Darwinian-Selectionist Explanation, Radical Theory Change, and the Observable-Unobservable Dichotomy

By Elay Shech

Department of Philosophy

6080 Haley Center

Auburn University, AL 36849-3715

eshech@auburn.edu

Forthcoming in *International Studies in the Philosophy of Science*

Please cite published version

Acknowledgements: Thank you to Guido Bacciagaluppi, Thomas Lockhart, Michael Watkins, and participants at Centre for Philosophical Psychology Seminar at Antwerpen Universiteit (23 May 2019 in Antwerp, Belgium), Utrecht Philosophy of Science Seminar at Utrecht University (7 June 2019 in Utrecht, Netherlands) and The Fourth Taiwan Metaphysics Colloquium: A Workshop on Realism-Antirealism Debates (8-10 January 2020, Department of Philosophy, National Taiwan University, Taipei, Taiwan) for helpful discussion and comments on earlier drafts of this paper.

ABSTRACT

In his recent 2018 book, *Resisting Scientific Realism*, K. Brad Wray provides a detailed, full-fledged defense of anti-realism about science. In this paper, I argue against the two main claims that constitute Wray's positive and novel argument for his position, viz., his suggested Darwinian-selectionist explanation of the success of science and his skepticism about unobservables based on radical theory change. My goal is not wholly negative though. Instead, I aim to identify the type of work that an anti-realist like Wray would need to undertake in order to further substantiate their position, viz., taking a stance on inductive inference and support, and the type of realist and anti-realist positions that seem viable.

1. Introduction. In his book, *Resisting Scientific Realism*, K. Brad Wray (2018) provides a detailed, full-fledged defense of anti-realism about science in the spirit of Van Fraassen's (1980) constructive empiricism—an anti-realism that takes the scientific enterprise seriously and views science as a paradigm of rationality. There are two main claims that constitute Wray's positive and novel argument for his position. First, he offers an anti-realist, Darwinian or "selectionist" explanation of the success of science which, he holds, is both a genuine competitor of and superior to the realist explanation, viz., that the best explanation of the success of science is that our theories are approximately true. Second, he argues that radical theory change is part and parcel of the development of science so that "our contemporary theories are apt to be replaced in the future by theories that make significantly different assumptions about" unobservable entities and their behaviors (Wray 2018, 143). Importantly, Wray's anti-realism and pessimism regarding the fate of our current theories is restricted to unobservables. As he explains, realist and anti-realist alike agree that scientific knowledge and precision progresses with respect to observable phenomena. The anti-realist's skepticism then "is a skepticism about our alleged knowledge of unobservable entities that are posited to account for the phenomena" (Wray 2018, 204).

In this paper, I will argue against said claims, thereby adding my voice to the camp resisting Wray-style scientific anti-realism (e.g., French (2020), Vickers (2020), Psillos (2020)). In particular, I suggest in Section 2 that there is no viable Darwinian-selectionist explanation of the success of science. Or, said differently, that Wray conflates a logical-ontic explanatory project, which is the proper issue at hand, with a pragmatic-doxastic explanatory project. Additionally, I consider and reply to an objection to the effect that I have changed the topic instead of addressing Wray in terms of the current debate vis-à-vis theories as a unit of analysis. In Section 3, I argue that anti-realism about unobservables is misguided and, at best, commits Wray to a stronger form of skepticism than he would wish to endorse. Section 4 places my criticism in the context of a larger literature on the observable-unobservable distinction. My goal is not wholly negative though. Instead, I aim to identify the type of work that an anti-realist like Wray would need to undertake in order to further substantiate such a position, and the kind of realist and anti-realist positions that seem viable. Accordingly, I conclude in Section 5 that taking a stance on induction, confirmation, and inductive inference and support is key, and I also consider three objections having to do with the theory of induction that I work with, other anti-realist views that Wray could perhaps appeal to, and my overall aim.

Before starting in earnest, it is worthwhile to sketch John D. Norton's material theory of induction (MTI) since I will be drawing on insights from the material theory throughout this essay when discussing how induction and scientific confirmation work. Specifically, Norton (2003; 2021) has argued that formal theories of induction such as enumerative induction, hypothetico-deduction confirmation, Bayesian confirmation theory, etc., which provide universal schemas that are meant to identify the inductions that are licit and those that are not, stand against an insurmountable difficulty when having to distinguish cogent from non-cogent inferences that are formally equivalent (an example of which I give in Section 2).[1] Instead, he offers a material account of induction wherein "the admissibility of an induction is ultimately traced back to a matter of fact, not to a universal schema.. … *All inductions ultimately derive their licenses from facts pertinent to the matter of the induction*" (Norton 2003, 650; original emphasis). The MTI, then, is "material" instead of "formal" since inferences are warranted by the matter or contents of propositions instead of universal-formal schemas or rules of inference like the modes ponens. For example, consider the inductive inference from P to C (Norton 2021, Chapter 1):

Premise P:	This sample of salt A has crystallographic form B.
Conclusion C:	All samples of salt A have crystallographic form B.

On the MTI, what warrants said inference is "a fact hard won from the preceding century's work on crystals" (47), viz., "Generally, each crystalline substance has a single characteristic crystallographic form" (Norton 2021, 43). In this example, the inductive risk associated with said inference is that some crystalline substances may form crystals belonging to more than one crystallographic system. There is always inductive risk associated with cases of ampliative-

[1] I will not defend Norton's theory or claims here. He dedicates two books (Norton 2021; Manuscript) and many papers to the matter (e.g., Norton 2003; 2014). That a formal approach to induction is ostensibly misguided is also identified in Goodman's (1955) "new riddle of induction."

inductive inference. In turn, the local background facts that power the inductive inference are themselves supported by other instances of induction that are licensed by further facts.[2]

This is the framework of induction that I will be adopting and working with, and it is worthwhile to note (as I emphasize in Section 5) that it is empiricist-friendly. Additionally, the MTI is broad in scope since by "induction" and "inductive inference" Norton has in mind *any ampliative inference* between propositions including analogical inference, abduction, conduction, projection, etc., and *any notion of inductive support* between evidence and theory such as hypothetico-deductive or Bayesian confirmation theory.

2. The Darwinian-selectionist explanation and inductive inference*.* The standard realist explanation of the success of science is that our best theories are true or approximately true. Of course, there may be various peculiar sociological or pragmatic factors for why a particular theory is chosen over a competitor. But such reasons have no bearing on the truth of propositions under dispute in the realist/anti-realist debate, viz., propositions about the existence and behavior of paradigmatic unobservable entities like atoms and electrons. Hence, the type of scientific success that matters for the realist/anti-realist debate is the *predictive* (and retrodictive) success of science. This is to say, realists/anti-realists can rightly demand an explanation of successful prediction in science since such success is pertinent to the (approximate) truth of the propositions of science and theory. To some realists, explanatory power along with other theoretical virtues such as simplicity, parsimony, fecundity, novelty, being non-ad hoc, etc., also matters, since these virtues are a mark of truth.[3] However, Van Fraassen (1980) famously argued that such virtues are pragmatic, rather than epistemic. Unless one wishes to take those arguments head on (which I do not), the common denominator, so to speak, between realists and anti-realists alike concerns the predictive success of science. However, it is well known that false theories (e.g., the caloric theory of heat or the phlogiston theory of combustion) afford successful predictions as well, and so it isn't clear how truth is

[2] For worries about an infinite regress problem see Norton (2014) and Norton (Manuscript).
[3] For instance, see Lipton (1991/2004), Leplin (1997), Psillos (1999), and Schindler (2018).

supposed to *generally* explain predictive success. Instead, Wray offers, develops, and defends a Darwinian-selectionist explanation of the success of science à la Van Fraassen:[4]

> [A]ny theory that does not enable scientists to make accurate predictions is not apt to be around very long. No scientist will waste her career working with such a theory. As a result, any theory that is still around, that is, any theory that is still being used by scientists, is apt to be successful. Consequently, when philosophers of science look at the world of science, they should not be surprised to find only successful theories. The others have been eliminated or are on their way to being eliminated. . . . Our theories enable us to make accurate predictions, because scientist do not work with theories that do not enable them to make such predictions. Consequently, inaccurate theories are not represented in the population of theories accepted by scientists. Scientists who work with unsuccessful theories are as rare as mice that do not run from cats, and the fate of both is similar. (Wray 2018, 148-149)

Furthermore, Wray claims that in contrast with realists' explanation, the selectionist explanation enables us to explain why long-accepted theories come to be rejected. Specifically, newly observed phenomena and scientists' changing research interests are akin to a new evolutionary environment, thereby creating novel standards of success that theories must contend with. Theories that do not meet the new challenges are thus discarded (Wray 2018, 152-153). Also, he contends that the selectionist can explain why two competing and contradictory theories are both predictively successful: "When two competing theories both enable scientist to make accurate predictions … we should expect each theory to be accepted by some scientists" (Wray 2018, 155).

Unfortunately, there seems to me to be no viable selectionist explanation of the success of science. The reason is that a prediction based on a theory, law, model, etc., is an *inference*. When we are asking why science is predictively successful, we are asking why the inferences made in science are successful; why the inferential relations between the propositions that

[4] See Van Fraassen (1980, 39-40) for his original presentation.

constitute the contents of science or some particular theories are justified. Let us consider a simple case of a deductive inference to begin. Say that I predict successfully that "Socrates is mortal" and I do so on the basis of my theory that "All Greeks are mortal and that Socrates is Greek." What explains the predictive success of my theory? What explains my successful inference? The selectionist explanation would hold that our inferential practices enable us to make accurate inferences, because (rational) people do not work with inferential practices that do not enable them to make such inferences. Let us concede this claim for the sake of argument. Still, this explanation completely misses the point.[5] We are not asking about the *sociological* and *pragmatic* reasons that lead people or scientists to adopt certain inferential practices or work with certain scientific theories. Rather, we want to know *in virtue of what* exactly is it the case that some *particular* successful inference, or predictively accurate theory, is indeed so successful. Take the simple deductive example just discussed, it is (arguably) the fact that I used a correct rule of inference, along with the fact that the proposition "All Greeks are mortal and that Socrates is Greek" is *true*, that explains why the inference is successful. Or, perhaps on a deeper level, it is the fact that the concept of "Socrates" contains within it the concepts of "Greek" and "mortal."[6] But what is clear is that the Darwinian-selectionist explanation misses the mark.

Moving on to the *inductive* case, which is more pertinent to science, we can inquire into what explains the following successful inductive inference (based on Norton 2013, p. 649):

P1) Some samples of the element bismuth melt at 271 degrees C.

C1) Therefore, all samples of the element bismuth melt at 271 degrees C.

---

[5] I'm setting aside a well-known worry to the extent that such "explanations" are tautological. In the context of the biological sciences, there is usually a deeper story to tell in terms of traits, fitness, etc., with a corresponding debate in the philosophy of biology literature such as how to characterize fitness (see, e.g., Rosenberg and Bouchard 2020, Heine and Shech 2021). Charitably, I take it that the Darwinian-selectionist explanation suggested by Wray (2018) is part of the deeper sociological story to be told about rational and scientific practices.

[6] See, for instance, Shapiro (2007) for a discussion of linguistic-semantic, modal, and epistemic accounts of deductive inference.

The explanation concerns the presence of a background fact—a *true* proposition—to the effect that, *generally*,[7] chemical elements are uniform in exactly those properties that determine their melting point, viz., their elemental nature. It is the *truth* of this background fact that warrants the inductive inference and makes it the case that if P1 is true, C1 is *likely* to be true. This can be seen by considering the formally equivalent inductive inference:

P2) Some samples of wax melt at 91 degrees C.

C2) Therefore, all samples of wax melt at 91 degrees C.

Since "wax" is a generic name for various mixtures of hydrocarbons, it is *false* that, generally, wax samples are uniform in exactly those properties that determine their melting point. Consequently, the inductive inference is not warranted because there is no background fact that licenses the inference.

But what explains successful inferences and predictions when a theory is false? Something else. Presumably the "false theory" has captured counterfactual dependencies that are similar enough to that of the "true" theory, whatever it is. Perhaps also the true predictions can be explained by the list of factors identified in Wray (2018, Ch. 11), e.g., standards of accuracy change over time, models and theories are intentionally designed to account for data, shortcomings of theories are often ignored.[8] The important point is that inductive inferences and predictions in science are justified on a *case-by-case basis*—there is no general explanation for their success except to say that such inferences are powered and warranted by background facts and, of course, that these facts are all *true* (or approximately true). Insofar as the selectionist explanation offers insight into other issues such as why long-accepted theories

[7] Note the emphasis on "generally." It is not claimed that "*all* bismuth samples are uniform" since this would turn the inductive argument into a deductive one. Some elements like sulfur have different allotropic forms with different melting points (Norton 2003, n1).

[8] Another approach is to admit "… that there might be lucky flukes where false theories have (for mysterious reasons) managed to get things right, but that in the majority of cases the success of theories is best explained by their approximate truth" (Schindler 2018, 47).

come to be rejected, the realist can adopt this explanatory strategy while still maintaining that there is a deeper story to tell about cases of predictive-inferential success in science.[9]

There has been some past criticism[10] that resembles my own in some respects, although the emphasis has not been placed on ampliative-inductive inference. For example, Lipton (2004, 194) has claimed that the selectionist explanation "does not explain why a particular theory, which was selected for observational success, has this feature" (194). Similarly, Leplin (1997, 9) holds that "to explain why *particular theories*, those we happen to select, are successful, we must cite properties *of them* that have enabled them to satisfy our criteria." In reply Wray (2018, 168-169) says:

> I am prepared to acknowledge that the realist critics are correct on having more details about the mechanics responsible for the selection of our best theories in science. But indicating a need for further development is quite different from insisting that the explanation is bankrupt.

---

[9] Footnote and reference extracted for blind review purposes.

[10] The following includes past criticisms (Wray 2018, 161-163; original emphasis), wherein it is claimed that the selectionist explanation for the success of science:

> (1) can only explain *past* successes and gives us no reason to think that theories that have been empirically successful in the past will *continue* to be successful in the future. (Blackburn 2005, 178; Lipton 2004, 194; Psillos 1999, 97)
> (2) is compatible with a realist explanation for the success of science, so it is not a threat to the realist explanation. (Kulka 1996, S299; Lipton 2004, 193)
> (3) is not sufficiently deep, for it does not explain what is common to *all* empirically successful theories. (Kitcher 1993, 156; Psillos 1999, 96)
> (4) does not explain why any particular *successful* theory is successful. (Leplin 1997, 9; Lipton 2004, 194)
> (5) cannot account for the fact that we have *any* successful theories *at all*. (Blackburn 2005, 179)

In my view, a lot of the criticism is misguided. For example, consider (5). Wray's account for why we can have any successful theories in the first place consists of noting that since predictive success is relative to accepted standard, "it is more or less guaranteed that the theories that scientists accept will be successful to some degree." Although I find this line of reply unsatisfying, it is not clear to me that a realist explanation will do better. That approximately true theory affords (or even best explains) successful prediction does not imply (or even make likely) that we will be able to discover and construct predictively successful theories in the first place. What best explains that we have predictively successful theories at all? The fact that we happen, fortuitously, to live in a world that is conducive to inductive inferences. This need not have been the case. It is at least conceivable that our world was so extremely chaotic that most prediction would fail, even if we somehow had true theories.

I agree with Wray that demanding more details does not amount to a wholesale rejection of an explanatory strategy, but I do believe that the selectionist explanation is bankrupt. Namely, it seems to me that the anti-realist is conflating the issue at hand, what we can perhaps call a logical-ontic explanatory project that will have implications for the (approximate) truth of propositions about unobservables, with a pragmatic-doxastic explanatory project. That is to say, there is something about the world and the evidence we extract from it such that if some things are true (or approximately true), other things either must be or are likely to be true (or approximately true). There is also something about rational agents like ourselves wherein we somehow recognize and thus come to believe that if some things are true (or approximately true), other things either must be or are likely to be true (or approximately true). Realists and anti-realist alike can endorse a Darwinian-selectionist explanation regarding the pragmatic-doxastic issue: Our ability to recognize inferential structure and tendency to align our beliefs accordingly is explained by the fact that those who did not develop such cognitive faculties and dispositions did not survive.[11] Generalizing then, accepted scientific theories enable us to make accurate predictions because, all things being equal, scientist do not work with theories that do not enable them to make such predictions.[12] The pragmatic-doxastic issue, however, cannot be the focus of discussion because *it has no bearing* on whether a proposition like "electrons exist" is *likely to be true*. Instead, a charitable interpretation of the realist's claim that truth best explains the predictive success of science is that this has to do with the logical-ontic issue.

Specifically, what is it about the world, and about the empirical evidence extracted via observation and experimentation, such that if some things are true (or approximately true), other things are likely to be true (or approximately true)? Norton's MTI, I submit, sheds light on this issue. It holds that there will be no general, universal answer to this question. Instead, contingent facts pertinent to a particular situation at hand allow us to make the inferential leap

[11] I set aside stubborn, fictional characters such as the Tortoise in Carroll's (1985) parable.
[12] Of course, there will be some counterexamples since there are occasions in which the interests of scientist shift away from predictive success. However, looking to modern science as a whole, successful prediction and retrodiction, or, empirical adequacy and strength, is necessary for the acceptance of (let alone belief in) theories.

from some things being true to other being likely to be true.[13] Truth is part and parcel of the story. Insofar as the propositions describing the inductively obtained evidence that supports our best scientific theories are true (or approximately true)—and one would have to be an extreme sceptic to deny this—then the inductive inferences made based on this body of evidence is likely to be true. This, I take it, is what is meant by a reasonable realist when she claims that it would be miraculous if our theories were not approximately true, or, on the road to truth. All that said, it is worthwhile to clarify that I have *not* argued in this section that Wray's Darwinian explanation fails to meet Norton's standard of inference. Rather, I am suggesting that explaining the predictive success of science, which I argued is at the heart of the realist/anti-realist debate, is deeply interwoven in part with explaining its (primarily ampliative) inferential success. Whatever one's favorite theories of induction and confirmation are, the Darwinian-selectionist explanation fails to properly connect with the inferential issue.

However, one may object that I have misrepresented the issue at hand. The relevant question is not what explains the predictive-inferential success of science. Rather, what matters is explaining why "we accept the theories we do" (Wray 2020, 38). For example, following Leplin (1997, 9) and Stanford (2020, 26), we may want to know what features enable our theories to enjoy the success they do and why theories that survive our testing procedures are successful. Wray claims that the Darwinian-selectionist explanation provides such an explanation.[14]

In reply, first, recall that what is at stake in the scientific realist/anti-realist debate is whether claims about unobservables are likely or unlikely to be true. Relatedly, the debate is about the rationality (or lack thereof) of believing in the existence of (or remaining agnostic or even skeptical about) paradigmatic unobservable entities like electrons in light of the predictive success of science; it's about whether statements to the effect that unobservables exist and behave approximately as described by our best theories are likely to be true and thus ought to

[13] Or, if you believe that (say) Bayesianism is the one true formal theory of induction and confirmation, one can identify the Bayesian machinery as warranting inductive inferences.
[14] Psillos (2020), for instance, disagrees; while Stanford (2020) (following Leplin (1997)) claims that the selectionist explanation provides only part of the answer, viz., why theories survive.

be endorsed.[15] So, again, sociological and pragmatic factors, do not make it more or less likely that the statement “electrons exist” is true. Similarly, many factors identified in Wray (2018, Ch. 11) as pertaining to theory selection (e.g., standards of accuracy change over time, models and theories are intentionally designed to account for data, shortcomings of theories are often ignored) are also irrelevant.

Next, note that anti-realists in the spirit of Van Fraassen will hold that all theoretical virtues except for consistency and predictive-retrodictive success (or, empirical adequacy) are pragmatic. They will, for example, reject claims to the effect that we must believe in the existence of electrons because they are explanatorily indispensable to our best theories. Putting the two together then, we are left with asking what could account for the predictive-inferential success of science. Such an account has to do logical-evidential relations between the propositions of science (or some theory) about unobservables and the evidence that lends inferential support to such propositions. Truth, as I have argued above, is an inseparable part of the ontic-logical explanatory story. This does not mean, however, that we are automatically committed to the existence of unobservables—an issue that I take up in the next two sections. What does follow, though, is that the Darwinian-selectionist explanation of the success of science is irrelevant to the type of predictive-inferential success that is the proper issue at hand in the realist/anti-realist debate.

To end, I wish to consider an objection[16] to the effect that I have changed the topic since the contemporary realism/anti-realism debate is framed in terms of *theories* as a unit of analysis. Wray too is concerned with theories and not hypotheses about the melting points of particular elements or the crystallographic forms of samples of salts. Talk of induction and

[15] One may object that the scientific realism debate isn’t about what one should rationally endorse. After all, Van Fraassen and Wray don’t argue that it's irrational to be a scientific realist. Nor need a scientific realist argue that it's irrational to be a constructive empiricist. Both sides can recognize the rationality of one another's positions while nonetheless disagreeing about which is superior. In reply, and first, all that matters for my argument is that realists think that one ought to believe in claims like “electrons exist” because they are likely to be approximately true given the evidence that supports our best science, while anti-realist hold either that one need not believe such claims or that such claims are not likely to be true. Second, looking to Van Fraassen (1980, 2004), I maintain that the issue at heart is fundamentally about whether it is rational for an anti-realist to remain agnostic about unobservables.

[16] Thank you to an anonymous reviewer for raising this objection.

Norton’s MTI seems, at best, to change the topic and, at worst, not relevant. In reply, note the following three points.

First, admitting that theories are the unit of analysis relevant to explaining the success of science, how can it be that theories are “true” or “approximately true” as the realist claims? Presumably what is meant is that the propositions (or sentences) of some theory that are about the world are true or approximately true. Similarly, if a theory appeals to scientific models and other representations, then what is meant when we say that such models are true or accurate is that the propositional-sentential inferences made about the world and licensed by said models are true or approximately true.[17] Such propositions, for instance, that a fundamental particle like the electron exists, that its charge is $1.6*10^{-19}$ C, that its charge to mass ratio is $1.75*10^{11}$ C/kg, etc., are supported through inductive relations to other propositions describing the relevant evidence. All that is needed in order to apply logic is such a set of propositions (viz., the propositions of a theory and the propositions describing the evidence and other background facts) and in cases of inductive relations—which is typically the case when empirical evidence lends support to a theory—we can apply Norton’s MTI. Moreover, even if we do not wish to discuss inferences between the propositions of science but instead prefer to talk in terms of degrees of support between propositions, or the inductive support afforded a proposition of a theory in light of the evidence, e.g., “the strength of inductive support evolutionary theory derives from the fossil record or what Big Bang cosmology derives from the cosmic microwave background radiation” then the MTI still applies (Norton 2021, 340). The basic quantity will be “$[T|E]$,” which is the strength of inductive support afforded proposition T by proposition E, where T refers to propositions of the *theory* of interest, and E to the corresponding evidence and relevant background facts (Norton 2021, 448). In some contexts, for example, it will be appropriate, to represent such support probabilistically $P(T|E)$, and then we can apply the usual machinery of, say, Bayesian confirmation theory.[18]

[17] In other words, what I am saying here does not imply taking a stance on the syntactic versus semantic interpretation of theories. As long as theories imply a set of propositions that can be true/false then we can put logic, including Norton’s inductive logic, to work.

[18] For details, see Norton (2021), especially chapters 12-16. It is worthwhile to note that there are various contexts where Bayesian confirmation theory will not be applicable because it isn’t licensed by the relevant background facts.

Second, consider Wray's (2018) primary example of the Copernican revolution in astronomy and the candidate theories that may be (according to the realist) true or false, viz., the Ptolemaic versus Copernican theories in astronomy. Surely the relation of evidential support between the propositions describing the observable-empirical evidence of astronomy and the propositions describing the contents of Ptolemaic and Copernican theories in astronomy is, *fundamentally*, ampliative-inductive (although some relations will no doubt be deductive). If the relation between evidence and theory is, fundamentally, ampliative-inductive, then a theory of the logic of induction like the MTI is needed in order to decide on issues such as which inductive inferences are licit and what strength of inductive support is afforded the theory by the evidence. One may retort that contrary to my claims, "science is *not* about induction." Moreover, framing the realism/anti-realism debate in terms of competing justifications for the success of inductive inferences seems to structure the debate in a way that favors realism from the start. Hence, this seems like an illegitimate move in the debate. In reply, insofar as we can agree that the success of inductive inferences matters, i.e., inferences from inductively-empirically collected evidence to theoretical conclusions about the existence of unobservable entities like electrons that behave roughly as our best theories suggest, then I deny the charge that formulating the realism/anti-realism debate as such is illegitimate. The process of collecting evidence which speaks against or lends credit to—but does not deductively imply—specific propositions at issue in the realism/anti-realism debate favors neither realism nor anti-realism. Rather, were I to formulate the debate in terms of explanatory and causal concepts, and/or theoretical virtues, this may favor the realist since she holds that such concepts and virtues are epistemic while the anti-realist takes these to be pragmatic. Which brings me to the claim that "science is not about induction" or "scientific *theories* are not about induction." I do not deny that there are various facets to science that are not inductive in character (e.g., appealing to mathematical representations and derivation) but, given that by induction I mean any ampliative inference between propositions that isn't deductive or any notion of inductive support, it is hard to understand how one can question the idea that science is an inductive enterprise and that theories are grounded inductively in evidence. What are the other options? Science is not purely deductive, and if it isn't inductive-ampliative in character how are we to understand scientific inference? I submit that without making sense of the relation of evidential-inductive-ampliative support, "science becomes just another 'way of knowing,' to use a popular oxymoron of the skeptics. Without this relation, we do not know anything of the world. We 'know' but do not know. Without it, the ideas of science are no better than the fanciful creation stories of primitive mythologies" (Norton 2021, 2).

Third, one may continue attempting to resist and hold that by looking to the history of science, we can identify the properties of successful theories (viz., the so-called theoretical virtues such as simplicity, explanatory power, etc.) so that there is no need to talk about induction, evidence, and confirmation. Wray is replying to such philosophers noting that his Darwinian-selectionist explanation of the success of science is superior to their appeals to truth or approximate truth. However, and first, it then it isn't clear how we have gone beyond Van Fraassen's arguments that the theoretical merits are pragmatic instead of epistemic (as noted above). Second, the process of identifying the properties of successful theories and *inferring* that they are truth-conducive is an ampliative-inductive one so that a theory of inductive inference is presupposed and the MTI is appliable.

In sum, the concern can be posed as a dilemma: Either one has to address Wray on the terms of the contemporary debate vis-à-vis theories as a unit of analysis, or else one has to argue that debate should be changed so as to focus on induction. The objection then is that I have done neither. My reply is that I have done both. In the first horn, my critique of the Darwinian-selectionist explanation of the success of science via an appeal to the MTI is applicable even when discussing theories as the unit of analysis. In the second horn, an upshot of said critique (and the following two sections) is that a discussion of the inferential-predictive success of science tacitly presupposes a theory of induction because inferences and predictions based on theories and models are, fundamentally, ampliative-inductive.

3. The fate of theories and the observable-unobservable divide. Taking the above into account, it remains to be addressed whether we ought to epistemically privilege scientific claims regarding observables over unobservables. Let us then consider Wray's argument regarding the likely fate of our best theories:

> Every theory is only ever a partial representation of the world, thus every theory leads scientists to disregard some features of the world. Scientists' interests determine which features they disregard in their theories, and as they realize their research goals, their interests will change. Consequently, a theory that effectively served the interests of scientists at one time is apt to seem inadequate at some later time, when scientists have different research interests. At this later time, the theory is vulnerable to being discarded and replaced by a new theory that better serves current research interests. … My aim … has been to reexamine the history of science and reassess the significance of the pattern of theory change that seems to suggest that theories are apt to continue to be discarded indefinitely into the future. (Wray 2018, 187-188; 202)

Importantly, the type of pessimism and anti-realism that Wray thinks ought to be extracted from the above is one *solely* targeting *unobservable* entities and processes. We can maintain our realist intuitions and be entirely optimistic when dealing with observable phenomena.[19]

However, it seems that radical theory change is as much of a problem for an anti-realist like Wray, as it is for realists. Recall, the logical empiricists and positivists distinguished between theoretical and observational terms. If such a distinction were possible, one could be a realist only about the referents of observational terms. It is by now accepted, however, that

---

[19] It may be worth noting that as a deductive argument that starts from radical theory change and concludes that we ought to be anti-realists about unobservables, the argument isn't valid. Wray is correct to note that if "theories are only partial representation of the world" and if "scientist will be led to investigate phenomena that the accepted theories are not fit to account for" this will result in the emergence of new theories (Wray 2018, 186). Analogously, maps are also partial representations and our changing interest in, say, different regions implies that there will be many maps. However, none of this implies that our theories or maps will be *discarded* since they may form a consistent whole. Worse, if theories are discarded it isn't clear why it is only the unobservable parts that are discarded.

However, Wray's (2018) conclusion (on p. 202) suggests that radical theory change is problematic for the realist in a similar manner to the (in)famous pessimistic meta-induction, which holds that since the history of science is a history of discarded theories then, by *induction*, current theories are likely to be discarded too. Crucially, he seems to think that it is the responsibility of the realist to "identify some significant difference between today's theories and past theories," or between today's scientists and past scientists, in order to block the pessimistic inference (Wray 2018, 93; but also see 96-97). In contrast, I maintain that, insofar as such arguments are *inductive* then it is the anti-realist who has to identify background facts having to do with similarities between today's theories (or scientists) and past theories (or scientists) that *warrant the pessimistic inference*. Since I don't think that such background facts can be found, I submit that Wray's argument isn't cogent.

observation is theory-laden to some extent and this means that a strict theoretical-observational distinction is untenable. Instead, Wray follows Van Fraassen (1980) in making a *vague* distinction between observable and unobservable entities,[20] but he also maintains that radical theory change is part and parcel of the development of science. As noted by Bacciagaluppi (2019), this suggest that the observable-unobservable distinction is not rigid. What is presently unobservable may become observable in the future and, crucially, what is observable now may become unobservable latter. Why then, if the likely fate of our best theories is that they will be discarded, does Wray think he can continue to be a realist about observable phenomena and empirical laws? Why is scientific knowledge about observable phenomena stable according to Wray? It isn't clear that an answer is forthcoming. Thus, insofar as Wray's arguments are sound and cogent, they commit him to a stronger form of skepticism than the one he would wish to endorse.

It is worth emphasizing this last point in relation to our previous discussion of induction, which brings me to my main objection against Wray's argument from radical theory change. In particular, science is an inductive enterprise. The evidence that lends support to or speaks against various scientific claims concerns a relation of *inductive* support. But whether or not a particular inductive argument works, whether inductive inferences are warranted, does not depend on whether said claims are about observables or unobservables. For instance, considering our example with bismuth above, the inference is still warranted for "unobservable" samples of bismuth that are too small to be seen with the naked eye.[21] In other

[20] The observable-unobservable distinction is *vague*, but it is still *categorical*:

> That 'observable' is a vague predicate. There are many puzzles about vague predicates, and many sophisms designed to show that, in the presence of vagueness, no distinction can be drawn at all. In Sextus Empiricus, we find the argument that incest is not immoral, for touching your mother's big toe with your little finger is not immoral, and all the rest differs only by degree. But predicates in natural language are almost all vague, and there is no problem in their use; only in formulating the logic that governs them. A vague predicate is usable provided it has clear cases and clear counter-cases. Seeing with the unaided eye is a clear case of observation. Van Fraassen (1980, 16)

[21] To be clear, the inference is warranted for all actual (past, present, and future) observed samples of bismuth, and it is also warranted for all possible samples of bismuth (even those, say, that lie far away in regions of the universe that will never be epistemically accessible to us). The inference is not warranted for a bismuth atom since single atoms do not "melt," but it is warranted for a sample of bismuth too small for the naked eye to see and such a sample for Van Fraassen (and, I can only assume, also Wray) is "unobservable." Generally, phase transitions (like "melting") do exist in small systems that cannot be seen by the naked eye. Similar comments apply to the example in Section 1 from crystallography.

words, if Wray (or any anti-realist) thinks that there is a fundamental distinction between observables and unobservables that cuts across inductive-evidential support, such that we ought not ontologically commit to the latter, then this point needs to be argued for. It isn't clear that there is any well-received theory of induction and confirmation that works in this manner. But I want to suggests that anti-realists (such as Wray) can further substantiate their position by paying closer attention to induction and confirmation, and identifying why we ought to epistemically privilege claims regarding observable phenomena. In the next section, I'll place my concern in the context of the larger literature.

4. The observable-unobservable distinction and inductive inference. As it stands, Wray (2018, 2020) is not completely clear about his conception of the observable-unobservable distinction (as Shech (2020) and Vickers (2020) also note) or regarding what his scientific anti-realism is ultimately supposed to be (as Rowbottom (2019a) notes).[22] Still, he is fairly explicit that his position is in the spirit of Van Fraassen's constructive empiricism so I believe that it is fair to fill in the blanks, so to speak, by partly appealing to Van Fraassen's work.[23] To that effect, there have been various objections posed to the observable-unobservable distinction since the publication of *The Scientific Image* (Van Fraassen 1980) such as charges of circularity and

[22] For instance: "On page 58, for example, he equates 'theoretical knowledge' with 'knowledge of unobservable entities and processes'. Yet it's commonplace to think that theories needn't involve unobservable entities, which will make the discussion confusing…" (Rowbottom 2019a).

[23] Early on in his book, Wray (2018, 49) cites Van Fraassen to explain how he views anti-realism and the observable-unobservable distinction: "[A]nti-realists are not thoroughgoing skeptics. Anti-realists are skeptical, but only in a circumscribed manner. Specifically, they are skeptical about: (I) the claims our theories make about unobservable entities and processes (see, for example van Fraassen 1980)…" Later, he cites Stanford (2006, 3) and says that "[b]y the term 'unobservable,' I merely mean to capture the range of entities that [are] 'too fast or too slow or too rare or take place on too grand a scale for us to engage with in ordinary ways…'" (Wray 2018, 100). The "too fast or too slow" and "too grand a scale for us to engage with in ordinary ways" remarks suggest Van Fraassen's notion of the observable-unobservable distinction. But the "too rare" remark allows for a modified interpretation. Vickers' (2020, 15) argues that there is indeed a slight difference between Wray and Van Fraassen on the observable-unobservable distinction, and that this "… has dramatic consequences for one's degree of scientific scepticism." Still, his suspicion is that "Wray does not actually wish to depart from van Fraassen so dramatically; if he really did, then he surely would not have referenced van Fraassen in the way he does…" Ultimately, my point about the observable-unobservable distinction not making a difference for evidential-inductive support and inference holds on either interpretation (and, in fact, it is likely stronger if Wray moves away from Van Fraassen as Vickers suggests).

incoherence. In this section, I will discuss some of the criticisms that are perhaps similar to my concern in order to place my point in the context of the larger literature.

For example, Paul Churchland (1985) challenges the priority that Van Fraassen gives to spatiotemporal location instead of size in characterizing what is observable. For instance, we have observed (and will observe) various samples of bismuth melting at 271 degrees C—these are "observable" (in Van Fraassen's sense). So are similarly sized samples of bismuth that may be found in parts of the universe that our best scientific theories tell us are epistemically inaccessible to us and thus "unobservable" (but *not* in Van Fraassen's modal sense). Such samples are "observable" in the (modal) sense since, were they in our vicinity, we could observe them. However, if we create a sample of bismuth that is too small to see with the naked eye, a sample that necessitates use of a microscope, for instance, this will be an "unobservable" (in Van Fraassen's sense).[24] Consequently, Churchland (1985) maintains that the observable-unobservable distinction cannot "bear the great weight that van Fraassen puts on it" (Churchland 1985, 40). Similar sentiments are worded by Musgrave (1985, 205) who also holds that said distinction cannot bear the "epistemological burden" and Hacking (1981/1985, 135) who makes an analogous point bluntly:

> Taking van Fraassen's view to the extreme you would say that you have observed or seen something by the use of an optical instrument only if human beings with fairly normal vision could have seen that very thing with the naked eye. The ironist will retort: "What's so great about 20-20 human vision?"

To my mind, this line of attack is a bit too quick since it does makes sense for an *empiricist* to maintain that the "limits of perception should play a role in arriving at our epistemic attitudes toward science" (Van Fraassen 1985, 258). Nevertheless, *insofar as inductive inference is* powered and warranted by facts that are *not sensitive to the observable-unobservable distinction*, it isn't clear that an empiricist can consistently claim that only all observable (in Van

[24] Objects greater than 0.1 mm in size are typically visible to the naked eye, but bismuth nanoparticles can be as small as 40-50 nm (cf. Zhao et al. (2004)).

Fraassen's modal sense) samples of bismuth melt at 271 degrees C. And this point can be extended*, mutatis mutandis*, to one's own favorite account of induction and confirmation.

Other worries focus on our use of instruments like optical microscopes in order to extend the notion "observable" beyond what can be experienced with the unaided senses. For instance, Teller (2001) argues that while many instruments produce new "phenomena rather than providing 'windows' through which we look more deeply at phenomena that exist beforehand" (130), an instrument such as a "microscope no more produces an intermediate image in the production of phenomena than the eye ball produces a visual image of a tomato on [one's] retina of which one then becomes explicitly aware and interprets as an image of a tomato…" (133). In reply, Van Fraassen (2001) maintains, first, that seeing through an optical microscope is similar to seeing rainbows and reflections in water. He calls such observations "public hallucinations" and notes: "Some of these public hallucinations are actually pictures of real things: e.g. the reflection of a tree in the water. Some are not; e.g. the rainbow" (160). Second, he further submits that "without stretching ourselves very far, we can report on our sightings through a microscope in the same way that we report our rainbow-observations," suggesting that while reflections of a tree in the water correspond to an "observable" tree, the paramecia, mitochondria, cell walls, etc., seen in a microscope do not. His rationale is as follows:

> If you see a reflection of a tree in the water, you can also look at the tree and gather information about the geometric relations between the tree, the reflection, and your vantage point. The invariances in those relations are precisely what warrant the assertion that the reflection is a picture of the tree. If you say similarly about the microscope's images that they are pictures of e.g. paramecia, then you are asserting that there are certain invariant geometric relations between the object, image, and vantage point. But now you are *postulating* that these relations hold, rather than *gathering information* about whether that is so. (Van Fraassen 2001, 160; original emphasis)

The realist reply that suggests itself is that one is postulating relations between image-like phenomena also in the case of reflections of a tree in water (Alspector-Kelly 2004, 336-338), while the Van Fraassen sympathizer can either maintain that there is an epistemologically significant "difference in degree" in the case of "direct perception" (Kusch 2015, 177), or embrace a disjunctive view of perception where: "unaided veridical perception really is of actual physical objects, whereas perception with instrumentation results only in the experience of some kind of publicly observable phenomena akin to rainbows and reflections" (Monton and Mohler 2017). Another empiricist approach, suggested by Bueno (2018, 102), is that "observables" are objects to which we have "thick epistemic access," in the sense that such access "(i) is robust, (ii) can be refined, (iii) enables us to track the object, and is such that (iv) certain properties of the object itself play a role in how we come to know other properties of the object."[25] Bueno then argues that in the case of some objects seen through a microscope (like atoms) it's not clear "that the robustness condition, for example, is actually met" (105).

Nonetheless, as I see it, there is a sense in which the above approaches put the epistemic cart before the horse, so to speak. After all, if we are to take science seriously as anti-realists like Wray and Van Fraassen purport to do, we need to also take seriously how we come to have knowledge through science. As I already noted, science is an inductive enterprise: "the essential relation is inductive support. It obtains between the propositions of science and those that express the evidence on which it rests" (Norton 2021, 2). It then makes no *categorical* difference to the inductive argument whether one is "postulating" relations or "gathering information," whether we are considering direct perception of actual physical objects or perceptions with instrumentations that result in publicly observable phenomena, whether an ostensible object satisfies (i)-(iv) above or not—all such details, including said "invariances" which are "precisely what warrant the assertion that the reflection is a picture of the tree" amount to an inductive argument with more or less *inductive risk*. The inductive risk associated with concluding that a tree reflected in water is a "real thing" is reasonably less than the

[25] The concept of thick epistemic access is credited to Jody Azzouni (1997, 474-477; 2004) and is contrasted with thin epistemic access, which is the access that one has to an object through a theory that has five virtues: (i) simplicity, (ii) familiarity, (iii) scope, (iv) fecundity, and (v) success under testing (479).

conclusion that paramecia, mitochondria, cell walls, etc. are real things; but in both cases we have strong inductive support for such conclusions. In short, it's all induction, and it is reasonable for the scientific realist and anti-realist/empiricist alike to suspend belief in and remain agnostic about some entity in question only when the inductive risk is high enough, that is, only when the evidence is scarce.

All this is to say, if Wray (2018) is to offer us a novel manner by which to resist scientific realism, along with a coherent anti-realist position that takes science seriously, it is important that he take a stance on scientific induction and confirmation.[26] Van Fraassen famously realized that this is the case and he does take a stance: "I do not think that there is such a thing as Induction, in any form…" (Van Fraassen 2007, 343-344). This leads Van Fraassen to the adoption of epistemic voluntarism wherein pragmatic factors power inductive-ampliative inferences. However, as noted already by Ladyman (2007, 48-51), the problem with such a stance (if it can even truly make sense of the scientific enterprise) is that it isn't clear why an empiricist like Van Fraassen ought to be a constructive empiricist—with a corresponding modal sense of "observable"—instead of an "actualist empiricist" who believes only in actual (past, present, and future) observations, wherein this latter position amounts to no more than extreme inductive skepticism.

Still, my goal here is not to concentrate on Van Fraassen—that would need to be reserved for another paper. Instead, the point I wish to make is that Wray needs a story about induction and an explication of whether he is committed to the type of epistemic voluntarism that Van Fraassen embraces. Such details are important for deciding the nature and novelty of Wray's (2018) anti-realism, his stance on the observable-unobservable divide, and,

[26] Interestingly, Wray (2020) seems to recognize that the problem with certain scientific claims is a lack of evidence and an abundance of inductive risk. For example, after noting Wray's lack of clarity on the observable-unobservable distinction, Vickers (2020, 14) asks: "'Does Wray think we know that the outer core of the Earth is liquid metal?'" In reply, Wray (2020, 37) notes recent geological developments regarding the Earth's outer core and says: "I am led to conclude that geologists are finding new and surprising things about what lies beneath the Earth's surface … Hence, Vickers' confidence that our theory of the Earth's core is not likely subject to change in the future is unwarranted. Where he sees settled knowledge, I see uncertainty." Fair enough, perhaps we shouldn't be realists about such issues but, of course, it doesn't follow from this that there is a *categorical* difference between an "observable" and an "unobservable" (as Wray, following Van Fraassen, maintains), whereby one is rational in being a realist about the former and an anti-realist about the latter.

consequently, the cogency of his views.[27] At times, such as when he says that "Ptolemy's planets are not Copernicus' planets" since neither "the intension of the term "planet" nor the extension of the term "planet" is the same in both theories," it seems as though his anti-realism extends (well beyond constructive empiricism) also to observable objects (like "planets"). Yet, as noted at the start of the paper, he is also explicit about how his skepticism concerns specifically "unobservables."

In any case, the claim I make here is quite general. Particularly, there is a positive story that needs to be told regarding how science succeeds in making true claims about observable phenomena. Perhaps one need not go so far as many realists do by appealing to explanatory notions, causation, inference to the best explanation, simplicity, etc. But, minimally, we still need to make sense of inductive inference in science. In fact, I have chosen to work with Norton's theory of induction since it is very much empirical and deflationary in spirit. For example, the MTI does not appeal to inference to the best explanation or any explanatory notions, nor does it make use of the various other theoretical virtues such as simplicity, fecundity, etc., in order to warrant inductive inferences. It holds that there are no such general rules of induction. Accordingly, if induction is indeed powered by background facts, by true propositions, it is clear that we can make sense of the inductive, propositional-structure of the contents of science as realists do, viz., without embracing the observable-unobservable distinction. The question remains though, whether anti-realists such as Wray can consistently make sense of such a structure, of all the inferences regarding observable phenomena that we ascend to, while remaining skeptic or agnostic about inference regarding unobservables.

To end, it is worthwhile to note that there is a different route that an anti-realist like Wray (2018) may take, which is very much in line with the empiricist aversion to metaphysics, the insistence on experience as the ultimate source of justification, and perhaps also the lessons that one may potentially extract from the history of radical theory change in science. Specifically, on the one hand, issues of metaphysics-ontology that have to do with unobservables can concern many of the more commonplace claims of science such as those

[27] This is so since, for many aspiring anti-realist/empiricists, epistemic voluntarism may be too radical of a position to embrace, and it isn't clear how the type of inductive skepticism and blatant relativism that lurks in the background can accommodate the scientific enterprise.

regarding the existence of a fundamental particle like the electron, that its charge is $1.6*10^{-19}$ C, that its charge to mass ratio is $1.75*10^{11}$ C/kg, and so on. Famously, identifying constitutive properties and behaviors of unobservables, and confirming them via various independent lines of evidence, is a hallmark of strong inductive evidence for the existence of such unobservables.[28]

On the other hand, some of the metaphysical-ontological claims made by scientific realists concern the "deep," "true," and overall obscure nature of reality, such as whether electrons are "fundamentally" particles or excitations in fields, or whether the quantum wave function representing them is a real entity in itself living in a high-dimensional space, or regarding which interpretation of quantum mechanics is correct. Claims of the sort rarely (if ever) make use of solely inductive arguments that appeal to inductive-empirically gained scientific evidence to support their conclusions. Instead, they often appeal to abduction, notions of explanation and causation, and other theoretical virtues (which anti-realists/empiricists submit are pragmatic instead of epistemic), as well as coherence with one's other deep metaphysical commitments (which anti-realists/empiricists typically eschew).[29] While it isn't clear to me that the anti-realist can consistently make sense of science and reject inductive inference just because they are about "unobservables," the story is different when it comes to the deeper or more obscure metaphysical claims of some scientific realists. Specifically, the anti-realist and any sufficiently empirically minded philosopher can rationally remain agnostic about such claims since these go above and beyond the inductive-empirically gained evidence. The problem with such "unobservables" then is not that they are categorized as unobservable, or that scientific theories undergo radical change, but that there is a lack of inductive-empirical evidence to support one supposition over another. Such an anti-realism remains true to the idea that there is a line to be drawn between some claims in science that we ought to rationally commit to even in spite of the inductive risk involved, and those wherein

---

[28] See Salmon (1984, 213-227) for Jean Perrin's argument for the existence of atoms and Norton (2000) for a similar explication with the existence of the electron in the work of J. J. Thomson (what Norton calls the overdetermination of physical constants) and Niels Bohr (what Norton calls demonstrative induction).

[29] For example, Alan Baker (2005) and Mark Colyvan (2010) use abduction to argue that the scientific realists ought to also be a mathematical Platonist, and Baker (2016) appeals to theoretical virtues such as "scope generality" and "topic generality" in countenancing realists-nominalist objections.

we are being asked to stick our necks too far out.[30] Exactly where to draw the line cannot be determined generally and globally but will depend on the details of a case-by-case study, which is part and parcel of how scientists themselves ascend to beliefs about unobservables as the history of the discovery of molecules, atoms, electrons, etc., suggests.[31]

5. Objections and Conclusion. There is objection that suggests itself in light of my commitment to making sense of induction and confirmation via Norton's MTI. Specifically, why should we take the material theory seriously? If Wray's (or Van Fraassen's) anti-realism is inconsistent with the MTI, so much the worse for such a theory! Or so the objection goes. I have three lines of reply to this kind of objection, keeping in mind that defending the MTI itself is beyond the scope of this paper.

To start, I am fine with qualifying my claims to the effect that they depend in part on adopting the MTI, but the main point remains. Namely, one ought to take some sort of stance on how induction and confirmation work in science. Wray (2018) doesn't take such a stance. My suggestion then is that his position could be further strengthened by adopting a theory of induction that a realist is likely to accept as reasonable and rational, and then showing how one can coherently and consistently accept inductive inferences about observables while remaining agnostic about unobservables. Second, as mentioned above, the MTI is very much empiricist-friendly. While "most epistemologists who think mounting a direct response to inductive scepticism is possible … use some form of inference to the best explanation to defend the rationality of induction" (Ladyman 2007, 50), and Wright (2018) recently offers a non-abductive but *a priori* defense of induction, Norton's material theory of induction appeals neither to abduction nor to the a priori (Norton 2003, 2014, 2021, Manuscript). So, I am meeting the anti-

---

[30] Cf. Van Fraassen (2001, 163): "The point of constructive empiricism is not lost if the line is drawn in a somewhat different way from the way I draw it. The point would be lost only if no such line drawing is considered relevant to our understanding of science."

[31] Of course, one may frustratingly object: "but this is all that Wray and Van Fraassen want!" My reply is that their writings clearly indicate that they want more—they want to rationally maintain agnosticism (or skepticism in Wray's case) about unobservable aspects of reality (like atoms and electrons) for which the inductive-empirical evidence is overwhelmingly strong. I don't see how this can be done unless one also embraces a more radical skepticism than either Wray or Van Fraassen wish to, and this is so even in sprit of Wray's (2018) novel contributions, which is what I have argued for up until now.

realist/constructive empiricist more than halfway by working with Norton's theory. Last, it may very well be the case that even in the context of the MTI one can draw an observable-unobservable distinction of some sort that Wray (2018) would be satisfied with. The point then is that such work needs to be done by the anti-realists in order to substantiate their agnosticism or skepticism regarding unobservables.

Before ending, there are two additional potential objections that I'd like to consider. The first is that there are important anti-realist views in the literature that I do not discuss here but that address some of the issues that I noted. For instance, the worry that the observable-unobservable distinction isn't rigid in time, so that today's observable can be identified as tomorrow's unobservable, is (ostensibly) treated in (say) Rowbottom (2019b). Why can't Wray help himself to such views? One may also re-consider whether the core of anti-realism ultimately does depend on preserving the observable-unobservable distinction, in the light of recent work by (say) Stanford (2006, 2020). My reply is that, although I have attempted to place my claims in the context of the larger literature, in this paper I concentrate on Wray's (2018) important, novel, and recent contribution, and so tackling the anti-realism defended by, e.g., Rowbottom (2019b), Stanford (2006, 2020), is beyond our scope. I don't deny though that there may be other ways to enhance Wray's (2018) views, and such contributions would be welcome.

The second objection is a worry to the effect that we've lost track of what is being argued for in this paper and a concern regarding a lack of cohesion of the topics discussed. After all, doesn't (i) a Darwinian-selectionist explanation of scientific success, (ii) a history of radical theory change, and (iii) the observable-unobservable divide concern three different issues that ought to be handled separately? In reply, recall that my goal here is twofold: Namely, to interact with Wray's (2018) recent contribution—and I do so by arguing against the two main claims that constitute Wray's (2018) positive and novel argument for his brand of anti-realism—and to identify the type of work that an anti-realist like Wray would need to undertake in order to further substantiate their position, and the type of realist and anti-realist positions that seem viable. Wray's (2018) two main claims concern his (developed) (i) Darwinian-selectionist explanation of the success of science, which he holds is both a genuine competitor and superior to the realist explanation, and the claim that (ii) radical theory change grounds agnosticism or skepticism about (iii) the unobservable. This is one manner by which (i)-(iii) are related. In this paper, I have argued against both of these claims, noting in particular that, first, Wray conflates a logical-ontic explanatory project with a pragmatic-doxastic one and, second, that his rejection of unobservables due to radical theory change commits him to a stronger form of skepticism than that which he purports to embrace. In both cases, I suggested that the reason that Wray's position doesn't seem viable concerns his failure to take a positive stance on an account of induction and confirmation that can power his anti-realism, and this is a second manner by which (i)-(iii) are connected.

What I wish to further note is what scientific realists and anti-realists alike can agree on. Specifically, predictive-inferential success is (at least partly) explained by the truth of some particular propositions when the associated inference is warranted. The Darwinian-selectionist explanation then can be adopted by realists and anti-realists alike insofar as it sheds light on the sociological story pertaining to the pragmatic-doxastic issue. Still, it does not follow from any of this that we ought to commit to any ostensible entity discussed in science. After all, taking a stance on induction and confirmation (that doesn't amount to inductive skepticism) matters to both camps, and given that inductive support is a matter of degree, reasons for suspending belief or remaining agnostic have to do with the lack of empirical-inductive support, not with a principled and categorical (albeit at times vague) observable-unobservable distinction.

Insofar as it ought to be possible to distinguish between scientific realists and anti-realists it is (arguably) because some proponents of the former are willing to go beyond purely empirical-inductive inferences and adopt various theoretical virtues as epistemic, while the latter maintain that such virtues are pragmatic. Thus, it seems to me that a sophisticated anti-realist position that takes induction seriously and adapts itself to the changing nature of the observable-unobservable distinction (e.g., Bacciagaluppi 2019), and which is also reflective of how scientific evidence is accumulated and assessed, will come very close to a type of sophisticated realism that is not susceptible to Wray's (2018) critique. Such a realist position is *not* motivated by wholesale arguments like inference to the best explanation, which support a global thesis about all of science, viz., that we ought to commit to the existence of all theoretical postulates playing a special explanatory role. Instead, a realism that seems viable is a selective, local realism in which our commitment to unobservables is assessed on a case-by-case basis, taking the scientific evidence into account, as has been gestured at by, e.g., Asay (2019), Magnus & Callender (2004), Saatsi (2010), and Shech (2019, 2022). What such realism and anti-realism ultimately amounts to is left for further study.[32]

References

Alspector-Kelly, M. (2004), 'Seeing the Unobservable: Van Fraassen and the Limits of Experience,' *Synthese*, 140: 331–353.

Asay, J. (2019), 'Going local: a defense of methodological localism about scientific realism', *Synthese*, 196:587–609.

Azzouni, J. (1997) 'Thick Epistemic Access: Distinguishing the Mathematical from the Empirical,' *Journal of Philosophy* 94 472-484.

Azzouni, J. (2004) *Deflating Existential Consequence: A Case for Nominalism*, New York: Oxford University Press.

Bacciagaluppi, G. (2019), 'Adaptive Empiricism', in G. M. Dariano, A Robbiati Bianchi, and S. Veca (eds.), Lost in Physics and Metaphysics Questioni di Realismo Scientico (Istituto Lombardo Accademia di Scienze e Lettere), pp. 99-113, https://doi.org/10.4081/incontri.2019.465 .

Baker, A. 2005, "Are There Genuine Mathematical Explanations of Physical Phenomena?", *Mind*, 114(454): 223–238.

Baker, A. 2016, "Mathematics and Explanatory Generality", *Philosophia Mathematica*, (III) Vol. 25 No. 2: 194-208

Blackburn, S. (2005), *Truth: A Guide*. Oxford: Oxford University Press.

Bueno, O. (2018), 'Empiricism,' forthcoming in Juha Saatsi (ed.), *Routledge Handbook on Scientific Realism*, London: Routledge, 96-107.

Bueno, O. (2011) "When Physics and Biology Meet: The Nanoscale Case", *Studies in History and Philosophy of Biological and Biomedical Sciences*, 42:180-189.

Carroll, Lewis. (1895), "What the Tortoise Said to Achilles." *Mind*, 4(14):278–280.

[32] Admittedly, there will be concerns such as that "going local" will merely reduce the debate to questions that only scientists, not philosophers, can address (Dickens 2016).

Churchland, P., (1985), “The Ontological Status of Observables: In Praise of the Superempirical Virtues”, in Churchland and Hooker 1985, pp. 35–47.

Churchland, P., and Hooker, C. (eds.), (1985), *Images of Science: Essays on Realism and Empiricism* (with a reply from Bas C. van Fraassen), Chicago: University of Chicago Press.

Colyvan, M. (2010), “There is No Easy Road to Nominalism”, *Mind*, 119(474): 285–306.

Dickens, P. (2016), *A Critical Introduction to Scientific Realism*. Bloomsbury Academic.

French, S. (2020), “Resistance is futile!,” *Metascience* 29:5–10.

Hacking, I. (1981) “Do We See through a Microscope?”, Pacific Philosophical Quarterly 62 305-322. (Reprinted in P.M. Churchland and C.A. Hooker (eds.) Images of Science: Essays on Realism and Empiricism, with a Reply by Bas C. van Fraassen, Chicago: The University of Chicago Press, 132-152.)

Heine, K. D. and E. Shech (2021), “Roles of Mitonuclear Ecology and Sex in Conceptualizing Evolutionary Fitness.” *Biology & Philosophy*, 36(29).

Kitcher, P. (1993), *Advancement of Science: Science without Legend, Objectivity without Illusions*. Oxford: Oxford University Press.

Kusch, M., (2015), ‘Microscopes and the Theory-Ladenness of Experience in Bas van Fraassen’s Recent Work,’ *Journal for General Philosophy of Science*, 46: 167–182.

Kulka, A. (1996), ‘Antirealist Explanations of the Success of Science,’ *Philosophy of Science*, 63 (Proceedings), S298-S305.

Ladyman, J. (2007), ‘The Epistemology of Constructive Empiricism,’ Monton, B. (ed.), *Images of Empiricism: Essays on Science and Stances*, with a Reply from Bas C. van Fraassen, Oxford: Oxford University Press, pp. 46-61.

Leplin, J. (1997), *A Novel Defense of Scientific Realism*. Oxford: Oxford University Press.

Lipton, P. (1991/2004), *Inference to the Best Explanation*. 2nd Ed. London: Routledge.

Magnus, P. D., and C. Callender. 2004. ‘Realist Ennui and the Base Rate Fallacy.’ *Philosophy of Science* 71: 320–338.

Monton, B. and C. Mohler, (2017), ‘Constructive Empiricism,’ The Stanford Encyclopedia of Philosophy (Summer 2017 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/sum2017/entries/constructive-empiricism/>.

Norton, J. (2003), ‘A Material Theory of Induction’, *Philosophy of Science,* 70 pp. 647-70.

Norton, J. (2000), ‘How We Know About Electrons,’ in R. Nola and H. Sankey, eds., *After Popper, Kuhn and Feyerabend; Recent Issues in Theories of Scientific Method*. Dordrecht Kluwer, pp. 67- 97.

Norton, J. (2014), ‘A Material Dissolution of the Problem of Induction’, *Synthese*. **191**, pp. 671-690.

Norton, J. D. (2021), *The Material Theory of Induction*. University of Calgary Press.

Norton, J. D. (Manuscript), *The Large-Scale Structure of Inductive Inference*. https://sites.pitt.edu/~jdnorton/papers/material_large/material_large.html

Psillos, S. (1999), *Scientific Realism: How Science Tracks Truth*. London: Routledge.

Psillos, S. (2020), ‘Resisting Scientific Anti-realism,’ *Metascience* 29:17–24.

Rosenberg, A. and F, Bouchard, (2015), ‘Fitness,’ *The Stanford Encyclopedia of Philosophy* (Spring 2020 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/spr2020/entries/fitness/>.

Rowbottom, D. (2019a), ‘Review of Brad K. Wray Resisting Scientific Realism,’ *Notre Dame Philosophical Reviews* https://ndpr.nd.edu/news/resisting-scientific-realism

Rowbottom, D. (2019b), *The Instrument of Science: Scientific Anti-Realism Revitalised*. Routledge.

Saatsi, J. (2010), ‘Form vs. Content-Driven Arguments for Realism,’ in P. D. Magnus and J Busch (eds.) *New Waves in Philosophy of Science*, Basingstoke: Palgrave Macmillan, pp. 8-28.

Salmon, W. C. (1984), *Scientific Explanation and the Causal Structure of the World*, Princeton University Press, Princeton.

Schindler, S. (2018), *Theoretical Virtues in Science: Uncovering Reality through Theory*, Cambridge University Press.

Shapiro, S. (2007), “Varieties of Pluralism and Relativism for Logic.” In S. D. Hales (Ed.) A Companion to Relativism. Wiley-Blackwell: 526-553.

Shech, E. (2019), ‘Historical Inductions Meet the Material Theory,’ *Philosophy of Science*, 86(5):918–929.

Shech, E. (2020), ‘Review of Brad K. Wray Resisting Scientific Realism,’ *Philosophia*,48:861–866.

Shech, E. (2022), “Middle Path Realism and Anti-realism: Review of Timothy D. Lyons’ and Peter Vicker’s (Eds.) Contemporary Scientific Realism: The Challenge from the History of Science.” *Metascience*.

Stanford, K. P. (2006), *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*. Oxford University Press.

Stanford, K. P. (2020), ‘Resisting Scientific Realism with or Without van Fraassen’s Darwinian Explanation. *Metascience* 29:25–31.

Teller, P., (2001) ‘Whither Constructive Empiricism?’ *Philosophical Studies*, 106: 123–150.

Van Fraassen, B. C. (1980), *The Scientific Image*. Oxford: Clarendon Press.

Van Fraassen, B., (1985), “Empiricism in the Philosophy of Science”, in Churchland and Hooker 1985, pp. 245–308.

Van Fraassen, B., (2001), ‘Constructive Empiricism Now’ *Philosophical Studies*, 106: 151–170.

Van Fraassen, B. (2004), *The Empirical Stance*. Yale University Press.

Van Fraassen, B. (2007), ‘From a View of Science to a New Empiricism,’ Monton, B. (ed.), *Images of Empiricism: Essays on Science and Stances*, with a Reply from Bas C. van Fraassen, Oxford: Oxford University Press, pp. 337-383.

Vickers, P. (2020), ‘Resisting Scientific Anti-realism,’ *Metascience* 29:11–16.

Wray, K. B. (2018), *Resisting Scientific Realism*. Cambridge: Cambridge University Press.

Wray, K. B. (2020), ‘Still resisting: replies to my critics,’ *Metascience* 29:33-40.

Wright, J. (2018), ‘An Epistemic Foundation for Scientific Realism: Defending Realism without Inference to the Best Explanation,’ Cham: Springer.

Zhao, Y., Z. Zhangb, & H. Dang (2004), ‘A simple way to prepare bismuth nanoparticles,’ *Materials Letters* 58: 790–793.