurt, H. G. (ed.), *Leibniz: A Collection of Critical Essays*. Garden City, NY: Anchor Books.

Hawley, K. (2009), "Identity and Indiscernibility." *Mind*, Vol. 118, No. 469, pp. 101-119.

Hawthorne, J. & Sider, T. (2002), "Locations." *Philosophical Topics*, Vol. 30, No. 1, pp. 53-76.

Hofer, C. (1996), "The Metaphysics of Space-Time Substantivalism." *The Journal of Philosophy*, Vol. 93, No.1, pp. 5-27.

Hofstadter, D. & Dennett, D. (eds.). (1981), *The Mind's I: Fantasies and Reflections on Self and Soul*. New York: Basic Books.

Horwich (1978), "On the Existence of Time, Space, and Space-Time." *Noûs*, Vol. 12, No. 4, pp. 397-419.

Huggett, N. (1999), *Space from Zeno to Einstein: classic readings with a contemporary commentary*. Cambridge, MA: MIT Press.

Ismael, J. & van Fraassen, B. (2003), "Symmetry as a guide to superfluous theoretical structure." In Brading, K. & Castellani, E. (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press.

Ismael, J. (2001), *Essays on Symmetry*. New York: Garland.

Jauernig, A. (2008). "The modal strength of Leibniz's Principle of the Identity of Indiscernibles." *Oxford Studies in Early Modern Philosophy*, Vol. 4, pp. 191-225.

Jolley, N. (2005), *Leibniz*. Abingdon: Routledge.

Kaplan, D. (1975) "How to Russell a Frege-Church." *The Journal of Philosophy*, Vol. 72, No. 19, pp. 716-729.

Kim, J. (1993), *Supervenience and Mind*. Cambridge: Cambridge University Press.

Kment, B. (2012), "Haecceitism, Chance, and Counterfactuals." *Philosophical Review*, Vol. 121, No. 4, pp. 573-609.

Knox, E. (2014), "Newtonian Spacetime Structure In Light of the Equivalence Principle." *The British Journal for the Philosophy of Science*, Vol. 65, No. 4, pp. 863-880.

Kosso, P. (2000), "The Empirical Status of Symmetries in Physics." *The British Journal for the Philosophy of Science*, Vol. 51, pp. 81-98.

Ladyman, J. (2007), "Scientific Structuralism: On the Identity and Diversity of Objects in a Structure." *Aristotelian Society Supplementary Volume*, Vol. 81, No. 1, pp. 23-43.

Ladyman, J. (2014), "Structural Realism." *Stanford Encyclopaedia of Philosophy*. Available online.

Ladyman, J. (2016), "Are There Individuals in Physics, and If So, What Are They?" In In Guay, A. & Pradeu, T (eds.), *Individuals Across the Sciences*. Oxford: Oxford University Press.

Ladyman, J., Linnebo, Ø. & Pettigrew, R. (2012), "Identity and Discernibility in Philosophy and Logic." *Review of Symbolic Logic*, Vol. 5, No. 1, pp. 162-186.

Ladyman, J. & Ross, D. (2007), *Every Thing Must Go: Metaphysics Naturalized*. Oxford: Oxford University Press.

Lam, V. & Wüthrich, C. (forthcoming), "No Categorical Support for Radical Ontic Structural Realism." *The British Journal for the Philosophy of Science*.

Lewis, D. (1968), "Counterpart Theory and Quantified Modal Logic." *Journal of Philosophy*, Vol. 65, No. 5, pp. 113-126.

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

Lewis, D. (1979), "Attitudes de Dicto and de Se." *Philosophical Review*, Vol. 88, No. 4, pp. 513-543.

Lewis, D. (1983a), "New Work for a Theory of Universals." *Australasian Journal of Philosophy*, Vol. 61, pp. 343-377.

Lewis, D. (1983b), "Individuation by Acquaintance and by Stipulation." *Philosophical Review*, Vol. 92, No. 1, pp. 3-32.

Lewis, D. (1986), *On the Plurality of Worlds*. Oxford: Blackwell.

Look, B. (2002), "Review: *Substance and Individuation in Leibniz*." *The Review of Metaphysics*, Vol. 55, No. 4, pp. 849-850.

Look, B. (2013), "Leibniz's Modal Metaphysics." *Stanford Encyclopaedia of Philosophy*. Available online.

Magidor, O. (forthcoming), "The Myth of the *De Se*." *Philosophical Perspectives*.

Mates, B. (1986), *The Philosophy of Leibniz: Metaphysics and Language*. Oxford: Oxford University Press.

Maudlin, T. (1993), "Buckets of Water and Waves of Space: Why Spacetime Is Probably a Substance." *Philosophy of Science*, Vol. 68, No. 2, pp. 183-203.

Maunu, A. (2005), "Generalist Transworld Identitism (or, Identity Through Possible Worlds Without Nonqualitative Thisnesses)." *Logique et Analyse*, Vol. 48, No. 189-192, pp. 151-158.

Melia, J. (1999), "Hole, Haecceitism and Two Conceptions of Determinism." *The British Journal for the Philosophy of Science*, Vol. 50, pp. 639-664.

Mondadori, F. (1973), "Reference, Essentialism, and Modality in Leibniz's Metaphysics." *Studia Leibnitiana*, Vol. 5, No. 1, pp. 74-101.

Mondadori, F. (1975), "Leibniz and the Doctrine of Inter-World Identity." *Studia Leibnitiana*, Vol. 7, No. 1, pp. 21-57.

Morganti, M. (2008), "Weak Discernibility, Quantum Mechanics and the Generalist Picture." *Facta Philosophica*, Vol. 10, No. 1-2, pp. 155-183.

Mugnai, M. (2004), "Review: *Substance and Individuation in Leibniz*." *The Leibniz Review*, Vol. 14, pp. 57-63.

Muller, F. (2015), "The Rise of Relationals." *Mind*, Vol. 124, No. 493, pp. 201-237.

Muller, F. & Seevinck, M. (2009), "Discerning Elementary Particles." *Philosophy of Science*, Vol. 76, No. 2, pp. 179-200.

Nelson, A. (2004), "Review: *Substance and Individuation in Leibniz*." *Philosophical Review*, Vol. 113, No. 1, pp. 136-139.

Nozick, R. (2001), *Invariances: The Structure of the Objective World*. Cambridge, MA: Harvard University Press.

O'Leary-Hawthorne, J. & Cortens, A. (1995), "Towards Ontological Nihilism." *Philosophical Studies*, Vol. 79, No. 2, pp. 143-165.

O'Leary-Hawthorne, J. & Cover, J. (1996), "Haecceitism and Anti-Haecceitism in Leibniz's Philosophy." *Noûs*, Vol. 30, No. 1, pp. 1-30.

Parkinson, G. H. R. (1965), *Logic and Reality in Leibniz's Metaphysics*. Oxford: Oxford University Press.

Pniower, J. (2005), *Particles, Objects, and Physics*. D.Phil Thesis, University of Oxford.

Pooley, O. (2002), *The Reality of Spacetime*. D.Phil Thesis, University of Oxford.

Pooley, O. (2006), "Points, Particles, and Structural Realism." In Rickles, D., French, S. & Saatsi, J. (eds.), *The Structural Foundations of Quantum Gravity*.

Oxford: Oxford University Press.

Pooley, O. (2013), "Substantivalist and Relationalist Approaches to Spacetime." In R. Batterman (ed.), *The Oxford Handbook of Philosophy of Physics*. Oxford: Oxford University Press.

Pooley, O. (MS), *The Reality of Spacetime*. Book Manuscript.

Quine, W. V. (1969), *Ontological Relativity and Other Essays*. New York: Columbia University Press.

Quine, W. V. (1970), "On the Reasons for Indeterminacy of Translation." *The Journal of Philosophy*, Vol. 67, No. 6, pp. 178-183.

Quine, W. V. (1975), "On Empirically Equivalent Systems of the World." *Erkenntnis*, Vol. 9, No. 3, pp. 313-328.

Quine, W. V. (1976), "Grades of Discriminability." *The Journal of Philosophy*, Vol. 73, No. 5, pp. 113-116.

Rescher, N. (1967), *The Philosophy of Leibniz*. Englewood, NJ: Prentice Hall.

Rescher, N. (2003), *On Leibniz*. Pittsburgh, PA: University of Pittsburgh Press.

Rickles, D. (2008), *Symmetry, Structure, and Spacetime*. Amsterdam: Elsevier Press.

Roberts, J. (2008), "A Puzzle about Laws, Symmetries, and Measurable Quantities." *The British Journal for the Philosophy of Science*, Vol. 59, No. 2, pp. 143-168.

Rodriguez-Pereyra, G. (1999), "Leibniz's Argument for the Identity of Indiscernibles in His Correspondence with Clarke." *Australasian Journal of Philosophy*, Vol. 77, No. 4, pp. 429-438.

Rodriguez-Pereyra, G. (2004), "The Bundle Theory is Compatible with Distinct but Indiscernible Particulars." *Analysis*, Vol. 64, No. 1, pp. 72-81.

Rodriguez-Pereyra, G. (2014), *Leibniz's Principle of Identity of Indiscernibles*. Oxford: Oxford University Press.

Russell, B. (1900), *A Critical Exposition of the Philosophy of Leibniz*. Cambridge: Cambridge University Press.

Russell, B. (1903), *The Principles of Mathematics*. London: Routledge.

Russell, J. S. (2011), *Possible Worlds and the Objective World*. PhD Thesis, New York University.

Russell, J. S. (2013), "Actuality for Counterpart Theorists." *Mind*, Vol. 122, No. 485, pp. 85-134.

- Russell, J. S. (2014), "On Where Things Could Be." *Philosophy of Science*, Vol. 81, No. 1, pp. 60-80.
- Russell, J. S. (2015), "Possible Worlds and the Objective World." *Philosophy and Phenomenological Research*, Vol. 90, No. 2, pp. 389-422.
- Russell, J. S. (MS), "Quality and Quantifiers." Available online.
- Rynasiewicz, R. (1992), "Rings, Holes, and Substantivalism: On the Program of Leibniz Algebras." *Philosophy of Science*, Vol. 59, No. 4, pp. 572-589.
- Rynasiewicz, R. (1994), "The Lessons of the Hole Argument." *The British Journal for the Philosophy of Science*, Vol. 45, No. 2, pp. 407-436.
- Saunders, S. (2003a), "Physics and Leibniz's Principles." In K. Brading & E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press.
- Saunders, S. (2003b), "Structural Realism, Again." *Synthese*, Vol. 136, No. 1, pp. 127-133.
- Saunders, S. (2007), "Mirroring as an A Priori Symmetry." *Philosophy of Science*, Vol. 74, No. 4, pp. 452-480.
- Saunders, S. (2010), "Chance in the Everett Interpretation." In Saunders, S., Barrett, J., Kent, A., & Wallace, D. (eds.), *Many Worlds? Everett, Quantum Theory, & Reality*. Oxford: Oxford University Press.
- Saunders, S. (2013), "Indistinguishability." In Batterman, R. (ed.), *The Oxford Handbook of Philosophy of Physics*. Oxford: Oxford University Press.
- Saunders, S. (2016), "On the Emergence of Individuals in Physics." In Guay, A. & Pradeu, T (eds.), *Individuals Across the Sciences*. Oxford: Oxford University Press.
- Saunders, S. & Wallace, D. (2008), "Branching and Uncertainty." *British Journal for the Philosophy of Science*, Vol. 59, No. 3, pp. 293-305.
- Sider, T. (2002), "The Ersatz Pluriverse." *Journal of Philosophy*, Vol. 99, No. 6, pp. 279-315.
- Sider, T. (2011), *Writing the Book of the World*. Oxford: Oxford University Press.
- Sklar, L. (1974), *Space, Time, and Spacetime*. Berkeley, CA: University of California Press.
- Skow, B. (2005), *Once Upon A Spacetime*. PhD Thesis, New York University.
- Skow, B. (2008), "Haecceitism, Anti-Haecceitism, and Possible Worlds."

*Philosophical Quarterly*, Vol. 58, No. 230, pp. 98-107.

Sleigh, Jr., R. C. (1990), *Leibniz & Arnauld: A Commentary On Their Correspondence*. New Haven, CT: Yale University Press.

Stachel, J. (2002), "'The Relations Between Things' versus 'The Things Between Relations': The Deeper Meaning of the Hole Argument." In Malament, D. (ed.), *Reading Natural Philosophy: Essays in the History and Philosophy of Science and Mathematics*. Chicago, IL: Open Court.

Stalnaker, R. (2012), *Mere Possibilities: Metaphysical Foundations of Modal Semantics*. Princeton, NJ: Princeton University Press.

Stein, H. (1991), "On Relativity Theory and Openness of the Future." *Philosophy of Science*, Vol. 58, No. 2, pp. 147-167.

Strawson, P. (1959), *Individuals: An Essay in Descriptive Metaphysics*. London: Routledge.

Swanson, N. & Halvorson, H. (MS), "On North's 'The Structure of Physics'." Available online at <http://philsci-archive.pitt.edu/9314/>.

Teh, N. (2016), "Galileo's Gauge: Understanding the Empirical Significance of Gauge Symmetry." *Philosophy of Science*, Vol. 83, No. 1, pp. 93-118.

Teller, P. (1987), "Space-Time as a Physical Quantity." In Achinstein, P & Kagon, R. (eds.), *Kelvin's Baltimore Lectures and Modern Theoretical Physics*. Cambridge, MA: MIT Press.

Thébault, K. (2012), "Symplectic Reduction and the Problem of Time in Non-relativistic Mechanics." *British Journal for the Philosophy of Science*, Vol. 63, pp. 789-824.

Thomas, T. (MS), "Old Work for Counterpart Theorists." Sent in personal correspondence.

Turner, J. (2011), "Ontological Nihilism." *Oxford Studies in Metaphysics*, Vol. 6, pp. 1-54.

Turner, J. (forthcoming), "Can We Do Without Fundamental Individuals?" In Barnes, E. (ed.), *Current Controversies in Metaphysics*. London: Routledge.

Vailati, E. (1997), *Leibniz and Clarke: A Study of Their Correspondence*. Oxford: Oxford University Press.

van Fraassen, B. (1980), *The Scientific Image*. Oxford: Clarendon Press.

van Fraassen, B. (1991), *Quantum Mechanics: An Empiricist View*. Oxford: Clarendon Press.

Wallace, D. (2012), *The Emergent Multiverse: Quantum Theory According to the Everett Interpretation*. Oxford: Oxford University Press.

Weatherall, J. (forthcoming, *a*). "Are Newtonian gravitation and geometrized Newtonian gravitation theoretically equivalent?" *Erkenntnis*.

Weatherall, J. (forthcoming, *b*), "Regarding the 'Hole Argument'." *The British Journal for Philosophy of Science*.

Whipple, J. (2013), "Leibniz's Exoteric Philosophy." *Stanford Encyclopaedia of Philosophy*. Available online.

Wilson, A. (2013), "Objective Probability in Everettian Quantum Mechanics." *The British Journal for the Philosophy of Science*, Vol. 64, No. 4, pp. 709-737.

Wilson, M. D. (1979), "Possible Gods." *The Review of Metaphysics*, Vol. 32, No. 4, pp. 717-733.

Wilson, N. L. (1959), "Substances Without Substrata." *The Review of Metaphysics*. Vol. 12, No. 4, pp. 521-539.

Wüthrich, C. (2009), "Challenging the Spacetime Structuralist." *Philosophy of Science*, Vol. 76, No. 5, pp. 1039-1051.