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ORIGINAL RESEARCH

# Theoretical virtues and the methodological analogy between science and metaphysics

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**Abstract**
Metaphysicians often claim that some metaphysical theory should (or shouldn't) be believed because it exhibits (or fails to exhibit) theoretical virtues such as simplicity. Metaphysicians also sometimes claim that the legitimacy of these sorts of appeals to theoretical virtues are vindicated by the similar appeals to theoretical virtues which scientists make in scientific theory choice. One objection to this methodological move is to claim that the metaphysician misdescribes the role that theoretical virtues play within science. In this paper I defend the metaphysician's use of theoretical virtues against this objection.

**Keywords** Theoretical virtues · Theory choice · Metametaphysics · Simplicity · Inference to the best explanation

## 1 Introduction

In recent years, the methodology of metaphysics has received a great deal of attention.[1] There is ongoing debate regarding what methodology metaphysicians actually employ, what methodology they *should* employ, and whether or not the methodology they do or should employ is capable of giving us justified beliefs regarding metaphysical matters.

Metaphysicians often appeal to *theoretical virtues*, and in particular that some theory should (or shouldn't) be believed because it exhibits (or fails to exhibits) cer-

[1] See, among many others, Williamson (2007), Ladyman et al. (2007), Thomasson (2007), Thomasson (2015), Huemer (2009), Chalmers et al. (2009), Ladyman (2012), Paul (2012), Ross et al. (2013), Kriegel (2013), Willard (2013, 2014), Blatti and Lapointe (2016), Benovsky (2016), Cappelen et al. (2016), Brenner (2017), Chakravartty (2017), Saatsi (2017), Beebee (2018), Bradley (2018a, b), Finocchiaro (2019), Finocchiaro (2021), Barber (2020), Bryant (2020), and Sider (2020).

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tain theoretical virtues such as simplicity or elegance.[2] Appeals to theoretical virtues within metaphysics are sometimes supported by noting that such theoretical virtues are appealed to in science as well. So, the thought goes, if theoretical virtues are truth-conducive in science, then presumably they are truth-conducive in metaphysics as well.[3] This argumentative strategy has received a great deal of criticism. The most common complaint is that there is some sort of discontinuity between science and metaphysics which blocks the transition from the idea that theoretical virtues are truth-conducive in science to the conclusion that theoretical virtues are truth-conducive in metaphysics (or truth-conducive in metaphysics in a robust manner capable of giving us significant guidance regarding which metaphysical theories to believe).[4]

But another response to metaphysicians' attempt to argue that theoretical virtues are truth-conducive in metaphysics on the basis of the fact that they are truth-conducive in science is to contend that this sort of argument is based on an inaccurate picture of the role that theoretical virtues play within science.[5] We might contend, for example, that theoretical virtues are not truth-conducive in science, and so conclude on that basis that the metaphysicians' attempt to appropriate the epistemic credentials of theoretical virtues within science for the metaphysicians' own ends are illegitimate. In this paper I address this concern. Of course, individual metaphysicians may have inaccurate pictures of the role that theoretical virtues play within science. But I do not think that they are mistaken about the crucial point that theoretical virtues are rightly seen as being truth-conducive within science, whatever else we should think about the role that theoretical virtues play within science. To that end, I will examine some recent arguments to the effect that the metaphysicians' appeal to the role that theoretical virtues play within science is illegitimate because it is not true that theoretical virtues are truth-conducive in science. To some extent this debate recapitulates debates within the philosophy of science proper regarding scientific realism and, more generally, whether or when we are justified in believing that scientific theories are true.[6] But the present debate is conducted with a different end in mind, namely in order to assess the epistemic credentials of metaphysics, and specifically to assess the epistemic credentials of appeals to theoretical virtues within metaphysics.

Here is the plan for the remainder of this paper. In Sect. 2 I suggest that we think of appeals to theoretical virtues in both science and metaphysics in Bayesian terms. As we'll see, thinking of theoretical virtues in the way I suggest here allows us to respond to some recent objections which have been raised to the notion that theoretical virtues

[2] My concern throughout this paper is with theoretical virtues conceived in this manner, as *truth-conducive* virtues, in the sense that theories which exhibit the virtues are thereby more likely to be true. It is a separate matter whether or when theoretical virtues are, say, *pragmatic* virtues, virtues which are good-making features of a theory in some respect which does not make the theory more likely to be true. Virtues conceived in this latter pragmatic sense might, for example, make a theory easier to formulate or test, even if they do not thereby make the theory more likely to be true).

[3] C.f. Sider (2008, p. 6), Paul (2012). and Brenner (2017).

[4] See, e.g., Huemer (2009), Shalkowski (2010), Kriegel (2013), French (2014), Willard (2014), Thomasson (2015) and Saatsi (2017): §4.1 (Bradley, 2018a; Bryant, 2020). For responses to many of the concerns which these philosophers raise see Brenner (2017), especially §3 (although note that Brenner focuses in particular on defending the theoretical virtue of simplicity), as well as Bradley (2018b).

[5] Ladyman (2012) and Bueno and Shalkowski (2020).

[6] C.f. van Fraassen (1980).

are truth-conducive. In Sect. 3 I respond to a number of objections which have recently been raised to the idea that theoretical virtues, or non-empirical theoretical virtues in particular, are truth-conducive in science. One thing that all of these objections have in common is that the philosophers who develop these objections take them to undermine the metaphysician's attempt to legitimize appeals to theoretical virtues in metaphysics on the basis of their alleged legitimacy in science. Section 4 concludes the paper.

## 2 Theoretical virtues and Bayes' theorem

I suggest that appeals to theoretical virtues are best understood in Bayesian terms, a point I will elaborate in this section.[7] The goal of this section is not to describe a certain way of thinking of theoretical virtues, assume that it is correct, and then declare victory against those who think that theoretical virtues are not truth-conducive. In other words, the goal of *this section* is not to defend the idea that theoretical virtues are truth-conducive. Rather, this section merely describes the approach to theoretical virtues to which I am sympathetic. In subsequent sections of the paper I will show that, given this Bayesian approach to theoretical virtues, we can offer a response to the objections to theoretical virtues in science and metaphysics which I will consider—the objections do not undermine theoretical virtues, conceived in this Bayesian manner.[8]

Here's what I have in mind. Let's say we have several competing hypotheses. In order to determine the probability of any such hypothesis H, conditional on some evidence E, we can determine the values of the probabilities cited on the right side of Bayes' theorem:

$$P(H \mid E) = \frac{P(E|H)\,P(H)}{P(E)}.$$

Since $P(E)$ will be the same for each of the competing hypotheses which are under consideration, it will not help us discriminate between them. So, in order to determine which hypothesis is most probable, we'll need to figure out, for each of our competing hypotheses $H_1,\ldots,H_n$, $P(H_n)$ (i.e., the prior probability of the hypothesis), and $P(E \mid H_n)$ (i.e., the likelihood of the hypothesis—that is, the probability of the evidence, given the hypothesis).

Different theoretical virtues might enter into the probability of a hypothesis in different ways. For example, if empirical accuracy is a truth-conducive theoretical virtue, then presumably it makes a hypothesis more probable by raising its likelihood. By contrast, non-empirical theoretical virtues such as simplicity are, if truth-conducive at all, best thought of as entering into the intrinsic probability of a hypothesis H—i.e., the prior probability of H, prior to conditionalizing on any evidence. So, to say that simplicity is a mark of truth is to say that a theory's simplicity enters into its prior probability, in the sense that, ceteris paribus, simpler theories have higher prior probabilities. This seems to me to be very plausible, but it is not entirely uncontroversial. Huemer, for example, has recently argued that simplicity is truth-conducive,

[7] Of course, the idea that theoretical virtues, and theory confirmation more generally, should be thought of in Bayesian terms is not new. See, e.g., Salmon (1990).

[8] And, in fact, one of the objections I consider, discussed in Sect. 3.5, fails whether or not we adopt a Bayesian approach to theoretical virtues.

not because simpler theories have higher priors, but because simpler theories tend to be compatible with a narrower range of evidence, and so tend to receive greater confirmation when we conditionalize on that evidence.[9] In other words, simplicity is truth-conducive because it makes certain likelihood ratios higher—i.e., $\frac{P(E|H)}{P(E|\neg H)}$ will tend to be higher for simpler hypotheses H and confirmatory evidence E. I don't think this account of why simplicity is truth-conducive is correct. In general, for any finite set of evidence we acquire, there will be some possible wildly complex theory which entails that evidence. In this case the complex theory will be compatible with an extremely narrow range of evidence. Nevertheless, the theory should be rejected because it is wildly complex, despite the fact that it is only compatible with such a narrow range of evidence. Of course, that's just one argument for the claim that simplicity is truth-conducive for some reason other than that, ceteris paribus, simpler theories have higher prior probabilities. But further defense of the notion that simplicity is truth-conducive because, ceteris paribus, simpler theories have higher prior probabilities is beyond the scope of this paper.

In Sect. 3 I will respond to a number of objections which have been made to the idea that non-empirical theoretical virtues such as simplicity are truth conducive, in either science or metaphysics. But it is one thing to offer responses to objections to the idea that theoretical virtues are truth-conducive in science or metaphysics, and it is another thing to show that we should believe that non-empirical theoretical virtues such as simplicity are truth-conducive in the first place. Here I do not plan to offer positive arguments in favor of theoretical virtues such as simplicity being truth-conducive. In fact, I'm not sure that there are any such positive arguments, at least not successful ones. But this does not mean that we are not justified in thinking that theoretical virtues are truth conducive, since we may have a priori and non-inferential grounds which support thinking these theoretical virtues are truth conducive. What's more, our accepting that these theoretical virtues are truth conducive may be an unavoidable component in any response to wide-ranging skepticism.[10] I mention this here in order to note that more can be said on this issue, but it will have to be said on another occasion. This paper is primarily defensive.

I'll now examine a number of recent objections to the effect that the picture just sketched of the role that theoretical virtues play within science is mistaken. The objections I discuss are given by Otávio Bueno and Scott Shalkowski,[11] and James Ladyman.[12] Some of the objections are idiosyncratic, but others are not. These latter objections strike me as the objections which are most likely to occur to those who object to the account of the role that theoretical virtues play within science which I've just sketched. For example, the objection I discuss in Sect. 3.3 claims that the history of science shows that theoretical virtues are not truth-conducive, since, for example, it shows that simple theories do not have a great track record for being true. My impression is that many opponents of theoretical virtues find this sort of objection

[9] Huemer (2009, pp. 221–225). Here Huemer follows Rosenkrantz (1977, Chap. 5) and Schwarz (1978, pp. 461–464). Forster and Sober (1994) prominently defend a similar idea.

[10] On both these points I follow Swinburne (1997, 2001, pp. 83–102).

[11] Bueno and Shalkowski (2020).

[12] Ladyman (2012).

compelling, and so it is important for the proponent of theoretical virtues to be able to show that the history of science does not in fact undermine the notion that theoretical virtues are truth-conducive.

Throughout the subsequent discussion I will often take simplicity as a paradigm example of a theoretical virtue, in part because it simplifies the discussion, but more importantly because simplicity is one of the more prominent (and contentious) of the theoretical virtues, in both science and metaphysics.

## 3 Objections

### 3.1 Objection 1: Either theoretical virtues are intrinsic or they are not. Both options are objectionable

Objection: "Proponents of arguments from theoretical virtues face a dilemma. Either theoretical virtues are intrinsic or they are not. If they are intrinsic, since truth is not an intrinsic feature of a theory, there is no guarantee that the satisfaction of the theoretical virtues will make the theory more likely to be true. ... If theoretical virtues are instead extrinsic, then determining that a theory is virtuous requires accounting for what goes on beyond the theory and in the world, thus losing the entire point of appealing to the virtues in the first place, which is breaking deadlocks that remain from considering extrinsic factors, as in empirically equivalent theories."[13]

Response: Neither horn of the dilemma seems to be very problematic. Start with the concern about intrinsic theoretical virtues. The simplicity of a theory does seem to be an intrinsic feature of the theory. Does it follow that, since truth is not an intrinsic feature of a theory, simplicity is not a mark of truth? No. Intrinsic features of theories (such as their simplicity) might very well *correlate* with extrinsic features of those theories. And Bueno and Shalkowski haven't provided any general arguments to the effect that intrinsic features of a theory cannot correlate with extrinsic features of the theory.[14] Compare: whether a cake is tasty is an extrinsic feature of the cake, since whether a cake is tasty is a matter of whether we enjoy the taste of the cake. But whether a cake is tasty is surely *correlated* with intrinsic features of the cake (e.g., its saccharine content). Now, admittedly, it is a bit less mysterious that a cake's intrinsic properties might be correlated with whether it is tasty than it is that a theory's simplicity would be correlated with whether or not it is true. After all, we can see how the cake's intrinsic properties can cause it to have other, extrinsic, properties, and it is harder to see how a theory's being simpler than some other theory should *make* it true. I concede that it is unclear how a theory's being simple could make it true. But I'm not sure that is why simpler theories are more likely to be true—that is, it's not clear to me that simpler theories are more likely to be true because their simplicity somehow makes them true. Regardless, my point here is just that there is no in principle obstacle to a theory's intrinsic properties (in this case, its simplicity) being correlated with some extrinsic properties (in this case, the theory's being true).

[13] Bueno and Shalkowski (2020, p. 458).

[14] Thanks to Nevin Climenhaga for suggesting this objection to Bueno and Shalkowski's argument.

What about the second horn of Bueno's and Shalkowski's dilemma? Perhaps some theoretical virtues are extrinsic rather than intrinsic. Does it follow that those theoretical virtues would then be useless in allowing us to decide between competing theories? I don't see why. Bueno and Shalkowski here assume that "the entire point of appealing to the virtues in the first place ... [is to break] deadlocks that remain from considering extrinsic factors, as in empirically equivalent theories." Below, in response to Objection 2, I'll argue that theoretical virtues have a role to play beyond allowing us to decide between empirically equivalent theories. Here I would just like to note that some appeals to theoretical virtues seem to explicitly involve appeals to empirical considerations, and so do not seem to be designed to *bypass* empirical considerations. For example, one theoretical virtue which Bueno and Shalkowski discuss is explanatory power. But whether a theory has explanatory power seems to be an extrinsic feature of the theory, since it is a matter of the theory's being able to explain some phenomena external to the theory (e.g., some physical phenomena discovered on the basis of empirical observation).

### 3.2 Objection 2: How do theoretical virtues stack up against other theoretical desiderata?

Objection: Even if theoretical virtues *are* truth-conducive, we are still owed some account of how satisfaction of theoretical virtues stacks up against other theoretical desiderata. For example, we might wonder whether empirical considerations always trump non-empirical theoretical virtues, so that non-empirical theoretical virtues can only help us decide between competing theories if those theories are both equally empirically adequate. In other words, "maintaining the truth conduciveness of theoretical virtues requires an account of exactly how they have such epistemic merit and where they fit in the hierarchy of truth-conducive considerations."[15]

Response: As stated, this isn't an objection so much as it is a challenge. But perhaps the idea is that if the challenge cannot be met then this should make us more skeptical of the idea that theoretical virtues are truth-conducive. In any case, I think that the Bayesian approach to theoretical virtues goes some way toward answering this challenge. One way to characterize the "hierarchy" of truth-conducive considerations which enter into the probability of a theory is in terms of the probabilities cited on the right side of Bayes' theorem, so that the truth-conducive considerations which enter into the probability of a theory H, given some evidence E, are $P(E \mid H)$, $P(H)$, and $P(E)$. On the view with which I am sympathetic, the simplicity of a theory enters into its prior probability, $P(H)$. Given this way of thinking about theoretical virtues such as simplicity, there's no reason to think that competing theories' relative satisfaction of theoretical virtues will only help us decide between them if they are equally capable of predicting our empirical evidence. A theory's probability is partially determined by its prior probability, a prior probability which is in turn partially determined, I suggest, by the theory's satisfaction of theoretical virtues such as simplicity. In order to decide between competing theories we must take account of their prior probabilities,

[15] Bueno and Shalkowski (2020, p. 459).

whether or not the theories are equally empirically adequate. By extension, we must take account of their satisfaction of any theoretical virtues, such as simplicity, which factor into their prior probabilities. We can also see that there is no reason to think that empirical considerations always trump non-empirical theoretical virtues such as simplicity. While simplicity, I have suggested, factors into a theory's prior probability, empirical considerations would presumably factor into likelihoods, e.g., $P(E \mid H)$. In principle a high prior probability can always swamp a low likelihood, and a low prior probability can always swamp a high likelihood. $P(H_1 \mid E)$ might be higher (or lower) than $P(H_2 \mid E)$, even if $P(E \mid H_2)$ is higher (or lower) than $P(E \mid H_1)$, as long as $P(H_1)$ is sufficiently larger (or smaller) than $P(H_2)$. And in fact, as Schindler[16] notes, the history of science furnishes us with examples where scientists (in retrospect correctly) accepted theories as true on the basis of their satisfaction of non-empirical theoretical virtues, despite the fact that those theories conflicted with some of our empirical evidence. One way to think of such cases is in the terms I've just described, where a theory's high prior probability (informed by its satisfaction of non-empirical theoretical virtues such as simplicity) leads us to think that the theory is probably true, despite the fact that the probability of our evidence conditional on the theory is relatively low, insofar as the theory conflicts with some of our empirical evidence.

This brings us to the question of how theoretical virtues fare in light of what we know about the role they have played in the history of science. It is to this important question that I now turn.

### 3.3 Objection 3: The history of science undermines the idea that theoretical virtues are truth-conducive in science

Objection: The metaphysician thinks that theoretical virtues are truth-conducive in science, and then from there concludes that they are also truth-conducive in metaphysics. But, Bueno and Shalkowski[17] contend, the history of science undermines this picture of the role that theoretical virtues play within science, since the history of science shows us that science does not rely on (truth-conducive) theoretical virtues. Bueno and Shalkowski claim, in fact, not only that theoretical virtues have not shown themselves to be truth-conducive within science, but that they have shown themselves *not* to be truth-conducive within science. Their case for this claim involves several case studies from the history of science. Here is one example (further examples will be discussed below). Consider the transition from a Ptolemaic model of the solar system to Copernicus's model of the solar system. At the time at which the transition took place, the Ptolemaic model was more theoretically virtuous than the Copernican model, insofar as it exhibited greater degrees of unification, explanatory power, and simplicity. Yet the Ptolemaic model is false. With the development of Newtonian physics a heliocentric model of the solar system may have come to acquire those theoretical virtues which the Copernican model of the solar system, in its initial formulation, lacked. Newtonian physics is both unified, simple, and explanatory. Nevertheless, we now know that Newtonian physics is false. This is bad news for the proponent of theoretical virtues:

[16] Schindler (2018, pp. 183–184).

[17] Bueno and Shalkowski (2020).

"If what was perhaps the most theoretically virtuous physical theory turns out to have been false, it is hard to see why being virtuous could be properly treated as a mark of truth."[18]

Response: How should the proponent of theoretical virtues respond to this line of reasoning?

Bueno and Shalkowski claim that their case studies show that the satisfaction of theoretical virtues is not *sufficient* for a theory to be true, since, e.g., Newtonian physics was theoretically virtuous, but turned out to be false. What's more, they claim, the case studies show that the satisfaction of theoretical virtues are not *necessary* for a theory to be true, since, e.g., non-relativistic quantum mechanics was accepted despite the fact that it is not theoretically virtuous. We should conclude: "Thus, the satisfaction of theoretical virtues is neither necessary nor sufficient for a theory to be true."[19]

Here it's important that we correctly identify the way that metaphysicians and others appeal to theoretical virtues. I doubt that anyone has ever seriously suggested that the satisfaction of theoretical virtues is necessary or sufficient for truth. Take simplicity as an example. It is easy to think of simple theories which are clearly false (e.g., "nothing exists, and nothing ever happens"), and so simplicity is obviously not sufficient for truth. Similarly, it is easy to think of very complex theories which are clearly true (just make a big long conjunction of true statements), and so simplicity is obviously not necessary for truth.

Those who think that theoretical virtues are truth conducive think that the satisfaction of theoretical virtues is a *mark* of truth, in the sense that theories which exhibit theoretical virtues are thereby *more likely* to be true. Above I've suggested that theories which exhibit certain non-empirical theoretical virtues such as simplicity are more likely to be true because their satisfaction of those theoretical virtues confers on them higher *prior probabilities*.

Given this way of thinking of theoretical virtues, we can see why the case studies cited by Bueno and Shalkowski do not show that theoretical virtues are not truth-conducive. Take, for example, their suggestion that Newtonian physics was highly theoretically virtuous, and yet turned out to be false. Doesn't this show that theoretical virtues lead us astray? No, I don't think so. At certain points in the past, given the information available to physicists at the time, Newtonian physics was highly probable. Part of the reason Newtonian physics was highly probable was because of favorable likelihood ratios: various pieces of evidence were highly probable given Newtonian physics, but improbable given some relevant competing physical theories. For example, Newtonian physics did a good job accounting for the elliptical orbits of the planets, while Aristotelian physics did not. Another reason Newtonian physics was highly probable was because it has a relatively high prior probability, in part because it exhibits theoretical virtues such as simplicity. Newtonian physics is, nevertheless, false, despite the fact that it was highly probable given the information available to us at certain points in the past. The reason Newtonian physics is improbable is because we have conditionalized on new evidence, evidence which is poorly accounted for by Newtonian physics, but well accounted for by competing theories (e.g., the observed

[18] Bueno and Shalkowski (2020, p. 463).

[19] Bueno and Shalkowski (2020, p. 464).

deflection of light around the sun, which is well accounted for by general relativity, but not by Newtonian physics). But just because Newtonian physics became improbable once we conditionalized on new evidence, this does not show that our initial estimation of its prior probability was incorrect. It is compatible with Newtonian physics's current improbability, conditional on current evidence, that Newtonian physics was nevertheless highly probable at various points in the past, given the information available at the time, and highly probable in part because it was theoretically virtuous. A theory's becoming improbable given a change in likelihoods tells us nothing about its priors, and nothing about whether/how theoretical virtues like simplicity enter into the priors.

We should also challenge Bueno's and Shalkowski's contention that scientists have learned that certain theories are true, or false, without taking into consideration their satisfaction of theoretical virtues. Start with their contention that scientists have learned that various theories, including Newtonian physics, are false. It seems to me that scientists have learned that these theories are false in part by relying on theoretical virtues. We can make any theory, such as Newtonian physics, accommodate new observations either by making ad hoc modifications to the theory, or by challenging the observations. Why didn't scientists do one of those two things when, say, they acquired new observations which seemed to falsify Newtonian physics?[20] For example, when Arthur Eddington reported astronomical observations incompatible with Newtonian physics, why didn't scientists conclude that, say, sneaky gremlins had tinkered with Eddington's instruments and made him seem to see or measure things which weren't really there (just as sneaky gremlins were sometimes claimed (seriously or not) to tinker with airplanes during World War II)? Arguably, the reason scientists didn't do that is because they made tacit appeal to theoretical virtues—for example, the gremlin hypothesis should be rejected because it is wildly complex. Equivalently: Newtonian physics, modified so as to include the scheming gremlins, is a wildly complex theory. It requires that we postulate a new class of beings who we otherwise have no reason to posit, with specific desires and abilities (e.g., the desire and ability to tamper with scientific instruments). Perhaps the gremlin hypothesis fares poorly with respect to its satisfaction of other theoretical virtues as well. Even so, its gratuitous complexity seems to be a large part of the reason it was and is not taken seriously.

Why bring gremlins into the discussion? Why not just stick to real examples from the history of science? My response is that the fact that nobody proposed the gremlin hypothesis is itself an instructive fact regarding the real history of science. And it nicely brings out the point I want to make: that we sometimes reject theories because they are gratuitously complex, and we do this so automatically and without conscious deliberation that we often fail to realize we are doing so. The point would not be so well illustrated by some hypothesis which people did seriously consider—after all,

[20] Well, some scientists *did* make ad hoc modifications to Newtonian physics in order to save it from falsification, and some scientists did challenge the observations which seemed to falsify Newtonian physics (e.g., they questioned the reliability of Eddington's measuring instruments)—cf. Earman and Glymour (1980). The question I ask in the main body of the paper should really be read as "why didn't the community of scientists at large, or some relevant subset of that community (e.g., those individual scientists whose work falsified Newtonian physics), simply make ad hoc modifications to Newtonian physics, or reject the observations which seemed to falsify Newtonian physics?" For ease of exposition, however, I have confined this qualification to a footnote.

I am concerned with the grounds we might have to reject a theory, and so I prefer illustrations which involve theories which everyone is and always has been inclined to reject, in order to think through why it is that nobody takes the theories seriously. The gremlin hypothesis is also easy to state and easy to understand. What's more, as we will see in Sect. 3.5, part of Ladyman's case against metaphysicians' appeals to non-empirical theoretical virtues relies on the alleged impropriety of examples such as this one—i.e., trumped up ad hoc modifications to theories, cited in order to illustrate how we allegedly face severe underdetermination with respect to our empirical evidence. So, it's helpful to have an example of just this sort of trumped up ad hoc modification to a theory (in this case, Newtonian physics).[21]

It might be suggested that the gremlin hypothesis was not rejected because it fares poorly with respect to considerations regarding non-empirical theoretical virtues such as simplicity. Rather, the thought goes, the gremlin hypothesis was rejected because scientists had no empirical evidence that gremlins were involved. But this is simply false: the gremlin hypothesis, suitably spelled out,[22] entails that we have the empirical evidence which we in fact have, and so our empirical evidence is ipso facto evidence for the gremlin hypothesis. What was wrong with the gremlin hypothesis was not that it could not account for the empirical evidence (since by hypothesis it *could* account for the empirical evidence), but rather that it was simpler to accept general relativity, which could also account for the evidence. I actually don't want to put too much stress on this latter point regarding simplicity. The main point I want to make here is also supported by the notion that we should reject the gremlin hypothesis for some reason other than the fact that it is needlessly complex. My main point is just that considerations regarding factors other than empirical adequacy are clearly at play here. So, it is not clear that the case studies Bueno and Shalkowski appeal to, where some scientific theories are falsified despite the fact that the theories in question are highly virtuous, are not simply cases where a theory is falsified because we come to endorse some incompatible theory (or disjunction of theories) in light of their greater satisfaction of relevant (non-empirical) theoretical virtues.

What's more, we can only conclude, on the basis of new evidence, that some theory is falsified, if we have some idea of what prior probability we should assign to the theory. If a theory has a sufficiently high prior then the theory may nevertheless be highly probable, even after we conditionalize on the problematic new piece of evidence. So we can only draw the conclusion that the case studies Bueno and Shalkowski appeal to are ones in which scientific theories were falsified if we make some judgements regarding the prior probabilities of the theories which have allegedly been falsified. How do we make those judgements? Plausibly, we make these judgements on the basis of some sort of assessment of the theoretical virtues exhibited by the theories in

[21] It is also instructive to note that thought experiments featuring demons (of which gremlins are a variant or species) have a venerable place in the history of science (Canales, 2020). Why not philosophy of science too?

[22] What I have in mind is a complex hypothesis to the effect that sneaky gremlins tinkered with Eddington's instruments to make it *seem* as if the predictions made by general relativity were correct, even though they were not correct. The gremlin hypothesis, then, made the same empirical predictions on the occasion of Eddington's famous expedition as general relativity did.

question. At any rate, absent this sort of procedure for assigning priors, it's hard to see on what basis the assignment of priors is being made.

So, plausibly, scientists have falsified the theories cited in Bueno's and Shalkowski's case studies with the (at least tacit) aid of considerations regarding theoretical virtues. We should say something similar about the case studies involving scientists coming to believe new theories. For example, Bueno and Shalkowski say that scientists made the transition to non-relativistic quantum theory despite the fact that it is not theoretically virtuous. From this example they conclude that "If the commonly-cited theoretical virtues really are markers of truth, they seem not to be driving crucial aspects of scientific activity and, so, the roles those virtues play in the sciences are not good guides to good method for metaphysicians."[23] It seems doubtful to me that the transition to non-relativistic quantum theory was not informed by considerations regarding theoretical virtues, at least tacitly. Why, for example, would anyone have thought that the Schrödinger equation correctly accounted for physicists' experimental observations, when those experimental observations could presumably all be accounted for by some more complex and/or ad hoc equation? Or, why think that those experimental observations were accounted for by the Schrödinger equation, when they could also be accounted for by our appealing to sneaky gremlins who guide our experimental results to make it *seem* as if reality is governed by the Schrödinger equation when it really isn't? Presumably we should reject these alternative explanations for our observations because they are not theoretically virtuous, for example because they are more complex than the explanation of the observations in terms of the world's being governed by or describable by the Schrödinger equation. Without such an appeal to theoretical virtues, it is hard to see why we should think it more probable that the world is governed by the Schrödinger equation than that it is governed by some alternative empirically equivalent theory.

So, again, Bueno's and Shalkowski's case studies do not show what they claim they show. They do not show that scientists do not rely on theoretical virtues in theory choice, and they do not show that scientists need not rely on theoretical virtues in theory choice.

I have repeatedly said that scientists have appealed to theoretical virtues, and must appeal to theoretical virtues in order to discriminate between empirically equivalent theories. But I have often claimed that scientists have or must have at the very least "tacitly" relied on theoretical virtues. This mirrors Bueno's and Shalkowski's own argumentative strategy: Bueno and Shalkowski do not argue from detailed historical case studies that the scientists involved in the case studies did not take themselves to rely on theoretical virtues to help guide theory choice. Rather, they argue that theoretical virtues *could not* have guided theory choice in these case studies, since if they were guiding theory choice then they would have resulted in different developments (e.g., then scientists would not have made the transition to non-relativistic quantum theory). Similarly, I have argued that the case studies only make sense under the assumption that the scientists involved *did* at least tacitly appeal to theoretical virtues as indicators of truth. It would strengthen my case if I cited further historical case studies where scientists were (in retrospect correctly) guided by considerations regarding theoretical

[23] Bueno and Shalkowski (2020, p. 464).

virtues in theory choice, either explicitly or implicitly. Here I would like to refer the reader to Schindler[24] for some recent historically informed arguments for the claim that in the history of science scientists have frequently appealed to theoretical virtues, and they did so in ways which, in retrospect, seem to have brought scientists closer to the truth. In some of Schindler's case studies the scientists involved *explicitly* appeal to theoretical virtues in support of the truth of their theories, contrary to Bueno's and Shalkowski's claim that theoretical virtues do not guide theory choice in science in any epistemically meaningful way. To give just one example, Schindler notes that theoretical virtues such as "Symmetry, unification, and simplicity ... are explicitly mentioned in the literature as reasons for why physicists adopted the [Glashow-Weinberg-Salam] model [of electromagnetic and weak interactions]."[25] Similarly, on the basis of a quantitative assessment of the frequency with which scientists explicitly invoke theoretical virtues, Mizrahi[26] has recently argued that scientists do indeed often explicitly appeal to theoretical virtues such as simplicity. The sciences Mizrahi examines are diverse, including the papers in JSTOR listed under "Biological Sciences," "Botany & Plant Sciences," "Ecology & Evolutionary Biology," "Astronomy," "Chemistry," "Physics," "Anthropology," "Psychology," and "Sociology." (While Mizrahi concludes that scientists do indeed make explicit appeals to theoretical virtues (including simplicity), he thinks that scientists do not explicitly appeal to these virtues as frequently as we would expect them to if those theoretical virtues play a major role in guiding scientific theory choice. Nevertheless, as Mizrahi concedes, scientists might be guided by theoretical virtues such as simplicity even when those scientists do not make *explicit* reference to those theoretical virtues. I have argued that this is often the case. Mizrahi's work simply confirms that scientists often explicitly appeal to the theoretical virtues as well.)

### 3.4 Objection 4: Many appeals to theoretical virtues are circular

Objection: Appeals to theoretical virtues are often illegitimate because, at least for some theoretical virtues, in order to establish that some theory exhibits the theoretical virtues in question we must first assume that the theory is true. Many appeals to theoretical virtues work like this: the theory in question is alleged to exhibit some theoretical virtue because, *if* the theory were true, then it would exhibit the theoretical virtue. For example, it might be claimed that a theory is explanatory because *if* the theory were true then it would explain some relevant phenomena. This move is illegitimate, Bueno and Shalkowski claim: that a theory *would* explain some relevant phenomena were the theory true does not show that the theory actually *does* explain the phenomena, and by the same token it does not show that the theory is true. Obviously, an appeal to a theory's satisfaction of theoretical virtues will not show that that theory is true if we can only establish that the theory exhibits the virtues in question if we assume that the theory is true: "Affirming the virtue premise of any argument from theoretical virtues is a dialectically illicit affirmation. The virtue accrues to a theory only if it is

[24] Schindler (2018, Chap. 6).

[25] Schindler (2018, p. 183).

[26] Mizrahi (Forthcoming).

true, but the argument depending on the theory's virtue is precisely an argument *for* the theory's truth."[27]

Response: I don't think that we need to think that a theory is true in order to see that it exhibits some truth-conducive theoretical virtues. One way to see this is by thinking through this issue in Bayesian terms, where, e.g., virtues like simplicity enter into a theory's prior probability. A theory can have a certain prior, and we can think that it has a certain prior, even if it is not true, or even if we do not think that it is true. Similarly, for virtues which enter into a theory's likelihoods (e.g., empirical accuracy): we can think that the likelihoods have a certain value even if the theory does not turn out to be true, or even if we do not think that the theory is true.

Take two particular theoretical virtues as examples, since they happen to be the examples which Bueno and Shalkowski use here: explanatory power and simplicity.

Start with explanatory power. It is an interesting question just what explanatory power amounts to. At the very least, it has something to do with whether a theory puts forward an explanation for some phenomena.[28] For example, the hypothesis of common ancestry offers an explanation for why so many different species have similar traits (e.g., similar bone structures), something which may be left unexplained on other hypotheses regarding the origin and development of life. To that extent the hypothesis of common ancestry exhibits greater explanatory power than those other hypotheses. Now, assume that we have reason to believe that the world contains few if any unexplained phenomena. (Perhaps, for example, unexplained phenomena complicate our total picture of the world, since they represent more features of the world which must be put in by hand in order to describe the world, and perhaps we have reason to think that simpler theories are more likely to be true.)[29] If we have reason to think that the world will contain few if any unexplained phenomena, then the fact that a theory would explain some phenomena if it were true would in fact, pace Bueno and Shalkowski, provide some reason to think that the theory is true. This is because the theory would make more probable some evidence on which we conditionalize, namely that the world contains few if any unexplained phenomena. And we can see that the theory would make this evidence more probable even if we don't think that the theory is true. Of course, you might challenge the assumption that the world contains few if any unexplained phenomena. Fair enough. But here I am simply responding to the concern that an appeal to the particular theoretical virtue in question, the virtue of being explanatory, cannot give us any grounds for thinking a theory is true which do not presuppose that the theory is true. I think it's clear that, given a background assumption that the world contains few if any unexplained phenomena, a theory's exhibiting the theoretical virtue in question *can* give us a non-circular reason to think that it is true.

[27] Bueno and Shalkowski (2020, p. 467).

[28] I don't want to suggest that this is *all* that explanatory power amounts to. For more discussion see, e.g., Keas (2018).

[29] C.f. Brenner (2015, p. §2) and Schindler (2018, p. 12), who suggest that theoretical unification might be truth-conducive because unified theories are simpler, insofar as they leave us with fewer unexplained phenomena. Their reasoning here mirrors my reasoning regarding explanatory power.

What about the virtue of simplicity? On the face of it it seems like we can see that a theory is simple (or complex) without first deciding whether the theory is true (or false). We can see, for example, that a theory according to which nothing exists is quite simple, even if we can also see that the theory is false. So what's the problem? Bueno and Shalkowski draw our attention to the fact that appeals to the virtue of simplicity generally contain some proviso to the effect that we should not endorse a complex theory unless we have to, and it is these provisos which, they claim, make any appeal to the virtue of simplicity circular. Consider, for example, Ockham's Razor, which tells us not to multiply entities *without necessity*. How do we tell whether an entity is a "necessary" posit? We can only do so, Bueno and Shalkowski claim, if we first determine whether the theory which includes that ontological posit is true: if the theory is true, then its ontological posits are, of course, "necessary" in the relevant sense (i.e., we should include the ontological posits in our total theory, if we want a true and comprehensive total theory); while by contrast if the theory is false then its ontological posits need not be necessary, unless those ontological posits happen to be required by whatever theory turns out to be true.[30]

This problem can be resolved if we carefully attend to what the proviso contained in Ockham's Razor is meant to tell us. If complexity is a mark of falsehood then we should not complicate our total theory, unless by doing so we make the theory more probable in some other respect (by, e.g., increasing the likelihoods). So, "do not multiply entities *without necessity*" should be read as "don't lower the prior by complicating the theory, *unless* there is a correlative increase in the probability of the theory in some other respect (e.g., unless the resulting more complex theory has sufficiently higher likelihoods)." If that's what the "without necessity" qualification in Ockham's Razor amounts to, then we need not assume that a theory is true in order to see whether it is favored by Ockham's Razor.

### 3.5 Objection 5: Non-empirical theoretical virtues are not needed in order to resolve cases of scientific underdetermination

Objection: Ladyman[31] argues that non-empirical theoretical virtues do not play the role in science that the metaphysician defender of theoretical virtues thinks they do. The metaphysician claims that non-empirical theoretical virtues (such as simplicity) play an important role in scientific theory choice because they are needed to get around underdetermination of scientific theory by empirical data. Ladyman denies this. Whatever underdetermination we face in scientific theory choice, Ladyman claims, we need not appeal to non-empirical theoretical virtues in order to resolve those cases of underdetermination. Rather, we can resolve the underdetermination by gathering more empirical evidence.

Ladyman distinguishes between different sorts of underdetermination.[32] For starters, underdetermination can be either local or global. Local underdetermination is underdetermination with respect to theories within some limited domain. We might,

[30] Bueno and Shalkowski (2020, p. 467).

[31] Ladyman (2012).

[32] Ladyman (2012, p. 43).

for example, have underdetermination with respect to theories in optics, and this would be a case of local underdetermination since it involves a limited domain of inquiry (optics). By contrast, global underdetermination is underdetermination with respect to theories meant to describe the whole of reality. We can also distinguish between weak and strong underdetermination. Weak underdetermination is underdetermination with respect to all of the observations we've actually carried out up until now. Strong underdetermination by contrast is underdetermination with respect to all possible observations.

The metaphysician wants to appropriate the response to underdetermination which we make within science, and offer the same sort of response to the underdetermination we find in metaphysics. But the sort of underdetermination we find in metaphysics is of the global and strong variety. So, the metaphysician must maintain that the sort of underdetermination we face in science is similarly both global and strong. Unfortunately for the metaphysician, Ladyman claims, almost all of the scientific theories which have actually been proposed are local theories, meant to describe some particular portion of reality, or some particular feature of reality (e.g., optical phenomena). What's more, most cases of underdetermination in science are weak cases of underdetermination, where we have underdetermination of theories with respect to the empirical observations observed so far, but which can be resolved with the aid of further empirical observations. So, in order to respond to most of the cases of underdetermination we find in science we need not appeal to non-empirical theoretical virtues. Rather, we need only appeal to further empirical observations, brought in to break the underdetermination of scientific theories which we find with respect to past empirical observations.

Response: When Ladyman says that all or most cases of underdetermination within science are cases of local and weak underdetermination, what he really means is that all or most of the cases of underdetermination with respect to theories *which scientists have taken seriously* are local and weak. Ladyman notes that cases of global and strong underdetermination in either science or everyday life are invariably "cooked up," in the sense that they involve theories which are not taken seriously by scientists, but which are constructed merely in order to press an in principle worry regarding underdetermination. For example, theories according to which the physical world is an illusion foisted on us by Descartes's evil demon, or theories according to which the world came into existence five minutes ago, are global theories, and there are no actual or possible empirical observations which would rule them out. Outlandish hypotheses of this sort are never seriously considered in science or everyday life, and what's more their "artificiality leads to doubt about whether global underdetermination should be taken seriously."[33]

Here I think that it is important that we pause and ask *why* the outlandish theories in question are not taken seriously in either science or everyday life. It is not because we lack empirical evidence for the outlandish theories, since we *do* have empirical evidence for those theories—by hypothesis, the theories predict our actual empirical evidence. I maintain that the reason we do not take these outlandish theories seriously is because they obviously fare poorly with respect to their satisfaction of non-empirical

[33] Ladyman (2012, p. 44).

theoretical virtues. If that's right, then the fact that we don't take these sorts of theories seriously in either science or everyday life would not, pace Ladyman, indicate that we face no genuine problem of underdetermination with respect to these theories. Rather, the fact that we don't take these theories seriously would simply show that we have a *response* to the underdetermination they illustrate, since we automatically rule out the theories on the basis of their poor satisfaction of non-empirical theoretical virtues (non-empirical because, again, the theories are constructed so that they are immune to empirical falsification). So, when the metaphysician says that there is underdetermination of theory by empirical evidence in scientific theory choice, they do not (merely) mean that there is underdetermination with respect to the theories which scientists take seriously. Rather, they mean that there is underdetermination with respect to the empirical evidence we gather in scientific investigation, arising from the fact that there are multiple theories which we either have or could construct which are all capable of accounting for the empirical evidence. It is no use replying that many of the possible theories in question are not taken seriously by scientists, since, again, it's plausible that they are not taken seriously by scientists precisely because they fare so poorly with respect to their satisfaction of non-empirical theoretical virtues.

It's odd that Ladyman misses this point. For example, following Laudan and Leplin,[34] he writes that attempts to show that we face cases of strong and global underdetermination in science or everyday life all involve "cheap tricks" or "logico-semantic trickery." For example, for any empirical observations, and any theory meant to account for those observations, we can in principle construct another theory which purportedly illustrates the global and strong sort of underdetermination at issue. All we need to do is take some theory T meant to account for the observations, and construct a new theory according to which "the observable phenomena are as if T but T is not true" (this is essentially what I've done above with the sneaky gremlin theory). But we need not take the resulting case of purported underdetermination seriously because we need not take the new theory we've constructed seriously, insofar as our new theory "is clearly ad hoc in every sense that Popper and his followers elaborated."[35] The resulting theory *is* wildly ad hoc, of course, but why would that give us any grounds for not taking the resulting theory seriously? Presumably because being ad hoc is a theoretical vice, and failing to be ad hoc is a theoretical virtue. The theoretical virtue or vice in question is non-empirical, since whether a theory is ad hoc seems to be unconnected with whether it is empirically adequate. And in fact, the ad hoc "the observable phenomena are as if T but T is not true" theory is *tailor made* to be empirically adequate, assuming the theory T we have in mind is itself empirically adequate, which it must be if the newly constructed competing theory is meant to represent a case of underdetermination. So, once again, the fact that we do not take the newly constructed competitor to T seriously in either science or everyday life simply points toward the fact that we use *non-empirical* theoretical virtues to rule out that theory.[36]

[34] Laudan and Leplin (1991).

[35] Ladyman (2012, p. 45).

[36] There is the interesting further question of why a theory's being ad hoc should be objectionable. Is being ad hoc an irreducible theoretical vice (or failing to be ad hoc an irreducible theoretical virtue), or are ad hoc theories objectionable because they exhibit some other theoretical vice (or fail to exhibit some other

## 4 Conclusion

It is highly controversial whether or how metaphysicians are justified in believing one metaphysical thesis is more probable than another. Metaphysicians sometimes contend that their methodology can be modeled on the methodology employed within science, and in particular that appeals to truth-conducive theoretical virtues within science vindicate appeals to theoretical virtues in metaphysics. A common objection to this move is to claim that there is some relevant disanalogy between science and metaphysics which blocks the transition from thinking that theoretical virtues are truth-conducive in science to our thinking that they are truth-conducive in metaphysics. But another response to the methodological move is to claim that it rests on a mistaken characterization of the way that theoretical virtues function in science. In this paper I've argued that this objection fails, although my responses to the objection often appealed to a Bayesian manner of conceiving of theoretical virtues (and theory confirmation more generally), and I have not attempted to present a positive case for adopting this sort of Bayesian approach to theoretical virtues. Overall, my arguments lend some support to the idea that a metaphysical methodology based on the appeals to theoretical virtues which one finds in science might be on the right track. This is not to suggest that *all* of the theoretical virtues which metaphysicians appeal to are truth-conducive. For example, metaphysicians often seem to think that it is a theoretical virtue to be in accord with common sense. That sort of theoretical virtue is not appealed to in science, and so is not vindicated by a methodological analogy between metaphysics and science.[37]

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theoretical virtue)? This is not something I want to take a firm stance on here, but I do have a suggestion. Schindler (2018, Chap. 5) and Keas (2018) both analyze a theory's being ad hoc in terms of its failing to exhibit a certain sort of coherence, either coherence internal to the components of the theory, or coherence with background theories presupposed by the theory, or with other things we believe. This seems right to me, but I would also suggest that this sort of coherence is only desirable because it brings with it greater simplicity. If that's right, then a theory's being ad hoc might be objectionable simply because more ad hoc theories are, other things being equal, more complex. (Keas (2018, p. 2778) also sees a connection between complexity and ad hocness, although he thinks that a theory's being ad hoc cannot be *reduced* to its being complex in a certain respect.)

[37] Conflict of interest: the author has no conflict of interest to report.

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