KEEPING METAPHYSICS RESPECTABLE:
A METHODOLOGICAL CRITIQUE

by

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Abstract

With no consensus in sight, but an ever-proliferating array of theories, one should fear that contemporary "heavyweight" metaphysics is broken. Some, such as Amie L. Thomasson (2017), have taken this as motivation to abandon the ambitions of so-called "heavyweight" metaphysics and pursue projects of Carnapian inspiration instead. Others, such as myself, take these fears as motivation to troubleshoot and fix heavyweight metaphysics. In this task, I draw upon the critiques of metaphysical methodology from Juha Saatsi (2016; 2017b) and others, which scrutinize the use of Inference to the Best Explanation (IBE) that is so popular among metaphysicians today. Saatsi helpfully identifies two broad strategies for justifying and deploying IBE that correlate with two understandings of how metaphysical theories are confirmed: *in parallel* with scientific theories, since they address largely orthogonal questions; or *in conjunction* with scientific theories, since the subject matter of metaphysics is, in one way or another, continuous with science. (The latter strategy involves contemporary incarnations of the indispensability argument that appeal to explanatory indispensability). In the first two chapters I discuss how Saatsi's critiques apply to specific projects from exemplars of the discipline, Laurie Paul (2012a), Timothy Williamson (2013) and Ted Sider (2011). Throughout I attempt to point out pitfalls and suggest methodological improvements. In the third and final chapter I turn to a dilemma that arises from the choice between our two methodological strategies, a dilemma that concerns the very ambitions of metaphysics. These ambitions allegedly devolve into either a rivalry with science, or a mystical pursuit for answers to "esoteric" and "unanswerable" questions. In answering this dilemma, I hope to show how we might systematize our ambitions and get a better grip on our elusive subject matter.

Lay Summary

Metaphysics is the branch of philosophy in which is collected all our most "deep" questions about reality, questions such as "what is the nature of time?" and "what exists *fundamentally*: are we more than the sum of our parts?". An obvious question arises: how on Earth are we to answer such questions? Haunted by past accusations of being a nonsense armchair discipline, contemporary metaphysics (as practiced in North America) has turned to science for guidance on this difficult question of methodology. Thus *a priori* deduction is more or less out; quasi-scientific inference to the best explanation is in. *This*, it is hoped, is how we will answer metaphysical questions: by mimicking science or otherwise riding off its successes. While this methodology has been discussed piecemeal elsewhere, here I hope to provide a more systematic critique of it, with a focus on evaluating its efficacy in achieving the ambitions of metaphysics.

Preface

This thesis is the original, unpublished work of Cristian Trout, sole author.

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Acknowledgements

Dedication

To my wise friend J. Beley, a sucker for proper procedure.

Introduction

It is uncontroversial, I hope, to remark that contemporary "mainstream" metaphysics has not yet yielded much consensus in the core topics that characterize it, topics such as ontology, metaphysical modality, the nature of causation, of persistence, or of time. For this last, the lack of consensus is actually quantified by Bourget and Chalmers (2014, 477). Some defenders of the discipline are comfortable with this result. Ted Sider for one admits that metaphysics is "by its nature comparatively [more] speculative and uncertain" than the disciplines of science it means to emulate (2011, 12). Laurie Paul seems to agree (2012b, 20) and goes further, claiming that:

> It is of significant philosophical value to develop [a range of different] theories about what there is, even if we cannot prove that a theory is true or confirm it with a wide range of empirical results. That's just not what most of philosophy, especially metaphysics, involves. (25)

Others have taken this lack of convergence as a sign that something is wrong with mainstream "hard" metaphysics. Amie L. Thomasson (from whom I am borrowing the monikers "mainstream" and "hard") has raised such misgivings in recent talks (2016; 2017). Indeed, the lack of convergence should give us pause: beyond combating dogma and mental calcification, the "value" of gazing upon a diversity of possible answers is unclear. If we are practitioners of "heavyweight"[1] metaphysics, and the goal of this last is to get at the "true fundamental structure of reality" or whatever, we shouldn't be satisfied with just a proliferation of variegated accounts: those that best approximate the truth must be identified and cultivated; those that are false, weeded out.

Again, many heavyweight metaphysicians are not worried: they claim to have the means to judge between competing metaphysical theories. It is believed that by consulting theoretical virtues (parsimony, explanatory power, conservation of legacy theory etc.) and then making an Inference to the Best Explanation (IBE), metaphysics will sift through speculative proposals and eventually arrive at more definite answers (Paul 2012b, 21–22; Sider, Hawthorne, and Zimmerman 2008, 7; Sider 2011, 12; Williamson 2013, 423–429). The thought is that, if considerations for theoretical virtues work for science,[2] they should work for metaphysics as

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[1] In the sense of (Chalmers 2009).

[2] I.e., they are truth-conducive or something akin. Where possible, I try to remain neutral on epistemology.

well. Even if this method only gave us meek recommendations, having it in our toolkit would certainly be a boon.

Unfortunately for heavyweight metaphysics, the appeal to IBE in metaphysics has also come under fire recently (Saatsi 2016; 2017b; Shalkowski 2010; Huemer 2009). The general point is this: our best justifications for the use of IBE in science (i.e., scientific realist theories) do not carry over to metaphysics in any obvious way. Worse, some of the very same justifications for the use of IBE in science give us reason to believe these methods will *not* work in metaphysics.

Poor Methods or Senseless Ambitions?

In the same talks, Thomasson has taken up these points (along with others) as further evidence that there is just something wrong with heavyweight metaphysics: its ambitions are confused. She is among a growing number of metaphysicians developing deflationary approaches to metaphysics (Hale and Wright 2001; 2009; Hirsch 1993; 2002a; 2002b; 2009; Sidelle 2002; Sosa 1999; Thomasson 2015; 2020; Yablo 2009), approaches that often trace their origins back to Carnap (1950).

While these views are fascinating in and of themselves, and while many heavyweight metaphysicians seem content with directly arguing against these neo-Carnapian alternatives (see e.g., Thomasson 2015, Part II), this paper will not explore such topics. As an adherent of heavyweight metaphysics myself, happy to assume its ambitions are mostly sound (though see Chapter 3), the impulse behind this paper is instead my conviction that the methodological critiques from these skeptics *are on to something*: the methodology we employ in heavyweight metaphysics *is* suspect, and perhaps our poor showing is due (at least in part) to such methodological issues. So I welcome their critique, as it gives me something to look into with the hopes of improving. This is the task of the present paper: to examine these critiques, following where they lead, in order to determine what does or does not work *given* the ambitions of heavyweight metaphysics.

Scope and Approach

Before I get started, some clarifications are in order, starting with the meaning of "mainstream" "hard" and "heavyweight" metaphysics. As mentioned, these are labels coined by suspicious onlookers, referring loosely to a currently popular tradition in analytic metaphysics that usually traces its origins to Quine (1948). Here are some of the features that characterize this tradition: practitioners tend to claim there is a strong similarity or continuity between metaphysics and

the sciences, in methodology and or in subject matter; practitioners accordingly claim that metaphysics is more than "just" conceptual analysis or conceptual negotiation (Sider 2011, 5; Williamson 2013, 423; Paul 2010, §§2–3), taking a less "deflationary" and more "realist" attitude towards metaphysical questions (such as whether composite objects like tables exist, or whether statues and lumps of clay are distinct collocated entities or a single entity); practitioners correspondingly take these questions to be substantial (not merely verbal) and non-trivial to answer; finally, many, though certainly not all, have developed or endorsed a theory of "fundamentality" that usually hinges on a key term, such as "structural" (Sider 2011, Chap 6), "grounding" (Fine 2001; Schaffer 2009), "real" (Fine 2001; 2009), or "truth-making" (Armstrong 1997; 2004). Metaphysics as practiced by these philosophers is the metaphysical tradition referred to by "mainstream," "hard" and "heavyweight" metaphysics, and it is to these philosophers that I am speaking to most directly (though where I defend our ambitions I am also speaking to doubters such as Thomasson). In the interest of brevity, I will drop the labels going forward: unless otherwise specified the metaphysics I will be referring to is of this tradition.

Now, it should be noted that this tradition is by no means exact or homogenous, bringing into question the pertinence of any general methodological critique. Indeed we'll find that, in order to get any critique off the ground, we have to distinguish between at least two broad methodological strategies metaphysicians employ. Further complicating matters is sub-disciplinary variation in methodology as well as variations in attitude, with some philosophers taking a heavyweight attitude only toward certain questions of metaphysics. However, wherever and insofar as these philosophers do take up science-emulating methods with heavyweight attitudes, the discussions here should be germane. It won't apply to every practice of every metaphysician, but still a considerable majority: the critiques target central assumptions of this methodology, widespread among metaphysicians.

All that said, to ensure my discussion is grounded, relevant and avoids riding roughshod over nuance, I will frame it around a few contemporary exemplars from the tradition, namely Laurie Paul, Ted Sider and Tim Williamson, and their views on two paradigm topics of the tradition, namely ontology and metaphysical modality.

The paper proceeds as follows: Chapter 1 will distinguish two broad methodological strategies for justifying and deploying IBE in metaphysics, followed by an examination of critiques made against the first strategy; Chapter 2 will continue this examination, focusing on the second

strategy; finally, Chapter 3 will turn to the long-standing question of how metaphysics relates to the rest of science – whether it rivals, aids or is separate from the rest of science.

Chapter 1:

Can Metaphysical Methodology Parallel Scientific Methodology?

Metaphysicians often appeal to theoretical virtues via an Inference to the Best Explanation (IBE) in order to defend or challenge a position (Saatsi 2017b, §2). Indeed they are quite explicit about making this their methodology of choice (Paul 2012b, 21–22; Sider, Hawthorne, and Zimmerman 2008, 7; Sider 2011, 12; Williamson 2013, 423–429). The intuition behind this move is this: if metaphysics is in the same business as science, i.e., investigating the largely "mind-independent" world and not simply analyzing our conceptual or linguistic schemes, and if IBE works in science (considered a very successful enterprise), then IBE should work in metaphysics (Saatsi 2017b, §3). Though this intuition has been questioned from a variety of angles in recent years (Saatsi 2016; 2017b; Shalkowski 2010; Huemer 2009), I find Saatsi's critiques to be the most powerful;[3] this and the following chapter will focus on applying them to the views of various metaphysicians so as to better gauge their severity and import for specific metaphysical projects.

These critiques are particularly powerful because of how much they concede to the metaphysician: Saatsi is happy to grant that IBE is effective in everyday scenarios and science, and willing to grant the ambitions of heavyweight metaphysics are kosher. What he questions is the migration of these methods from the everyday and scientific context to the metaphysical context: he claims that while in the first two contexts we have plausible justification for trusting IBE, these justifications do not translate to the case of metaphysics. While this does make his critiques dependent on the philosophy of science, we will see it does not depend on any niche partisanship within the philosophy of science. In fact Saatsi's critiques are again so powerful because they are made *assuming* a *scientific realist*[4] outlook for the sake of argument,[5] the

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[3] As I mention below, this is because Saatsi is willing to humour the scientific realist inclinations that many metaphysicians appeal to when they defend their use of IBE (e.g., Paul 2012b, 22; Sider 2011, 12–13). The scope of Shalkowski's critique is slightly narrower in that it only targets those metaphysicians who endorse an empiricist epistemology (e.g., Quine 1969).

[4] The view that we should take science as not simply yielding empirically adequate models (merely yielding reliable predictions of experience) but that these models are importantly true, or approximately

outlook friendliest to metaphysics and the outlook many metaphysicians explicitly appeal to (or otherwise assume) when justifying their use of IBE in metaphysics (e.g., Paul 2012b, 22; Sider 2011, 12–13). Echoing Dorr (2008, 43) Saatsi just insists we be mindful of the details of the scientific realist theories actually defended by philosophers of science.

Following Saatsi (2017b, §3), we'll proceed by distinguishing two broad strategies metaphysicians employ in order to justify their use of IBE in metaphysics, looking at critiques of each in turn. The first rides on the intuition that if IBE is deployed successfully in science then it should be successful in metaphysics: it is thought that since metaphysics is similar enough to the sciences (e.g., in its ambitions), the methodology of metaphysics can and should *parallel* or *mimic* the methodology of sciences with success. The second strategy that metaphysicians deploy says that metaphysical posits are confirmed *in conjunction with* science, given a certain degree of confirmational holism observed in science.

The rest of this chapter will be devoted to discussion of the first strategy. This strategy has been given voice by various contemporary metaphysicians, but nowhere is it more full-throated than in Paul's writing (2012b): she begins by remarking the similarities between metaphysics and science (21), and then proceeds to claim that "if we accept inference to the best explanation [by appeal to theoretical desiderata such as simplicity, elegance etc.] in ordinary reasoning and in scientific theorizing, we should accept it in metaphysical theorizing" (22). Saatsi breaks down this second claim into two separate claims (2017b, 170) which, together with the observation that both metaphysics and science employ IBE, give us a positive reason to trust IBE in metaphysics:

(1.1)   Both metaphysics and science employ inference to the best explanation.
(1.2)   We have no reason to think that: if explanationism is truth-conducive in science, it is not so in metaphysics.
(1.3)   We have a positive reason to think that: if IBE is truth-conducive in science, it is also so in metaphysics.
(1.4)   Therefore the use of IBE in metaphysics is justified.

The critiques of this strategy will target (1.2) and (1.3) independently.

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true, or successfully refer to the entities they posit, or reflect mind-independent reality in some significant way. For more see (Chakravartty 2017, §1).
5 Consider for example (Saatsi 2017b, 173).

1.1 Critiques of the first strategy

Instead of rehashing Saatsi's work line by line, I think it would be more helpful to illustrate and discuss his critiques by applying them to specific claims from specific authors and then extrapolating from there. For examples of this first strategy we will be considering the work of Laurie Paul and Tim Williamson.

1.1.1 A problem for (1.2): sophisticated Scientific Realism

Premise (1.2), which says we have no reason to think that if IBE is truth-conducive in science then it is not truth-conducive in metaphysics, is the very least that strategy 1 metaphysicians endorse. Consider Paul's claims:

> A scientific realist should take such desiderata [e.g., parsimony, elegance, explanatory power] to be truth-conducive [...] If such theoretical desiderata are truth conducive in science, they are also truth conducive in metaphysics. [...] If the method can lead us closer to the truth in science, it can lead us closer to the truth in metaphysics. (2012b, 21)

Or consider Williamson's line, that if IBE is applicable to various special sciences outside of philosophy then "[g]ood methodology permits the application of abductive criteria of elegance and simplicity to theories of metaphysical modality" (2013, 424, 425). Clearly they endorse at least something like (1.2).

To this strategy's credit, (1.2) does not seem totally implausible. As Paul and Williamson imply and Saatsi admits, to the extent that metaphysics and science are "in the same epistemological boat" and are similar endeavours – both seek answers to questions about the mind-independent observable and unobservable world around us – we have defeasible reason to believe (1.2) (Saatsi 2017b, 172).

The trouble is, as Saatsi points out, there seem to be relevant differences between the application of IBE in scientific contexts and metaphysical contexts. Saatsi convincingly argues that the history and philosophy of science pushes anyone with realist leanings to adopt a qualified scientific realism, [6] in which IBE's reliability is curtailed in certain respects. Unfortunately, that curtailment tends to be on metaphysical territory:

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[6] Chakravartty corroborates this (Chakravartty 2017, §1.3).

> [A]gain and again in the history of science it is precisely the metaphysical and ontological assumptions concerning the nature of gravity and other forces, light, disease, life, genes, and so forth—the assumptions that were underwriting the best scientific understanding of the relevant phenomena—that have subsequently turned out to be false [...] [But] even if IBE cannot reliably function as a guide to the fundamental nature of things (as the history suggests), it is open for the realist to maintain that scientists, with their IBE-laden methods, nevertheless systematically "latch onto" reality with their theories in ways that largely account for the predictive and instrumental successes of science. (173, 174)

Not only is reliability on many metaphysical matters directly curtailed, but were a metaphysician to attempt to mimic the scientific realist in somehow qualifying how IBE is truth-conducive in their field, the metaphysician doesn't have the resources the scientific realist does: there doesn't seem to be any progress in predictive or instrumental success to speak of in metaphysics, which are precisely the features scientific realists rely on to carve out and defend IBE's qualified truth-conduciveness in science (174).

For metaphysics generally, this is concerning. But let us see how severe the issue really is when applied to our specific authors. For our first case study we'll look to Paul's recent work in ontology. Because it will become relevant later, I should specify that Paul is interested here in a much more neo-Aristotelian sort of ontological project.[7] In her words:

> An ontology is defined by its fundamental categories. Following Peter van Inwagen (2011, quoted in Paul 2017), I take divisions between fundamental categories to mark "real divisions between things" that determine the basic categorical structure of the world. Speaking metaphorically, the fundamental categorical structure of the world carves the world at its fundamental joints. (2017, 32)

This is not the more Quinean project of merely cataloguing what exists.

Unfortunately for Paul, the application of IBE to such an ontological project seems to fall squarely where sophisticated scientific realism abandons IBE's truth-conduciveness. Consider Paul's defense of a "one category ontology," the view that "denies we need more than one

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[7] Schaffer is to be credited for reviving this sort of ontological project, in opposition to Quine's project. See (Schaffer 2009) for more.

fundamental category[8] to support the ontological structure of the world" (2017, 32). Consider how she pursues such a defense on the grounds that "one category ontologies are deeply appealing, because their ontological simplicity gives them an unmatched elegance and sparseness" (32). And finally consider that it is with regards to fundamental ontological posits that IBE has repeatedly yielded falsehoods, showing it to be an unreliable guide to truth in such matters.

Indeed there is some irony in Paul's own writing regarding this point: she seems to criticize her rivals for indulging "our corpuscular intuitions and our attraction to classical-mechanical or particle-based depictions of the world" (2017, 46) suggesting that outdated assumptions about fundamental physics have misled these ontologists, while many physicists (e.g., string theorists) have willingly abandoned those intuitions (43). What goes unnoted is that if IBE (and intuition) *failed on these matters in the past* why should we think these methods will deliver now? Something about classical mechanics was right – but it wasn't its fundamental ontological picture: that is the sophisticated and *qualified* scientific realism we have to deal with, precisely Saatsi's point. Other "[f]amous, now-rejected ontological posits include gravitational force, caloric, phlogiston, luminiferous ether, electromagnetic ether, circular inertia, miasma, vortices, vital forces, and electron orbits, to name a few" (2017b, FN18). Granted, these examples from the history of science don't speak to *exactly* the same questions as those of Paul and other neo-Aristotelian ontologists: none of these failed ontological claims of science seem to be of the right level of generality or abstractness to make them directly about fundamental ontological categories. No one is or ever was proposing that the fundamental categories of which everything "is built" were marked out by luminiferous ether or phlogiston or vital forces or whatever. This remark is in keeping with how strategy 1 metaphysicians conceive of their discipline and its relationship with science: it parallels science and is constrained by it, but science rarely if ever makes direct pronouncements on metaphysical issues. How wide and what difference this gap in subject matter makes for is an open question not settled by anything either Saatsi or Paul says. However, I think Saatsi is right to raise the alarm: these examples are the closest science gets to broaching the questions neo-Aristotelian ontologists raise, and the results have not been promising. IBE's unreliability[9] on these matters does seem to be a reason to worry that even if IBE is (generally) truth-conducive in science, it might not be for neo-Aristotelian ontology, throwing (1.2) into question for Paul's work.

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[8] "Categorical fundamentality is understood in terms of the metaphysically prior, as that in which everything else in the world consists." (2017, 32)
[9] I should note that how we understand this unreliability does take away some of the bite in this critique, but not all of it (Saatsi 2017b, 171–172).

I move onto my second case study, Williamson's work on metaphysical modality and its connection to modal logic. As with Paul, it's worth describing what he takes his inquiry to be. Since Williamson is his own best expositor and since the details of his project will become important, I hope the reader will forgive extensive quotation. In *Modal Logic as Metaphysics* he explains:

> Logic and metaphysics overlap. Appropriately interpreted, different logical systems form the structural core of different metaphysical theories. The choice amongst such theories is to be made by abductive standards similar to those in the rest of science […] We fixed interpretations of the modal operators, as expressing metaphysical possibility and necessity, and of the quantifiers, as unrestricted, in accord with the ambitions of metaphysics. Modal logic in this form aims to discover which generalizations in such terms are true. The true generalizations constitute a quantified modal logic but we do not know ahead of enquiry which one […] [I]n a scientific spirit [our task is] to build and test theories that codify putatively true generalizations of the sort at issue, to find out which are true. Those theories are not about our language or thought, or any other actual or possible creatures' language or thought, except incidentally, since they are about everything whatsoever. (2013, 423)

And what is a metaphysical interpretation of the modal operators according to Williamson? In a subsequent paper he summarizes:[10]

> In ordinary thought we can get at it through counterfactuals, as what would be the case no matter what was the case [(Williamson 2007; quoted in 2016b)]. At a more theoretical level, we can approach the same metaphysical modality as the limiting case of a kind of modality we may call objective, by contrast with other categories such as epistemic modality. Physical [as well as nomic] possibility is an example of an objective modality; so are various grades of practical possibility. […] [O]bjective modalities correspond to what linguists classify as circumstantial readings of natural language modal auxiliaries such as 'can' and 'must'. The category is similar to that of objective interpretations of probability, by contrast with epistemic, subjective and purely logical interpretations. Metaphysical possibility is simply the maximal type of objective possibility, in the sense that

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[10] A more thorough elaboration of his conception of metaphysical modality can be found in (Williamson 2007, Chap 5; Williamson 2016a).

something is metaphysically possible if and only if it has at least one objective

type of possibility… (2016b, 707)

Incidentally, this "Reply" was prompted by methodological concerns similar to those of Saatsi: Sider (2016) argues that simplicity arguments (a particular form of IBE) are not applicable to all fields of inquiry, and in particular, not applicable to modal logic. Like Saatsi, Sider raises concerns over the track record of inquiry into modal logic, worried that it is pursued "mostly within philosophy," suggesting a lack of consensus or applicability[11] (694). Williamson responds by pointing to the successful development and application of modal logic in other disciplines (mathematics, computer science and economics to name a few). He then goes on to defend the track record of *alethic* modal logic under a metaphysical interpretation of the operators, pointing to some degree of consensus among philosophers on much of "the basics," allegedly more consensus than many other branches of metaphysics (2016b, 706, 707).

I won't investigate these claims further, but if Williamson is right, they should go some way toward inoculating Williamson against Saatsi's first critique. Saatsi may rightfully complain that this is not the relevant track record: Williamson and Sider focus on relatively recent work and mostly within philosophy, whereas what we should be concerned with is the long-term track record of (objective) alethic modal claims in science, since these would seem to be the closest analogs to metaphysical modal claims in philosophy. However, when I try to think of this track record, I draw something of blank. As far as I'm aware, the history and philosophy of science has much less to say about the track record of modal claims in science, poor or otherwise (Saatsi certainly never mentions anything in this regard). This seems to be because (at least under Williamson's understanding of objective interpretations of alethic modal operators as extensions of common counterfactual thinking) alethic modal claims are deeply tied to science as a predictive enterprise. Consider nomic necessity (arguably the closest to metaphysical necessity in its level of generality). We have been making claims about what is nomically necessary since at least Descartes and presumably with much success. I'm unaware of any laws of physics that we have held in radical error for any extended period of time. Even if we consider Newton's laws of classical mechanics as strictly speaking false (as opposed to simply limited in their domain of application),[12] their falseness doesn't seem to be anywhere near the degree of the ontological errors mentioned earlier. Thus, without further indication of a poor track record, I would agree

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[11] Sider takes this to show that the ideology of modal logic (the modal operators) is not fundamental (i.e., "joint-carving" or "structural") which in turn means modal logic is not a fundamental subject matter (nor reducible to one), and is therefore, non-substantive in some sense, unfit for simplicity arguments (Sider 2016, 692–695).

[12] As some philosophers of science do (Woodward 2003, Chap 5).

with Williamson: "[i]f metaphysical modality is just the limiting case of a category of modality many types of which are explored by the natural and social sciences, the presumption should be that we may legitimately apply the same sort of abductive methodology in theorizing about metaphysical modality that is applied in theorizing all over the natural and social sciences" (2016b, 708).

Put all together, it seems Saatsi has much less of a case for concern in Williamson: under Williamson's understanding of metaphysical modality (the correctness of which I am not commenting on), the sophistication of scientific realism does not seem to cast any particular doubt on IBE's truth-conduciveness in matters of metaphysical modality.

1.1.2   Another problem for (1.2): finding a naturalistic epistemology

Saatsi briefly raises another challenge for premise (1.2): conceiving of a plausible naturalistic epistemology[13] for the IBE-laden methodology of metaphysics (2017b, 174–176). Strategy 1 metaphysicians cannot, without argument, rely on the literature that has developed natural epistemologies for science since strategy 1 metaphysicians conceive of metaphysics as paralleling and mimicking science (especially in its methodology) but not as being encompassed by science in its subject matter (see Paul 2012b, 2–9), and so metaphysical theses are not (in general) confirmed or disconfirmed *with* science. Hence the need for a separate epistemology for metaphysics.

As before, Saatsi points to general differences between science and metaphysics (such as the lack of empirical feedback in the latter) to suggest general difficulties metaphysicians might have in developing a naturalistic epistemology as compared to philosophers of science. However, the severity of this challenge will again depend on the specific metaphysician and specific metaphysical project in question.

Williamson seems very aware of this challenge (2007, 136) spending considerable time developing such an epistemology for metaphysical modality (2007, Chap 5). Since he argues for understanding metaphysical modality as the limiting case of other objective modalities (2016a) which are in turn rooted in counterfactual thinking, he can ground his epistemology of

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[13] I.e., "to account for the truth-conduciveness of inference to the best explanation [in metaphysics] in a way that coheres sufficiently well with the rest of our naturalistic world view, according to which our capacities and ways of reasoning about the world are products of natural and cultural evolution, and every reliable mechanism of theorizing is reliable for a natural reason amenable to a scientific study" (Saatsi 2017b, 174). Of course the burden of such a task would only be felt by metaphysicians who want to ally themselves with naturalism, but I find this to be most metaphysicians (when referring to heavyweight metaphysicians in the analytic tradition, as I am).

metaphysical modality in our ability to judge parochial counterfactual cases. This ability to judge counterfactuals seems to involve distinct cognitive abilities (such as some kind of ability to simulate experiences in our minds) which, crucially, are recognized outside of philosophy and studied by cognitive science (2007, Chap 5, §3). Furthermore, I would add that an ability to judge accurately the physical counterfactuals typical of our ancestors' environment has plausible benefits making it a candidate for natural selection: it seems like a clear advantage for survival to be able to accurately judge how one's environment might change under counterfactual conditions, or what prey and predator might do in counterfactual circumstances. In short, Saatsi cannot charge Williamson with lacking a developed and plausible naturalistic epistemology (which Saatsi agrees is all that can be asked for) for this project in metaphysical modality.

Unfortunately the same cannot be said for Paul's project. Paul is very aware of work in cognitive science, pointing to research into ordinary experience and perception which show that mechanisms of our cognitive apparatus seem to bias our metaphysical understanding of the phenomena underlying our experience (2010; 2016). While cautioning against bias and other cognitive pitfalls is important and improves our methodology, it does not quite answer Saatsi's question of how *homo sapiens* could come to know anything about the fundamental ontological categories of the world in the first place. If anything it only makes the question more pressing.

For Paul's project, the only thing in the way of an epistemology seems to be first, an intuition that "we enjoy a direct grasp" of certain metaphysical aspects of reality (namely the nature of parthood); and second, the suggestion that we can rely on thought experiments and ordinary experience to help us (2012a, 221–222; 2012b, §§2.2–2.3, respectively). Both of these points are somewhat undercut by the findings of cognitive science raised in her other work (which she recognizes), and neither of them answer Saatsi's challenge in a satisfying way without further argument.

1.1.3   A Problem for (1.3): finding a positive reason to trust IBE

We move on to what I take to be the weakest of Saatsi's critiques: his doubts about (1.3), the premise that says we have a *positive* reason to think that if IBE is truth-conducive in science, it is also truth-conducive in metaphysics (2017b, 176–184). To start, I have to wonder if metaphysics really has the onus of producing a *positive* reason to trust IBE for metaphysical work. It seems to me that so long as we establish (1.2), we are warranted in taking the gamble and at least *trying* this method in the absence of any clearly better one. And perhaps establishing anything like (1.3) requires testing the methodology over the long run. Still, Saatsi is

right to discuss this premise since various metaphysicians do try to establish something like it. These attempts usually involve pointing out similarities between what makes an explanation good and successful in science and what allegedly makes one so in metaphysics. Saatsi discusses two of the most common explanatory virtues raised, that is, providing *understanding* and providing a *unifying account* of disparate phenomena (e.g., Swoyer 2008).

Generally I agree with Saatsi's admonition: metaphysicians tend to make "broad references to these features" in "extremely general and abstract terms" which "pays a mere lip service to the literature on scientific explanation" (177). He proceeds to make good on this claim by pointing to work in the philosophy of science that complicate these notions of *power to give understanding* and *unification*, just as he did with scientific realism. For example, the notion of "understanding" is shown to be too psychological and subjective a notion to be uncritically taken as an objective measure of an explanation's goodness. Furthermore where philosophers of science *have* been able to extract any kind of objective measure, they have only been able to do so by relying on features of scientific explanation not present in metaphysics (such as predictive and instrumental success) (2017b, 177–180).

However, some of these complications seem more like mere technicalities. For example, with regards to an explanation's ability to unify phenomena, Saatsi seems to only point out that, under leading contemporary accounts of unification (and how it might be an epistemic virtue), it is not construed as an explanatory virtue of *theories* (e.g., Relativistic Mechanics) but rather *individual explanations* (e.g., explanations of anomalies in Mercury's orbit) (182, 183). If there is any such explanatory virtue of theories, it is derivative. While this is a valuable insight, it does not sound too worrisome. Case in point: when Williamson appeals to the virtue of unification, it is only with respect to specific modal axioms in his proposed modal logic and not the entire logic as a whole (2013, 425, 426 and; 2016b, 708). For at least some metaphysical projects then, it seems these technicalities can be navigated.

1.2   Assessment of the first strategy

Strategy 1 aims to justify the use of IBE in metaphysics by appeal to alleged similarities between metaphysics and the rest of science, despite their being generally orthogonal to one another (claims in the rest of science do not generally impinge on metaphysics and vice versa). The most constructive critique of this strategy comes from Saatsi (2017b): he gives no sweeping knockdown argument but instead points to various challenges for the strategy that arise due to

complicating details from *the most metaphysics-friendly* philosophy of science. Not every challenge is as difficult as the next: for example, we saw that the challenge of finding a positive reason to trust IBE was not as threatening as, say, the sophistication of scientific realism. Furthermore, particular metaphysical projects fare better than others in the face of particular challenges: we found, for example, that inquiry into metaphysical modality (as Williamson understands this) is not as threatened as neo-Aristotelian ontology by the challenge of producing a plausible naturalistic epistemology for itself.

All in all then, strategy 1 certainly seems serviceable, at least for certain metaphysical projects. For those who pursue said strategy, hopefully this discussion helped identify where it is in most need of improvement. But for metaphysicians who find strategy 1 too rickety or broken to salvage, there is still strategy 2, the topic of the following chapter.

Chapter 2:

Are Metaphysical Theses Confirmed in Conjunction with those of Science?

Some metaphysicians prefer a different strategy for justifying their use of Inference to the Best Explanation (IBE). This second strategy is premised on the thought that metaphysical posits are confirmed *with* science, given a certain degree of confirmational holism. Following Quine (1951b, §VI; 1991, 268–269) confirmational holism is roughly taken to recommend we endorse all theoretical assumptions that "contribute to our best theory being the best," e.g., their having predictive power, explanatory power and or other virtues (Saatsi 2017b, 169). By contributing to a theory's success, these assumptions, no matter how abstract, are thought to "inherit a borrowed luster" and "merit our belief" (Sider 2011, 12). In argument form, the full strategy looks something like this:

(2.1)   Science[14] successfully gets closer to the truth by using IBE to sift and choose between various theories that are empirically adequate: IBE identifies which theories are "best" in light of the various theoretical virtues it appeals to.

(2.2)   Confirmational Holism[15] (CH): Confirmation in science is spread holistically across all the statements that contributed to a theory's success (a theory's success being that which confirms it, however this is specified).

(2.3)   Corollary of 2.2: we ought to be committed in an appropriately realist way to "*all* theoretical assumptions that are responsible for the successes—whether predictive, explanatory, or whatever—that provide realism-eliciting evidence for a given theory" (Saatsi 2017b, 168, emphasis original).

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[14] Following Quine (Quine 1997, 49), "science" is construed broadly to include everything from physics and chemistry to sociology and economics.

[15] This is also known as the Quine-Duhem thesis. See (Ariew 2020, §2.1) for history and discussion. CH is not to be confused with Semantic Holism (another Quinean thesis), the view that says the smallest units of meaning are not sentences or words, but systems of sentences large enough to have some sort of critical semantic mass. I should also note that there are a variety of non-equivalent formulations of CH, some of which Ariew discusses. Though perhaps another point on which to press metaphysicians, Saatsi does not lean on any specific formulation of CH so I leave the matter aside.

(2.4)   There is no first philosophy: metaphysics is just an extension of science which elaborates and compares deeper theoretical assumptions that science employs (or something along these lines).

(2.5)   Therefore the use of IBE is justified in metaphysics since IBE is already brought to bear (if only implicitly) on metaphysical matters, these deeper theoretical assumptions.

One will recognize premises (2.2) and (2.3) as key components of a Quinean indispensability argument,[16] and the strategy here is certainly related to such arguments. Indispensability arguments can be seen as examples of metaphysicians "bringing to light what theoretical assumptions are in play in our best overall theories of the world." There is a sense in which IBE (and other methods) have *already* been applied to the topic by scientists and any further use of IBE by metaphysicians is merely to make clear what theoretical devices (entities, propositions, pieces of ideology) are *indispensable* to science: these are what we should be committed to in an appropriately realist way. So for example, if the positing of a certain entity is indispensable to our most successful science, we should take this entity to exist.

My reconstruction of this second strategy, as with Saatsi's reconstruction of the first, is not meant to be taken as the exact argument given by any particular metaphysician, since I have not come across any such detailed rendition of the strategy. With Saatsi I "construe them as *aspiring* to the schema[s] presented here" (2017b, FN13, emphasis original). For more discussion of how exactly to understand this strategy see §3.2.1.

How (2.3) is meant to follow from (2.2) is not important to Saatsi's critique (only that it is a corollary), hence I omit the (surely debatable) premises needed to reach (2.3) from (2.2). Again, Saatsi is giving all the benefit of the doubt to metaphysicians with respect to questions in the philosophy of science. He concedes scientific realism is broadly correct: scientific realists, by these very credentials, do maintain at least *something like* (2.3) for at least some controversial assumptions (e.g., the existence of unobservables like quarks and bosons) and they do so (typically) by appeal to some form of CH.

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[16] In its original form, an argument that claims we should be committed to a certain theoretical entity (e.g., mathematical entities) given that we quantify over such entities *indispensably* in our best scientific theories. This form of argument has since been applied to more than just *entities* (i.e., a theory's ontology) and the "indispensability" in question has been given various qualifiers (e.g., explanatory indispensability). For more see (Colyvan 2019).

2.1 Critiques of the second strategy

Saatsi's critiques target premise (2.3). However, before I get into any details it is worth bracketing one concern the reader might have: is Saatsi misrepresenting metaphysicians? Do they really take the corollary of CH to be that we should commit ourselves to *all* theoretical assumptions? While it's true that particular metaphysicians have been happy to make particular restrictions on strategy 2 and only extract the kinds of commitments from science that they like,[17] I don't think Saatsi is misrepresenting metaphysicians when he claims they accept (2.3). Metaphysicians *do* accept (2.3) wholesale, as a corollary of CH, and if they have other strictures (usually metaphysically motivated), these are applied *afterwards*. Case in point, *early* in his book (Sider 2011, 12, 13, 97) Sider quite explicitly endorses (2.3), in reference to Quine and CH, before *later* applying restrictions to (2.3) based on what disciplines he considers to be "fundamental" and why (2011, 267; 2016). Broadly speaking, Saatsi's critique again points to sophistications in scientific realism that curtail the universal quantification in (2.3), limiting strategy 2 *before* any qualifications are added by the metaphysician. With this worry put aside, we can look at the details of Saatsi's comments. As before, in order to bring the critiques to life, I will be discussing them in the context of specific metaphysical projects. For this we will be aided by the writings of Williamson again,[18] and Sider.

2.1.1 *Explanatory Indispensability Arguments and Explanatory Roles*

Noting strategy 2's connections with indispensability arguments, Saatsi focuses his discussion on the traditional topic of such arguments, mathematical realism (i.e., Platonism). He begins by briefly commenting on the original Quine-Putnam indispensability argument, bringing to bear his earlier remarks about how our best theories of scientific realism curtail the reliability of IBE. He claims that the indispensability argument (and certain contemporary voicings of it, e.g., Psillos 2012, 53) concerns "ontological commitment of *literally true* theories," which in light of today's more sophisticated scientific realism makes it naïve or "over-optimistic" in certain ways,

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[17] For example, Quine only ever argued that we should be committed to *entities* our best theories quantified over, since Quine was only interested in the cataloguing of what exists and thought there was only one meaning for "exists," the meaning captured by the standard first order quantifiers. He may have had his philosophical qualms with Sider's similar appeals to confirmational holism to conclude that we should be committed to whatever ideology our best theories employ as being the most fundamental ideology, i.e., the ideology that "carves nature at its joints" (2011, 13). And as was noted earlier, Sider for his part argues that we should restrict our use of IBE to only what is either a fundamental subject matter or that which is reducible to a fundamental subject matter, limiting the conclusion of strategy 2 in his own way (2016).

[18] Williamson is precisely so interesting because he is one of the few to explicitly employ both strategy 1 and strategy 2. See (Williamson 2016b, 708). There are complications involved though: see §2.3 and §3.2.1 below.

since it takes the scientific method (IBE included) to yield literal truths where the history of science indicates otherwise (Saatsi 2017b, 185, emphasis original). In other words, by scientific realist lights, CH does not entail that we ought to take a realist attitude toward *all* theoretical devices used in successful theories. On its own, this remark casts doubt on (2.3), but only in a vague way: it suggests no *specific* ways in which (2.3) should be limited, and gives no clear way to determine what theoretical devices should be taken with a realist attitude and which shouldn't.

Thankfully, Saatsi says more. He moves on to his principal target, more recent "explanationist" variants of the indispensability argument which claim we should be committed to mathematical entities because they play an indispensable *explanatory role* (and not just because they must be quantified over) in various scientific theories. This qualification is meant to bolster the argument and help avoid criticisms of reading metaphysical theses off of our best scientific theories in too simplistic a manner. Saatsi rightly points out that this is all well and good, so long as this crucial notion of an indispensable "explanatory role" is given some analysis in connection with theories of explanation. If it isn't, there is no guarantee that explanatory indispensability demands ontological commitment from us.

To see this we turn to another paper of Saatsi's (2016) which elaborates distinctions between types of explanatory roles based on leading realist accounts of explanation in the philosophy of science. To start, he summarizes a threefold division due to Salmon (1984) between broad categories of explanation, a division debated but commonly referenced in the philosophy of science. The passage (Saatsi 2016, §4) is already clear and concise, but I will try to distill his exposition here: a handle on these distinctions will become vital further on. The three conceptions of explanation are the *epistemic, ontic* and *modal*. The division is premised on the distinct sources of explanatory power, that in virtue of which an explanation can be called explanatory.

Under the epistemic conception, the source of explanatory power lies in the explanation's ability to provide understanding, that is, its ability to help epistemic agents with limited cognitive capacities such as ourselves to comprehend the phenomena in question. There are numerous accounts of what it is to provide understanding but the traditional one comes from Hempel (1965): it is to show that the explanandum was to be expected, given the explanans. This conception of explanation seems much too psychological to be of any help to the metaphysician's plight so we put it aside.

Under the ontic conception, the source of explanatory power lies in the explanation's ability to situate the explanandum "within a broader ontic structure of the world," by citing any relevant worldly facts. Causal and mechanistic facts[19] are paradigmatic but not exclusive. As Saatsi notes:

> [N]ot all explanatory dependence is causal. A law can depend on other laws in an explanatory way, but laws do not cause other laws. An explanandum can depend on structural constitution in an explanatory way that is not causal, as in the case of glass's fragility being explained by its molecular structure. An explanandum can depend on more abstract (yet still real) structural features of the world, as in the case of a Lorentz contraction being explained in relativity in terms of the fundamental kinematic structure of reality. (Saatsi 2016, 1052)

This is the conception of explanation Saatsi thinks has the best chance of getting metaphysicians the connection they need between explanatory indispensability and ontological commitment.

Finally there is the modal conception of explanation, meant to capture explanations which show that "what did happen had to happen" (Salmon 1984, 293; quoted in Saatsi 2016). Saatsi thinks that, in its original formulation from Salmon, this conception is somewhat "inchoate" and hard to distinguish from the ontic conception. However, a recent characterization from Lange (2013) *is* of interest. According to Lange the modal conception applies distinctly to at least mathematical explanations in science where the ontic conception does not (510), since under his interpretation a modal explanation is one which shows "that the explanandum is inevitable in the sense that it holds independently of any contingent ontic structure at stake" (Saatsi 2016, 1053). Consider:

> The fact that twenty-three cannot be divided evenly by three explains why it is that Mother fails every time she tries to distribute exactly twenty-three strawberries evenly among her three children without cutting any (strawberries!).
> (Lange 2013, 488)

The explanation may refer to ontic structures (e.g., the mother having three children and twenty-three strawberries) but these are irrelevant to the explanation since they are assumed in the why question that prompts the explanation (i.e., "why does Mother fail each time she tries to distribute exactly twenty-three whole strawberries evenly among her three children?") (497). Instead the mathematical explanation explains by exploiting how the world must be *as a matter*

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[19] E.g., the fact that the baseball was struck with the bat caused it to go flying, which in turn caused the window to shatter when it struck the window.

of mathematical necessity, regardless of any (typically contingent) ontic structures of the world (496). Saatsi elaborates this conception because *prima facie* it looks helpful to the mathematical realist who is trying to get more mileage out of an indispensability argument, but he (and Lange) ultimately conclude that this modal conception of explanation is still orthogonal to the goals of mathematical realism: granting that such "distinctively mathematical" explanations exist and can reveal modal features of reality, does not obviously imply that mathematical entities exist. There is no particular reason to think that say, a mathematical fictionalist, could not deliver the same explanatory goods (Saatsi 2016, 1055). (This insight from Saatsi will become important later).

After singling out the ontic conception as the most likely candidate for supporting the connection between explanatory indispensability and ontological commitment, Saatsi claims that "within the ontic conception there is scope for critical, more fine-grained distinctions between different types of explanatory roles" (1056). The distinction of interest to him is what he calls *thick* and *thin* explanatory roles, characterized thus:

> *Thick explanatory role* is played by a fact that bears an ontic relation of explanatory relevance to the explanandum in question.

> *Thin explanatory role* is played by something that allows us to grasp, or (re)present, whatever plays a 'thick' explanatory role. (1056)

Although Saatsi never gives necessary and sufficient conditions for judging which role an explanatory element is playing, this does not severely diminish the distinction[20] or seriously impede our ability to judge specific cases, as we shall see. He spends most of his remaining ink (§§5–7) going through various contemporary accounts of ontic explanation, and finding evidence or room for such a distinction in each account. In this respect, Saatsi is convincing. He also suggests each time why the *thin explanatory role* is the more apt descriptor for mathematical entities which, if correct, would severe the wanted link between explanatory indispensability and ontological commitment. However, providing an irrefutable argument against the new explanatory indispensability arguments for Platonism is not his primary goal. Rather, like his later paper, it is to challenge metaphysicians on the finer points of their methodology, insisting they don't gloss over the details of scientific realism, naturalistic epistemology, and here, accounts of scientific explanation. And while in this paper he focuses on

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[20] Especially since various other authors have raised similar distinctions (cf. Psillos 1999; Yablo 2005; Yablo 2009; Hawley 2006).

mathematical realism, he suggests these details matter to other metaphysical projects which appeal to "explanation-driven arguments," whether that's an "explanationist" indispensability argument, or an appeal to the explanatory power of some or other theoretical device to argue it merits (or doesn't merit) a realist attitude (§8). Hence we arrive at a clear restriction on (2.3), at least in regards to explanation-driven arguments: a realist attitude is only merited toward those theoretical devices that play a *thick explanatory role* in scientific explanations. So let us consider the implications for some other metaphysical theses.

2.1.2   An ontological thesis: necessitism and possibilia

According to necessitism, necessarily everything is necessarily something. In its formal rendition: □∀x □∃y (x=y). Since this formula is valid under Williamson's preferred system of modal logic for metaphysical modality, Williamson is a necessitist. This system also validates the controversial Barcan Formula, "◇∃xFx → ∃x◇Fx" and its converse. Thus, on the uncontroversial assumption that John F. Kennedy and Marilyn Monroe could have had a child, Williamson is committed to the necessary existence of something that is the merely possible child of John F. Kennedy and Marilyn Monroe (for more see Williamson 2013, Chap 2). In a similar fashion, necessitists such as Williamson accept countless possibilia into their ontology that contingentists reject. Strictly speaking it is only this thesis, that countless possibilia exist, which we will be evaluating in this section.

How does Williamson arrive at this ontological conclusion? First, it's important to understand that for Williamson this ontological thesis is *a by-product* of his primary thesis: that modal logic S5 combined with quantifiers that range over a fixed domain is in some sense the "correct" system of modal logic for metaphysical modality (more on this later). Call this specific combination of logical axioms and model theory the W-system. Williamson focuses on mustering evidence for the truth of the W-system under its intended metaphysical interpretation of the modal operators. It is his commitment to the W-system (under its intended interpretation) that, according to Williamson, commits him to possibilia. In fact, he takes this commitment to possibilia to be one of the most damning pieces of evidence *against* the W-system (2013, Chap 1). If it turns out that neither his system nor the evidence he musters for it actually commit him to possibilia, this should be a welcome turn of events for Williamson (assuming he hasn't grown too attached to his possibilia). Let us investigate this on Williamson's behalf then, starting by checking whether the evidence he provides for the W-system's correctness really commits him to the existence of possibilia.

In *Modal Logic as Metaphysics* Williamson's methodology is abductive, in the vein of strategy 1 (2013, Afterword). However, in subsequent papers he also makes use of strategy 2 when he appeals to the natural sciences' implicit use of modal logic. He argues that the W-system is implicitly integral to the structure of *phase spaces* or *state spaces*,[21] the abstract mathematical representation of the space of possibilities for a physical system (2016b, §2; 2016a, §§5–6). Finally, Williamson does appeal to the explanatory power of phase spaces and by extension, the system of modal logic embedded in them (2016a, 472), taking this to provide evidence that the W-system is the "correct" one for metaphysical alethic modality (2016a, §5, §8; 2016b, §3) and that we should be committed to the existence of countless possibilia (2016a, §6). So we can ask, what kind of scientific explanations are the valid formulas of Williamson's proposed system implicitly or explicitly implicated in? Again, explanations of the epistemic type are of no help to the metaphysician anyway so we won't be considering these. Though the ontic type of explanation is most promising, let me first briefly touch on modal explanations (as Lange conceives these).

Recall that Lange conceives of a modal explanation as one that explains why something is inevitable *independent* of any causal relations or other contingent ontic structures (2013, 487). It seems to me that, under any objective reading[22] of the modal operators, valid formulae or theorems of the "correct" modal logical system should be able to play a similar role to that of the mathematical "facts" serving in the examples of mathematico-modal explanations Lange considers. If Williamson is right about the implicit commitment of the natural sciences to the W-system, then there may be *logico*-modal explanations (regarding for example, why all phase spaces whatsoever are a certain way) that involve theorems or valid formulas of this system. Such explanations may indicate that Williamson's *system* is "correct" in some sense, but, echoing Saatsi, it is not clear to me that they demand any sort of *ontological commitment* to possibilia. Just as mathematical fictionalism could plausibly supply the explanatory goods for *mathematico*-modal explanations, it's not clear why some sort of "necessitist fictionalism"

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[21] In dynamical systems theory, phase spaces are the mathematical representation of all possible states of a given system. A simple example from classical mechanics consists of all possible values of position and momentum through time for an object in a given system. This type of model is particularly useful when the initial conditions of the system (e.g., the starting position and momentum of the object) are unknown or where the slightest differences in initial conditions lead to vastly different outcomes for the system (i.e., chaotic systems). "State space" is mostly synonymous with "phase space" with the caveat that talk of state spaces usually connotes the treatment of states that are *discrete* (in which all or some of the variables that represent the states are discrete, e.g., the states of a chess board) instead of *continuous* (in which all of the variables that represent the states are continuous, e.g., the states of a double pendulum). This aligns with preferences of certain disciplines: computer scientists and engineers tend to talk of states spaces; physicists and mathematicians, phase spaces.
[22] In the sense of (Williamson 2016a, §1).

couldn't supply the goods for the *logico*-modal explanations in question. This necessitist fictionalist would take key formulas of the necessitist system (such as "$\Box\forall x\ \Box\exists y\ (x=y)$") to be strictly speaking false under their intended metaphysical (or physical)[23] interpretation and only "true within the story of necessitism" so to speak. If the mathematical fictionalist can maintain that only some mathematical claims are correct, even though the mark of their correctness is neither truth simpliciter nor truth of some nominalistic paraphrase, there should be a similar line of argument for the necessitist fictionalist to maintain the correctness of their *system*.[24] Thus the thesis about which system of modal logic is correct and the ontological thesis come apart: one can accept the W-system as being the correct one for metaphysical modality yet reject the existence of possibilia.

Again, this should come as welcome to a defender of the necessitist *system* such as Williamson, since the ontological commitment to possibilia is usually seen as unwelcome baggage. But is he committed to this baggage some other way? We turn now to the ontic type of scientific explanations. Recall, these are explanations which appeal to various "ontic" features of the world in their explanans, such as the momentum, solidity and trajectory of a certain baseball, the brittleness of a certain glass pane, and any relevant laws of nature to explain why the glass shattered when the baseball struck it. These are the type of scientific explanations Williamson has in mind when he claims that (modal logic laden) phase spaces have an indispensable explanatory power. Do these ontic explanations that use phase spaces commit him to possibilia? To answer this, we need to verify that possibilia play a *thick explanatory role* in the relevant explanations and not merely a *thin* role.

We will consider an example pulled from microbiology. Anderson et al. create a model that simulates the growth of tumorous cells under various genetic and environmental scenarios in order to better understand the causal mechanisms involved in the emergence of aggressive and

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[23] Strictly speaking, explanations in science would only involve *physical* interpretations of necessity and possibility (e.g., nomic necessity). This caveat does not worry Williamson however: see (2016b, 708; 2016a, §8).

[24] For example, Balaguer proposes "a mathematical sentence is fictionalistically correct if and only if it would have been true if there had actually existed abstract mathematical objects of the kinds that platonists have in mind" (Balaguer 2018, §2.2) and furthermore claims that this is the right way to define fictional correctness (for mathematics) because (among other things) it aligns much better with actual practice by grounding the definition in the conceptions and intuitions of practicing mathematicians (Balaguer 2001). Williamson could similarly claim that (under the intended metaphysical or physical interpretations) a formula of quantified modal logic is fictionalistically correct if and only if it would have been true had there in fact existed mere possibilia of the sort necessitists have in mind. The indispensability of phase space studies, the crippling limitations a contingentist system would impose on such studies, and scientists' "free use of quantification over possible individuals, possible states, sets of possible states and so on" (Williamson 2016a, 483) could all count as evidence for this definition.

invasive cancer cells (2006). The details of this model and its results are not critical for my purposes, but I should explain why this example was chosen. Firstly, because it is an example of an *agent-based* model, a model in which (many) variables stand for individuals (in this case tumorous cells). This is the type of model whose use would most clearly commit us to the ontological thesis of necessitism (Williamson 2016a, §6). Secondly, I think judgments about the explanatory role of possibilia in this example will be relatively clear cut.

Granting Williamson's interpretation of the scientists' mathematical model, let us assume that the model speaks of and makes (indispensable) use of merely possible cancer cells in order to answer questions about the macroscopic behavior of the system (such as what conditions induce a tumor fingering pattern, characteristic of invasive cancer). Do these possibilia play a *thick* or merely *thin* explanatory role? Judging from examples given in Saatsi (2016; 2017a) and Woodward (2019), it seems quite clear that the possibilia are playing a thin role here. Consider the parallels with a different explanation, this time from chemistry:

> [S]uppose that a mole of gas is confined to a container of volume V_1, at pressure P_1, and temperature T_1. The gas is then allowed to expand isothermally into a larger container of volume V_2. One standard way of explaining the behavior of the gas—its rate of diffusion and its subsequent equilibrium pressure P_2—appeals to the generalizations of phenomenological thermodynamics—e.g., the ideal gas law, Graham's law of diffusion, and so on. (Woodward 2019, §4.3)

In this example, any reference to individual trajectories of individual molecules seems *irrelevant* to the explanation of the macroscopic behavior of the gas, since "[t]here are a very large number of different possible trajectories of the individual molecules in addition to the trajectories actually taken that would produce the macroscopic outcome—the final pressure P_2—that we want to explain" (§4.3).[25] In other words, only the generalizations that describe how the variables P, T and V relate to each other are playing a thick explanatory role here; if reference to individual molecules plays any role at all, it is only a thin one. A similar point can be made for

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[25] This also suggests that agent-based models are only indispensable *epistemological tools* for the discovery of relevant laws and generalizations governing the types of systems in question (systems of cells, systems of gas molecules etc.), and *not* indispensable for *explanations* (since once we have learned of the generalizations, we no longer need to refer to any simulations with agent-based models). This agrees with how many scientists consider such models (the textbook reference: Axelrod 2001, 3–4). This is yet another stumbling block in the path from the scientific reference of a particular entity to an ontological commitment to that entity. Space doesn't permit further discussion: for the sake of argument we assume here that the possibilia are in fact *explanatorily* indispensable in the example from microbiology.

the microbiological[26] explanation from Anderson et al. What seems to play a thick role in their explanation of the macroscopic behavior of the tumor (such as the emergence of a fingering pattern), are generalizations that describe how various environmental factors affect the prevalence of certain phenotypes. The reference to many individual (merely possible) cells may be indispensable (perhaps for representing or otherwise grasping these generalizations) but only the generalizations themselves seem to play a thick explanatory role: much like the chemistry explanation, what matters is not the *specific* evolutions of *specific* lineages of cells (since many such evolutions would produce the same macroscopic outcome) but rather the statistical tendency for that macroscopic outcome under certain environmental conditions and given the mechanics of cell evolution. Therefore, possibilia only seem to play a *thin* explanatory role in this example, from which no ontological commitment to possibilia can be obviously extracted.

So much for that example one might say: perhaps a different one commits us to possibilia. Perhaps, but remark how the bar has been raised: possibilia must now be shown to play not just an explanatorily indispensable role, but a *thick* role. What we have shown here for the debate over possibilia is what Saatsi has shown for the debate over mathematical entities: we are not licensed to *directly* infer ontological commitment from explanatory indispensability. That is the methodological point we both stress. Strategy 2 is not defective, merely complicated: corollary (2.3) needs to be modified to say that, with regards to *entities* playing an indispensable (or very powerful) explanatory role, we ought to only be committed to those *entities* playing a *thick* explanatory role.

To sum up, neither the logico-modal nor ontic explanations involving phase spaces yield an ontological commitment to possibilia. Without further details of the scientific explanations in question, explanatory indispensability arguments on their own are not enough to settle such ontological theses. But what of Williamson's real prize, determining the "correct" modal logic for metaphysical modality? Are explanatory indispensability arguments capable of making such determinations? We turn to this next.

2.1.3   An ideological thesis: the modal logic for metaphysical necessity and possibility

In the previous section we explored how Williamson leans on strategy 2 to conclude that countless mere possibilia exist. However, ontology is not Williamson's focus. Recall,

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[26] Indeed it seems something similar can be said for a variety of explanations which involve explaining the macroscopic behavior of microscopically complex systems. See (Woodward 2019, §4.3).

Williamson's primary goal is to determine which system of modal logic (axioms + model theory) is the "correct" one under a metaphysical interpretation of the modal operators. This is a question of *ideology* by the lights of Quine's ontology-ideology dichotomy (1951a): it concerns what concepts we should accept into our language and what expressive power they afford us (cf. Williamson 2013, Chap 6–7). Unlike questions of ontology, which are answered by straightforward existence claims, accepting a certain element of ideology as "correct" need not be accompanied by anything like a "realist" attitude toward the ideology in question, or sentences expressed with said ideology.

Consider for example, a mathematical fictionalist, who claims that "a mathematical sentence is fictionalistically correct if and only if it would have been true if there had actually existed abstract mathematical objects of the kinds that platonists have in mind" (Balaguer 2018, §2.2; see also Balaguer 2001). Thus they can agree with Platonists that, for example, the truths of arithmetic are "correct" in some sense, even if literally speaking false. As was suggested earlier, this sort of anti-realist attitude seems available to theorists of metaphysical modality as well (see FN25 above), and there may even be reason to endorse such a view: to avoid committing oneself to possibilia. This view might seem quite exotic though, and anyway it's only hypothetical (no one, to my knowledge, has endorsed such a view). However, this is not the only anti-realist attitude in town. Thomasson (2020) carefully develops a modern form of modal conventionalism, according to which alethic modal claims are object language expressions of linguistic rules. Key to her program is the claim that (alethic) modal ideology and modal statements *do not serve a descriptive function*: in Huw Price's terminology, they do not serve as "e-representations" whose job it is "to co-vary with something else — typically, some external factor, or environmental condition" (2011, 20; quoted in Thomasson 2020, 8). Despite this anti-realist attitude, Thomasson can still agree that only some modal logics correctly capture the function they play in our reasoning (2020, §3.2, especially FN10). Finally, scientists themselves don't seem to think of state spaces or phase spaces as somehow modeling modal reality or anything of the sort. Instead they seem to think of them as an epistemological tool for acquiring synthetic data to analyze, as the formalization of a type of simulation thinking[27] that helps us better understand systems for which other methods of analysis are infeasible (Axelrod 2001, 3–4; quoted in Burguillo 2018, 1–2). Such simulation allows us, for example, to qualify the chaotic

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[27] To be clear, I am not suggesting that scientists are employing an *epistemic* interpretation of the modal operators. Williamson (Williamson 2016b, 704) is right to rule this out: scientists are not exploring what is *epistemically* necessary or possible for the system. However, this is beside the point. The point is that, employing alethic modal reasoning does not obviously commit us to there being *modal features of the world* that modal statements are *about*. If, for example, Thomasson's understanding of metaphysical modality can also codify modal logic S5 as being "correct", isn't this all science needs?

nature of systems that are governed by differential equations for which solutions are extremely difficult to produce (Strogatz 2015, 1–11).

Such attitudes conflict with Williamson's: he believes that "one role for logic is to supply a central structural core to scientific theories, including metaphysical theories" (2013, xi) and that, under the metaphysical interpretation of the modal operators, the task of modal logic "is in a scientific spirit to build and test theories that codify putatively true generalizations of the sort at issue", theories that "are not about our language or thought, or any other actual or possible creatures' language or thought, except incidentally, since they are about everything whatsoever" (423). Clearly then, mere "correctness" will not suffice for Williamson. However, given the attitudes surveyed above, I fear that truth under intended interpretation won't be enough either, since *we have no guarantee that the so-called metaphysical interpretation of the modal operators has in fact anything to do with what heavyweight metaphysics is supposed to be about:* we have no guarantee that it has anything to do with highly general and deep features of the world, or whatever (see §3.2.2 below for more). Williamson is a heavyweight realist about alethic modal logic: he does not believe the correct or true modal logic is so in virtue of some feature about us, our language, or our need for epistemic tools. Instead he must believe it is so in virtue of deep metaphysical truths (such as the existence of countless possibilia perhaps) or in virtue of nothing, since the truths of modal logic are simply the deepest sort of metaphysical truth. But this realist attitude may not be warranted – especially for someone who elsewhere appeals heavily to counterfactual and simulation thinking to characterize his understanding of metaphysical modality (2007, Chap 5; 2016a).

Let us call such questions of how and whether we should entertain a realist attitude toward certain elements of ideology (or sentences expressed in terms of them), questions of ideological commitment (cf. Hirsch 2013). Questions of ideological commitment are not new: for example, philosophers have long debated what logic or mathematics are *about*. Do they express deep worldly truths or "merely" truths of psychology? Are they merely formal systems, with no content? Are they merely useful fictions? The question for us is: *can such questions be answered with an application of IBE via strategy 2*?

Ted Sider believes so. Recall, corollary (2.3) states that we ought to be committed in an appropriately realist[28] way to all theoretical assumptions that are responsible for the successes

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[28] I'm purposefully being vague about what constitutes a "realist" attitude, since different philosophers will want to say different things about it. I am only using "anti-realist" and "realist" to mark a vague

of a given theory. Included in these theoretical assumptions are the ideological choices we make: which concepts we employ with which precisifications or associated rules of use. According to strategy 2 then, we have ideological commitments to the indispensable elements of our ideology, and this is precisely what Sider believes (2011, 12, 13, 97). Incidentally, he also holds an anti-realist view of metaphysical modality: relying on strategy 2, he employs IBE to conclude that "the metaphysically necessary truths" merely denotes a hodgepodge collection of truths that we have come to consider special, for reasons that have everything to do with us and nothing to do with deep metaphysical truths [29] (Chap 12). Williamson and Sider generally agree on methodology (2016, 690) but they disagree about the indispensability of the alethic modal operators. We won't be commenting on this last, but instead investigating their methodology: is this application of strategy 2 without reproach? We validated Saatsi's critiques with regards to ontological commitments – will they also apply to ideological commitments? In order to answer these questions, we need to better acquaint ourselves with Sider's framework.

A complete exegesis of the Siderian metaphysical enterprise is beyond the scope of this paper. A brief summary of the relevant details will have to suffice. According to Sider, there is a privileged way of describing the world: he believes that "for a representation to be fully successful, truth is not enough; the representation must also use the right concepts, so that the conceptual structure matches reality's structure" (Sider 2011, vii; for discussion see Hirsch 2013). To be clear, Sider is not talking about just some psychologistic demand, such as the demand that inquirers express their theories in a perspicuous manner. Failing to use the "right concepts" for Sider is to misrepresent reality in some deep sense. These "right concepts" form the structural ideology, the ideology that carves nature at its joints, that gets at the fundamental structure of the world so to speak. This is what characterizes Sider's realist attitude toward certain elements of ideology. It is of course open to Williamson to reject this *specific* realist attitude, but then he should look to develop and defend his own lest anti-realist attitudes win the day. To my knowledge, he does not do so. Since Sider does develop machinery for the explicit purpose of deciding when and how to regard ideology with a realist attitude, Sider is who we are investigating.

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contrast in general attitude between theorists. For my preferred explanation of what is going on here see §3.2.2 below.

[29] In Sider's terminology, he believes no modal ideology is structural. He claims:

> [N]ecessity does not carve at the joints. There are many candidate meanings for 'necessary' [...] our linguistic community might choose. Since none of these candidates carves at the joints, our linguistic community is free to choose whichever of these it likes. (Sider 2011, 269)

In other words, he takes questions of what modal ideology we should adopt to be *nonsubstantive* questions (Sider 2011, Chap 4). Notably, he reaches this conclusion by directly applying IBE via strategy 2 (Sider 2011, 267 but see also §§12.2–12.12, arguably one extended IBE).

Sider's realist attitude is descended from a family of such attitudes that we might call "world-privileging" attitudes: some elements of ideology are "objectively" privileged, privileged by the world so to speak, in that they line up with reality in some way. Sider's "structural ideology" is the generalized cousin of the "sparse predicates", an alternative account of what it is for certain ideology to be world-privileged: according to Armstrong the sparse predicates are special in that each one stands for a *universal*, as he understands these last; according to Lewis, they are special in that each stands for a *natural property*, as he understands these last (Sider 2011, Chap 6). Sider's generalization of these accounts is twofold. First, there is no focus on the predicate: a linguistic element of any grammatical category can be structural according to Sider, not just predicates. Second, Sider's account of world-privileged ideology is not "entity-based": structural ideology is not so-called because it stands for any *entity* in the world (such as the abstracta of Armstrong or Lewis' account). Despite his prose, officially Sider does not reify any such entity as a joint in nature (92). The fact that some element of ideology *X* is structural, that it lines up with the world's structure, is as fundamental a fact as it gets according to Sider (2011, §7.13). Again, I won't be commenting on the intelligibility or correctness of such realist attitudes here: for the time being, let us assume we can make sense of the idea that the world privileges certain ideology, and that "realism need not be ontic" (Nerlich 1982; quoted in Sider 2011, 92).

One task in Sider's project then is to identify our ideological commitments, as he understands this – to single out the structural ideology. As mentioned earlier, to carry out this task Sider applies IBE liberally to metaphysical debates, and said methodology is justified with strategy 2 (2011, §2.3). In addition, Sider frequently refers to explanatory power as a key theoretical virtue, precisely the theoretical virtue under scrutiny by Saatsi. So do Saatsi's critiques apply to Sider's methods for discovering world-privileged ideology? Not in any obvious way. Saatsi's discussion of thin and thick explanatory roles focused on their connection to *ontological* commitment, not *ideological* commitment. Furthermore, because Sider's account of what it is for a piece of ideology to be world-privileged is *not entity-based*, an ideological commitment for Sider will not collapse into an ontological commitment to some entity the privileged ideology stands for, such as a universal or natural property. If his account were entity-based, like Armstrong's or Lewis', we might be able to rerun Saatsi's methodological objections.[30] As it stands however, Saatsi's

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[30] It might go something like this. A metaphysician claims we have an ideological commitment to some predicate, say "has electric charge," since it affords indispensable explanatory power. This entails (according to an entity based account of world-privileged ideology) that we have an ontological commitment to a certain universal or natural property. We can then ask whether this entity (via our use of the corresponding ideology) really plays a thick explanatory role, carrying out a similar analysis to that which we carried out for Williamson's alleged ontological commitments to possibilia.

critiques appear to be orthogonal to Sider's investigation into our ideological commitments. Sider's application of strategy 2 presents itself as a distinct case for our methodological inquiry.

So what should we make of Sider's appeal to strategy 2 in his project? Does it pass muster? To lay out my cards: I think we'll find that, though Saatsi's specific critique may not apply, an analogous critique does.[31] Recall, the driving intuition behind Sider's methodology is that:

> A good [i.e., simple, powerfully explanatory etc.] theory isn't merely likely to be true. Its ideology is also likely to carve at the joints. For the conceptual decisions made in adopting that theory—and not just the theory's ontology—were vindicated; those conceptual decisions also took part in a theoretical success, and also inherit a borrowed luster. (12)

Somehow, our *best overall theory of the world* tracks *the world-privileged ideology*. But this is not obvious: does Sider have an independent argument to this conclusion?

There are suggestions of one in his work. Before discussing the promising formulation of this argument, let me dismiss an initially tempting but ultimately circular argument. It goes something like this: IBE ranks theories that are expressed in world-privileged ideology higher because being expressed in such ideology makes a theory comparatively *better*. Therefore, we can read the world-privileged ideology off of our *best* overall theory of the world, the overall theory of the world ranked highest by IBE. Whatever truth there may be in the initial premise of this argument, *it is no help if we do not know how to identify the world-privileged ideology*. Sider cannot offer up this premise as a response when the driving methodological question for his work is *how do we identify the world-privileged ideology?* Unfortunately, at times this can appear to be Sider's view.[32] At various points in *Writing the Book of the World* he makes claims such as "the epistemic achievement [of an explanation] will be diminished if cast in non-jointcarving terms" (64) and ""theories" based on bizarre, non-jointcarving classifications are unexplanatory even when true" (23 – see also §3.1 and §3.3). This is, at least in part, a call back to Sider's bold metaphysical thesis: that the goal of inquiry is not only to represent the world truly, but also to represent it with *perfectly world-privileged ideology* – to describe the world

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on its own terms so to speak (vii). Again, while this may be true, simply reminding inquirers of this extra obligation does little to help them succeed in fulfilling it.

I take the more promising formulation of his argument to be as follows:

(3.1)   The status of being world-privileged ideology can be conferred in degrees. In Sider's terms: there are not only perfectly jointcarving words but also comparatively more or less jointcarving words (2011, §7.11.1).

(3.2)   A word's degree of world-privilege is proportional to the degree[33] of explanatory power it confers, where this is measured by the number and depth of explanations couched in terms of that word.

(3.3)   Our best overall theory of the world is, among other things, the theory that best balances theoretical virtues such as simplicity and explanatory power (in the sense above) while satisfying all our explanatory needs.

(3.4)   Corollary of (3.3): our best overall theory of the world will tend toward using the shortest list of most explanatorily powerful words that still allow it to fulfill all our explanatory needs.

(3.5)   From (3.4) and (3.2): our best overall theory will tend toward using the most world-privileged ideology.

(3.6)   Therefore our best overall theory of the world is a reliable guide to the world-privileged ideology.

Though Sider never explicitly endorses this argument, there is evidence that this is what he has in mind when it comes to his methodology.[34] Sider all but endorses premise (3.2): his picture of science is one in which special science explanations are indeed explanatory but only in virtue of their being underpinned by reductions to fundamental physics[35] (2011, §7.8, §7.11.1, 267; see also 2016, 689–692). According to Sider, the ideology of fundamental physics combined with that of classical logic and set-theory[36] confers the greatest explanatory power: explanations from physics *and* the special sciences are *both* to be construed as "couched" in terms of this physics-mathematics driven ideology, since special science vocabulary is considered to be some sort of

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[33] I mention that world-privilege comes in degrees not only to elaborate more of Sider's view but also to make (3.2) sound more plausible. The connection between explanatory power and world-privilege would be quite strained if the former variable could vary widely in value (taking any natural number as a value) while the latter was a Boolean variable (taking only True or False as values).

[34] Compare how well this methodology lines up with Sider's approach in Chapters 11 and 12, chapters meant to exemplify the application of his methodology (Sider 2011, ix).

[35] He thinks of these reductions as the giving of a "metaphysical semantics" for a subject matter, and more specifically the giving of "metaphysical truth-conditions" for a sentence. See (Sider 2011, §7.8).

[36] Sider's preferred ideology – see (Sider 2011, Chap 13).

paraphrase[37] for complex expressions in the language of fundamental physics plus logic and set-theory.

Of course this is only the sketch of an argument: questions abound. For example, much rides on how "simplicity" is characterized in (3.3) in order to reach corollary (3.4): it's not clear how "simplicity" in our best theories is meant to transform into their favoring the "shortest list" of ideology. But simplicity is not the focus of our discussion here (though see §2.2 below). Instead, explanatory power and it's relation to world-privileged ideology is our focus. What should we make of (3.2) then? Again, the details matter. The exact formula for measuring explanatory power will be crucial, this time for the plausibility of the premise itself. As stated, the method of measurement doesn't seem nuanced enough: it doesn't seem impossible to concoct words which clearly aren't world-privileged but, by design, do fabulously on this measure for explanatory power, "tricking" the measure so to speak. Imagine we recast all of science in highly disjunctive terms – unlikely candidates for being world-privileged like "being an electron-or-cow" (Sider 2011, 2) – such that all our most explanatorily powerful terms are themselves highly disjunctive. As Dorr notes, highly disjunctive concepts can surely figure in explanations of phenomena described in highly disjunctive terms (Dorr 2013, 722). So why wouldn't we be able to scramble our language such that this is the case all the way down? In various writings Sider has recognized this problem: that explanatory power (as well as simplicity) can fail as epistemic guides when there is no restriction on what ideology we allow ourselves to use in formulating our theories (2011, §3.3, §4.5; 2016, 690; 2020, 18−19).[38] Indeed, he fears this could be IBE's undoing, for *all inquiry* (i.e., for *all of science*). Sider's reply is always the same: the best way of saving IBE from this problem is to restrict our attention to ideology that is world-privileged. In other words, Sider thinks IBE is only really applicable to fundamental matters (subject matter phrased in perfectly jointcarving terms). This may be a reasonable suggestion for all other inquirers (for whom Sider is trying to solve this problem) but if we are inquirers such as Sider, *inquirers searching for what the world-privileged ideology is*, he cannot save IBE with this suggestion. After all, if uncovering world-privileged ideology is the *prize* of our inquiry, a methodology that depends too heavily on *prior knowledge* of what ideology is world-privileged

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[37] For more see (Sider 2011, §7.7).

[38] He understands this as another instance of Goodman's "general problem of projection," the problem of showing that some predicates are indeed objectively "projectible" (e.g., "blue" and "green") while others are not (e.g., "grue" and "bleen"). The question is whether some predicates objectively match a "ready-made" world as opposed to all predicates being simply constructions that refer to a fundamentally unstructured world (Goodman 1955). Among other things, this "Goodmania" spells trouble for IBE, and induction (Sider 2011, §3.3, §4.5, §4.6). Protecting these essential epistemological tools *for science* is Sider's goal when he discusses this problem.

will not be of much use.[39] Circularity has crept back in and it threatens to undo Sider's own methodology. In sum, Sider cannot, at least for the purposes of metaphysical methodology, supplement his measure of explanatory power in a way that relies heavily on prior knowledge of what ideology is world-privileged, lest it defeat the purpose of the present argument and produce another unusable methodology. However, if the measure is not strengthened, countless ad hoc and arbitrary elements of ideology could count just as explanatory as the most intuitively world-privileged.

These are just some of the details to be worked out. But even assuming Sider successfully does so, there is still another problem with this argument. However plausible principle (3.2) may be, it would be quite *implausible* if it were *without exception*.[40] For example, given that we are creatures with limited cognitive abilities, surely some elements of ideology are merely indispensable grease to our cognitive gears, ideology that is world-*unprivileged* (it doesn't "line up" with the world in any way) but without which our explanatory abilities would be unacceptably crippled. There also may be reason to worry that some world-privileged ideology confers no especially great amount of explanatory power. In short it would be *unbelievably convenient* if (3.2) were without exception. Without an independent argument to support it, we should remain skeptical of this. Thus, just as Saatsi's distinction between thick and thin explanatory roles complicates corollary (2.3), exceptions to (3.2) would also complicate corollary (2.3): just as we are not licensed to infer ontological commitment from explanatory indispensability (as the entity in question may only play a thin role), we don't seem licensed to infer ideological commitment from mere explanatory indispensability (as the element of ideology in question may be an exception to (3.2)).

Just as before, this does not spell disaster for strategy 2. However, it does complicate its use: for inquiry into both our ontological and ideological commitments, the strategy needs supplementing. For example, perhaps Sider could provide case-by-case analyses to investigate whether a given element of ideology is not an exception to (3.2). Unfortunately, to my knowledge, these extra details have so far gone unheeded by metaphysicians who avail themselves of strategy 2 for this particular metaphysical project.

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[39] Sider may be able to get some mileage out of this suggestion, if he can show how some kind of reliable intuitions and pre-theoretical beliefs are enough to get the bootstrapping process rolling. Of course, this assumes we have reliable (and frankly mysterious) intuitions about which elements of ideology are world-privileged.

[40] Strangely, Sider doesn't seem to consider this possibility, even though he is disposed to agree in order to avoid "hard choices": somewhat incredibly, Sider maintains "there *is* a real question about which of propositional logic's connectives [e.g., "∧" or "∨"] carve at the joints" even though he is only led to accept such questions "by argument, against inclination" (Sider 2011, 217, emphasis original).

2.2   Assessment of the second strategy

Strategy 2 seeks to legitimize the use of IBE in metaphysics by bringing metaphysics under the fold of science, claiming that metaphysical questions can be answered by surveying science (where IBE is already applied), given a degree of confirmational holism and the implicit reliance on metaphysical theses in science.

Following Saatsi (2016; 2017b) we began our critique by focusing on questions about the relation between indispensable explanatory roles and ontological commitments, finding that the connection is not without exception. Where this connection is invoked by strategy 2 metaphysicians (e.g., Williamson 2016a), we saw how these exceptions complicate matters. Much later we found in Sider (2011) an appeal to a similar relation: that of a word's world-privilege being proportional to its explanatory power. This would yield ideological commitments for any explanatorily indispensable ideology. As with the earlier correlation, we have reason to be wary of potential exceptions, complicating any appeal to it by strategy 2 metaphysicians.

As with the critiques of strategy 1, neither of the critiques elaborated here unequivocally doom strategy 2, but instead demand supplements for the strategy, at least where explanatory power is the key theoretical virtue appealed to. Further research might pursue a systematic procedure for identifying when an entity is playing a thin explanatory role, or when an element of ideology is an exception to (3.2). Otherwise metaphysicians will have to rely on case-by-case analysis, as we did for Williamson's possibilia, and as Saatsi does for mathematical entities (2011). (For what it's worth, Saatsi is doubtful a systematic procedure can be devised (2016, 1060)).

Owing, I think, to the greater complexity and longer history of strategy 2, the critiques it gets are more specific and narrower in scope than those laid on strategy 1. For example, I only had room here to discuss critiques of one theoretical virtue, explanatory power. This does not mean there is less to critique in strategy 2 (though it may mean the strategy is more mature). For example, discussions of similar length could be had over the other theoretical virtues commonly invoked, parsimony and other forms of simplicity being particularly high profile targets. With regards to this last, Huemer (2009) makes good headway, giving parsimony the same treatment Saatsi (2016) gives explanatory power: he details under what conditions parsimony can be construed as truth-conducive according to our best philosophy of science (again granting scientific realism), and then proceeds to examine whether those conditions are met in the context of various metaphysical disputes. In the two examples of metaphysical disputes he chooses and for the specific conditions of truth-conduciveness he evaluates in those examples, he concludes,

rightly I think, that the conditions are not met. However, Tallant (2013) argues that some of these conditions are met for at least some metaphysical disputes (namely, that of presentism vs. eternalism). While important first steps, these papers are only just the start: in Cowling's terminology (2013), Huemer and Tallant primarily discuss *quantitative ontological parsimony*, which concerns minimizing the number of ontological commitments. Quantitative ontological parsimony would most directly concern disputes such as Williamson's acceptance of possibilia against those who reject possibilia. In addition to quantitative ontological parsimony, there is also *qualitative ontological parsimony*,[41] as well as *quantitative ideological parsimony* and *qualitative ideological parsimony*.[42] As explained in Cowling's paper, this last is most relevant to Sider's metaphysical project of uncovering the world-privileged ideology.[43] Cowling makes an interesting preliminary defense of appeals to this type of parsimony in metaphysics, but clearly there is much more to be said about appeals to the various forms of simplicity in metaphysics.

2.3 The Bigger Picture

We began by laying out two strategies for how to justify and deploy IBE in metaphysics. With our critiques of both strategies complete, it's time to take stock. How did the strategies fare overall?

First, a reminder of intent: my goal is to critique the methodology of practicing metaphysicians in the hopes of improving our chances at achieving the aims of our inquiry. The point of focusing on specific philosophers was twofold: first, to see how these general critiques looked on the ground, in actual practice; and second, to compare how these critiques played out in different sub-fields of metaphysics. It is *not* my intention to rate philosophers. If the critiques tend to stick more to Sider or Paul's work I think it is because their "post-modal" approach to metaphysics is much younger and underdeveloped, as compared to work on metaphysical

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[41] Which concerns minimizing the number of *kinds* of ontological commitments a theory has.

[42] Which concern, respectively, minimizing the number of ideological commitments and minimizing the number of *kinds* of ideological commitments a theory has.

[43] Though I would question this. Qualitative ideological parsimony, as Cowling understands this, is certainly an interesting theoretical virtue, but I'm not sure it has as much bearing on Sider's project as he makes it out to. Recall, under Sider's account of world-privileging, elements of world-privileged ideology do not stand for *countable things* but instead must be understood as referring to irreducibly *qualitative features* of the world (Sider 2011, §6.3). The simplicity Sider refers to in regards to "structure" is one of shape, not size: it concerns the presumption of a *non-gerrymandered* world and has no obvious connection with minimizing the number of anything. Until it is made clear exactly what this simplicity in shape consists in (or at least how to measure it), it won't be clear to me how this expectation of a world simple in shape is respected by minimizing the *number* of (kinds of) ideological commitments a theory takes.

modality (Sider 2020, Chap 1). In short: I would not draw conclusions about any particular *philosophers* or their *theoretical theses* based on the present *methodological* critique.

With that clear, what is the takeaway? Is the use of IBE justified in metaphysics? Of course there is no easy answer. Two things stand out to me however. First, nowhere were the critiques fatal. Each weakness revealed poses a challenge for the working metaphysician to be mindful of, but none seemed absolutely intractable. For example, the critiques of strategy 2 more or less pointed to further conditions on the kinds of theoretical devices that could garner realist credentials (i.e., they can't simply be explanatorily indispensable). To improve the reliability of our methodology, these extra conditions need elaboration (and then need to be heeded of course), but this seems doable. The second takeaway is that the results are not at all homogenous. While neither strategy seems more or less problematic overall, they each have weaknesses of varying severity depending on the different projects metaphysicians pursue. For example, the challenge of developing a naturalistic epistemology looks much more imposing for neo-Aristotelian ontology (as conceived by Paul), than for metaphysical modality (as conceived by Williamson).

These results might lead metaphysicians to try mixing and matching: deploy both strategies, so long as neither is too problematic and they each offer an independent line of argument to pursue. As mentioned before, Williamson is rare in that he explicitly does this (2016b, 708). But it's not obvious that mixing and matching is permissible. Recall, strategy 1 is premised on the idea that questions of metaphysics are *generally orthogonal to those of science* while strategy 2 is premised on exactly the opposite thought. It's not obvious that strategy 1 and 2 are reconcilable, at least in general. This question merits further research.

What about shopping around instead, picking the strategy that poses the least difficulties for your project? Again, it's not clear this is permissible. Presumably, the subject matter of one's project will determine which strategy is appropriate: if the subject matter is largely independent of science, there is no choice but to take strategy 1; if the subject matter is tightly bound up with science, conclusions arrived at via strategy 1 would not be impermissible but would presumably be outweighed by any conclusions arrived at via strategy 2. No doubt there are also subject matters somewhere in the middle (which would also permit the mixing and matching of strategies). To determine any of this however, metaphysicians will need some sort of handle on their subject matter. Generalities will no doubt be of limited help (where even coherent): a metametaphysics for each branch of metaphysics will be to some extent required.

This raises one final dilemma of sorts, regarding the subject matter and ambitions of metaphysics. Roughly put: assuming naturalism, [44] if a given question of metaphysics is addressed by science, then this metaphysical pursuit has a rivalry with science; if a given question is *not* addressed by science, then these pursuits are slipping into mysticism. This dilemma is due to our friends the skeptics of mainstream "heavyweight" metaphysics, erected here in acknowledgement of (what I take to be) legitimate suspicions about our ambitions in metaphysics. Since answering this dilemma should bear interesting metametaphysical fruit and does ultimately stem from our methodological needs, this is the task I set for myself in the next chapter.

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[44] I.e., assuming that there is no first philosophy, that science is our only guide to understanding the world, or something to this effect.

Chapter 3:

Is Metaphysics in a Rivalry with Science or Slipping into Mysticism?

Metaphysicians face a question regarding their projects: do they revolve around the same questions that other sciences raise, or do they revolve around distinct questions, distinct subject matter? Neither answer appears false across the board: it simply depends on the metaphysical project in question. No doubt some projects encompass both distinct topics and topics identical to those addressed by certain sciences. Such may be the case for Williamson's project: the question of the correct modal logic for metaphysical modality is distinct, but arguably overlaps greatly with the question of the correct modal logic for *physical* modality, something scientists supposedly take an (implicit) stance on.

This question of subject matter becomes all the more pressing in the face of choosing the appropriate methodological strategy for one's project. As we have seen, metaphysicians have developed two sorts of strategies for deploying Inference to the Best Explanation (IBE). However, each strategy is loosely premised on the opposite metametaphysical intuition about our subject matter: that a given question of metaphysics is, or is not, already addressed (if not answered) by science. If it is not so addressed, the metaphysician can only borrow scientific methodology, not its results (strategy 1); if it *is* addressed, then the metaphysician can borrow the relevant results of science to discover what metaphysical theories are confirmed or disconfirmed in tandem with scientific theories (strategy 2). Thus, metaphysicians must make clear with themselves what their subject matter is.

Immediately a dilemma becomes apparent. If a certain question of metaphysics is addressed by science, then what business does the metaphysician have in opining on the matter? Or if instead the question is not in general addressed by science, what exactly is the metaphysician opining on? First philosophy? This would go against the naturalism popular with so many metaphysicians (including myself: I will assume naturalism is true in what follows). It seems that either way then, the metaphysician has some explaining to do – twice the explaining if they wish to pursue both the strategies discussed.

This is the dilemma we'll be exploring in this chapter. While it is less directly a methodological problem, this dilemma still concerns us here since it seems to stem from our methodological needs: we each need at least one of the two methodological strategies discussed above; each strategy rides on an intuition about the nature of our subject matter (is it or is not already addressed by science?); yet each intuition leads to one horn of the dilemma. Thus, if we can't answer the dilemma, we should abandon all of our present justifications for an IBE driven methodology in metaphysics (or of course abandon the enterprise of heavyweight metaphysics). Since I don't plan on abandoning heavyweight metaphysics just yet, and I think both metametaphysical intuitions are more or less correct (in varying proportions, depending on the metaphysical project), I will be outlining here what the responses to this dilemma should look like. Metaphysicians should welcome the task of responding to this dilemma: developing such a response for one's project can help in getting a better handle on one's subject matter. I'm convinced that getting such a handle will be essential to improving the reliability of our methodology, allowing us to more masterfully apply the appropriate strategy to the appropriate project. Before we can begin answering the dilemma however, we should better familiarize ourselves with it.

3.1 Raising the Dilemma

Just like the methodological critiques of Chapters 1 and 2, the present dilemma is due to our friends the skeptics of mainstream heavyweight metaphysics: I've constructed it from their various comments and objections to heavyweight metaphysicians. And just as before, it will be my default assumption that there is something *correct* or *rational* about these objections. We have something to learn from them.

Though the dilemma doesn't manifest itself clearly in every metaphysical project, at least one of the horns usually does,[45] ready to be joined by its other half. This is to be expected: different

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[45] For example, Thomasson (Thomasson 2020), defends a deflationary account of alethic modality, arguing that alethic modal language is best interpreted as signaling an object-language expression of semantic rules, the most contemporary incarnation of a long-running conventionalist tradition (see Thomasson 2020, 15). In putting forward her deflationary account she brings into question projects such as Williamson's (which aims at uncovering certain alleged truths about the world, namely, the alleged modal truths, those related to what is objectively necessary and what is objectively possible). Thomasson suggests the error in such projects stems from a confusion over the function of modal language: "heavyweight modal realists" mistakenly assume that modal language serves to *describe* and *track* when in fact they serve to signal *prescriptive force*. As for concerns over a rivalry with science, we see at least some discussion of it around Williamson's own work (Williamson 2013, xii; Divers 2014, §2.a; Williamson 2014, 744–746). For more on this see §3.2.1 below.

projects are usually more susceptible to one horn than the other, given that the philosophers pursuing them usually align themselves with one intuition about their subject rather than the other, and pursue one strategy rather than the other.

Hofweber provides a helpful framing for this dilemma at a general level (2009, §1), but I believe the most thoroughly developed instance of the dilemma comes from Amie L. Thomasson's critiques of neo-Quinean ontology[46] as practiced by heavyweight metaphysicians. Thomasson has deep misgivings about any ontological project that isn't the simple (and comparatively easy) cataloguing of what, *at face value*, science and common sense take to exist (2009; 2014; 2015). If we agree ontology is in the business of making an "easy" catalogue (in the above sense) then to deny the existence of ordinary objects (tables, chairs etc.) and many scientific objects (pulsars, amino acids etc.) would be to pick a fight with science (and common sense), a fight metaphysics is sure to lose (Thomasson 2017, 8:52). If we reject this characterization of ontology, we had better have a good story about what exactly we *are* doing. Searching for what *fundamentally* exists? For what *really* exists? For the world-privileged catalogue of terms?[47] For what exists*? (Where "to exist*" and "∃*" are stipulated, respectively, to be the "perfectly jointcarving" cousins of the ordinary English "to exist" and the standard quantifier "∃").[48] Thomasson's persistent and persuasive questioning of our ontological ambitions should make any ontologist think twice. Thomasson (2009) argues that debates among heavyweight ontologists devolve into verbal disputes or else revolve around unanswerable questions, ultimately because of the vague or otherwise confused use of the words "thing" and "object". Thomasson follows this up with another paper (2014), this time making the case that any questions about world-privileged catalogues of what exists (what *really* exists, in some sense) would be bizarre "danglers:"

> [T]he answers do not matter, we do not know how to articulate them or give them content, there is nothing to determine the correct answer to them, and we can do all the same things the language was designed to do regardless of the correct answer to the question.

With regards to the project of uncovering the world-privileged ideology and then describing the world in terms of this ideology, Hirsch (Hirsch 1993) questions the rationality of such a project. Sider (Sider 2011) can be read as offering a rationale in response. However, Hirsch (Hirsch 2013) and Dasgupta (Dasgupta 2018) respectively question the intelligibility and objectivity of said rationale.

[46] In contrast with neo-Aristotelian ontology; "ontology" in this context is the investigation into what exists, what there is.

[47] "Terms" here being used in its technical sense, denoting "object designating words", i.e., "words that stand for a thing".

[48] This is Sider's "Ontologese gambit", the strategy of shifting to specialized language to carry out ontology. For more see (Sider 2011, §9.3). For discussion see (Hirsch 2008; Sider 2014). For more context on this sort of strategy see (Van Inwagen 2014, 1–14).

At that stage, one might begin to wonder: why think there are such remaining, legitimate, meaningful questions at all? (528)

Sider answers Thomasson's deflationism by arguing that it amounts to denying there is ontological structure, i.e., it amounts to claiming that neither "∃" nor anything semantically close to it is world-privileged – there is no perfectly jointcarving cousin of "∃" (2011, §9.9). Total metaphysical deflationism would be to deny there is any structure, that there are any world-privileged elements of ideology (§5.6). Sider then remarks that these denials rid the deflationist of any "epistemic high ground," since any such denial of structure is itself a deep and difficult metaphysical claim (83).

There may be some truth to Sider's claim that metametaphysics is "just more metaphysics": no doubt it accurately describes some works of metametaphysics. However, after spending a book's worth developing her deflationary, common sense approach to ontology, I think Thomasson is right to resist Sider's portrayal of her, insisting instead that she simply withholds judgment on questions that she believes to be confused or in serious need of clarification (Thomasson 2015, §10.3; see also Warren 2016, §3). This certainly seems to be a reasonable position while her "simple realist" ontology stands as a viable alternative to any offering from heavyweight ontologists.

Thus an instance of the dilemma takes shape: either questions of (neo-Quinean) ontology are the ordinary questions of "what exists", of "what there is", questions we all seem to understand and have access to; or they are something else, something distinct. If they are the ordinary questions, then they are "easy" to answer: simply defer to science and common sense. To dispute common sense and (face value) scientific answers to these questions would be to pick a fight metaphysics cannot win. If the questions are something beyond the ordinary questions (e.g., they aren't being asked in ordinary English but instead in Ontologese), then we have no rivalry with science but instead sound very mysterious. What is the difference between "what exists*?" (the question in Ontologese) and "what exists?" (the question in ordinary English)? What would it be for a list of terms to be the world-privileged list? That it picks out all and only what *really* exists?

Instances of the dilemma in other branches of metaphysics raise analogous questions for those branches (see FN46 above). Now, some may feel these skeptic friendly rhetorical questions are unfair, and perhaps they are. After all, many metaphysicians *have* spent considerable time developing their particular project, working hard to clear away mystery and develop methods for

answering the questions they pose. It's not as if no one has answers for the skeptic: the field *has* taken a notably metametaphysical turn in the last few decades, indicating an awareness of these issues.

In my more skeptical moments though, I can't help but worry that many of these responses jump the gun, putting forward a metametaphysics that is too detailed, too specific for what evidence can support. For my part, I prefer sticking to the bigger picture in a field as speculative as metaphysics.[49] It's not just a worry about losing sight of the proverbial forest, but a fear that the forest might in fact be an ocean and what we've been thinking of as trees are fish. It doesn't seem prudent to enter the thicket of theory with such little guarantee that we aren't looking at the matter profoundly wrong.

Consider, for example, how Sider's response to Thomasson requires some sort of faith in the intelligibility and obviousness of reality having "structure". Sider's claim that "metametaphysics is just more metaphysics" is a double-edged sword: if metaphysics is mysterious then our metametaphysics is mysterious as well. I sympathize with skeptics who are dubious of metametaphysical frameworks that appeal to notions as mysterious as the questions they are meant to demystify.

Consider as well how the situation in metaphysics is the reverse of epistemology: metaphysical skeptics are like Mooreans (defenders of common sense) while mainstream metaphysicians are like epistemological skeptics (always complicating matters more than seems warranted). Though I don't consider myself a common sense philosopher, common sense does count for something.

Factor in the severity of our potential error and I'm compelled to believe that it would be irrational *not* to worry that our projects are confused or that we are playing pseudo-scientist, despite any metametaphysical hypotheses we may have. Some will no doubt still not feel the pressure of the dilemma. Fair enough: it's not as if I have given a deductive proof of its threat. However, it might still be wise to recognize the dilemma, if only for the sake of argument: our defense against metaphysical skepticism would likely be more effective if it began by acknowledging the reasonableness of the skeptical position. By erecting this dilemma I am trying to concede as much as possible to a skeptic such as Thomasson and suspend disbelief in what little I think I know about the subject matter of metaphysics: it would make for a powerful

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[49] For this reason, I do appreciate Chapters 7 and 8 of Sider (Sider 2011), as well as (Tahko 2018) and especially Chapter 1 of (Sider 2020). But these do not answer the skeptic of heavyweight metaphysics (nor intend to, I should add).

response to the skeptic if from that skeptical position we can still make our way back to *any* less deflationary metaphysical conviction. Finally, entertaining skeptical worries can be a healthy exercise (when not over-indulged in). Only one of two things can happen: we gain a better grip on our subject matter, or we discover that we were investigating mirages all along and can stop wasting our time doing so. Either one is a win in my book.

3.2   Wrangling the Dilemma

I approach this dilemma with sympathies torn: I wish to defend heavyweight metaphysics but also give skeptics of heavyweight metaphysics their due. As such, my strategy will involve neither directly attacking deflationary accounts of metaphysics (as seen in, e.g., Thomasson 2015, Part II) nor simply detailing my preferred account of metaphysics without really questioning the intelligibility of our ambitions. I don't mean to dismiss or ignore the metametaphysical work of other metaphysicians (indeed, I will still refer to it as I go along): I simply wish to explore a different approach.

I will try to start where the skeptic stands and move from there. I will assume metaphysics *does threaten* to rival science, at least in principle. I will assume metaphysical notions *are unintelligible*, until shown otherwise. I will not rely on any deep metametaphysical premises but instead only rely on common sense observations to move forward. I will however be trying to escape the skeptical position.

While I can't possibly *settle* these matters in such a small space, I do hope to suggest paths out of the skeptical position, which should dull the horns. At the very least, I hope to provide a *template* for how to answer each horn. And I will be responding to *both* horns: after all this is not a strict dilemma[50] of the form P∨¬P, so responding to both does not obviously entail a contradiction. Furthermore, as I mentioned earlier, ideally we would clear both of our methodological strategies of their respective roles in the dilemma. Ultimately I will claim that, while neither charge is always warranted, they certainly can be in principle, something the working metaphysician must be mindful of.

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[50] I still take it to be a dilemma since the denial of one horn seems to lend support to the other. I won't try and prove this but just concede it to the skeptic.

3.2.1   The Charge of Rivalry

No one wants a rivalry with established sciences, least of all an aspiring one like heavyweight metaphysics. It doesn't take a skeptic of metaphysics to realize this is a bad policy. Paul's claims that metaphysics deals with matters *distinct* from that of other sciences (2012b, 2) are indeed prompted by hardliner naturalists who protest that metaphysics does not pay enough respect to the details of actual science (Ladyman and Ross 2007, Chap 1). Paul's response to the threat of rivalry presents one extreme: it is to abandon any claims of significant overlap with science, to distance one's project from science as much as possible, claiming that one's questions are almost entirely orthogonal and distinct from those of the established sciences. In short, it is to change subject. Depending on how hard Paul presses toward this extreme, she might get herself off the hook of this horn, but in return she will feel the other horn of the dilemma all the more acutely – the charge of mysticism.

Others don't go so far, but still recognize the danger of rivalry. Indeed, the charge of rivalry has been wielded by metaphysicians against their fellow metaphysician: consider how Williamson dismisses any discussion of Lewis' plurality of possible worlds on the grounds that Lewis is postulating a "non-standard cosmological theory" which is in "competition" with those of physics (2013, xii; for discussion see Divers 2014, §2.a; and Williamson 2014, 744–746). However, Williamson is still happy to accept that scientists take a stance (if only implicitly) on a good portion of his subject matter of choice, metaphysical modality. Namely, he believes they take a stance on the appropriate modal logic for many objective readings of the alethic modal operators (e.g., nomic necessity or physical necessity of various types), something he takes to overlap significantly with his own claims about the appropriate modal logic for metaphysical modality (2016a, §8). The thought, presumably, is that his research makes proper deference or somehow pays proper respect to the relevant science, where Lewis' did not. Let's see if all this can't be made clearer and more precise.

Williamson's charge against Lewis is an example of what might be called the weak charge of rivalry: accusing a metaphysician of opining on subjects that established sciences already opine on. The norm this charge appeals to is simply that metaphysicians should not (in their capacity as metaphysician)[51] opine on subjects that established sciences already opine on. Why? There seem to be two sorts of motivations one can give. One thought is that to do so would be

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[51] Of course, as Hofweber notes, individuals who happen to be metaphysicians can contribute to the scientific enterprise – just not by bringing conclusions of metaphysical theory to bear on the science (Hofweber 2009, 263).

"immodest": to opine on these subjects would involve holding metaphysics above science with the mistaken belief that science requires vindication from philosophy (Hofweber 2009, 262–263). The other thought is that philosophers just can't compete, that science simply has a more successful track record at discovering the truth of the matter. As Williamson puts it: "I know which side my money is on" (2014, 745).

However, this is not the only way to formulate the charge of rivalry. In its stronger form, the charge of rivalry is an accusation of *directly contradicting* a claim from an established science. The norm at play here is something similar to that of French and McKenzie's compatibility principle:

> *The compatibility principle:* the constraint that any metaphysical theory invoking entities *x* and deployed at some time *t* should be compatible with at least some independent, well-supported, overall 'serious' scientific theory that directly describes or that is otherwise relevant to those entities, should such a theory exist at that time. (2016, 37)

While their principle has a focus on ontology, I see no reason why it couldn't be extended to demand that metaphysical theories never include statements that are incompatible with all relevant reputable scientific theories.

Guilt of the strong charge seems to entail guilt of the weak charge, but not vice versa. Consider again Williamson's accusations against Lewis. It's hard to deny, as Williamson points out, that physicists do at times opine on the existence of spatiotemporal regions disconnected from our own: witness, for example, the Many Worlds Interpretation (MWI) of quantum mechanics. This, one might feel, shows that Lewis' hypothesizing about the existence of a pluriverse is out of place, being made "outside the proper domain of natural science" (Williamson 2014, 745). Thus Lewis might be guilty of the weak charge. As an aside, the trouble for Williamson is that if he is right about science being committed to possibilia (2016a, §6) then how is his earlier work not also guilty of the weak charge? Williamson's *Modal Logic as Metaphysics* opines on the existence of possibilia with abductive reasoning that doesn't consult any established science, the same way Lewis (1986) opines on disconnected spatiotemporal regions. It appears Williamson is trying to have his cake and eat it too (a hazard of appealing to both strategy 1 and strategy 2). Returning to Lewis though, what of the strong charge? I haven't heard of physicists explicitly *denying* the existence of specific disconnected spatiotemporal regions, as if they have proof this is not the case. At most, to my knowledge, they will say they don't have enough evidence to

conclude there is a multiverse. To be sure, Lewis' modal realism is more extravagant than MWI (I don't believe MWI entails the existence of talking donkey heads), but this does not mean they are in conflict. *Prima facie,* they appear *compatible*. Thus, Lewis is *not* guilty of the strong charge.

How is one to respond to such accusations? If the norms that underpin the charges are correct, the metaphysician's options are limited. If accused of the strong charge, they need to show their work is not incompatible with all relevant scientific theories of repute. (Or of course abandon their theory and repent). If accused of the weak charge, they must show that they weren't in fact opining on the same subject matter as the established science, or else drop their ambitions, foregoing judgment on the matter in question and resigning themselves to total deference.

I will not be investigating the norms at play in these charges. I concede that, in general, they seem reasonable and correct. However, I will be suggesting that they aren't without exception: I will suggest there are situations in which metaphysicians (in their capacity as metaphysician) have every right to not only opine on the same questions that scientists raise but also to contradict their answers to these questions. How is this possible assuming, as I did at the outset, some form of naturalism (some sort of rejection of there being a first philosophy)? As a first response, consider whether analogous norms are without exception. Inquirers of any stripe should be wary to opine on questions that other competent inquirers are already investigating, let alone *contradict* their conclusions: this is simply to demonstrate humility and respect the autonomy of another's inquiry. This is all the more true the less sure we are that our expertise is relevant to the inquiry at hand. A biologist certainly (in their capacity as a biologist) has no business opining on questions about black holes. However, this norm is not without exception. A chemist and a psychologist may both be entitled to opine on a particular criminal investigation, and disagree with one another regarding who they suspect the criminal is. Some questions do not fall neatly under one discipline and require various types of expertise to be answered. Of course, this last claim depends on the somewhat arbitrary delineation of disciplines – after all, we might simply say both the chemist and the psychologist fall under the category of forensic scientists. But how we delineate disciplines is really beside the point. The deeper point is that *many questions cannot be answered by just one inquirer* since in general one is not capable of studying or gathering all the details required to answer the question with any certainty. No one is an expert on everything. We still need both forensic psychologists *and* forensic chemists (and so on). And these inquirers do not (or at least should not) see their approach as *above* the other's but simply as two equally important perspectives, each bringing equally legitimate,

relevant but distinct details to the table. The question for metaphysicians is: do we bring anything new to the table? Do metaphysicians offer an expertise that is *distinct, legitimate* (by naturalist lights), and *relevant* to answering questions other scientists already investigate?

I think the answer is "yes", or at least it's not obviously "no". I will try to develop at least one example of a distinct, legitimate and relevant form of expertise that metaphysicians offer, but of course particular metaphysicians may prefer to develop a different story. However, if they answer "no" (i.e., they concede the norms that underpin the charge of rivalry are without exception) then they seem to face a choice: either abandon strategy 2, placing all bets on strategy 1 in the hopes of applying IBE to questions distinct from those of science, pursuing an "ambitious" but potentially mystical metaphysics (Hofweber 2009); or simply abandon ambitious metaphysics altogether, deferring entirely to science for answers on whatever questions they may have had, all but taking its claims at face value (see e.g., what Chalmers (2009) calls "lightweight" ontological realism). Strategy 2 must either be abandoned or almost entirely gutted, reduced to simply consulting the nearest textbook or briefly glancing about us. In the latter case, there wouldn't be much left of heavyweight metaphysics either. Of course, if one has no allegiance to heavyweight metaphysics, happy to confine metaphysics to linguistic and conceptual analysis, there is no need to choose: ambitious *heavyweight* metaphysics was always a mistake and we should instead pursue an alternative metaphysics, ambitious in its own right – something in the vein of Strawson's descriptive metaphysics (1959). The above might not be a complete map of the conceptual space, but hopefully it provides some orientation. Anyway, since I do have hope for heavyweight metaphysics and don't think strategy 2 should be abandoned or gutted, I take it that I must elaborate a distinct, legitimate and relevant form of expertise that metaphysicians can bring to bear on questions that science already opines on.

My answer will be schematic for a number of reasons. First, I haven't the space. Second, it's really just a certain reading of Quine, no doubt familiar to many (though hopefully made more clear and distinct here).[52] Third, my over-arching aim in this chapter is to provide a *template* for how to respond to each horn of the dilemma. Maintaining this bird's eye view is a feature, not a bug: sometimes providing a framework we can all agree upon and within which we can carry out further research (in this case, for metametaphysics) is more beneficial than diving straight for

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[52] I think general characterizations of metaphysics as the project "of making explicit what had been tacit, and precise what had been vague" in science (Quine 2013, 254) have been especially confusing: when making explicit what has been tacit, there is all the difference in doing so *with complete deference to the rest of science* and doing so *as equals of other scientists, bringing to bear a distinct, legitimate and relevant perspective.*

the final answer (see §3.1 above). That said, templates are more vivid when colored in, so on with the example.

Scientists regularly opine on questions of what there is, what exists. As Quine puts it:

> Given physical objects in general, the natural scientist is the man to decide about wombats and unicorns. Given classes, or whatever other broad realm of objects the mathematician needs, it is for the mathematician to say whether in particular there are any even prime numbers or any cubic numbers that are sums of pairs of cubic numbers. (2013, 255–254)

Yet ever since Quine, metaphysicians have also been making declarations on such questions: by investigating the "uncritical acceptance of the realm of physical objects itself, or of classes, etc." ontologists may come to conclude that strictly speaking minerals, planets and pulsars do not exist (Van Inwagen 1990; Merricks 2001). They can mollify matters however they like (they might say they aren't *eliminating* planets but merely *identifying* them particles arranged planet-wise) but ultimately they must either admit to contradicting the natural scientist and engaging in revisionary ontology, or deny it with a change of subject and face the charge of mysticism. Some, such as Merricks, are frank in their admission about being revisionary, while others swing between admission and denial (compare Sider 2011, 84 with §9.3). Here I assume we admit to contradicting the scientist: we face the charge of rivalry in both its weak and its strong form.

What gives? How can metaphysicians justify butting in? I allege: because metaphysicians are experts on what exists *all things considered*. The "all things considered" perspective that metaphysicians bring to bear is a distinct, legitimate and relevant perspective for questions of what exists. What is this perspective? It consists of trying to amass the totality of scientific knowledge, our overall theory of the world, and trying to construct from this the *best* overall theory of the world according to the usual theoretical desiderata (parsimony, explanatory power, minimal mutilation etc.). It is the application of such desiderata with the *whole* of science in view, in contrast with the piecemeal application that each specific science employs.

This perspective, this expertise, is distinct from the expertise of other scientists: scientists do not generally consult the entirety of scientific knowledge to determine answers to their questions of what there is. In determining whether wombats exist, zoologists don't consult what physicists are saying about neutrinos. And rightly so: the expertise of the zoologist lies in determining which *specific* animals exist, assuming animals *in general* exist. From an "all things considered" perspective however, whether any animals exist is an open question. So the ontologist's angle is

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distinct, but is it legitimate by naturalist lights? Yes, since *it is parasitic on the rest of science*: it takes as input the total mass of true descriptions that science acquires (our overall theory of the world), processes that body of truths by applying the theoretical desiderata, and outputs our *best* overall theory of the world. If the information acquired by science is kosher, and the application of theoretical desiderata (as seen in the rest of science) is kosher, then the outputs of the "all things considered" perspective should also be kosher. This is not some first philosophy, but *last science*: if you will, it is the ultimate meta-analysis.[53] More generally, if we think the *best* zoological theory (best according to the usual desiderata) is the zoological theory most likely to be true, then why wouldn't we think the best *overall theory of the world* is the overall theory of the world most likely to be true?

Finally though, how is this perspective relevant? How does the application of pragmatic considerations to our overall theory of the world have any bearing on whether wombats exist? We might accept the *terminology* of "animal" on a pragmatic basis, but what determines whether "animal" can be predicated of anything (i.e., whether any animals exist) seems determined by nothing other than an empirical fact (e.g., the existence of a particular wombat). But isn't this last entirely up to the zoologist to determine? I think that, strictly speaking, the answer is "no", but here is where things get controversial. My answer hinges on Quine's doctrine of a "more thorough pragmatism": if it is accepted, then the zoologist's expertise is not the only which is relevant to determining whether wombats exist. According to this doctrine, *all scientific hypotheses* (and not only those concerning which linguistic frameworks we adopt) are subject to pragmatic considerations: considerations of simplicity, explanatory power, minimal mutilation of extant theory etc. (1951b, 43). Actually, all that is needed to get ontology off the ground is the doctrine that all *scientific existence claims* are subject to these pragmatic considerations. If this is true, pragmatic considerations emanating from zoology should come to bear on the question of wombats (e.g., in the form of an IBE) – something that already happens in practice, for better or worse.[54] But now, if that is correct, then what reason do we have for barring pragmatic considerations that emanate from an "all things considered" perspective? They too are relevant to the question of wombats – as well as the question of neutrinos, marriages, planets and any other alleged entity. Thus, I contend, "the all things considered" perspective is also *relevant* to answering existence questions.

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[53] In the technical scientific sense of "meta-analysis".
[54] See, for example, how Pauli posited the existence of the neutrino as a "remedy to save the "exchange theorem" of statistics and the energy theorem" (Brown 1978, 27).

A more detailed discussion and defense of Quine's doctrine (and its twin, a more thorough empiricism)[55] will have to be dealt with elsewhere. Curiously, this more thorough pragmatism is seldom recognized by contemporary ontologists for its crucial importance in Quine-inspired ontological projects. Thus, although cognizant of the threat this doctrine poses, a skeptic of ontology such as Thomasson spends little ink on it – only enough for a quick dismissal, since her opponents don't seem to make any substantial defense of it (2015, 49–50). This is a poor showing on the neo-Quinean ontologists' part I think: they have left a potentially vital premise undefended.

But neither this, nor other details of the specific expertise defended here concern us much. Perhaps my defense of the "all things considered" perspective was convincing, perhaps not. The real takeaway, I hope, is *how to answer the charge of rivalry*: metaphysicians need to find a form of expertise that they can offer which is *distinct* from those offered by other scientists, *legitimate* by naturalist lights (if one adheres to some form of naturalism), and *relevant* to whatever question it is that they wish to opine on. If they do this, I see no reason why they can't opine on the question, and yes, even contradict other scientists. But what of the other maneuver metaphysicians are fond of, namely, denying any significant overlap with science and instead changing the subject (Paul 2012b; Sider 2014)? This is what gives rise to the charge of mysticism.

3.2.2   The Charge of Mysticism

The charge of mysticism is quite simple really: it accuses various metaphysical notions of being so vague or confused that questions framed in terms of these notions can only lead to verbal disputes or mystical pursuits. The combination of two positions makes metaphysicians susceptible to this charge. First is the claim that they are investigating questions not addressed by the rest of science, that their subject matter is distinct from anything the rest of science explores. A metaphysician might hold this belief for a variety of reasons, but in any case, it is this claim that steers them away from the horn of rivalry and toward the horn of mysticism.

However, this claim isn't enough to garner accusations of mysticism: it takes more for this horn to materialize. To see this, we need only look to those who levy this charge, neo-Carnapian and Strawsonian "descriptive" metaphysicians: they are happy to define and pursue questions that they take to be left for the picking by the rest of science (Hofweber 2009; Thomasson 2020,

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[55] The view that *empirical* considerations (i.e., maintaining consistency with empirical observation) also come to bear on all scientific claims, including the most abstract, such as those of mathematics (Quine 1991, 269–270).

209–215). The difference between their ambitions and those of heavyweight metaphysicians is (among other things) one of scope it seems. These "deflationary" metaphysicians *explicitly confine themselves* to studying "just" the details of human language and conceptual schemes, while the heavyweight metaphysicians' self-conception is of someone who investigates a broader swath of reality – a swath of reality that no doubt includes our conceptual and linguistic schemes, *but only incidentally* (Sider 2011, 5; Williamson 2013, 423; Paul 2010, §§2–3). Characterizations include: "the fundamental structure of reality", "modal reality", and "the fundamental categories of what compose reality".

It is this self-conception, accompanied by the claim of having a subject matter distinct from the rest of science, which get the metaphysician into trouble and lead to charges of mysticism. Nowhere is this more apparent than in the topic of the day, research into "the fundamental". For the uninitiated, I will try to briefly outline this research. To start, I would note that there seem to be various different notions of "the fundamental" being employed in this research. There are at least two important variants, corresponding to two sorts of questions frequently posed by metaphysicians:

> **The Question of Hierarchical Fundamentality (QHF):** What is "at the bottom" in the hierarchy of the world's total structure (assuming the world's total structure *itself has structure* and that structure forms a *well-founded tower*)? In short, what is fundamental in the hierarchy of the world's structure?

> **The Question of Categorical Fundamentality (QCF):** What are the most abstract divisions that accurately characterize what the world is made up of? In short, what are the fundamental categories of the world?

I'll use "H-fundamental" and "C-fundamental" to refer to each variant of "fundamental" respectively. "H-fundamental" admits at least two further variations depending on whether it is applied to *entities* or *facts/truths/propositions*.

To help orient the reader, here are some sample answers. One might believe that the H-fundamental entities are the particles of the Standard Model, qualia, and nothing else, while also believing that the H-fundamental facts/truths/propositions are those of physics, mathematics and nothing else (because one is a fictionalist about mathematical entities, and believes that the facts of physics determine all the qualia facts). Furthermore, one might be a bundle theorist of some sort, believing that universals are C-fundamental and nothing else. If one takes "what the world is made up of" to simply stand for "the H-fundamental entities", QHF and QCF can be

understood as intersecting. This is not obligatory however: one might believe instead, e.g., as Dipert (1997), that the fundamental categories truly describe everything we would call an entity and not just the H-fundamental entities (i.e., chairs as well as electrons). Besides, on the face of it, QHF and QCF appear orthogonal: one asks "what is foundational, what is at the bottom, of the world's *total* structure?", while the other asks "what is anything and everything's general "shape", what is the structure of *any instance* of the world's structure?". It does not seem wise to equivocate between C and H-fundamentality.[56]

Further details of this research program can be found elsewhere.[57] However, these details do little to assuage the accusations of mysticism that have been levied against this research. Sider's thesis that descriptions phrased in "fundamental" ideology are objectively privileged, is a thesis that is "hard-going" for Hirsch (2013, 712–713) and will "hasten a revival of some form of logical positivism" (2013, 709). According to Thomasson, research into the H-fundamental entities involves "quizzical" (2014) and or "unanswerable" questions (2009; see also 2015, 306–308). More generally, Dasgupta questions how the pursuit of the "fundamental" could be "objectively better" than pursuit of, say, the "schmundamental" (2018). Others don't mince their words: Hofweber openly wonders if the questions that this "esoteric" metaphysics raises are even meaningful, given that they are so far removed from questions phrased in ordinary English (2009).

I must admit, I do fear this research program is mystifying metaphysics. I do not have a precise definition for what counts as "mystical", unlike Hofweber's "esoteric" or Thomasson's "unanswerable questions". My fear of mystification is instead a testament to the impressive

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[56] Consider how Sider draws no distinction between different types of fundamentality, without a clear reason: both "electron" and "bare particular" (or "thin particular" as Sider prefers (2006)) are highly joint-carving according to Sider (2011, 18; 2013), both standing for something "fundamental" to reality's structure. Yet it is not at all obvious they are "fundamental" in the same sense. This doesn't make the claims obviously *false*, but it certainly isn't prudent methodologically speaking. Firstly, it's not at all obvious that a single methodology (e.g., *just* strategy 2) will be successful at uncovering *both* the C *and* H-fundamental. Secondly, as suggested above, it seems to be a matter of inquiry how the C and H-fundamental relate: we should not *assume* (as Paul and many of her interlocutors do) that in QCF "what the world is made up of" simply stands for "the H-fundamental entities".

[57] Tahko (2018) provides a helpful overview of H-fundamentality and introduces the various dependency relations meant to characterize relative H-fundamentality (e.g., the more fundamental facts are those said to "determine" or "ground" less fundamental facts.). His article highlights how in fact QHF depends on a number of assumptions, such as the dependency relation being asymmetric and transitive, and that the chains of dependencies *terminate* at some "bottom level" (i.e., the chains are well-founded in some sense). Tahko and Lowe (2020) discuss H-fundamentality as applied to entities, while Bliss and Trogdon (2016) provide further discussion on specifically the grounding relation. Sider (2011, Chap 8) provides an interesting comparison between the various proposed dependency relations. An overview of contemporary approaches to QCF and C-fundamentality is more difficult to find but Paul (2012a, §3; 2012b) helps fill in some of the blanks.

literature from skeptics of heavyweight metaphysics – not just their attacks, but their alternative accounts of what is going on when one asks about, say, what exists (Thomasson 2015; Hofweber 2016). I have no doubt there is *something* to this research program, that *some* of it can be demystified. Indeed, some research into the H-fundamental, especially that which emphasizes the elaboration of reductions and bridge laws between "high-level" facts and more "fundamental" facts (cf. Sider 2011, §§7.4–7.8; Tahko 2018, §1.3) has a lot in common with the "all things considered" approach discussed earlier. These researchers therefore may prefer to go the rivalry route, rebuffing the charge of mysticism by claiming that, all along, they were inquiring into the same questions scientists pose. So be it. This surely will not suit everyone though: some clearly think they have hold of a subject matter distinct from anything science investigates (Schaffer 2009; Paul 2012b). These must face the charge of mysticism head-on.

Perhaps they aren't worried though. So what if some philosophers just don't get it? We'll never convince everyone that what we investigate is meaningful and worthwhile. This is true enough, but it doesn't make for a commensurate response once we emphasize what is at stake for the metaphysician. Answering the charge of mysticism isn't just an exercise in appeasing skeptics: getting a firm handle on one's subject matter is imperative for the success of any inquiry.

To see why, let me briefly return to the problem for Williamson from §2.1.3. There it was phrased as a question of whether alethic modal ideology deserved being taken with a "realist" attitude. Personally I don't find talk of "realism" and "anti-realism" to be the most perspicuous or enlightening (I only availed myself of it earlier because it was the least worst off-the-shelf terminology that suited my needs). I think a much clearer statement of the problem for Williamson is *whether alethic modal ideology is ever relevant to metaphysics*. If this sounds baffling, I can sympathize, but such problems are precisely the kind that arise from not having a firm grip on one's subject matter. This bears elaboration.

To explain what I mean by "relevant", consider how only some choices of ideology, of language, of linguistic framework, can be said to be *relevant or make a significant difference* to a given task. For example, the choice of whether or not to use a framework that includes the word "lepton" (with certain rules of use) is relevant and important to physics: making the right choice is critical to achieving the goals of physics. The choice between a framework with the imperial or metric system of measurement may be relevant to physics, but not that important: we would be able to achieve the goals of physics just as well either way (albeit more easily in the latter case…). Finally, the choice of whether to include "genome" or not is not even *relevant* to physics (let alone important): it makes no difference whatsoever in helping us achieve the goals of physics.

Of course, for biology, the inclusion of "genome" is relevant and important, whereas I presume "lepton" is not; what framework choices are relevant is a relative matter. So to reiterate, the problem for Williamson is this: he needs a guarantee that there are ideological choices regarding the alethic modal operators that *are relevant and important for metaphysics, as he conceives of metaphysics*. Simply claiming that one is investigating the metaphysical interpretation of "□" *does nothing to guarantee there is any such interpretation relevant to metaphysics*. It does nothing to prove that the choice of whether or not to use a framework that includes "□" under *any* of its proposed rules of use is at all relevant to metaphysics – in just the same way that no debate surrounding "genome" is at all relevant to physics or astronomy. Declaring that one is studying the "astronomical interpretation" of "genome" does nothing to alleviate matters: either one is signaling the invention of a completely new word, and one must argue for the merits of its introduction with your assigned meaning (which is decidedly not what Williamson is doing (see e.g., 2007, Chap 5)); or one is assuming there already exist interpretations of the word that are relevant to your subject matter.

Now typically, these last are pretty safe assumptions: typically people don't make mistakes like appealing to the astronomical interpretation of "genome". In most disciplines the subject matter is clear enough that it's obvious which terms are relevant and which are not: practitioners can pretty easily put together a word cloud for their discipline without much controversy. But "most disciplines" metaphysics is not: any proposed word cloud for metaphysics would be subject to heated debate because, compared to the sciences, *we typically have a poor collective grip on our subject matter*. The putatively metaphysical is *not* indisputably metaphysical. Therefore assumptions about what is relevant to metaphysics are not safe assumptions. Sider's deflation of metaphysical modality is effectively that: an argument that concludes Williamson's work is irrelevant to metaphysics. Sider might prefer to say Williamson's work revolves around metaphysically nonsubstantive questions (2011, Chap 12), on a par with questions of whether or not "Martini" applies to sour green apple liqueur served in a V-shaped glass (Chap 4). I'm not sure this is much different. Similarly, Paul fears spatiotemporalists[58] in mereology aren't using "suitably categorical terms" to frame their views, i.e., suitably *metaphysics-related* terms (2012a, 240): there is a suggestion that their views are slightly *off-topic* (§3, especially 224). In metaphysics, we *do* question whether our colleagues are (despite themselves) discussing matters somewhat or completely irrelevant to metaphysics. It's not that metaphysicians are meaner or something! It's simply that, when one's subject matter is as elusive as this, these are genuine concerns!

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[58] Those who take the fundamental structure of the world to be spatiotemporal.

As noted earlier, Williamson has done much work to elaborate his conception of metaphysical modality, fleshing out what the alleged metaphysical interpretation of "□" is meant to amount to (2007, Chap 5; 2016a). But it is precisely because his conception of "metaphysical" modality is grounded in our understanding of counterfactual thinking that there is a real danger Williamson is "merely" uncovering deep-seated linguistic rules,[59] or deep-seated psychological rules of a certain simulation thinking. In other words, his work might end up being ground-breaking psychology or an insightful elaboration on scientific epistemology and methodology,[60] but by the same token his work might *not be relevant to metaphysics* as heavyweight metaphysicians such as Williamson conceive of it (2013, 424).

I am not saying I am sure of this. I am saying that until we have a clearer idea of the subject matter and goals of heavyweight metaphysics we *can't* be sure: we will be stuck discussing whether the work of our colleagues is even relevant to our ambitions. And though such questions are neither the healthiest of signs nor the most enjoyable to engage in, they *are* legitimate. To live and let live is all well and good but this kind of thinking can be taken too far: if metaphysics devolves into each losing themselves in their own idiolect, metaphysics will have failed. If we want to work *in collaboration* to answer our questions then we require shared, agreed upon goals and methods. Of course things are not so dire yet, but the rise in metaphysical skepticism and deflationary views does not bode well. These early charges of mysticism should be a wakeup call: we must do better.

In order to legitimize their work, the appropriate response for some will no doubt be to break away from metaphysics, dropping their self-conception as one who investigates reality's deepest and most general truths. Others, on reflection, may find that they *were* chasing shadows – it is not a given that every specific charge of mysticism is unwarranted! But for the resolute, the true believers, what is the appropriate response?

They cannot gesture to their subject matter with just more metaphysical jargon, not while the jargon in question is still suspect. I think then, in general, the goal should be to find and bring to light the source (or sources) of metaphysical questions that lie *outside* of theoretical metaphysics. We can see examples of this already: in this vein, some have taken up Hofweber's challenge to locate questions of "what grounds what" in ordinary parlance (Bliss and Trogdon

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[59] As Thomasson (Thomasson 2020) would have it.
[60] See §2.1.3 for more. If either of these scenarios turns out to be the case, this doesn't really undermine Williamson's work, but just shifts its framing. It just means his work is relevant to other researchers, researchers who need to bring to bear their own set of methodological and theoretical considerations on his work.

2016, §8). After all, if being "expressed in perfectly ordinary terms, accessible to all" is enough to legitimize Hofweber's inquiry (2009, 275), why shouldn't it legitimize this project of allegedly "esoteric" metaphysics? This is a fair response, especially when accompanied with arguments to the effect that questions of ground in ordinary parlance cannot be reduced to questions of some other sort (such as entailment, supervenience or conceptual priority) (Raven 2012). However, I'm not sure it is the most convincing: many expressions of ordinary parlance are metaphorical and vague: talk of "what *holds in virtue* of what" seems like a paradigm example of this. Not exactly the firmest foundation for an aspiring science. In addition, this response does not explicitly show that inquiry into what grounds what will ultimately respect the metaphysician's self-conception as one who investigates "deep" truths of "mind-independent" reality: ideally the metaphysician's response would guarantee this.

Anyway, I think we can do better than to point to ordinary parlance in locating the source of certain metaphysical questions outside of metaphysics. I propose we borrow another page from science's playbook: we should be looking to make precise *baptisms* of our respective subject matters, via *descriptions that we have independent reason to believe are true of some stuff.*[61] As with the charge of rivalry, I will illustrate with an example. And as before, my primary aim is to show that what follows makes for a good *template* of how to respond to the charge of mysticism: the fate of the exact example I choose does not concern us so much here.

The example revolves around what I will call "disuniformity in the world". Let us start with the baptism:

> Let "disuniformity in the world" stand for that which whose distribution is largely independent of us and whose accurate detection is required for acquiring more information – in short, that which makes information difficult and expensive to acquire.

Incidentally, this "disuniformity in the world" appears to be identical to what Floridi calls "*dedomena*" ("data" in Greek) (2019, §1.3). He makes an independent case for this subject matter (2011), which can only be good for me. However, since his baptism and arguments are much more philosophical in their trappings, I present my own case for "disuniformity in the world" instead. Speaking of which, much more needs to be said before I convince anyone I have

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[61] I use "stuff" in order to remain neutral on questions of fundamental categories: I don't want to appear to be endorsing a fundamental category that includes particulars.

hold of anything. So here is an argument to the effect that the description above is actually true of some stuff:

(4.1) Some descriptions of how matters stand are more detailed than others: some true descriptions convey more information[62] than others about how matters stand. (e.g., "this carpet is crimson" conveys more information than "this carpet is a shade of red".)

(4.2) From science (perhaps combined with everyday observation) we can extract the *most detailed description of the world* (of all aspects of reality) that we have to date, and that is true (or best approximates the truth).

(4.3) But science is not finished: there are more detailed descriptions of the world to be had (which are also true or approximate truth).

(4.4) From (4.1) and (4.3), there is more (true) information[63] to be had.

(4.5) Carrying science further is difficult: acquiring more detailed descriptions of the world (that are true) is difficult. It is difficult in that it requires, among other things, the careful development and deployment of scientific methods and instruments of measurement.

(4.6) From (4.1) and (4.5), acquiring more (true) information (more than what is conveyed in our current most detailed description of the world) is difficult.

(4.7) The best explanation for (4.6) is that further (true) information is only acquired by the *accurate* detection of *some stuff whose distribution is largely independent of the information gatherer*.

(4.8) From (4.4) and (4.7), there is that which whose distribution is largely independent of us (as information gatherers) and whose accurate detection is required for acquiring more information. There is disuniformity in the world.

Some explanations are in order. First, a comment about information: though the notion of information is crucial to this argument, I am not implicitly beholden to any esoteric or

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[62] There are various understandings of how to quantify semantic information: examples can be found in (Floridi 2019, §4). The details of such understandings won't concern us – only the general point, that information can be quantified.

[63] I should mention that there are conceptions of information such that information is always truthful: on such views *mis*information and *dis*information are not species of information and the epithet "true" would be considered redundant when talking about information (Floridi 2019, §3.2.3). To my understanding, this slightly changes the subject matter under discussion but does not invalidate other conceptions of information. For my purposes I keep truthfulness and information apart since it's useful for the clarity of my argument to sideline the former and focus on the latter. In other contexts, it's more useful to consider information as something that cleaves closer to truth (Floridi 2019, §4.1, §4.2).

idiosyncratic theory of information.[64] All that is important for the purposes of the argument is that *true information be more expensive than simply truth*. It's not clear that truth on its own could support the claim of *difficulty* in (4.6) whose explanation is what motivates positing disuniformity in the world.

Consider how (relatively) cheap truth is, or at least true sentences. Anyone can no doubt say a number of true things about nearly any subject matter. That's not difficult at all, so long as one glosses over enough detail. Here is a true sentence: most things that fly have wings. Does this make me an aerospace engineer, qualified to design airplanes? Of course not: I need to be able to say *detailed* true things about flight in order to be so qualified. This takes training and the study of our knowledge of flight, knowledge acquired over centuries by the careful development and deployment of scientific methods and instruments of measurement. Again, true descriptions are cheap; *detailed* true descriptions are not. More bluntly, if sentences can be defined recursively and double negation preserves the truth conditions of the sentence it is applied to, then with one true sentence we can go to town, producing an infinite number of distinct true sentences. If that isn't enough we could always translate those sentences into different languages, multiplying them by the number of languages we have available. Of course, in uttering all these true sentences one will have *said nothing new,* conveying *no more information than if one had uttered just one of these sentences*. And that is precisely my point: information is much rarer than a true sentence. If you will, it is "true" semantic content that is rare. Could I appeal to true propositions instead of information then? This depends on how propositions are individuated. Consider the propositions expressed in the following two (true) descriptions of my carpet: "my carpet is a shade of red" and "my carpet is crimson". We do not typically talk of the latter proposition as somehow "containing" the former proposition, in the sense that the former is already referred to by the latter. We instead treat them as distinct, as two, instead of as one with the other as part. The latter treatment seems to add an undue complexity to any theory of propositions. At most we would say that we can analytically deduce the former from the latter. This is all very natural given the legacy of such concepts as 'sentence,' 'proposition' and 'truth': they are best suited for logic, linguistics, and the philosophy of language. By the same token however, they are not suited to my argument: like sentences, this standard way of individuating propositions makes them still too abundant for my purposes.

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[64] For example, the argument is perfectly compatible with some sort of contextualism about information, i.e., the view that what counts as "new" or "distinct" information depends on the context of utterance, perhaps by relativizing to the level of abstraction at play in a given context (Floridi 2019, §3.2.2). Premise (4.6) would simply need to be modified to read "In the context of trying to give as detailed a description of the world as possible, further true information is difficult to acquire." This does not seem any less true, than how it currently reads.

Science would not be as difficult if it were interested in maximizing the number of just any true propositions: the ones that gloss over details are easy to cook up, especially if propositions are individuated by what concepts or non-coextensive predicates figure in them. There may be some manner of individuating propositions that might suit my needs, but why force it? "Proposition" just isn't perspicuous: "information" is what is wanted.

If the difficulty of acquiring more (true) information is conceded, then a question imposes itself: why is it difficult? What explains the need for the careful development and deployment of special methods or instruments in acquiring more (true) information? Here we get to (4.7), the premise most in need of support. The difficulty in acquiring more true information about the world can in part be explained by our observations of the world needing to be *accurate* – truth-tracking or whatever (insert epistemology debates here). But that is not enough: recall my platitudinous statement about most flying things having wings, or our recursively generated true sentences. Truths like these will require accurate observations of the world (or sound deductions), but they otherwise aren't that difficult to acquire. They certainly don't require special instruments of measurement. On its own then, the requirement of *accuracy* is not enough to explain the difficulty of science. After all, science is not the activity of acquiring just *any* true description of how matters stand (those are cheap!), but the *most detailed true description* of how matters stand. It seeks (true) *information*. So what makes (true) information so rare that it requires special methods and tools to acquire? The best explanation (indeed the *only* explanation as far as I can tell) is that information involves the detection of some stuff whose distribution is largely out of the hands of the one gathering information. The apt name for this stuff seems to be "disuniformity in the world" (given that we talk of more precise instruments allowing us to make finer *distinctions*).

It's important that the distribution of this stuff be largely independent of the information gatherer, *qua* information gatherer. If it wasn't, we again wouldn't be able to account for the difficulty of science: if we were in charge of the distribution of disuniformity in the world – if we could produce it at will or we knew (ahead of inquiry) where it was – its detection would be trivial, making the gathering of more information trivial in turn. Of course, as creatures of the world, our actions can produce disuniformity in the world. There are countless banal cases of this (someone carving their initials in a tree, or otherwise leaving their mark in any way) but a particularly interesting case is language: as members of linguistic communities we partake in the creation and modification of words. A lexicographer, *qua* member of a linguistic community, may very well have a hand in producing some of the worldly disuniformity that they study, such

as in the creation of a new, distinct word. But the lexicographer, *qua* lexicographer, does not abuse this power: the lexicographer doesn't make up words and promote their use *just so that they can then easily fill up more pages* in their dictionary and advertize that *theirs* contains *more* information. That is the mark of a perverse lexicographer. Indeed that is the mark of a perverse scientist. They did acquire more information, but only by abusing their power to produce disuniformity in the world, by tampering with the portion of reality they were describing. Creating more disuniformity in the world *just so that one can describe it* is most certainly *not* what science is about. Thus, this stuff whose detection gives rise to information must have a distribution largely or entirely independent of the information gatherer.

Of course it's eminently *obvious* that we had no part in determining the lay of the Milky Way, or the structure of the atom. That the world is out there, and that the distribution and configuration of its details is largely independent of us, is just common sense. Perhaps philosophers (such as Goodman) can buck common sense, but then they must also explain the *empirical fact* that *science is not trivial but hard*. They must explain its need for *ever finer tools* and *careful prospecting* in its quest for (true) information. They must explain why Rutherford's gold foil experiments (which revealed that the distribution of positive charge and mass was not uniform inside the atom but instead concentrated at its center) are so noteworthy. They must explain why it took the careful use of specially developed tools and techniques to acquire whatever it is they acquired.

The only story I can think of that explains all this is that: Rutherford and his associates were smart enough to know where to look; their instruments were fine enough to detect the further disuniformity they hypothesized; and finally, the world did in fact hold deposits of further disuniformity there. The scientists struck gold in this case, but conceivably the world could have been otherwise: the atom's positive charge and mass could have been uniformly distributed. The atom could have better lived up to its name, containing no further internal structure. There could have been "nothing more to see" there, in just the same way that there was "nothing to see" where Vulcan was hypothesized to be. In the case of Vulcan, scientists knew there had to be more to the world's detail than what Newtonian mechanics and current astronomy described, but they happened to be looking in the wrong place for those further details, in spite of Le Verrier's careful prospecting. The quest for ever more (true) information about our world *is no easy task*. Thus, we must posit some stuff whose accurate detection and independent distribution account for the difficulty of acquiring more true information about the world.

Finally, let me be clear what this conclusion does *not* entail. It does not (or at least, not obviously) entail that the statements of science are "projectible," or that there is some elite class of predicates that stand for nature's joints. Nor does it disprove the hypothesis that I am a brain in a vat (BIV). Assuming BIV, my conclusion only entails that the evil scientist must have programmed their simulation with a distribution of distinct, varied, disuniform stimulations for my brain to uncover.

With baptism, argument, and clarifications in hand, we can finally state at least one reply to the charge of mysticism: what is so mysterious about inquiring into the general aspects of disuniformity in the world? What is so confused about seeking information on worldly disuniformity, seeking the most detailed true description that applies to any instance of worldly disuniformity? Let us call this research, the science of *dedomena* (following Floridi's suggestion (2019, §1.3)). Here is a subject matter that is, to my knowledge, untouched by any other science. This is the project of detailing what it is to be a detail of the world, of describing the general structure of any worldly disuniformity. Forget, for the moment, *how* we are to achieve this ambition (that requires its own paper), and instead just consider: what is so confused about it?

My example is complete. (For the curious, it is meant to stand as a demystified project in the vicinity of C-fundamental research: consider how crucial the notions of identity, discernability and distinctness are in discussions of the C-fundamental,[65] notions seemingly closely related to disuniformity in the world). Whether the specific project outlined here is convincing or not is beside the point though. What is more important is to emphasize how this *type* of reply makes for a powerful reply to the charge of mysticism. Hopefully it is clear now how this type of reply is more powerful than locating the source of a metaphysical question in quotidian claims. First, it attempts reference to a more *precisely delineated* subject matter. (Because of this, this type of reply can guarantee that, if our subject is not a mirage, it certainly isn't "just" an aspect of our linguistic or conceptual scheme). Second, this type of reply does more to leverage *empirical observation and argument* to conclude that a certain subject matter is there to be had and inquired about. Third and finally, these replies can successfully legitimize our projects *even if the baptisms involved in such replies do not in fact succeed* (i.e., the description used is not in fact true of anything). All that is required is that the baptism's success *was* (or *still is*) at least *plausible*. After all, we don't generally know whether such baptisms fail *ahead of inquiry*. It took

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[65] These notions are central, for example, in the debates between bundle theorists and particularists. They can also be central in debates between theorists of the same camp. Take, for example, Dipert (1997), a particular brand of bundle theorist who argues there are only dyadic properties (and only one property at that!). He defends his position based on an uncommon understanding of *distinctness* – a graph-theoretic understanding to be specific.

a great deal of searching before it became clear Vulcan did not exist, and even after this was clear, the past research did not suddenly become illegitimate "esoteric" astronomy.

3.3   Milking the Dilemma

This chapter has sought to answer a dilemma that metaphysicians face, a tough choice that lines up with the choice between two ways of understanding the questions we pose in metaphysics: either they are already addressed by science, and we have an apparent rivalry with science; or they are not, and we are (by naturalist lights) committing heresy for pursuing mystical questions. Which path one chooses will depend on one's own metaphysical predilections and ambitions. Every metaphysical project will no doubt require its own particular metametaphysics: no two metaphysical projects are exactly alike.

I have briefly sketched a sample answer for each horn of the dilemma, hopefully providing some evidence that both paths are viable. In the case of the first horn, that example was the "all things considered" perspective for neo-Quinean ontology; for the second horn, it was the science of dedomena. Even if these example replies are not themselves convincing, I hope at the very least to have provided a *template* for answering the two principal charges skeptics have lodged against the ambitions of heavyweight metaphysicians. For the charge of rivalry, this involves showing that metaphysicians offer a *distinct*, *legitimate* and *relevant* expertise for the given question that we are investigating alongside science. For the charge of mysticism, this involves providing the *clear baptism* of a distinct subject matter via precise description, *supported by an argument* to the effect that this description is true of some stuff.

I also hope to have shown that engaging with these charges in detail is a worthwhile exercise, that this dilemma provides a helpful framework for conducting metametaphysics. It can provide orientation by giving us a better feel for the theoretical landscape, helping us map out where one piece of the literature lands in relation to another, and where further research could be conducted. It can reveal general metametaphysical truths, such as that metaphysics is *not homogenous*; searching for a one-size fits all metametaphysics and methodology is undoubtedly a mistake. Finally, and related to this last, engaging with this dilemma can yield more precise criteria for how to tighten up one's project in heavyweight metaphysics, challenging each to get a better handle on their subject matter, which in turn should help each determine the appropriate methodology for their project.

Conclusion

The hunch of this paper was that metaphysicians need more rigorous and reliable methodology if they are ever to convincingly answer the questions they pose. I hoped skeptics of metaphysics would provide some clues as to where to make improvements, and they have not disappointed. With the assumption that they had something to teach us, engaging in detail with their criticisms has revealed a number of pitfalls the working metaphysician must be wary of.

Broadly speaking, Saatsi's critiques (2016; 2017b) challenge us to mind the details of serious theories from the philosophy of science. For example, the details of serious scientific realism complicate any IBE driven methodology in metaphysics. In Chapter 1 we saw how these details prevent scientific methods from being easily imported into metaphysics: the reliability of these methods is not obviously guaranteed in the context of metaphysics. In Chapter 2 we saw how these details bar us from reading metaphysical commitments off of science's sleeve: we cannot, for example, infer that an entity exists simply because that entity is explanatorily indispensable to science. Of course this only scratches the surface: for example, appeals to theoretical virtues *other* than explanatory power await further research. Ideally the metaphysician's appeals to these other virtues (various forms of parsimony, conservativeness etc.) would be given the same treatment Saatsi gives to explanatory indispensability.

Together, the charges of mysticism and rivalry (credited to a number of authors) challenge the metaphysician to get a better handle on their subject matter and the angle they can offer on that subject matter. The dilemma these charges form provide a helpful framework within which the metaphysician can go about meeting these challenges. A number of paths are available: which is appropriate will no doubt depend on the specific metaphysical project at hand. A good place to start is in determining whether the project revolves more around questions *already addressed* by science, or questions *not already addressed* by science. This will help in determining what steps should be taken to get a better handle on one's subject matter, as well as help determine which methodological strategy is most appropriate for the project.

Studying these issues makes it clear that metaphysical projects are not homogenous. However, it should be stressed that heterogeneity does not mean attempts at systematizing our discipline (at organizing and clarifying the projects that make it up) are a waste of time. On the contrary, it makes systematization all the more pressing: the two-pronged threat of confounding matters or talking past one another, each in our own idiolect, is all the more acute. A separate metametaphysics may be required for each project – but not each metaphysician! Within a given

project (e.g., C or H-fundamental research) *we need to agree* on what exactly our subject matter *is* and *is not,* as well as the appropriate methodology for its investigation. Failing to do so will make collective progress on that project much more difficult, if even feasible. Hence, the systematization of our ambitions (and available methodology) is vital. As I hope to have made clear in §3.2.2, this does not mean halting all research until we have a *nature-revealing definition* of our subject matter. After all, that is only ever found at the *end* of inquiry, not the beginning. Nothing so polished can be demanded from research that has just commenced or is still ongoing. What *is* in order is a *clear*, and more or less *precise* ostension of our subject matter, *supported by an argument* to the effect that the ostension is successful.

I fear none of these lessons are very punchy or eye-opening. Methodology isn't exactly riveting, I know. But it's vital to every theoretical enterprise. If we think of theory as the output of a function, then that function is our methodology, taking as input our data, our observations,[66] our intuitions, and so on. It doesn't matter how high quality our input is if our function doesn't do what we want it to do: methodology is half of the puzzle in any inquiry. Furthermore, it seems to me that successful methodology within the context of a particular discipline typically *remains successful* in that context, no matter what new input is thrown at it. Therefore, it would be wise to get our methodology right *earlier* rather than *later*.

This is not achieved overnight however. Let us not forget that several generations passed before scientific methodology reached the level of maturity it now displays. For example, double-blind experiments and reviews were not standard practice until the mid-20th century, four centuries after the publication of Bacon's *Novum Organum.* And issues still dog scientific methodology today (such as publication bias). On a smaller scale, there are similar stories to be told of individual sciences: witness the long and difficult road from Freudian psychoanalysis to modern psychology, from biblically cross-referenced natural history to modern paleontology.

Thus we can expect there to be much work ahead of us in fashioning a reliable methodology for metaphysics. We've made a good start, but we have a long ways to go. We must apply our minds to this meta-level task with all the care and finesse that we display in our answers to metaphysical questions at the object-level. Despite its unexciting appearance, I personally found exploring our methodology as rewarding as exploring the object-level questions themselves, if not more so. It gives one hope of our eventually producing confirmed answers to the dizzyingly

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[66] If we think of *data* or *our observations* as the output of a function as well, then this other function looks like *even more* methodology (e.g., the operation of an oscilloscope, or any such instrument), which takes as input that aspect of reality under observation. Good quality data and accurate observations also require good methodology. Good methodology is just a must.

lofty questions we foolishly dare to ask. It makes one think: maybe metaphysics *can* be helped after all.

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