A Pragmatic, Existentialist Approach to the Scientific Realism Debate

by

Curtis Joseph Forbes

A thesis submitted in conformity with the requirements
for the degree of Doctor of Philosophy

Institute for the History and Philosophy of Science and Technology
University of Toronto

A Pragmatic, Existentialist Approach to the Scientific Realism Debate

Curtis Joseph Forbes
Doctor of Philosophy

Institute for the History and Philosophy of Science and Technology
University of Toronto
2017

Abstract

The debate between scientific realists and anti-realist empiricists is often treated as a question about whose position is true, or rationally preferable *per se*. This debate is widely considered to have been argued to a stalemate. I suggest we accept this stalemate and instead investigate when and whether scientific realism or anti-realist empiricism is more useful, and thereby rationally preferable, for certain individuals. I develop a methodological framework for doing this, then put it to use by investigating the practical benefits for working scientists of adopting one view over the other. This investigation is historical rather than philosophical, and therefore empirical rather than theoretical, focused around a case study of the way philosophical commitments influenced the scientific practice of late 19^{th} c. electrodynamics researchers in positive and negative ways.

For Mary Butterfield,

whose trust and testimony helped me learn the importance of listening,

and thereby made me a feminist,

and thereby made me a better thinker.

Acknowledgements

First and foremost, thanks to both of my mothers, for showing me how to love well. I miss the first one dearly, and if my love could've saved her, she would've lived forever. Nevertheless, I'm extremely lucky and grateful to have my other mother in my life, along with my Dad, my Papa, my Nana, my Opa, and especially my Oma, my greatest inspiration. The few virtues I have, I owe to them.

My strongest thanks to Anjan Chakravartty for years of candid comments, criticism, and encouragement. It's metaphysically impossible for me to ever thank him enough, but he has my eternal gratitude.

My supervisor James Robert Brown deserves special thanks for responding to many of my wilder claims in a manner that was consistently fresh, skeptical, insightful, supportive, and helpful, all at the same time. I'm certain of very few things, but I'm certain that no other supervisor would've been as good for me.

Chen-Pang Yeang's influence on me has been diverse and greatly productive. Studying the history of physics with him planted the seeds of this entire work, exposed me to some of the greatest books I've ever read, and imbued me with a love of historiography. What I'll remember most, however, is building a replica of Hertz's spark-gap transmitter/receiver apparatus with him in my backyard, the best edutainment I've ever had. So, thanks to him, for all of that.

Thanks as well to Lorraine Code for her writing, thinking, and the directed reading course we did together. I don't address her work here directly, but suffused throughout my project is her insight that subjective factors must be accounted for in our epistemologies. I've internalized that insight so deeply that I now struggle to adequately express its influence on my thinking, other than to simply acknowledge my debt here.

Many professional academics gave me varying degrees of help, advice, insight, input, and support over the years: Catherine Elgin, Kit Fine, Robin Hendry, James Ladyman, Mark Newman, K. Brad Wray, Scott Woodcock, Bas van Fraassen, Peter Vickers, Arthur Fine, Samuel Schindler, Patrick Rysiew, Philip Kremer, P. Kyle Stanford, Kerry McKenzie, Paul Hoyningen-Huene, Howard Sankey, Kevin Haggerty, Matt Brown, Mark Solovey, Joseph Berkovitz, Colin Howson, Margaret Morrison, Lucia Dacome, Charles Mills, and many conference-goers whose names I either never caught or have forgotten. Thanks to all of them.

Particular thanks go out to Sean O'Connell and Wes Cooper for being kind and encouraging mentors, and to Jeff Foss for assuring me that I don't always have to be right, so long as I'm not boring. Thanks as well to Hasok Chang for being the most pleasant human being I've ever met, and for encouraging me to do history as a philosopher. Additionally, thanks to Paul Teller for being a philosophical hero that treated me like an old friend from the moment I met him, and ever since. And, of course, thanks to Tim Lyons for the most extensive and thoughtful external examiner report anyone has ever seen.

Thanks to Kyle Menken, Darin Gette, Kira Lussier, Nico Saldias, Marine Gobert, Kelin Emmett, Karen Deuel, Hasko von Kriegstein, Danny Strewlow, Dave Gaber, Hector MacIntyre, Kevin Kuchinski, Ross Reid, Sasha McNicoll, Ellie Louson, Jaipreet Virdi, Sarah Qidwai, James Digiovanna, Isaac Record, Boaz Miller, Chris Belanger, Ari Gross, Mike Thicke, Allan Olley, Greg Lusk, Cory Lewis, Mike Stuart, Alex Koo, Gwyndaf Garbutt, Bruce Petrie, and the late Nigel Verney for unique, meaningful, and ever delightful friendships that helped me succeed academically, in diverse ways. Thanks to my roommates—Josh, Robyn, Katlyn, British Mark, and Regular Mark—for tolerating me cloistering myself at home during the final stages of writing. Thanks to Jesse Boner for giving me my all-time favourite job, right when I needed it most. Thanks to Dave Suarez for several casual yet careful conversations on some of the philosophical issues dealt with here, most of which were hopefully more helpful for me than aggravating for him. Thanks to James Davies for helping me understand the nature of music, and for teaching me to play the guitar; apologies to him for not practicing more. Thanks to Paul Jarvey for always being excited about everything I do, or even think about doing. Thanks to Jennifer Whitson for teaching me how to write. Thanks to Audrey Yap for being Data to my Barclay. Thanks to Aaron Wright and Noah Stemeroff for being Spocks to my Scotty. Thanks to Colin Stephens for making me realize that pragmatism isn't bananas. Thanks to Jess Hall for finding it hilarious that I sometimes question my abilities. Thanks to Akwasi Owusu-Bempah for demonstrating that guys like us can do important academic work, and that cookies needn't only be a sometimes treat. Thanks to Adam Richter for being a better journal manager than me, and taking over my duties right when I needed to be thinking about nothing other than 19th century electrodynamics. Thanks to Denise Horsley for sharing a smile with me every single time I've ever seen her, and for so much more.

Thanks to all my friends in the Philippines—Rey, Simona, Vincent, Jayson, Craig, Ramil, Rossele, Garley, Jaypee, Rudelyn, Daniel, Shella, and everyone else—just for being solid people. Collectively they saw me through a soul-searching leave of absence from academia, during which the key features of this work were conceived. I miss them constantly, and I never would have been able to put all of this together had there not been people in this world as great as they all are.

Thanks to Bianca Torchia for making me recognize my own freedom and accountability in all things. She alone made me the best version of myself, at least for a time. I'll never forget that.

Thanks to my favourite writer Ellen Etchingham for reminding me how much I love hockey during the decade of darkness, for countless good times, and for showing me that non-epistemic factors inevitably and rightly influence our epistemic judgements.

Thanks to Ty Blakeman for periodically coaxing me out of my writing den, only to then explore many of the most important issues arising out of human existence with me. Sometimes I worry that what makes a question "philosophical" is that it's unanswerable, and thereby not worth thinking about; Ty regularly quells my fatalist impulses by reminding me not only that it's important to keep asking these questions, but impossible to stop.

It goes without saying for anyone who knows me well that I'd be a lesser man without a friend like Christopher Alton, so thanks to Connie and Bill for bringing him into existence. If I could have one wish it would be that he lives for as long as he chooses, continuing to make the world a better place just by being himself.

Lastly, thanks to the gods of copper and blue for Leon Draisaitl, and to Leon himself for exemplifying humility and dedication despite his providence. May he be an Oiler forever.

Table of Contents

For a landlady considering a lodger, it is important to know his income, but still more important to know his philosophy

G. K. Chesterton, *Heretics*

The final arbitrator in philosophy is not how we think but what we do

Ian Hacking, *Representing and Intervening*

Introduction: Philosophy for One

Philosophers are often motivated by the question "what should I think about *x*?", but their arguments typically aim to answer a much broader question: "what should *everyone* think about *x*?" To determine which position they (or me, or you, or any rational individual) should take on a philosophical issue, philosophers tend to argue that some position "is so certain that no reasonable man [sic] could doubt it" (Russell 1912, p.1). The philosopher's usual strategy for figuring out what to think, therefore, is an ambitious one: figure out what every rational person should think, and then think that. Sometimes this aim is achievable, and philosophical arguments can show people that it's always rational to think one thing, and irrational to ever think otherwise.

This strategy is not always successful, however, and sometimes seems doomed to failure. An increasingly common judgement amongst philosophers of science is that it fails when it comes to the scientific realism debate, which has been "argued to a stalemate" (Monton 2007, p.3). It would seem several, mutually exclusive positions on this issue are all rationally permissible, with no position appearing so certain that no reasonable person could doubt it. To be sure, this is not a consensus position, but it's a widely held one, and it's assumed in much of what follows. I begin by presenting some reasons to agree, but my main question is where this leaves those trying to decide whether to be a realist or an anti-realist themselves, or those hoping to convince someone else to change their mind on such matters. I argue that, even if philosophical argument can't show which position everyone should take, it can still show which position some individual should take. That is, rational arguments, based on evidence, can show that I (or you, or some other rational person) should accept one position and reject the other without determining that everyone else should do the same.

My aim is to resolve the following sort of worry: if philosophical arguments can't ground people's choice between rationally permissible options, then everyone's position is just a matter of taste, like a preference for vanilla over chocolate. Tastes are not accountable to rationality or evidence, so they can't be productively argued over. Only things like rhetoric, cajolery, wheedling, seduction, bribery, or violence can change people's minds on such matters. But the scientific realism debate addresses an issue many find very important: is the modern scientific worldview true? Scientific realists believe it is, while anti-realist empiricists remain agnostic, but it would be shocking if realists and anti-realists were just expressing different tastes, immune to the influence of evidence and philosophical argument. I show that they are not. Rational consideration of the evidence can still change someone's mind about being a realist, or an anti-realist, even if we assume that realism and anti-realism are both rationally permissible positions *per se*.

My argument is structured as follows: the first chapter discusses the traditional scientific realism debate, understood as the effort to determine whether scientific realism or anti-realist empiricism is true, or rationally preferable *per se*. The aim is to show why this debate seems to have been argued to an intractable stalemate, where realism and anti-realism can both be understood as rationally permissible positions. The second chapter outlines and elaborates the voluntarist understanding of this stalemate developed primarily by Bas van Fraassen (1980, 1984, 1985, 1989, 1992, 2000, 2002, 2004, 2005a). My focus is on showing how people form rational preferences for one position over alternatives as a function of their agent-relative values. But, I argue, it is not as immediately obvious which position is best for someone, given their values, as is often assumed by the voluntarist analysis. Certain kinds of evidence can show people that they've made the wrong choice, given their values, and that the rational thing to do would be to adopt a different position. The third chapter develops a methodological framework for finding the relevant kind of evidence, and the fourth uses it to find some. Specifically, I look to the history of science for evidence that can help working scientists decide whether to be a realist or an anti-realist, given their professional values as working scientists. I do this by providing a detailed case study of late 19th century European electrodynamics, which shows that scientific realists make better working scientists in specific research contexts; disquietingly for some, it also shows that anti-realists make better working scientists in other research contexts. But the idea is just that, if a working scientist were to consider this evidence, it would be rational to adopt the position that has promoted productive research practices in research contexts sufficiently similar to their own, and irrational to do otherwise.

Thus, I conclude, rational arguments based in evidence can show specific individuals whether they should accept realism or anti-realism, even without showing everyone else they should do the same thing. To be clear, I don't aim to show that every rational person should adopt one position and reject the other, only that some rational people should do so, in certain circumstances. That's enough to show that evidence and philosophical argument can still be used to change people's minds on this issue, so their positions on these matters are not mere matters of taste. And that's my ultimate aim.

I call this a "pragmatic, existentialist approach to the scientific realism debate," making good on van Fraassen's (2000, p.273) suggestion that his voluntarist framework allows pragmatic and existentialist concerns to play an important role in deciding philosophical questions about science. This approach is "pragmatic" because it allows many contextual, practical, value-based, and non-epistemic considerations to impact our assessment of philosophical positions. It's "existentialist" because it allows that many of the factors involved in such assessments can be understood as free choices, unanalyzable acts of will, personal commitments that need not (or even cannot) be debated. And while the voluntarist framework in which this approach is developed introduces an element of relativism into our epistemic lives, it doesn't make rationality or evidence irrelevant. Evidence and philosophical argument can't tell us whether scientific realism is true, or rationally preferable *per se*, but they can still tell each of us whether it's rationally preferable to be a scientific realist, ourselves.

The scientific realism debate may have been argued to a stalemate as a debate over which position is best for everyone, but there's still a way to productively argue about which position is best for each of us. This is an important result, given many philosophers' motivations for debating scientific realism in the first place: figuring out what they should think. My thesis, simply put, is that when it comes to the issue of scientific realism, philosophical arguments can still help them do that.

Chapter One – The Scientific Realism Debate as a Stalemated Debate.

1) Introduction

The aim of this chapter is to show why many philosophers of science believe that the scientific realism debate has been "argued to a stalemate" (Monton 2007, p.3), at least as a debate about which position is true, or rationally preferable *per se*. Not only has no extant argument established realism or anti-realism as the 'correct' philosophical interpretation of science, but it seems unlikely that any argument ever will.[1] To see why, it will help to keep two types of scientific realism carefully distinct: an epistemological version, and an axiological version. Doing so will make it much easier to explicate and understand the failings of the arguments generally marshalled for and against each of them.

Arthur Fine's *The Shaky Game* (1986a, esp. Ch.7) is notable as a classic text on realism that uses a similar distinction to also argue that the debate between scientific realists and anti-realists is irreconcilable.[2] I outline the distinction between axiological and epistemological versions of scientific realism in the next section, but I begin by first discussing Fine's position on the scientific realism issue. He summarized his position quite forcefully: "Realism is dead" (1986a, p.112). What he means is that no argument can be offered that realism (or anti-realism) is the only tenable philosophy of science, so we should stop arguing about this issue. I'll provide some reasons to agree in what follows, but one difference between his ultimate conclusion and my own should be made clear at the outset.

---

[1] This conclusion is increasingly represented in the literature on this subject, particularly in the earlier work of Allison Wylie (1986), Andre Kukla (1998), and Arthur Fine (1986a, 1986b), but also more recently in the work of Paul Teller (2011), Nancy Cartwright (2007), Peter Lipton (2004), Anjan Chakravartty (2004, 2007a, 2007b, 2011a), and Bas van Fraassen (2000).

[2] Fine (1986a) looks at the difference between arguments for realism at the "ground level"—where we are asked to explain why scientific theories are successful—and at the "methodological level"—where we are asked to give a philosophical account of why specific methods are embedded in scientific practice, showing that both types of arguments fail but for different reasons. His terminology is different than the more contemporary terminology used here (which is closer to that found in Lyons (2005 and 2012), for example) but his analysis of the relationship between these "levels" of realist argument is the same as the relationship between arguments for epistemological and axiological scientific realism I describe.

Fine goes so far as to liken a commitment to scientific realism or anti-realism to a religious leap of faith, and on that much we agree. He writes:

> In support of realism there seem to be only those 'reasons of the heart' which, as Pascal says, reason does not know. Indeed, I have long felt that belief in realism involves a profound leap of faith, not at all dissimilar from the faith that animates deep religious convictions. I would welcome engagement with realists on this understanding, just as I enjoy conversation on a similar basis with my religious friends. The dialogue will proceed more fruitfully, I think, when the realists finally stop pretending to a rational support for their faith, which they do not have. Then we can all enjoy their intricate and sometimes beautiful philosophical constructions (of, e.g., knowledge, or reference, etc.), even though to us, as nonbelievers, they may seem only wonder-full castles in the air. (1986a, p.116, n.4)

The reference to Pascal and the grounding of religious faith here is telling and instructive. While religious faith may generally be grounded by "reasons of the heart," Pascal did mount a pragmatic argument in favour of adopting the Christian faith and all the ontological baggage that comes with it. Once one accounts for such practical considerations, Pascal argued, the choice is obvious for anyone who sees faith as a live option, in which case one should go through the motions of having faith as if one already has it, until one actually does. But Fine doesn't think there can be any such practical considerations in favour of being a realist or an anti-realist; scientific practice seems equally consistent with either position (Fine 1986b, forthcoming; cf. Kukla 1998, p.32, Hendry 2001). If there's no practical difference between being a realist and an anti-realist, and there is no sound argument that one view is true and the other false, Fine thinks continuing the scientific realism debate would be a waste of time and thought. His argument is a pragmatic one: there is nothing practical to be gained in continuing the debate, so we should stop.[3]

I argue there's still something practical to be gained by continuing the scientific realism debate, albeit continuing it in a specific way. For contra Fine (1986b, forthcoming), I argue that there *is* a practical difference between being a realist and an anti-realist.[4] After all, there's no intuitive reason to think people's philosophical commitments would be behaviourally inert; rather, we

[3] Arguments similar to Fine's can be found in Rorty (1999) and Rouse (2002a, 2002b); for a counterargument based in an appeal to history that is similar to the argument I offer here, see McArthur (2006).

[4] As the fourth Chapter makes clear, this central part of my argument is largely an elaboration of Robin Hendry's (1995, 2001) arguments against Fine. While Hendry does not conduct the kind of historical investigations into these matters that I conduct here, my debt to his published insights and suggestive comments cannot be overstated.

should probably expect that realists and anti-realists would behave differently, for example, as working scientists, as they'd be motivated towards different sorts of activities and prone towards different kinds of inferences. As I illustrate in chapter four through a study of late 19th century European electrodynamics research, history provides empirical support for those intuitions: realists and anti-realists practice science quite differently, potentially in regular and predictable ways (Hendry 2001; cf. Hendry 1995, Wray 2015). What's more, this study suggests that realists and anti-realists tend to be more successful than each other in certain research contexts. If further studies show that such patterns are historically robust, contemporary scientists might use this information to help them make a practical decision about whether to be a realist or an anti-realist, should they find themselves in a research context where people with one attitude tend to be more successful than the other. Thus, we have practical reasons to investigate the practical benefits of realism and anti-realism, to help people decide which position is better for them.

Responding to this project, Fine (forthcoming) expresses skepticism that it will succeed, defending his claim that being a realist or an anti-realist makes no practical difference for working scientists. Scientific progress, he argues, comes from all camps. I think history shows realism is better in certain contexts, and anti-realism better in others, but Fine (forthcoming) responds:

> I don't think so. That is, I do not think there are reliable 'best practice' guides that link generic scientific tasks (build theories, measure parameters, look for novel phenomena, etc.) with meta-attitudes like realism, or instrumentalism, or empiricism. Except for the Feyerabendian 'anything goes' (or as I prefer 'anything might go'), there are no dialectical laws for scientific practice. Just as realism (along with other metaphysical attitudes) fails to be rationally required for understanding science, it also fails in a general or generic way to be required for doing science ('scientific progress'). (p.1)

Fine goes on to show how good work can be done whether scientists consider theoretical postulates real or mere useful fictions. He also works through some hypothetical scenarios to show that a realist and an anti-realist can be led to produce the same result, or develop the same technique, despite their philosophical differences. He concludes: "I see no practice motivated by a search for truth that could not be motivated just as strongly in a quest for reliability" (ibid., p.3).

Three responses are appropriate here. Firstly, Fine is equivocating between possibility and actuality: I don't argue that realist and anti-realists *couldn't* be motivated to undertake the same activities, only that history shows they *aren't*, and that we should expect they *won't* be in the future (Hendry 2001). Secondly, philosophical arguments are not empirical studies. I believe in the power of the philosopher's armchair, but I don't think it can reveal broad statistical facts about how scientists with identifiable philosophical outlooks tend to behave, and that's what I want to investigate. As Hendry makes the point: "a priori arguments alone should not convince us" (ibid., S36). So, thirdly and most importantly, we'll never know whether philosophical outlooks have predictable effects on people's behavior unless we go looking. Perhaps we'll find that no patterns can be identified, and no "best practices" guides can be written, but we may well find the opposite. Thus, investigating the practical consequences of adopting a realist or an anti-realist outlook on science—which I prepare for, begin, and promote here—is not a waste of time, practically speaking, given the practical character of the arguments that may result.

The present chapter deals exclusively with the traditional scientific realism debate, understood as the attempt to determine which position every rational person should adopt simply because it is true, correct, or rationally preferable *per se*. The chapter that follows this one will discuss the idea of philosophical progress in general, explicate the framework of "meta-epistemological voluntarism" that is often used to help make sense of the stalemate, and argue that progress in this debate may still be possible if we focus on practical arguments for adopting realism or anti-realism. The third chapter deals with matters of methodology, producing a framework through which we can conduct the kind of empirical investigations needed to ground such practical arguments. The final chapter puts that framework to work, showing how studying the history of science could support practical arguments for adopting scientific realism in specific contexts of scientific practice and (perhaps surprisingly to some) anti-realist empiricism in other contexts. I begin by explicating the differences between axiological and epistemological types of scientific realism, the arguments typically presented in their favour, their interrelations, and their anti-realist contrasts. This will show why Fine is right about this much: philosophical arguments seem unable to establish one position as true or rationally preferable *per se*.

2) Two Types of Scientific Realism

Over the last several decades, scientific realism's advocates and detractors have characterized and catalogued many different varieties of scientific realism to either address or press various issues facing the basic position.[5] Since their initial formulation many of these varieties have undergone even further refinement, often speciating into a range of even more fine-grained subtypes, full of nuance and contrast in the hopes of accounting for all the various philosophical, historical, and scientific considerations involved in trying to offer a compelling philosophical account of science's aims and accomplishments.[6] Understanding these divisions is often important for precisifying the exact claims involved in any specific form of scientific realism, and therein for assessing the cogency of the various arguments and motivations offered for or against that particular position. In this chapter, however, I will not be concerned with parsing or discussing these various divisions, or people's reasons for creating them. I will focus on more general questions: what exactly is scientific realism a theory about, and what are the arguments for and against it?

Despite all the variation amongst types and subtypes of scientific realism, there are two general categories that any philosophical thesis going by the name "scientific realism" will usually fall into. On the one hand, there are epistemological theories about what we can (and by implication do) know on the basis of scientific inquiry. Most types of scientific realism fall into this category. As theories about the potential (and actual) knowledge provided by scientific methods of investigation, these theories serve as proposed answers to epistemological questions such as "what can and do we know about reality through science?" A characteristically realist answer would be that we can and do know a great deal on the basis of scientific inquiry, not just about observable phenomena but also about their modal features and unobservable causes, because the evidence shows that the theories of modern science are in large part *true*. Taking a

[5] To name but a few, there is convergent realism (attributed by Laudan (1981, p.20) to Putnam, Boyd, and Newton-Smith), structural realism (French and Ladyman 2011), entity realism (associated with Hacking 1983), semirealism (Chakravartty 2007), deployment realism (Psillos 1999), naturalised realism (Ruttkamp-Bloem 2013), deflationary realism (Peters 2012), methodological realism (Wray 2015), exemplar realism (Saatsi 2015), and whig realism (Solomon 2001).

[6] Structural realism, for instance, now comes in ontic, epistemic, and eliminative ontic versions, with a variety of different strategies for motivating or denying each one (French and Ladyman 2011).

position on such epistemological issues allows us to answer more specific questions about what we know through particular scientific theories such as "what can and do we know on the basis of this particular scientific theory, given the evidence we have for it?" The realist might say, for example, that given the evidence we have for General Relativity we can now confidently affirm that we understand the *cause* of gravitational phenomena, for (unlike Newtonian Mechanics) Einstein's theory adequately *explains* why massive bodies all move towards each other. To be an "epistemological scientific realist," then, is to believe that science can (and likely does) provide us with true knowledge about reality; we know more than how phenomena appear to observers and their scientific instruments, we know *why* they appear that way.

On the other hand, "scientific realism" sometimes refers to a type of axiological thesis, that is, a theory about the aims governing scientific inquiry, a proposed answer to axiological questions such as "what is the defining aim of science?" More specifically, an axiological approach to understanding science tries to determine a governing telos, end-in-view, or criterion of success for science that allows us to understand it as an eminently rational activity aimed towards a specific goal. A characteristically realist answer would be that science aims to uncover the true nature of reality, not just in its actual and observable aspects but also in its modal and unobservable aspects, and to do so in a way that permits the explanation of known phenomena in terms of their true causes. Taking a position on such axiological issues allows us to answer more specific questions about the rationale or motivations for certain aspects of scientific practice. One might rationalize theory testing, for example, by saying that it helps determine if a theory is a good explanation of some phenomenon, and therefore likely true, by making sure it's consistent with the observable facts. To be an "axiological scientific realist," then, is to interpret science as an activity directed towards truthfully depicting reality, in a manner capable of explaining our observations.

Epistemological and axiological versions of scientific realism are regularly espoused together.[7] This is understandable, for there is some overlap between them. For example, they share an understanding of what it means for a scientific claim to be acceptable: accepting a given

[7] Sankey (2001), for example, carefully distinguishes the two views but defends them both; Psillos (2000a), by contrast, develops an argument for axiological realism but ties the views together so intimately that at times it seems as if he's defending both.

scientific theory (e.g. General Relativity), class of entity (e.g. electrons), or set of structural relations (e.g. those described by Maxwell's equations) means to believe in the truth of that theory, the existence of such entities, or the reality of the modal relations between properties described in those structures. But it's easy to conflate these two types of scientific realism because, as I will argue, the main argument for epistemological scientific realism uses axiological realism as a premise. But strictly speaking they can be maintained independently of each other, such that someone might consistently accept one but not the other.

It would not be inconsistent for someone to maintain, for example, that science aims for true and explanatory theories but that it is impossible to ever actually achieve this aim, i.e. one could affirm axiological scientific realism but deny epistemological scientific realism. Inevitably failing to achieve their primary objective is a familiar outcome for human endeavours, such as when a law enforcement agency aims but inevitably fails to fully enforce the law, and science can be understood similarly (e.g. Lyons 2005, 2012). Conversely, it would not be inconsistent to claim that the aim of science is something less than or different than true, explanatory theories, but that true and explanatory theories are nevertheless regularly produced by the pursuit of those aims. This is also a familiar outcome for human endeavours, such as when a corporation's efforts to increase net profits leads it to reduce its waste and thereby its environmental impact, without the organization ever specifically aiming to "go green." While I am unaware of anyone defending such a view in print, it is at least conceivable that someone could view science similarly without contradiction. This would mean accepting some form of epistemological scientific realism without accepting any form of axiological scientific realism: science, it would be said, gives us a true picture of the nature of reality and the causes of various phenomena, though painting that picture is not its governing end-in-view.

With a rough picture of these two forms of scientific realism in place, I now turn to a more detailed characterization of epistemological scientific realism and the arguments that have been offered for and against it. Following that I outline its anti-realist contrasts, focusing on constructive empiricism, and show why the realist's arguments for their position are not compelling to the anti-realist. I then show how the main argument for epistemological scientific realism tends to assume axiological scientific realism, and then proceed to offer a more detailed characterization of the latter position, its anti-realist contrasts, and the arguments for and against it in subsequent sections.

<u>3) Epistemological Scientific Realism</u>

By far the most common form of scientific realism is epistemological scientific realism. Its general spirit is "the view that scientific theories correctly describe the nature of a mind-independent world" (Chakravartty 2007, p.4). To best understand this version of scientific realism, a tripartite division found throughout the literature is quite useful. According to realists such as Chakravartty (2007, p.9) and Psillos (1999), scientific realism is constituted by something like a conjunction of the following three theses:

> **Metaphysical Thesis**: There exists a mind-independent world. This world is populated not only by many of the observable entities, processes, and relations of everyday experience, but also perhaps by the unobservable entities, processes, and relations posited by scientific theories.
>
> **Semantic Thesis**: Scientific claims, descriptions, or representations of the way the mind-independent world is have truth values, when the appropriate portions of them are construed in literal terms. Whether these claims refer to observable or unobservable objects, their truth should be understood in terms of the real existence of those objects, as described.
>
> **Epistemic Thesis**: Under certain conditions we are warranted in accepting that specific scientific claims, descriptions, or representations regarding the mind-independent world are approximately true, even regarding the unobservable aspects of our world, i.e. under certain conditions we should believe in the existence of unobservable entities, processes, or relations posited by scientific theories.

This tripartite characterization of epistemological scientific realism is most useful in understanding the various forms of anti-realism that oppose it, for each of these three components has specific anti-realist contrasts, as discussed in the next section. As stated here it already incorporates several qualifications that have proven extremely important in the defence of scientific realism against various anti-realist challenges (e.g. regarding the approximate nature of scientific truth, the impossibility of interpreting some parts of theories literally, etc.). It's also likely to prove generally satisfactory for characterizing most subtypes of epistemological scientific realism, for together with the claim that the relevant epistemic conditions obtain (e.g. we have sufficient evidence for our current best theories), this tripartite characterization captures the general realist contention: that much of modern science gives us knowledge of both the observable *and unobservabl*e aspects of our world.

Let's address the main argument against this position first, which goes as follows: if people in the past had been realists, and accepted that their current best science was true, they would have been wrong. Newtonians, for example, sometimes portrayed Newton's theory as the final word in mechanics and cosmology, an image drawn from the mind of God even. But it turns out that some of Newton's conceptions of fundamental things like mass, time, and space prove entirely unworkable when applied universally. That is, we now know his theory is false, in fundamental ways. So, unless we can show that modern science is somehow on a more secure epistemic footing than theories like Newton's were, and that the word of current scientists on matters of ontology is therefore more reliable than the word of those who advocated for the truth of past (but certainly false) theories, we should believe (contra realism) that modern science does not give us knowledge about unobservable reality, as its theories are likely radically false. But since the realist's argument for their claim that the theories of modern science are approximately true (discussed below), if it were applied in the past, would have led people to believe in the truth of theories we now know to be deeply flawed, employing that argument seems to be an unreliable method of belief formation. That is, a realist attitude towards our current best theories seems to be on no more secure footing than a realist attitude would have been towards the best theories of previous eras, which evidently would not have been on a very secure footing.

This argument has a lengthy pedigree, going back at least to Leo Tolstoy (1904) and Henri Poincaré (1900) in the early 20th century, but its most forceful statement in recent memory comes from Larry Laudan (1981). Today it's known as the "Pessimistic Meta-Induction," or PI for short, for it seems to take the form of a higher-level inductive inference about the reliability of our lower-level inductive inferences: even the most widely accepted theories, no matter how well established or successful they seem, have turned out to be false, so by induction we should expect our current theories to be false as well.[8] Do we not, then, have inductive evidence that accepting the truth of contemporary scientific theories is an unreliable method of belief formation?

By weakening and qualifying the basic idea that successful scientific theories are likely true, realists have been able to turn this argument on its head, using the same inductive form to defend a kind of epistemic optimism.[9] For scientists were certainly wrong about many things, but they've also been *right* about many things. In a move made most explicitly by Kitcher (1993) and Psillos (1999), the reliability of realist methods of belief formation is defended by first dividing scientific theories into smaller parts. Non-*ad hoc* criteria for which parts can be properly judged as true (and therefore belief-worthy), given certain kinds of evidence, are then proffered. The key to this strategy's success is making sure that, from their own perspective, historical actors would have been able to use such criteria to pick out those portions that are not today considered false. In that case, the realist's proposed belief-forming processes could be judged as reliable, rather than unreliable as the PI suggests.[10]

At least in their own judgement, several realists have developed reliable strategies for identifying which portions of scientific theories are likely to be retained in future theories and which are not (e.g. Psillos 1999, Chakravartty 2007). In doing so, they contend, the PI can be rebutted by a suitably sophisticated form of epistemological scientific realism, one whose prescriptions for belief seem reliable given the history of science. For if past realists had formed their commitments using the rules of belief-formation provided by such sophisticated forms of realism, history would have validated rather than discredited their commitments. The way that various brands of epistemological scientific realism determine which portions of a scientific

---

[8] For an account of the PI as a deductive argument, taking the form of a "meta-modus tollens," see Lyons (2002, 2003, and 2013).

[9] Most contemporary realists tend to argue that only *mature* theories should be considered as candidates for realist commitment, where maturity is often understood as a capacity to achieve certain forms like predicting novel phenomena (e.g. Musgrave 1988, Psillos 1999). This allows realists to weaken the inductive strength of the PI by arguing that many of the false but previously accepted theories cited by anti-realists were, in fact, never successful enough to warrant consideration for realist commitment.

[10] For an excellent discussion of this strategy, and an attempt to address some of the challenges faced by it, see Votsis (2011); for an argument that rebutting the PI in this manner may be much easier than many realists have understood it to be, see Vickers (2016). For an argument that the strategy only allows for a modest form of realism that may not satisfy many would-be realists, see Harker (2013)

theory are belief-worthy is borne out in part by how they explicate qualification terms like "portion," "part," "approximate," "appropriate," etc., but the key point is that these strategies seem workable to many would-be realists. Anti-realists may attack realism through a pessimistic view of scientific change, and judge their criticisms to be devastating, but many realists believe they have ways of supporting their continued optimism.[11]

Arguments based on scientific change are not the only anti-realist challenges faced by the aspirant epistemological scientific realist. For example, realism can be challenged by noting that, apparently as a matter of pure logic, any successful theory has at least one (or infinitely many) ontologically distinct yet empirically equivalent alternatives, regardless of whether they've been formulated (Earman 1993). The evidence will always underdetermine our choice between such rival theories, so it seems like we can never determine which of them is true. Realists tend to think they can and have successfully responded to such challenges (e.g. Leplin 1997), and while some anti-realists agree that such challenges are specious (Laudan 1990, cf. Laudan and Leplin 1991) other anti-realists disagree (Douven 2000).

P. Kyle Stanford has emerged as realism's foremost detractor in recent times, and has forcefully pressed similar arguments, along with several others. The basic problem is that scientific theories can only be empirically evaluated by comparing them to relevant competitors. But scientists often fail to compare theories they're evaluating against relevant competitors, either because they fail to conceive of such alternatives (Stanford 2006), because social factors influencing scientific practice incentivize them not to do so (Stanford 2015), or for more mysterious reasons that allow them to deem certain competitors irrelevant. So, when scientists conclude that some theory is best supported by the empirical evidence, and realists interpret this as evidence that the theory is true, the reasoning begins to seem based on gerrymandering,

[11] Chakravartty's notion of "detection properties" (2004, 2007), for instance, seems capable of discriminating between properties that have been retained even through high-level theory changes, so these properties can count as belief-worthy portions of successful scientific theories, serving as fodder for an optimistic induction on the history of science capable of rebutting the PI. Worrall (1989; cf. 1994) argues that a distinctly structural form of scientific realism might offer us the "best of both worlds," limiting our commitments in a way that helps avoid the PI yet allowing us to remain (reasonably) optimistic about the truth of our current best theories.

and the conclusion therefore rather suspect, given that many rivals theories were eliminated from consideration on non-empirical grounds.[12]

While such arguments certainly call into question the ability of scientific methods of theory evaluation to pick out the true theory amongst all the empirically viable options, realists tend not to be too concerned about them. First, realists are usually not engaged in a normative analysis of scientific methods which might judge, criticize, or improve the way that scientists evaluate their theories. Implicit within realism is the assumption that science is a rational enterprise, so if scientists habitually refuse to consider all alternatives, realists assume they must have a good reason, even if no one can explain what that reason it is. Second, these issues of underdetermination and unconsidered alternatives are, formally speaking, species of the problem of induction, which challenges the reliability of all forms of ampliative inference. As such, the challenge is not so much to realism as it is to scientific inference itself, whether it's interpreted according to realism or (most forms of) anti-realism. So, given how generic they are, responding to such challenges has simply not been a priority for realists (Chakravartty 2007, p.28). They're happy to ignore such challenges, I suspect, because it's a problem for any view that assumes science is a rational enterprise, an assumption that many of their anti-realist opponents share.[13]

The main argument in favour of epistemological scientific realism is the "No-Miracles Argument," or NMA for short. The name derives from a remark by Hilary Putnam, who said that scientific realism "is the only philosophy that doesn't make the success of science a miracle" (1975a, p.73). This, he said, is the "positive" argument for scientific realism (ibid., p.72), where a negative argument for some position is based in the failures of its potential alternatives. The negative argument for scientific realism, accordingly, simply says that alternative philosophies of science (such as logical positivism, realism's main rival when Putnam was writing) have all

[12] See Lyons (2009) for more on the issue of how the existence of competing scientific theories bears on the coherence of scientific realism; see Lyons (2013) for an analysis of the relation between underdetermination arguments and the PI.

[13] On the issue of why an anti-realist should permit some ampliative inference, but need not permit as much as the realist, see van Fraassen (1980, p.73) and Monton and van Fraassen (2003, p.407); for arguments against this strategy, see Rosen (1994) and Alspector-Kelly (2001)

proven unworkable for one reason or another, leaving realism as the only live option. We can render the negative argument for scientific realism as a premised argument quite simply:

P1: No non-realist account of science is workable.
P2: A realist account of science is workable.
C1: A workable account of science will be realist.

The first premise would be justified by going through all of realism's competitors and identifying their problems. The second premise would be justified by showing that realism does not share those problems. This conclusion just says that realism is the only game in town.

At the time Putnam was writing the negative argument certainly seemed sound, and since most people are only willing to accept philosophical accounts that seem workable, it rightly led to the widespread acceptance of scientific realism. The logical empiricists, operationalists, and pragmatists alike attempted to interpret the semantics of many scientific claims (e.g. about unobservables) non-literally, but this proved unworkable. But realists prefer to interpret all claims literally, such that claims about unobservables like electrons or microbes are semantically no different than ordinary claims about observables like tables and chairs. This approach seems comparatively unproblematic, leaving realism as the only viable philosophical interpretation of science. While none of the anti-realist philosophies of science on offer when Putnam was writing seemed viable, as we will see this is no longer the case. Constructive empiricism shares the realist's literal interpretation of scientific claims, and many see it as a workable anti-realist competitor to scientific realism. Today, the negative argument is no longer sound, and scientific realism is not the only game in town.

A positive argument, by contrast, is supposed to give a reason to directly infer a given position, without consideration of the workability of rival positions. At first glance, the NMA may appear to be a negative argument. It begins by noting that modern science has been incredibly successful, e.g. in the development of new technologies, the explanation of our observations, or the prediction of novel and unexpected phenomena. It then claims that the approximate truth of the theories involved in those successes—which is tantamount to the epistemological scientific realist's thesis with respect to those theories—is the *only* explanation of their success. "How could General Relativity predict time dilation—which had never been observed before it was sought out to test the theory—unless it were true? Some sort of miraculous coincidence, as any non-realist would have us believe?!", the realist asks. The question is rhetorical for she feels she already has the answer: a miraculous coincidence on a cosmic scale is not even an explanation; realism is the only explanation.

At this point the realist must appeal to a rule or principle of abductive inference known as "inference to the best explanation," or IBE.[14] IBE tells us to "infer what would, if true, provide the best explanation of [the] evidence" (Lipton 1991, p. 1). So according to this principle, a given claim's being the best available explanation of our observations is a reason to infer it. As the realist understands it this is because explanatory power carries epistemic weight; if some claim displays superior explanatory power, this is, *ceteris paribus*, evidence that it is true (Hausman 1982, Musgrave 1985, Clendinnen 1989). As we will see, anti-realists often interpret IBE differently (if they accept it at all), such that explanatory power is not evidence of truth. Thus, accepting IBE does not imply that explanatory power and truth are correlated, that's just how the realist understands it. IBE is indifferent to realism or anti-realism (see Chakravartty 2011b, sec. 3.2).

To capture the realist's understanding that a claim's explanatory superiority over its rivals is evidence of its likely truth, and help make sense of IBE's role in the realist's NMA, we can state IBE as a conditional statement that can be used as a premise in an argument:

> IBE(R): If some claim x is the best available explanation of some phenomenon that needs to be explained, x is likely true.

The NMA is a positive argument because it assumes IBE is reliable, and a canonical rule of rational inference as the realist interprets it, and that IBE(R) is therefore true. This is in no way

[14] Gilbert Harman (1965) was the first to use this terminology, but he claimed that the pattern of inference it refers to had long been widely accepted within science and philosophy, stretching back (at least) to Descartes's *Discourse on Method* (1637). Harman, like many who came after him, and (at least implicitly) many who came before him, ascribed to this practice a foundational role in our epistemology, going so far as to assert that all non-deductive inference is just inference to the best explanation. For an extensive treatment of IBE, see Lipton (1991).

assumed by a negative argument for scientific realism. On these assumptions, we can render the NMA as follows:

> P1: Scientific theory *t* has been very successful (e.g. in making novel predictions), and that success is a phenomenon that needs to be explained.
>
> P2: The best available explanation of *t*'s success is that *t* provides us with knowledge about some specific aspects of the mind-independent world (e.g. regarding the existence of certain unobservable entities, causal processes, or modal relations).
>
> C: It is likely true that *t* provides us with knowledge about some specific aspects of the mind-independent world (e.g. regarding the existence of certain unobservable entities, causal processes, or modal relations). (by IBE(R))

The conclusion here—which is basically that theory *t* is likely true—is precisely the attitude that an epistemological scientific realist would take towards *t* if they felt warranted in accepting it. Nowhere is it claimed that anti-realist philosophies are unworkable, just that anything other than realism would be an impoverished explanation of *t*'s success, and therefore less likely to be true. By arguing that realism offers the best explanation of a scientific theory's success, and that realism is therefore likely true, NMA-type arguments apply IBE in a way that necessarily treats explanatory power as evidence of truth.

There are two challenges that may be immediately brought against the NMA. One might argue that realism doesn't offer the best explanation, never mind the *only* explanation, as has been done for example in van Fraassen (1980, Ch.2), Wray (2007), and Mizrahi (2012). But one might also wonder why anyone should think that IBE(R) is true in the first place, or that IBE is reliable and a canonical rule of rational inference. One might find it intuitive and obvious that the best explanation is likely true, and be inclined to argue for the truth of IBE(R) as follows: if a theory is true, it will be the best explanation, so when a theory seems like the best explanation, it is likely true. But even with the qualifiers "seems like" and "likely" this is, at best, a qualified instance of the logical fallacy known as affirming the consequent. So, intuitive or otherwise, there's reason to be suspicious that explanatory strength should be counted as a kind of evidence.

There are further reasons to worry. Even if IBE can reliably help us select the true theory from amongst a pool of competitors, we have no guarantee that the true theory is in that pool; we may just be choosing the best of a bad lot (van Fraassen 1989, p.143). Perhaps even worse, revising our beliefs using IBE seems to make us assign probabilities in non-standard ways, opening us up to being "Dutch-booked," i.e. it makes it possible for someone to place bets against us that cannot fail to win (ibid., p.161-170; cf. Day and Kincaid 1994, p.273-4). Additional worries abound given the absence of any consensus regarding what makes something an explanation, never mind what makes one the "best" (Chakravartty 2011b, sec.3.2). Given all these difficulties, it's not unreasonable to ask the realist who justifies their position by using it in the NMA to first justify IBE itself (and their interpretation of it).

Especially amongst realists, IBE tends to be justified naturalistically (cf. Ganson 2001). We all seem to use IBE quite regularly in everyday life, and philosophers seem to make regular use of it in defending their views (Day and Kincaid 1994, p.272-3), but perhaps more importantly scientists seem to employ it all the time, particularly when they evaluate competing scientific theories. In fact, one might argue that scientists *need* to use IBE, e.g. to choose between empirically equivalent rivals. So, assuming science is an eminently rational enterprise, and scientists make regular or central use of IBE, it must be a rational inference rule. Thus, we can (and should) apply it in other contexts, on naturalistic grounds. At least, that's how IBE is often defended as a rational rule of inference.

Feeling they're justified in using IBE, the realist looks to evaluate competing philosophies of science according to their ability to explain a scientific theory's surprising successes (e.g. its ability to make accurate predictions), treating the success of a scientific theory as itself an observable phenomenon that calls out for explanation. The NMA's support of realism is based on the judgement that realism provides the best explanation. The conclusion that realism is correct, the realist will stress, is reached not only by using a method of inference supposedly accredited by scientific methods of inference (i.e. IBE), but also by employing scientific standards of relative explanatory power. For even in the absence of any consensus regarding what exactly makes one explanation better than another, the "No-Miracles Argument" gets its name from the realist's contention that the only explanation of a theory's predictive success besides its truth is some sort of miraculous cosmic coincidence, where a false theory makes true predictions. But since science clearly does not count miracles as good explanations, the epistemological scientific realist's claim that successful theories are likely true is "the only scientific explanation of the success of science, and hence part of any adequate scientific description of science and its relation to its objects" (Putnam, 1975a, p.73). In this way, the question of whether scientific methods can produce knowledge of the unobservable nature of reality has been answered affirmatively, and apparently *on scientific grounds*. Thus, the NMA purports to give us a positive reason to infer epistemological scientific realism directly: it's the conclusion we're led to by following scientific methods of theory evaluation and standards of explanatory power.[15]

I return to IBE's status as a rule of rational inference in section five, but it's importance for the epistemological scientific realist should be emphasized, as I am unaware of any argument for their position that is not some form of NMA. Ernan McMullin's arguments (1984, 1985), for instance, look at the way that scientists successfully work with idealizations, but they still ask us to *explain* that success, and defends realism using the same 'realists are just using scientific rules of abductive inference to reach philosophical conclusions' move that most NMA-type, IBE-based argument do (1984, p.16; cf. 1985, p.262, 264). Other arguments for epistemological scientific realism appeal to theoretical or instrumental consilience (e.g. Hacking 1983, Salmon 1984, Franklin 1986). Many of our most predictive theories gained their unique predictive power by positing certain unobservable entities, they note, which allowed the unification of previously disparate theories into a single theory; furthermore, those entities can now be detected by a variety of instrumental methods that use very different physical processes. But the inference to realism from such robust and surprising consilience is still based in the idea that realism is the only *explanation* of it, i.e. that it would be a miracle if those entities didn't exist, given those successes. As such, arguments for realism that focus on consilience are just a specific type of NMA. There is also a class of arguments often called "methodological," "explanationist," or "vindicationist" that asks us to explain the presence in scientific practice of certain methods that seem to depend on realist assumptions (Boyd 1981, 1984; cf. Fine 1986a, Ch.7, Hendry 1995). The argument is that, if certain scientific *methods* are based on realist

[15] See Fine (1986a, ch.7) for an argument that any argument for realism about science would need to satisfy more stringent standards of demonstration than those used in science to convince the open-minded non-believer, just as a set-theoretic proof of set-theory's consistency could not convince anyone unconvinced of its consistency.

assumptions, the only explanation of their success is that those assumptions are true. Here again we have a kind of NMA, and the same can be said for any attempt to ground realism by appealing to the way that our theories allow us to manipulate reality (Hacking 1983; Aronson, Harré, and Way 1995, Ch.9), produce accurate novel predictions (Musgrave 1988), or any other kind of success. I submit that any positive argument for epistemological scientific realism will be an NMA, and thus will assume the rationality and reliability of IBE, understood as an injunction to treat explanatory power as evidence of truth.[16] So, unless the epistemological scientific realist can justify IBE and their interpretation of it, they will have no compelling positive argument for their position.

It should be noted that not all realists uphold the rationality and reliability of IBE, and accordingly not all realists support their position through NMA-type arguments. Chakravartty, for instance, acknowledges that in his judgement, even as a scientific realist, "[a]s arguments go, the miracle argument is surprisingly poor, all things considered" (2007, p.24; cf. Fitzpatrick 2013, Achinstein 2002). The NMA is indeed a poor argument, as we'll see in the next two sections, but the lack of a positive argument for realism is of little concern for Chakravartty, as he is more concerned with developing a metaphysical framework for understanding realism's ontological claims than offering arguments for accepting them (Chakravartty 2007, p.xi). As we'll see, many anti-realists (van Fraassen most of all) find the NMA unconvincing because they deny that scientists need to make use of IBE, but first we should get clear about the anti-realist contrasts to epistemological scientific realism, in particular van Fraassen's constructive empiricism.

4) Anti-Realist Contrasts to Epistemological Scientific Realism

I've identified the type of scientific realism I've been discussing as "epistemological" because the epistemic component is more important than the metaphysical and semantic components in this sense: few modern philosophers of science see any anti-realist alternative to the realist's

[16] This includes "retail" rather than "wholesale" arguments for realism, as discussed in Fitzpatrick (2013), Achinstein (2002), Saatsi (2015), and Magnus and Calendar (2004).

metaphysical and semantic theses as viable options (Psillos 2000b). Few if any modern metaphysicians or philosophers of science are idealists, for example, who oppose realism's metaphysical thesis by maintaining that all reality is mind-dependent. Many fine-grained judgments about what counts as an appropriate metaphysical framework may vary from philosopher to philosopher, but today most will uphold some form of metaphysical realism. Additionally, most are happy to understand scientific claims literally, and see scientific theories as assertoric (i.e. truth-apt) statements about what is or is not the case.[17] Efforts to systematically interpret talk of unobservables and counterfactuals non-literally—e.g. as a kind of systematic shorthand for talk about observables and actuals—are generally seen as unworkable these days, even by anti-realist lights, so most philosophers adhere by default to a literal semantics for scientific theories (see van Fraassen 1980, p.3-4, 10; cf. Chakravartty 2007, Ch.1). And while social constructivism plays an important methodological role in the social, anthropological, and historical study of science—e.g. by interpreting scientific claims as reflecting social and political interests rather than as literal descriptions of reality—few philosophers of science concerned with issues involving scientific realism see anti-realist constructivism as a viable option.[18]

These are the sorts of considerations that fuel the negative argument for epistemological scientific realism that Putnam (1975a) referred to: there does not seem to be a viable or appealing alternative to a realistic interpretation of the sciences, at least when it comes to metaphysics and semantics. But at least one anti-realist position accepts both the realist's semantic and metaphysical theses, but rejects their epistemic thesis: constructive empiricism. First presented by van Fraassen in *The Scientific Image* (1980), this position is taken very seriously by many would-be scientific realists as an important rival that needs to be forcefully rebutted. Constructive empiricists, as it were, are the non-believers that epistemological scientific realists would be most interested in converting, and as such, constructive empiricism has become something of a standard anti-realist foil for defences of epistemological forms of scientific realism. Constructive empiricism does specify an axiological component as well, but its dispute

[17] Contra, for example, Dummett (1978); cf. Putnam (1975a). See footnote 20 below for further clarification on this matter.

[18] While some would-be realists take social constructivism seriously (e.g. Kukla 2000 and Giere 2006), most see it as a fatally flawed metaphysical and semantic framework for understanding science. This makes sense of why many extensive treatments of scientific realism neglect issues concerning social constructivism entirely (e.g. Chakravartty 2007, Psillos 1999).

with the type of realism discussed so far is expressly a disagreement over the epistemology of science, so it seems fair to call this form of scientific realism the "epistemological" version.[19]

For clarity, the realist's epistemic thesis contrasts with any significant rejection of the idea that science can or does produce knowledge about things like unobservables, causal processes, and modal relations. People sometimes reject this idea for political, spiritual, or even practical reasons. Some forms of social constructivism, for example, include denials of realism's metaphysical and semantic theses (Kukla 2000), but they all reject the epistemic thesis, and many seem to do this for blatantly political reasons (Hacking 1999). Perhaps they see scientific realism as a doctrinaire component of a political apparatus that maintains systems of social injustice, helping to hegemonically enforce the illegitimate intellectual authority of science. Such people may oppose scientific realism in the hopes of disrupting the political status quo in progressive ways, upending the unjust epistemic privilege that science is afforded in our society (partially) through realism's widespread acceptance. Similarly, many individuals and organizations reject realist interpretations of modern science—e.g. about the age of the Earth, or the origin of species—because these claims directly contradict central tenets of their spiritual faith. What's so interesting about constructive empiricism for the epistemological scientific realist is that its opposition seems motivated primarily by epistemological, philosophical considerations. Unless otherwise stated, from here on out I confine my discussion to the conflict between scientific realists and constructive empiricists, for simplicity's sake.

At base, constructive empiricism is built on the idea that the results and methods of science can be fully accepted and made sense of without thinking that its theories are true with respect to unobservables or counterfactuals (van Fraassen 2004, 2005b). The idea is just that, to accept science as an exemplar of rationality, one only needs to think that it produces knowledge about observable, actual things—such as trees, tables, torsion balances, and galvanometer displays—not knowledge about unobservable, non-actual things—such as atoms, causal pathways, and what would have happened if things had been different. Thus, the most we need to say about

[19] In fact, constructive empiricism is primarily concerned with the axiology of science. If we assume that science has been successful, however, certain epistemological conclusions follow from constructive empiricism's axiological thesis in a manner similar to how epistemological conclusions follow from a realist axiology under the same assumption.

what makes for an acceptable scientific theory, according to the constructive empiricist, is that it "saves the phenomena," i.e. that it is empirically adequate. Assuming we want to see science as an exemplar of rational, empirical inquiry, and its theories as significant and reliable, seeing acceptable and accepted scientific theories as empirically adequate is all that's required (van Fraassen 2004; 2005b, p.112).

To say that a theory is empirically adequate, according to van Fraassen's original explication of the concept, is to say that it counts amongst its models "at least one model that all the actual phenomena fit inside" (1980, p.12; cf. Ch.3), i.e. it can accurately represent all the observable, actual facts. This implies nothing about whether it gets the unobservable or modal facts correct. An empirically adequate theory or model *may* get the unobservable or modal facts right, but it may also get them wrong; to say a theory or model is empirically adequate is to remain agnostic about such matters. So, according to constructive empiricism an acceptable scientific theory is true with respect to the observable and actual facts, regardless of whether it's true with respect to anything else.

In contrast to the realist's epistemic thesis offered above, we can give something like the following as a description of constructive empiricism's epistemic thesis:

> **Constructive Empiricism's Epistemic Thesis**: Under certain conditions we are warranted in accepting that specific scientific claims, descriptions, or representations regarding the mind-independent world are empirically adequate, i.e. under certain conditions we should believe that they accurately represent the actual and observable aspects of the mind-independent world, but not necessarily its unobservable or modal aspects.

This understanding of what science can (and potentially does) achieve is, roughly, the attitude towards theory acceptance recommended by Osiander in his preface to Copernicus's *De Revolutionibus* (1543), where to be acceptable for scientific purposes:

> hypotheses need not be true nor even probable. On the contrary, if they provide a calculus consistent with the observations, that alone is enough [...] the astronomer will take as his first choice that hypothesis which is the easiest to grasp. The philosopher will perhaps rather seek the semblance of truth. (preface)

A similar if more phenomenologically focused kind of anti-realist empiricism was recommended by Ernst Mach, at least until his later years, with respect to the atomic hypothesis. Similar positions were maintained by the French positivists and British empiricists that preceded him, as well as the logical positivists who succeeded him. Generally speaking, an anti-realist *empiricist* epistemology of science maintains that, as a rational effort to theoretically represent the world, science need uncover nothing more than the regular connections between observable objects, as they are revealed to our epistemic community's perceptual apparatuses. Constructive empiricism is but the latest in a long line of anti-realist empiricist philosophies, and is currently the most viable way to interpret the methods of science as rational, and its results as well established, without suggesting they have revealed the true nature of reality.

Van Fraassen has consistently described himself as an "aspirant" empiricist philosopher of science (1980, p.2; 1991, p.483, n.5; 2002, p.xii), aiming to make anti-realist empiricism acceptable again. As he puts it, his "own view is that empiricism is correct, but could not live in the linguistic form the positivists gave it" (1980, p.3). He describes his entire project as a rehabilitation of anti-realist empiricism in the modern philosophical milieu, following the fall of logical positivism:

> Logical Positivism […] even if one is quite charitable about what counts as a development rather than a change of position, had a rather spectacular crash. So, let us forget these labels which never do more than impose a momentary order on the shifting sands of philosophical fortune, and let us see what problems are faced by an aspirant empiricist today. What sort of philosophical account is possible of the aim and structure of science? (1980, p.2)

The anti-realist empiricist account of science that van Fraassen develops is this: there is a mind-independent, external world (i.e. metaphysical realism), and scientific claims are attempts to literally describe that world (i.e. semantic realism). This commitment to literally interpreting scientific claims extends to all the cherished claims of realists regarding the existence of certain unobservables and causal processes, and potentially the truth of modal facts as well.[20] The

[20] Although van Fraassen denies that counterfactuals have truth values (1980, p. 13), Kukla (1995, p.431-2) notes that van Fraassen is not exactly committed to the realist's semantic thesis, as I here suggest he is for simplicity's sake. Van Fraassen explains that, when developing his anti-realist account of science's axiology and epistemology in *The Scientific Image* (1980), he chose to "set aside [semantic] issues, at that point, for the sake of keeping the debate over scientific realism undistracted" (Ladyman et al. 2011, p.434). His prior (1967, 1969, and 1977) and subsequent (1997, 2004, 2006b) work on semantics develops a pragmatic approach, rather than a correspondence

constructive empiricist, thus, has no bone to pick with the realist when it comes to their basic metaphysical and semantic commitments, only with the epistemic thesis that, under the right conditions, science can provide evidence that some theory states the truth about unobservables or modal facts. Exhaustive experimental testing could show that a theory gets all the observable, actual facts correct, certainly, and this might convince us that a theory is empirically adequate, i.e. that it "saves the phenomena" in the sense discussed above. But what empirical evidence could we ever have that the theory gets the unobservable or modal facts right when, *qua* human beings with limited perceptual capacities, we can only ever measure our theories against the observable, actual facts? We cannot perceive electrons, for example; we can only *infer* their existence from our observations, e.g. as the best explanation of the read-outs on our scientific instruments. Modal facts are likewise beyond the reach of empirical evidence, for our observations and experimental tests are necessarily constrained to actual situations. Any kind of inference from the observable, actual facts to unobservable, modal facts, via IBE, is epistemologically suspect for an empiricist like van Fraassen. He sees the empirical content of a theory—that is, the stuff we can have evidence for—as exhausted by the assertions it makes about actual observables. There's no need to think a theory's super-empirical content—regarding unobservables, causal processes, and counterfactuals—can be reliably judged using empirical evidence, or inferred because of its explanatory power, so the anti-realist empiricist treats such judgements with a kind of skeptical agnosticism.

Realists will counter that we *can* provide super-empirical evidence for the existence of certain unobservables and the truth of certain counterfactuals: the explanatory power of the hypotheses that posit them. Constructive empiricists understand scientific theory evaluation differently, however; on their account, explanatory power is not treated as evidence of super-empirical truth. But they don't deny that scientists often appreciate theories in terms of their explanatory virtues, and often explicitly recommend acceptance of theories that display such virtues over theories that do not. That much is an undeniable empirical fact about how science is practiced and how scientists sometimes defend their own inferences and theoretical preferences. Here, for instance, is a quote from Darwin's *On the Origin of Species* where he seems to construct a kind

or literal one, which makes sense of how he seems to both accept (for the sake of argument) that counterfactual claims are to be interpreted literally and yet (ultimately) deny that they should be.

of NMA, and justifies IBE because it has been accepted and successfully applied by other scientists:

> [I]t can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified. It has recently been objected that this is an unsafe method of arguing; but it is a method used in judging of the common events of life, and has often been used by the greatest natural philosophers. (Darwin 1859, Ch.15, as cited in Okasha 2000)

Realists might see this as Darwin endorsing the use of IBE as part of proper scientific method, textual evidence that scientists take explanatory power to indicate a claim's likely truth, a rule they can help themselves to and use to support their position.[21] But as Darwin notes, people often object to its use because it seems "unsafe," i.e. unreliable. So even within scientific circles its status as a canon of rational inference is contested. And while it may be the case that "the greatest natural philosophers" have used IBE, even treating explanatory power as a mark of truth, this doesn't make it any more valid or reliable than 1500 years of using Aristotelean physics makes a vacuum impossible.

Nevertheless, the asking of "why-questions" and a concern for explanatory theories is certainly part of how science is widely practiced. Constructive empiricism "saves" this phenomenon as follows: scientists often prefer explanatorily powerful theories because explanatory power is a pragmatic virtue of theories, not an epistemic virtue, i.e. it makes a theory useful, not more likely to be true (van Fraassen 1980, Ch.4, 5). On this understanding, Darwin is a bit confused. His theory's explanatory power gives us reason to think adopting it would be useful, so he is right to promote it in this way. Explanatory theories are often easier to work with cognitively, for example, but this gives us no extra reason to think they're likely true, over and above their ability to get the empirical facts right.

This will be discussed at length in the next section, but the short story is van Fraassen portrays science as an eminently rational enterprise without seeing IBE as an essential part of its inferential toolkit. And even when scientists do accept the best explanation, because it is the

[21] For an account of IBE as a rational method of inference that does not validate its use in the NMA, see Ben-Menahem (1990).

best explanation, van Fraassen argues that we don't need to see them as inferring the truth of that explanation to understand what they're doing as within the bounds of rationality. So, from his perspective, by assuming scientists use IBE and see explanatory power as a kind of super-empirical evidence, realists are begging the question. Some realists and anti-realists certainly believe they have strong, positive arguments for their position that their opponents should recognize as compelling, but I have never found an argument for either position that is truly non-circular, i.e. that proceeds without making assumptions that the other side sees as being essentially what's at issue. What's needed is an independent reason to accept one view and reject the other, one that doesn't rely on disputed premises. Through the rest of this chapter I'll present some reasons to think this can't be done, and to accept that the debate (at least between realists and constructive empiricists) has been "argued to a stalemate" (Monton 2007, p.3). Whether this assessment is accurate, subsequent chapters will assume it is, going on to investigate how scientific realists and anti-realist empiricists might nevertheless continue to engage in substantial debate.

The main reason to think no negative argument will ever be able to establish realism or constructive empiricism as the correct account of science's epistemology is that both seem to account for everything we know about how science operates. Establishing this conclusion has, in some sense, been van Fraassen's primary aim all along. His main arguments do not seek to establish that constructive empiricism is the only way to see science as an exemplar of rational empirical inquiry, and accept the theories of modern science as well-established; he mainly argues that we don't *need* to see scientific evidence as establishing acceptable theories as true to see science as rational (2005b). In short, we don't need to be realists to see and value science as eminently rational, for constructive empiricism is a coherent anti-realist alternative that does that too. But this doesn't establish, of course, that the theories of modern science *aren't* true in approximate but significant ways with respect to unobservables, i.e. that epistemological scientific realism is false. If we accept scientific realism as a coherent and adequate philosophy of science, accepting constructive empiricism as coherent and adequate as well just means that rational consideration of the evidence underdetermines our choice between them. In that case, there can be no negative argument in favour of either position so long as the other stands as a coherent alternative.

Realists have attempted to salvage the negative argument for their position by trying to show that constructive empiricism does not, in fact, present a coherent anti-realist alternative to epistemological scientific realism. Several criticisms target constructive empiricism's way of distinguishing between observable and unobservable objects, for example. The observable things are, roughly, anything for which there are circumstances under which we'd be able to perceive them without the aid of scientific instruments (van Fraassen 1980, p.16). Paul Churchland (1985) questions whether this distinction is as clear and epistemologically relevant as van Fraassen insists it is. For instance, a distant object like Jupiter is considered an observable object while something like a microbe is not, even though both are currently only seen with the aid of optical instruments. It's possible we may travel to see Jupiter with our own eyes, true, but what if humans had been unable to control their spatial position? Natural philosophers long considered it impossible to enter the celestial sphere, but eventually we figured out how to do it; did Jupiter suddenly become observable once we became capable of space travel? In that case, suppose we learned how to shrink ourselves down so that we're small enough to observe microbes directly; would they become observable then, too? Alternatively, suppose we had evolved to have electron-microscopes for eyes in the first place, and could have seen microbes without the assistance of any instruments; would they then be observable? This distinction seems highly contingent and variable, then, which Churchland thinks casts doubt on its precision and epistemic import. Furthermore, it seems quite agent-relative, for looking at microbes through an optical microscope (which does not qualify as observing microbes) is based on the same physical processes as looking at ordinary objects through corrective lenses; is the constructive empiricist saying that someone with spectacles can't have a warranted belief in the existence of ordinary objects, simply because without looking their low-powered microscopes all they see are fuzzy patches? The point is that the distinction seems vague, arbitrary, subjective, and contingent, so constructive empiricism seems incoherent as a result.

Van Fraassen rebuts these objections by elaborating the communal nature of the observable/unobservable distinction (1985, 2005b), and by noting that a distinction is neither arbitrary because it is alterable, epistemologically irrelevant because it is vague, nor subjective because it is relative (1980, p.16). He argues that science itself tells us what is observable and unobservable: theories of human physiology tell us the limits of our community's perceptual capacities, and any in testing any theory scientists will identify certain empirical substructures in its models to represent observable states of affairs (van Fraassen 1980, p.56-9). Those substructures constitute the empirical content of that theory, and they're based on the perceptual capacities of our entire epistemic community, not one individual. Thus, what counts as observable for the visually impaired is no different from what counts as observable for anyone else. Furthermore, if we began to recognize aliens with electron-microscope eyes as part of our community, or devised a way to shrink ourselves down and see microbes directly, our sense of what's observable and unobservable would rightly shift along with our communities or scientific theories. And while many distinctions are vague, that does not make drawing them impossible or unimportant: night is not day because of dusk and dawn, sorites paradoxes do not give me a full head of hair, and using a microscope to look at paramecia is not epistemologically on par with looking at one's favourite tree through a window. So, at any given time there remains an important epistemological difference, based on the perceptual capacities of our epistemic community, between observable entities and unobservable ones: we can perceive the former, but only ever infer the latter. With such responses, the constructive empiricist contends, the coherence of their position is maintained.[22]

Let's assume the constructive empiricist is right, and their position is a coherent philosophy of science. Let's also assume that scientific realism is a coherent philosophy of science, and see where that leaves us. At the very least it means there can be no negative argument for either position. As we'll see in what follows, positive arguments don't fare any better.

5) Circular Arguments: The Constructive Empiricist's Challenge to IBE

In this section I explain something I've already indicated: the NMA is ultimately a question-begging argument, and therein unconvincing for anti-realists. Specifically, the NMA for epistemological scientific realism tends to assume axiological scientific realism— the thesis that the aim of science is to develop true theories—to justify its use of IBE. This is something the constructive empiricist denies, claiming instead that the aim of science is to develop empirically

[22] There are several further realist challenges to constructive empiricism, each of which has been rebutted to the satisfaction the position's defenders. I do not address these arguments and counterarguments here. They can be found throughout Churchland and Hooker (1985), in Monton (2017, sec. 3), and in many other places.

adequate theories. We can see why the NMA is question begging using these simple statements of the realist's and the constructive empiricist's axiological theses, which will be elaborated in the next section.

Recall that realists generally justify their use of IBE naturalistically, i.e. by claiming that it is not just regularly but centrally and necessarily used by working scientists, *qua* scientists. For the NMA to be compelling, three things would need to hold in succession: first, scientists must not just invariably but *necessarily* use IBE as a matter of fundamental scientific methodology, and do so as the realist understands the rule, treating explanatory power as evidence of truth; second, the success of science must be a phenomenon in need of explanation; third, the realist's explanation of scientific success must be the "best" available explanation. To show why the NMA does not compel him to accept realism, van Fraassen (1980, Ch.2) denies all three, showing in the process not only that the realist's claims about IBE's role in scientific inference cannot be established without begging the question, but also that even if they could be we would not be inextricably led to accept epistemological scientific realism.

A naturalistic justification of IBE, as the realist interprets the rule, can't be based on observations of scientists in action. For van Fraassen's claim is that scientists don't *need* to use this rule, even if they sometimes do. If he's right, and we can see science as a rational enterprise aimed at theoretically representing the world without seeing IBE as an essential part of that enterprise, then realists can't help themselves to the rule in arguing against the them; that would be to assume exactly what's at issue.

IBE is often defended by going even further, however, asserting that humans in general, not just scientists specifically, regularly need to infer the truth of the best explanation. This is supposed to be not just as a matter of human psychology but a requirement of evolutionary fitness: we need to treat explanatory power as evidence of truth to survive. Consider this everyday instance discussed by van Fraassen (1980):

> I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears—and I infer that a mouse has come to live with me. Not merely that these apparent signs of mousely presence will continue, not merely that all the observable phenomena will be as if there is a mouse; but that there really is a mouse. (p.19-20)

Granting that all rational people would be prone to infer the existence of a mouse from "apparent signs of mousely presence," and that people who did not would likely prove evolutionarily unfit (e.g. when the apparent signs are of a rattlesnake in the bed rather than a mouse in the wainscoting), van Fraassen nevertheless questions whether rational people are here inferring that the theory that best explains their observations is true, i.e. whether they're using IBE as the realist understands it. He suggests a rival hypothesis: "we are always willing to believe that the theory which best explains the evidence, is empirically adequate" (1980, p.20). Since mice are observables, this everyday example gets us to the same conclusion ("there's a mouse in the wainscoting") whether we use the realist's interpretation of IBE or the constructive empiricist's. The requirements of evolutionary fitness do not support either interpretation over the other, and the same goes for the requirements of scientific theory assessment.[23] In this way, the constructive empiricist need not deny that scientists or ordinary people search for explanations, and will prefer a claim if it's explanatorily powerful; she only needs to deny that IBE is treated as a truth-conductive method of inference beyond the level of the observable.

The dispute here is over the kind of value rational agents should place in explanatory power. On the realist's account, scientists search for explanations because explanatory power indicates truth. If the aim of science is to find true theories, IBE is essential to scientific theory assessment because sometimes it's the *only* way to get beyond observation in deciding between empirically equivalent rivals. If two theories are empirically indistinguishable yet mutually exclusive, for example, an assessment of their relative explanatory power breaks the tie and tells us which one is true, assuming explanatory power is evidence of truth. Thus, explanatory power *must* be treated as evidence of truth, for otherwise there's no way to achieve what the realist sees as science's central aim: the identification of true theories.[24] But the constructive empiricist thinks

[23] see the dialogue in Foss (1984), Bourgeois (1987), and Foss (1991) for some interesting discussion of this move that casts doubt on the idea that van Fraassen's rule gives the same conclusion as the realist's IBE, without showing that one or the other is the correct account of scientific or everyday inference patterns.

[24] For clarity, realists are often impressed by more specific super-empirical virtues of a theory (virtues beyond its ability to accommodate the observable, actual facts), e.g. its ability to not just save but predict the facts, its elegance, its ability to help us manipulate objects and systems of interest, its coherence with background knowledge, or its ability to unify disparate phenomena into one theory. In the absence of any narrow understanding of what makes a theory explanatory, I interpret the realist's claim that such super-empirical virtues count as a reason to accept the truth of theory as the claim that such virtues demonstrate a theory's explanatory strength: when a theory is able to help us manipulate reality, for instance, this counts as evidence of its truth for the realist because its being true would best explain that ability.

the aim of science is not developing and identifying true theories, only empirically adequate ones. So, according to them, "the value of this search for science is that the search for explanation is *ipso facto* a search for empirically adequate, empirically strong theories" (1980, p.157; cf. Ch.4). Realists see the search for explanations as sometimes the *only* way to achieve science's aims, but constructive empiricists see it as more instrumentally, for assessing a theory's explanatory strength is just *one* way to assess its empirical adequacy.

This is how a debate over the epistemology of science reduces to a debate over the axiology of science (cf. van Fraassen 2002, p.238, n.32). If the aim of science is truth (i.e. axiological scientific realism), scientific theory assessment must assume that explanatory power is evidence of truth to work towards that goal. In that case, the epistemological scientific realist's use of IBE in the NMA can be naturalistically justified, and the argument's conclusion established (i.e. epistemological scientific realism). The problem is, of course, that constructive empiricism presents an alternative axiological account of science, so the realist's account can't be assumed without begging the question. By presenting a coherent anti-realist account of the aims of science, van Fraassen subverts any attempt at a negative argument for axiological scientific realism. So, epistemological scientific realists would need a *positive* and non-question begging argument for their axiological claims to ground any naturalistic justification of IBE, validate its use in the NMA, and thereby establish epistemological scientific realism. Alternatively, they'd need to show that axiological constructive empiricism is incoherent, which would provide them with a negative argument for their own position. I argue through the following sections that realists have neither a positive nor a negative argument for their axiological thesis, that they likely never will, and that the realist's NMA for their epistemological thesis with therefore never be non-question begging. But before moving on to these issues it's worth briefly looking at van Fraassen's two other challenges to the realist's NMA. For his point is that, even if IBE could be naturalistically justified, the NMA still wouldn't be a persuasive argument.

Van Fraassen's second challenge grants, for the sake of argument, that explanatory power is evidence of truth. But even then, he writes:

> the realist will need his special extra premiss that every universal regularity in nature needs an explanation, before the rule will make realists of us all […] these arguments for realism

> succeed only if the demand for explanation is supreme—if the task of science is unfinished, *ipso facto*, as long as any pervasive regularity is left unexplained. (1980, p.21-23)

And yet, he argues, there is at least one well-established school of twentieth century physics that rejects the unlimited demand for explanation by accepting a fully indeterministic interpretation of quantum probabilities and (contra Einstein, Podolsky, and Rosen 1935) denying the need to postulate any hidden variables to explain these probabilities to "complete" the theory (van Fraassen 1980, p.23, 30, and 95; cf. 2004, 2008, Ch.12 and 13). But if some scientists seem satisfied with leaving some universal regularities unexplained, the realist seems wrong to suggest that *all* regularities need to be explained. So perhaps we don't need to make NMA-type arguments, if the success of scientific theories is just not one of the regularities in need of explanation. Committed realists will of course side with those physicists that deny the acceptability of fully indeterministic theories and endorse efforts to develop hidden variable theories; but again, van Fraassen's conclusion is not that realists are clearly wrong, just that our observations of science in action, and our analyses of what scientific method requires, do not clearly support their picture over his. Some people may want to explain the success of science, but there's no reason to think we *need* to; we can probably get by without such explanations, as van Fraassen wants to, just as many scientifically respectable 20th century physicists got by without trying to explain quantum statistics.

Having cast doubt on the idea that we're compelled to explain the success of science, after first casting doubt on the idea that explanatory power counts as evidence of truth rather than empirical adequacy, van Fraassen grants the realist demand for an explanation of scientific success, again for the sake of argument. He then argues that the best explanation is not, in fact, the "realist explanation with the Scholastic look" (p.39), i.e. that any sufficiently successful theory must be *adequatio ad rem*. The more scientifically acceptable explanation is the Darwinian explanation, he asserts, writing that "any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature" (1980, p.40). And, as Wray (2007) stresses, this evolutionary explanation might rightly be judged as a *superior* explanation to the realist's explanation of scientific success because it is also capable of explaining the eventual *failure* of many once successful theories. For once thriving species are often driven to extinction when a better adapted species takes over their niche, just as once dominant theories are abandoned when a more empirically adequate theory is formulated for their domain. Wray suggests that realism cannot explain such failures, so it is not even the best explanation of the success of science, by scientific standards of explanatory strength.[25]

Realists have responded to this last point by extending the Darwinian metaphor. For purely Darwinian explanations based only in natural selection are incomplete, even vacuous explanations of a species's fitness in its environment. Van Fraassen is, in effect, explaining the fact that a theory gets any given observable fact right by appealing to the fact that it gets *all* the observable facts right. This is equivalent to explaining the fact that one organism is fit by appealing to the fact that *all* members of its species are fit, or the fact that some crow is black because *all* crows are black (Musgrave 1988, p.242). A complete evolutionary explanation of an organism's fitness requires the identification of some features about *that* organism that make it fit, e.g. a molecular biological account of the mechanisms of genotypic inheritance and phenotypic expression, a physiological account of how its teeth are suited to its diet, and perhaps an ecological explanation of how the particular niche it occupies is homeostatically related to those of other species that share its environment.[26] A realist explanation of a theory's success—that the theory is true in certain respects—would be analogous to such a genotypic, physiological, or ecological explanation. Its *truth* is what makes *that* theory successful, and the falsity of other theories is what makes them unsuccessful.

Then again, the anti-realist can challenge this analogy. We can test the reliability of IBE at the level of the observable by tearing open wainscoting to see whether a mouse has, in fact, come to live with us. We can test the reliability of IBE in evolutionary explanations by sequencing an organism's genome to confirm it possesses the hypothesized fitness-conferring genotype, inspect its teeth and diet, or use fossils to understand its phylogeny. But there is no analogous way to

[25] Mizrahi (2012) makes another important point: realism doesn't offer any testable predictions, which scientists would expect any good explanation to do.

[26] For more on this point, see Kitcher (1993), Leplin (1997), and Musgrave (1988); for a purportedly genetic explanation of theoretical success from an anti-realist perspective, see Stanford (2000); for a defence of the strength of van Fraassen's origin explanation, see Wray (2007) and Fine (1986a); for alternative anti-realist explanations that do not claim to be genetic, see Laudan (1984). For an excellent (if slightly dated) summary of several different views, see Kukla (1996)

test the reliability of IBE when using it to infer a theory's truth from its empirical successes, as any corroborating assay would just be another empirical success, the very thing to be explained. So, the constructive empiricist suggests, the realist's "genetic" explanation of a theory's success may not add any additional value to the anti-realist's "selectionist" one, beyond the satisfaction of the realist's metaphysical curiosity and desire for deeper explanations. The realist may in turn contend that scientists will often infer that there is a genetic reason for a given phenotypic trait without being able to independently identify the genes responsible, so it would be appropriate to do the same with respect to the truth of scientific theories, even without independent assays. But here again, it's the realist's prior assumptions about how IBE is deployed in science that determines what they ultimately believe is prudent to infer when a scientific theory displays some remarkable form of success.

Van Fraassen has several other objections to the use of IBE, as briefly discussed above in section three, but the point by now should be clear: one's prior commitment to a realist or an anti-realist empiricist image of science determines the conclusions one will draw when a scientific theory is successful. Realists will think it's true, constructive empiricists will think it's empirically adequate, and each can show that their position is internally coherent, e.g. that they're applying the canons of rational inference, as they understand them.[27] But they can't show that theirs is the *only* internally coherent position, and therefore the only rationally viable one. Scientific realism and anti-realist empiricism remain internally coherent positions despite their opposition, and as a result advocates of neither position can mount a negative argument in its favour.

Van Fraassen does offer a "positive argument" for constructive empiricism, presumably meant to be convincing for people that do not already accept his position (e.g. realists): "it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics" (1980, p.73). But the superiority of constructive empiricism in making sense of scientific activity is highly debatable, and has been challenged by realists on several different fronts, often focused around the way that science is deployed as a predictive tool in non-experimental settings like science policymaking (e.g. Hooker 1985, Kukla 1994, Psillos 2000a).

[27] See Fine (1986a, p.114-116) for an argument that the realist can't even do that much.

Furthermore, just as anti-realists are unlikely to be swayed by the realist's contention that explanatory power counts as evidence, realists are unlikely to be swayed by van Fraassen's commitment to deflationary metaphysics; the avoidance of metaphysical speculation about the nature of unobservable reality is simply not a *value* that realists prioritize in the same way as anti-realist empiricists.

This leaves us with the following stalemate: scientific realism and constructive empiricism are both coherent epistemologies of science, consistent with the mutually agreed upon canons of rationality such as internal coherence and consistency with the available evidence. The positive arguments offered in their favour are valid if one assumes ungrounded assumptions about the sorts of aims and values that guide scientific inquiry. Realists think science aims for truth, and constructive empiricists think it aims for something more epistemologically humble, but as it stands those are contested assumptions. Nevertheless, if we assume that science aims to uncover the truth about unobservable reality for whatever reason, the NMA may go through and rationality compels us towards some form of epistemological scientific realism.[28] The most likely reason for someone to make that assumption would be if they're personally curious about such truths, and come to interpret scientific practice as enacting their values by seeing it as a search for true theories. But if someone instead values metaphysical minimalism, and interprets the aim, practice, and methodological requirements of science according to *their* values, they will see science as the search for something less than true theories, e.g. empirically adequate theories. Neither person seems to be acting irrationally, but because they're both guided by different values they're unable to convince their opponent to change their position. Both positions, it seems, are rationally permissible, and there seem to be no prospects for using arguments and evidence to convince someone who accepts one position to change their mind.

An epistemological scientific realist or a constructive empiricist, of course, could show that their position is the only rational one if they could establish, on independent grounds, that the governing aims and central methods of science are necessarily and fundamentally either realist or anti-realist. Simply put, the NMA will be a compelling argument for epistemological

[28] Though as Lyons (2005) shows, accepting axiological scientific realism need not lead us to accept epistemological scientific realism.

scientific realism only if we have a compelling argument for axiological scientific realism. Unfortunately for those who wish to resolve this stalemate between epistemological scientific realism and constructive empiricism, there are no definitive, non-question begging arguments for or against axiological scientific realism, and there probably never will be, as we will see through the next four sections.

6) Axiological Theories of Science

As the preceding discussion has shown, disputes over the persuasiveness of the NMA come down to disputes over what the aims, methods, values, and canons of rational inference governing scientific inquiry are. To be sure, arguments have been offered in favour of axiological scientific realism in an attempt to establish it without begging the question. As we will see through the next three sections, these arguments ultimately fail, so the arguments for epistemological scientific realism planted atop axiological scientific realism have not been placed on solid ground. But it's worth addressing these arguments, for treating axiological scientific realism, and the failure of arguments proffered for it, in isolation will make it as clear as possible why exactly the NMA proves unconvincing for epistemological scientific realism's non-believers.

At this point, one might begin to wonder what it means to say that a collaborative human activity like scientific inquiry has its own aims, independently of the various aims of whichever individual actors are participating in it, i.e. what it means to give an axiological account of science. An axiological theory of science is a very specific type of answer to the question "what is science?" There other ways one might answer such a question. One might give a sociological, geographical, or historical answer, for example by saying that science is something practiced by a certain group of people, delineated along various sociological, geographical, and historical lines. But to define a human activity *axiologically* is to identify the criteria of success operant in and definitive of the very activity itself, independent of any opinions, strategies, or motivations of anyone participating in the activity. The idea is that we needn't take as definitive or authoritative any participant's self-conception of their activities, or description of their personal motivations, to understand what counts as success in an activity like the scientific representation of nature. This is why, for instance, van Fraassen can disagree with scientists who insist that they take explanatory power as evidence of a theory's truth, and that they see the aim of science as discovering the truth about unobservables. Rather than relying on the reports of someone engaged in an activity, to define science axiologically we need to identify the standards of success that "appear concretely in science itself in theory choice and evaluation" (van Fraassen 2004, p.794). Simply put, the question is about what modern science takes to be an acceptably complete theory. Answering this question will show us the end-in-view that the modern scientific community directs itself towards, its ultimate aim, telos, or raison d'être. If that end is the development of true theories, we'll know that science needs to make use of IBE; if it's merely empirically adequate theories, we'll know that it doesn't.

For clarity, consider the following analogy. Someone might ask "what is ice hockey?" One answer would be that it's a sport whose highest leagues are the NHL and KHL. We could add that players make use of various implements such as skates and sticks, that it's played primarily throughout the Western world but is making inroads in Asia, that it was first played using recognizably modern rules in Montreal on March 3rd 1875, that it has some family resemblance and genealogical association with Bandy, and so on. This would give us some sociological, geographical, and historical answers to our question, but it would not give us an axiological answer. An axiological answer would specify an aim that is internal to the game of hockey itself, but without necessarily telling us why any individual person chooses to play hockey. A player might say, for example, that they play hockey "just for fun," for exercise, to find camaraderie, to inculcate cooperative or competitive virtues, or to appear impressive to their friends. But those are individual goals, not the goal of the activity itself. Similarly, simply explicating a particular strategy of play would not provide an axiological answer to our question, whether that strategy is getting pucks deep, maximizing scoring chances, playing the trap, or just givin'r 110%. Even listening to a professional explain the standards they hold themselves to will not answer our axiological question, whether they tell us that they will count themselves as successful when they've made a certain amount of money, broken certain scoring records, maintained a certain +/- rating, had their name etched on Lord Stanley's Cup, or consistently laid out everyone that gets too rough with the superstars on their team. For an axiological answer will give the standard of success *internal to the activity itself,* as manifested in the community's recognition of success, independently of the criteria any of the individual actors hold themselves to. And in hockey, we have a clear answer to the axiological question: at the end of every game the successful team, the "winner," is always going to be whichever team has the most goals on the board, making "scoring more goals than the other team" the operant criterion of success for hockey, as an activity. This will be true whether we're talking about game seven of the Stanley Cup finals or a game of shinny in a cul-de-sac. The central aim, end-in-view, or telos of playing hockey has nothing to do with individual opinions, strategies, or motivations for participation, be they professional or amateur; the aim of hockey, axiologically speaking, is defined by the standards of success the community of hockey players holds itself to and enshrines in their rulebooks, which gives fundamental value to having the most goals.

Despite there being no official rulebook, some philosophers of science believe an analogous axiological aim can be discerned for scientific inquiry, a criterion of success manifest in scientific communities and methodologies, independent of any individual scientist or research team's opinions, motivations, or research strategies. So, whereas epistemological scientific realism is a claim about what we can (and do) know on the basis of scientific inquiry, axiological scientific realism is a claim about what science, as a human endeavor, aims to accomplish; specifically, axiological scientific realism is the claim that success in the activity of science, in the above sense, is the production of true theories, capable of explaining various phenomena of interest. Another way to put this would be to say that a research team has metaphorically "won" a game of science in some domain when they have produced a true theory, and identified it as such through its ability to explain all the phenomena of interest to scientists in that domain. A complete scientific theory, according to the realist, is a true scientific theory, just as a complete game of hockey is one in which one team has recorded more goals than the other.[29] Once that standard has been achieved, the results can be accepted and reported, whether to the local papers or to the field's top journals.

For clarity, we can formulate this view as follows:

[29] There are never ties in hockey, so games go into overtime (and sometimes a shootout) if they're tied after regulation time.

> **Axiological Scientific Realism**: Science aims to give us theories that reveal the truth about the unobservable world, and/or the modal structure of reality, capable of explaining phenomena of interest.

By contrast, constructive empiricism, as originally explicated by van Fraassen (1980), includes the following axiological claim:

> **Axiological Constructive Empiricism**: "science aims to give us theories which are empirically adequate" (p.12), capable of saving phenomena of interest.

Success in the activity of science, according to the constructive empiricist, is something less than the production of true, explanatory theories. The standards of modern science, it is claimed, deems theories acceptable if they are judged capable of including the observable facts of the actual world in at least one of their models. Producing theories that are true with respect to unobservables, and explain the appearances in terms of an underlying reality, is at best supererogatory; so long as a theory is empirically adequate, it is winner.

As with his defence of epistemological constructive empiricism, in defending axiological constructive empiricism van Fraassen is just claiming that there is a way to make sense of science as a rational enterprise without realism. He's not claiming that his view is the only coherent one, just that it is coherent. There are many other anti-realist contrasts to axiological scientific realism that may also be coherent. Social constructivism can be understood as opposing both axiological scientific realism and constructive empiricism, for example, by claiming that the "end-in-view" for science is some set of social conditions, rather than true or empirically adequate theories (Khalifa, 2010). Pragmatism or instrumentalism, somewhat similarly, might be understood as the idea that science is aimed not towards the discovery of truth but the promotion of human flourishing, for example through the production of useful (i.e. predictively powerful) theories. But van Fraassen's constructive empiricism is by far the preeminent anti-realist alternative to axiological scientific realism, claiming solidarity with a long history of empiricist challenges to more metaphysically-inclined visions of science, linking van Fraassen's position, through the logical positivists, to Mach, Helmholtz, Kant, and ultimately to Hume and the anti-scholastic nominalists that came before him (see van Fraassen 2002).

While arguments for and against epistemological scientific realism are far more common in the philosophical literature, a few arguments have been specifically marshalled for axiological scientific realism. Through an abstract conception of the rules and demands of scientific practice, these arguments for axiological scientific realism purport to show that scientists can only function if they work towards realist aims, i.e. that scientists would lose some important tools, techniques, or methods if they maintained anti-realist standards of success. The general thrust is that science could not operate if it was not governed by realist aims and held to realist standards, because important aspects of scientific practice cannot be well-motivated by anti-realist axiologies, so the aims of science are, therefore, fundamentally realist. Here I will deal with two such arguments: Popper's methodological argument (Popper 1983; cf. Wray 2015, Fine 1986a, Ch.7) and Psillos's rendition of Putnam's "conjunction argument" (Psillos 1999, 2000a; cf. Hooker 1985).

7) The Methodological Argument for Axiological Scientific Realism

Several attempts have been made to establish scientific realism by paying attention to scientific methodology in one way or another (e.g. Boyd 1981, 1984; Sankey 2001; Putnam 1975; Leplin 1986; cf. Hendry 1995, Fine 1986a, Ch.7, Wray 2015). I take Karl Popper as my exemplar for this argument, but the basic strategy is always the same: identify some aspects of scientific method that cannot be understood as rational, or well-motivated, unless the aim of science is identifying true theories (or at least that scientists will not be able to effectively operate unless they see this as their aim); conclude that the aim of science is truth.

Popper's contention was that science, as an activity, is properly directed towards the production of explanations of observable phenomena in terms of unobservable causes, followed by the explanation of those unobservable causes in terms of other, deeper unobservable causes, and so on and so on (1983). Only by trying to find the truth, he thought, could scientists properly implement the procedure of conjecture and refutation (better known as the hypothetico-deductive method) which he saw as characteristic of science and essential to how it achieves its unique kind of epistemic progress.

As Popper saw it, science is unique and eminently rational because, while deeply creative and speculative, it is also fundamentally critical and based in an entirely deductive methodology (cf. Musgrave 2004). Scientists, like metaphysicians and theologians, freely speculate about the hidden mysteries of reality, but their hypotheses are distinguished by their being empirically falsifiable. Science, metaphysics, and theology all proceed by forwarding bold hypotheses about the nature of unobservable reality to explain what they observe, but to be scientific a hypothesis must be at risk of being contradicted by observation or experiment. If a theory makes inaccurate predictions, we can show deductively that it must be false. If it makes accurate predictions, on the other hand, we can't show that it's true, we only know that it *might* be true. This allows scientists to evaluate their theories by trying to falsify them, deducing observable predictions from them and trying to show that those predictions are wrong through so-called "crucial experiments." On this account, science doesn't tell us what's likely true, only what's certainly false, and that's still an important kind of epistemic progress. Whether or not we are ever warranted in concluding that some theory is true (epistemological realism), it's the speculative effort to formulate true and explanatory theories and then rigorously test them (axiological realism) that drives scientists towards the prediction and discovery of new and surprising phenomena. The discovery of time dilation, new periodic elements, quantum entanglement, and the existence of electromagnetic radiation all resulted from scientists' efforts to produce theories capable of providing deeper and broader explanations whose truth could be tested in terms of their precise, novel predictions; or at least, that was Popper's contention.

Popper offered a negative argument for his axiological realism, asserting the hypothetic-deductive method only made sense if the aim was to formulate true, explanatory theories (1972). The alternative to axiological realism, in his eyes, was some form of conventionalism, where science is understood to be about constructing useful and empirically adequate theories, with no concern for the truth of those theories (Wray 2015). But if scientists simply construct theories conventionally to accommodate the observed regularities of nature, he thought, they might simply stipulate a set of laws that all models of empirical phenomena must be based on, come what may. They could then introduce *ad hoc* hypotheses to accommodate any new phenomena that might be discovered, thereby saving both the phenomena and the theoretical system of their choosing. But if science proceeded by that logic, he thought, the whole system of laws and empirical generalizations constructed through such procedures would become untestable, for scientists could always save any law or generalization they wished to, even if an experimental result initially seemed to falsify it. In that case we might never have discovered gravitational waves, electrons, or even oxygen, as scientists might have introduced innumerable metaphorical epicycles into Newton's system to "save the phenomena" rather than overturning its entire system of laws in favour of Einstein's.[30] Indeed, Popper thought, actively testing our scientific worldview makes little sense on a conventionalist understanding of the aims of science; and yet, science only makes progress because scientists test their laws, theories, and hypotheses, often elaborately and at great expense. Thus, he argued, the aims of science are *characteristically, properly, and necessarily* realist.[31]

To put some flesh on this picture of science consider the tests of Einstein's theory of General Relativity conducted in the years following its formulation. Newton explicitly recognized that his account of gravitation was simply a description of the phenomenon, and famously did not frame hypotheses regarding the ultimate "reason for these properties of gravity" (Newton 1726). Nevertheless, Newton's followers eventually came to see an explanation in the primitive property of gravitational attraction itself: gravitational attraction was a power given to objects by the quantity of mass, which draws them towards each other across empty space over time. This account of gravity was itself capable of explaining many observed phenomena of motion, both in the heavens and on earth, and predicting many hitherto unobserved ones, all through one simple hypothesis: all massive objects, in virtue of possessing mass, attract each other according to the Law of Gravitation. But within the Newtonian framework the property "mass" that explained all these phenomena itself remained unexplained.

As Popper saw it such unexplained explainers are perfectly good explanations. Not everything needs to be explained to serve as an explanation of something else (do I need to explain why

[30] The introduction of a Lorentz contraction might have provided an *ad hoc* way to save Newton's theory from some anomalies that favoured General Relativity, for instance, which eventually predicted the existence of gravitational waves; Maxwell's Field Theory, through which the modern theory of the electron was developed, might have been subjected to *ad hoc* modifications by a devoted Newtonian simply because it violates Newton's Third Law of Motion (unlike contemporary action-at-a-distance theories); and some anomalies in phlogiston theory regarding the transfer of mass during chemical reactions, which showed up most starkly when modelling those reactions within Newton's theory, could have been accommodated by assuming in *ad hoc* ways that phlogiston sometimes had a negative mass (but not always).

[31] See Hendry (1995) and Fine (1986a, Ch.7) for good summaries of more contemporary versions of the methodological argument, and Boyd (1981, 1984) for an example.

water expands when it freezes to explain why pipes burst in the winter?). But by looking for the cause of gravitation Einstein made further progress, descending to a deeper level of explanation by boldly conjecturing that gravitational attraction was the result of spacetime being warped by massive objects. This hypothesis implied some new predictions, at serious odds with our prior expectations, meaning its truth could be tested by seeing whether those were false predictions. On Popper's conception that's what Eddington effectively set out to do by checking to see whether starlight would be bent by the sun's gravity to the degree that Einstein's theory predicted it would. As it turns out, against many people's expectations, Einstein's predictions of what would be observed held true, and the predictions of Newton's theory did not. Through that crucial experiment we learned that Newton's theory was certainly false but that Einstein's might still be true, exactly the kind of epistemic progress that science produces on Popper's account.

Popper would argue that unless science was aimed towards the discovery of truth, this would not have happened. As he saw it, Einstein formulated his theory to provide a deeper explanation of gravitational phenomena, which wouldn't make sense unless he knew the point of science was to look for the true causes of things. And if Eddington hadn't known the point of experiment was to assess a theory's potential truth, he wouldn't have gone to great effort to test Einstein's theory, as he did. But if he hadn't, we'd never have learned that light was effected by gravity to that degree. And if scientists didn't know that the point was to determine which theory is true, and reject theories that are false, they might have simply saved Newton's theory after Eddington's results by adding a few *ad hoc* hypotheses.[32] And lastly, if scientists didn't want to test the truth of Einstein's theory even further, they wouldn't have been motivated to investigate its other, even more shocking novel predictions, such as time dilation. The only way that science can make progress, Popper thought, from the formulation of theories, to the testing of them against each other and the direction of future research, was by undertaking the realist aim of trying to formulate and identify true theories, capable of explaining various phenomena of interest.

---

[32] e.g. that measuring instruments shrink in gravitational fields, that the gravitational constant needed to be corrected in certain circumstances, etc.

There are, of course, historical objections we might make to this Popperian rendering of the events surrounding the formulation and empirical testing of General Relativity.[33] Nevertheless, that is irrelevant to Popper's main point: what I've called "axiological scientific realism" correctly identifies the governing aim of science. It should be upheld by philosophers on those grounds, he thought, for only realist aims make sense of the procedures by which science makes progress. But even more importantly, he thought, a realist axiology should be upheld by working scientists, lest they not be driven to do many of the things that makes science a unique, effective, and progressive form of inquiry. In short, both the philosopher of science trying to understand the nature of science and the working scientist trying to operate effectively should accept axiological scientific realism, for it is the only philosophy of science that makes sense of the logic of hypothesis testing by which scientific progress is achieved, and motivates people to carry out that method.[34]

While he was a committed axiological scientific realist, Popper's attitude towards epistemological scientific realism was more complicated. There are several places in his writing where he carefully makes clear that the most anyone could claim for a theory that has passed all tests is that it *might* be true, simply because it isn't demonstrably false (1959; see esp. 1972). Specifically, he suggests we'll never be *justified* in believing that a scientific theory is true or even likely true when it proves predictively successful. Nevertheless, he seems to have had an unshakable intuition that, as it progresses, science does get "nearer to the truth" (1982, p.57), and he constantly struggled to explain what scientific progress meant in realist terms. To this end he famously developed a definition of verisimilitude or "truth-likeness" that he thought would allow him, even within his hypothetico-deductive conception of the scientific method, to understand scientific progress in epistemologically realist terms, as the idea that our current best scientific theories contain more truth (or are more "like" the truth) than their predecessors, even

---

33 There is the well-known influence of Mach's positivism on Einstein's formulation of both Special and General Relativity, for instance, as well as Einstein's advocacy of physicists acting as "unscrupulous opportunists" in epistemological matters (Einstein 1949, p.684). Additionally, there is Eddington's obvious (and eventually desperate) effort to validate Einstein over Newton (for partly political reasons) rather than conduct a proper test (Stanley 2003, Collins and Pinch 1998).

34 For an excellent recent treatment of the methodological argument for scientific realism, as espoused by Popper and others, and methodological arguments for anti-realism, see Wray (2015), who also makes clear that this can really only succeed as an argument about what scientists should accept, not what philosophers should. For a relatively contemporary defence of this argument, see Leplin (1986).

if they are strictly speaking certainly false. However, for formally demonstrable reasons, this definition failed to work as intended.[35] Nevertheless, we might suppose that Popper *wanted* to make an argument like the NMA, using the predictive and experimental success of modern science to support an assertion that modern science contains more truth than past science. To see how a Popperian NMA might be constructed, suppose that his argument for axiological realism were sound, i.e. that no anti-realist axiology could rationalize key scientific practices. In that case, a viable notion of truth-likeness could be used to help formulate the hypothesis that some theory is closer to the truth than its rivals, because that would be the best explanation of its ability to pass the experimental tests that its rivals failed to pass. This claim could then be favoured until proven wrong, as our current best guess, allowing us (even if only tentatively) to give a kind of scientific explanation of scientific success akin to the NMA, from within a Popperian framework.[36]

Of course, if we accept that constructive empiricism presents a coherent understanding of how science operates progressively while aiming for something less than true and explanatory theories, then Popper's argument for axiological scientific realism is no longer valid. In that case, this strategy of building a Popperian NMA will not succeed. I will explain why this is the case in section nine, after first laying out another argument for axiological scientific realism: the so-called "conjunction argument."

8) The Conjunction Argument for Axiological Scientific Realism

Whereas Popper's methodological argument looks to support axiological scientific realism against some form of conventionalism, a different argument can (and has) been directed

[35] as demonstrated independently by Tichý (1974) and Miller (1974).

[36] This seems to be the approach taken by Ilkka Niiniluoto, who uses an updated notion of verisimilitude (1987) to defend a broadly Popperian epistemology of science (1999) while also claiming that IBE is generally truth-conducive, and that the NMA is a strong argument that employs IBE (1998). Niiniluoto offers a naturalistic reassurance that IBE is a reliable method of belief-formation that is not grounded in scientific practice, method, or axiology, but rather in our evolved inferential instincts (1998, p.13). Insofar as we might wish to apply IBE to the level of the unobservable, however, Niiniluoto's argument provides at best a very weak assurance that it is reliable, for two reasons: first, there's never been any selection pressure on us to get the facts about unobservables right, and second, there's ample historical evidence that humanity's untrained inferential instincts are extremely unreliable with respect the unobservable world.

specifically against an axiological rendering of constructive empiricism. Its original presentation is due to Putnam (1975a), but an excellent summary has been given by Psillos (2000a):

> Realists argue there is something to be gained by believing [a] theory to be true. Envisage two theories T1 and T2 which are accepted as true. One can form their conjunction T1&T2 and claim that since T1 and T2 are true, so is T1&T2. One then comes to believe T1&T2 and starts applying it to the explanation of the phenomena. Is it only explanation that can be gained, though? In general T1&T2 will entail more observational consequences than T1 and T2 taken individually. So there is certainly something extra to be gained by believing the theories: extra observational consequences that would not have become available if the theories had been taken in isolation. […] This argument has become known as the 'conjunction argument.' (p.68-69)

The contention here is that, if scientists are not aiming for true theories, and therefore do not believe that acceptable theories are *true* but rather that they are merely *empirically adequate*, then they will not believe that all the novel observational consequences that result from such a conjunction will hold true. This is because, as Psillos explains:

> There is a crucial difference between truth and empirical adequacy. Although, truth is preserved under conjunction, empirical adequacy is not, at least not necessarily. So although 'T1 is true' and 'T2 is true' entail that 'T1& T2 is true', 'T1 is empirically adequate' and 'T2 is empirically adequate' do not entail that 'T1&T2 is empirically adequate'. The conjunction of two empirically adequate theories might even be inconsistent. (ibid.)

Since constructive empiricists do not see acceptable theories as true theories, it is argued, they cannot see a scientist who believes the untested predictions resulting from the conjunction of two independently established theories as rational. But scientists, it is supposed, *need* to form those additional beliefs to properly practice science, i.e. they need to accept theories as true. And while a constructive empiricist can't account for why it's rational to do so, the realist easily can: if the conjuncts are true, their conjunction must be as well, along with all its untested consequences. So, if scientific progress depends on scientists believing the new observational consequences resulting from the conjunction of two already accepted theories, but the constructive empiricist can't see that as a rational action despite claiming that science is a rational enterprise, constructive empiricism is incoherent. That that case, the governing axiology of science cannot be what they say it is, i.e. it must be a realist axiology.

This is also a negative argument for axiological scientific realism, and the issue it hinges on is whether scientific progress depends on scientists forming their beliefs in this way. We might argue that scientists form their beliefs in realist ways when they're called upon to guide policymaking, for example, as this often requires making novel and untested predictions by conjoining several theories. For example, when making policy proposals for how to respond to climate change, scientists base their judgments on climate models that have been built using several, independently tested models in conjunction—thermodynamical models of radiative heat exchanges in the atmosphere, ecological models of carbon capture, fluid dynamical models of ocean currents, chemical models of industrial processes, etc. And yet, they suggest to policymakers that these models are likely to be predictive, even in their most novel observational consequences such as the prediction of run-away global warming, which strictly speaking has never been observed or put to an empirical test. Despite such predictions never having been independently tested, scientists generally seem to believe them with the same conviction that they believe the thoroughly tested conjuncts that were used to produce the larger predictive model. To issue such predictions with confidence they would need to think that the untested predictions resulting from the conjunction of independently tested theories are likely to be borne out empirically, as they would if they accepted the conjuncts were true (but not if they accepted them as empirically adequate). So, in their role as policy advisors. it would seem scientists need to be realists, given the kinds of inferences they need to make when using their theories to inform important kinds of decision making we expect them to be able to. If we consider such activities to be a part of "science" proper, we'll expect any axiology of science to be able to account for them, and the realist's conception of science's aims seems to do a better job of that than the constructive empiricist's (cf. Hooker, 1985).

9) Anti-Realist Responses to Arguments for Axiological Scientific Realism

Before evaluating these arguments, we should be clear about what they might show, if successful. Both Popper's methodological argument and the conjunction argument are practice-oriented, negative arguments for axiological scientific realism. The idea, in general, is to show that some essential feature of scientific practice—at least as it is ideally conceived—cannot be rationally motivated on anything but a realist understanding of the aims of science.

If the constructive empiricist was forced to declare an essential scientific practice rationally unmotivated, that would contradict their assumption that science is a rational enterprise. This would make constructive empiricism incoherent, and therefore untenable. With no workable alternative, axiological scientific realism would then be the only tenable axiological account of science. This could compel the would-be anti-realist to admit defeat and accept a realist axiology, given the absence of any viable alternative. Both parties would then agree that scientists sometimes *need* to operate according to realist aims, and potentially make use of IBE in a way that treats explanatory power as evidence of truth (e.g. when choosing between empirically equivalent rivals). The epistemological scientific realist would then have a non-question begging, naturalistic justification of IBE, which would make the NMA a sound argument. So, if either of these arguments for axiological scientific realism held up to scrutiny, the NMA could go through, establishing that epistemological scientific realism was the rational position to hold.[37]

Nevertheless, these arguments do not show that axiological constructive empiricism is incoherent, for there's a simple strategy that allows the constructive empiricist to account for most any practice that seems exclusively motivated by a realist axiology: assert that this practice is rationally motivated on pragmatic grounds, much as van Fraassen does with the search for explanations.[38] It's the existence of this strategy that makes Fine think not just that extant arguments for axiological realism are unconvincing, but that all as-of-yet unformulated arguments will also be unconvincing. Whatever practice realists claim only a realist axiology can account for, the anti-realist can simply declare them pragmatically motivated. Or as Andre Kukla put it, there can be "no end to the pragmatic virtues that may motivate a piece of scientific work" (1998, p.33). Thus, in the end, no abstract considerations of scientific method or empirical facts about how science is practiced can speak in favour of either a realist or an anti-realist axiology or epistemology of science.

[37] Assuming, of course, that the anti-realist empiricist could be convinced that the success of science needed to be explained, and that realism was in fact the best explanation.

[38] For van Fraassen's take on this strategy, see his (1980, Ch.4 and 5). For more systematic discussions, see Fine (1986b, p.154) and (Kukla 1998, p.32).

To see how this strategy works, let's address three of the scientific practices Popper argued could only be accounted for by a realist axiology: the formulation of new and precise theories through the search for true and explanatory theories, the refusal of *ad hoc* modifications to save established or favoured theories, and the testing of theories through experiment. We've already seen how the constructive empiricist understands scientists' search for explanations: what scientists consider explanatorily powerful theories are, *ipso facto*, empirically adequate theories, so searching for explanations proves to be an effective way to develop empirically adequate theories (which, as the constructive empiricist sees it, is science's true aim). Van Fraassen goes so far as to suggest that, if it is simply a brute psychological fact that a realist self-conception is especially efficacious for working scientists, "[w]e might even suggest a loyalty oath [to realism] for scientists" (1980, p.93). But from a philosophical perspective, he argues, any criticism of constructive empiricism based on realism's methodological fruitfulness in the practice of science raises "a totally false issue [...] for the interpretation of science, and the correct view of its methodology, are two separate topics" (ibid.). An axiological interpretation of science is an attempt to identify science's governing aim, telos, or end-in-view as defined by the criteria of success internal to the activity itself; it is not an attempt to identify which interpretation is most beneficial for working scientists to adopt, as a matter of psychological fact. A hockey player may contribute more if her coach tells her she's out there to block shots, dump and chase, or wait for a pass at the blue line, but the winning team will always be the one that scores the most goals. That scientists often achieve success by searching for explanations, and that they could accordingly be prescribed (even mandated?) to search for explanations, does not count against the constructive empiricist's claim that scientific theories need only be evaluated in terms of their empirical adequacy.

Thus, according to constructive empiricism "the search for explanation is valued in science because it consists *for the most part* in the search for theories which are simpler, more unified, and more likely to be empirically adequate" (ibid., emphasis in original), not because it implies that explanatorily powerful theories are more likely to be true. Which is all to say, in brief, that on van Fraassen's account the search for explanatorily powerful theories is not justified on epistemic grounds, for explanatory power is not evidence of a theory's truth; rather, it's justified on pragmatic grounds, because it's an evidently effective means of achieving the epistemic aim of producing increasingly empirically adequate theories. Thus, the constructive empiricist has no problem accounting for the fact that the search for explanations seems to animate a lot of good scientific work; indeed, if science works better when scientists adopt a realist axiology, perhaps because that self-conception leads them to work harder at developing explanatorily powerful (and thereby empirically strong) theories, the constructive empiricist would gladly recommend that all scientists operate according to a realist axiology, *but only on pragmatic grounds*. But there is no demand on science that it must try to explain "in the absence of any gain for empirical results" (1980, 34; cf. Feyerabend 1964, Hendry 1995 p.69-70).

Van Fraassen accounts for scientists' refusal to accept *ad hoc*, theory-saving hypotheses in the same way, as a demonstrably effective means of producing empirically stronger, easier to work with, and more unified theories. In general the constructive empiricist will see the super-empirical virtues of theories—such as non-*ad hoc*ness, simplicity, explanatory power, breadth, depth, etc.—as valuable for such pragmatic reasons. They may even recommend that scientists search for theories that possess such virtues, if it helps them achieve their governing aim of producing increasingly empirically adequate theories, but again, that is an empirical, psychological question rather than a philosophical one. The constructive empiricist doggedly denies that the super-empirical virtues are valuable for *epistemic* reasons, i.e. that scientists, or anyone else, can reliably infer the truth of a theory's claims about the modal or unobservable features of our world based on its possession of such super-empirical virtues. For the constructive empiricist, the only epistemic virtues that a theory can possess—that is, the only virtues that bear on its belief-worthiness, or likeliness to be true or empirically adequate—are the empirical virtues of getting more observable facts right and fewer wrong. A theory's possession of super-empirical virtues, including non-*ad hoc*ness, can still count as a good reason to adopt that theory, they just don't count as good reasons to think the theory is true, or empirically adequate absent an increase in empirical virtues. So, whatever scientific practices a realist might point to, claiming that they are instrumental or even essential to achieving scientific progress, such that scientists will only be motivated to pursue them if they conceive of themselves as aiming to formulate true and explanatory theories, or if they accept their theories as true (not merely empirically adequate), the constructive empiricist has a ready reply: scientists are realists for pragmatic reasons, as a realist self-image is an effective psychological aid in the process of developing empirically strong and adequate theories, but scientific theories are still judged by the entire community of scientists according to the epistemic standards of empirical adequacy. That, the constructive empiricist contends, provides a coherent understanding of science as a rational, goal-directed activity at least as much as the realist's understanding that scientific theories are judged according to the epistemic standards of truth.

When it comes to conducting crucial experiments, however, constructive empiricism sees a clear epistemic reason for this practice: theories need to be tested for their relative empirical adequacy. To illustrate, let's continue with Popper's favourite example, but render it according to a constructive empiricist's understanding of science. According to the constructive empiricist, Eddington was not testing the truth of Einstein's explanation of gravitational attraction in terms of mass's capacity to warp the fabric of spacetime. In proffering his theory Einstein explicitly demonstrated that it could provide empirically adequate models of all established gravitational phenomena, including some that had proven anomalous in a Newtonian framework, such as the observed perihelion of mercury. This suggests Einstein's theory is more empirically adequate, capable of providing a model for more observable, actual facts than Newton's. But Einstein's theory also implied some shocking deviations from established theory regarding hitherto unobserved but observable circumstances. So, Eddington and his team worked hard to create and observe one those circumstances, to further assess the empirical adequacy of Einstein's theory. What the results showed was nothing more than that, in those circumstances as well, Einstein's theory was more capable of accommodating the observable, actual facts than Newton's, i.e. that it was *more empirically adequate*. Like the observed perihelion of mercury that preceded it and the observation of time dilation that succeeded it, Eddington's experiment simply counted as an observation that, after it had been made, all theories of macroscopic motion were henceforth expected to be able to accommodate in their models. Newton's theory couldn't provide models for these phenomena, so it was rejected, but Einstein's could, so it was accepted, not as true or explanatory but as *empirically adequate*. Thus, theory-testing is straightforwardly motivated by the aim of formulating empirically adequate theories, and on epistemic rather than pragmatic grounds.

Note that this account of scientific theory-testing operates entirely at the level of the observable; at no point does the constructive empiricist think that a test of a theory provides evidence for the truth or falsity of the claims it makes about unobservables, or counterfactuals. This points to an important difference between a realist and a constructive empiricist's conception of scientific experiments, one which will be addressed in more detail throughout the fourth chapter. On an empiricist understanding, scientific instruments and experimental apparatuses are not devices that give us epistemic access to facts about the unobservable aspects of reality, as realists understand them; they are not windows into the unobservable world, they are engines of creation that simply enrich the observable world, bringing new phenomena into existence that all candidate theories must henceforth "save" (van Fraassen 2008, Ch.4). So, on a constructive empiricist account of the aims of science, developing theories into their most testable form, and then testing them, makes perfect sense in terms of the central epistemic aims of science.

So much for Popper's methodological argument for axiological scientific realism. Insofar as we agree that the constructive empiricist's anti-realist axiology can rationalize (i.e. provide a rational motivation for) all the same practices as Popper can, his negative argument fails.[39] When it comes to the so-called "conjunction argument," the constructive empiricist's response to the realist's objection takes on a somewhat different character: declare that *believing* untested, novel predictions generated by conjoining independently tested theories is not, really, a properly scientific activity (cf. Kukla 1998, 31-34; Hooker, 1985). In empiricist circles this idea has a long pedigree, stretching back at least to Hume's articulation of the problem of induction. The basic insight here is that prognostication, even using our current best scientific theories, is an irredeemably risky business, and that scientists *qua* eminently rational and cautious epistemic agents should not get involved in it. They still may, but such practices are outside of science proper, for success in scientific theory building and evaluation does not require believing the predictions of our theories, especially not the untested, novel ones.

To be sure, scientists do need to draw out predictions from their theories and hypotheses to be

[39] In fact, there's a good argument that constructive empiricism can rationalize all the same practices and more. Popper's philosophy of science is significantly more normative than van Fraassen's and is committed to condemning scientists as behaving badly if they (for example) conduct an experiment without a firm hypothesis and then simply write the results into their theory, as is often done in metrological work. For van Fraassen such work makes complete sense, and is part of why he calls his brand of empiricism 'constructive,' "to indicate [his] view that scientific activity is one of construction rather than discovery" (1980, p.5). Rather than theories always being built as bold hypotheses to be testing through crucial experiments, van Fraassen can understand people like Millikan as eminently rational for using experiment to build theory by other means (ibid., p.77), whereas Popper cannot.

effective scientists, but only because they need to *test* rivals against each other in terms of their empirical consequences, in the manner outlined above. What the constructive empiricist denies is that scientists ever need to *believe* these predictions to run such tests, i.e. to be effective scientists.[40] So, according to the constructive empiricist, science does require that predictions are made, sometimes through the conjunction of independently tested theories, but this is only required insofar as those predictions are used as a means of testing the empirical adequacy of those (conjunctions of) theories. But scientists, *qua* scientists, don't need to treat acceptable theories as true because they never need to *believe* that the observational consequences that result from conjoining two such theories will be true. To achieve their aims scientists only need to note that specific observational consequences result from such a conjunction and then empirically test those predictions to determine whether the conjunction is, in fact, empirically adequate.

Given the central role of scientific experts and scientific theories in much of our public policymaking, this can be a counterintuitive idea, so I will elaborate. Accepting two theories as true allows scientists to confidently believe "extra observational consequences" that result from their conjunction, for unlike empirical adequacy truth is preserved under conjunction. Believing these extra observations consequences proves useful to scientists when they're called upon to make policy recommendations and plan for the future, certainly. But on the constructive empiricist's account, planning for the future and making policy recommendations are just not a part of what they consider scientific inquiry proper: conjoining-to-*test* is part of science, and is well motivated within a constructive empiricist conception of science, but conjoining-to-*believe* is not. This may seem arbitrary, but it makes sense once we realize that constructive empiricism is only an axiology of science as a *representational practice*, not as a practical endeavor. In describing what it means to think of science axiologically, van Fraassen (2004) writes:

> Science is a representation of nature, in mathematical form, accomplishing by this means … a certain end that philosophers debate. Criteria of success or completeness for scientific representation must be related to this end, but appear concretely in science itself in theory choice and evaluation. (p.794)

[40] indeed, they'd likely be well advised to withhold belief until the experiment is conducted, e.g. to resist confirmation bias

While scientific theories are often used to make important predictions, the reliability of their untested predictions can play no role "in theory choice and evaluation" before they're tested. So, if science is understood only as an effort to represent nature in mathematical form, there's no need for scientists to believe untested predictions, or to make accurate predictions at all outside of testing contexts. Van Fraassen does write that "[t]he process of putting theories together, in joint projects of explanation, prediction, and control, is a process the philosopher of science must be able to describe" (1980, p.85). And as part of theory choice and evaluation, the constructive empiricist can describe and rationalize that process. But insofar as the scientist is recommending *belief* in the accuracy of those predictions, the constructive empiricist would argue, they put on a different hat. For when a scientist, as a scientist, conjoins two already well-tested and accepted theories together, van Fraassen says, they:

> must always, even if tacitly, reason at least as follows in such a case: if I believe T and T′ to be true, then I also believe that (T and T′) is true, and hence that it is empirically adequate. But in this new area of application T and T′ are genuinely being used in conjunction; therefore, I will have a chance to see whether (T and T′) really is empirically adequate, as I believe. That belief is not supported yet to the extent that my previous evidence supports the claims of empirical adequacy for T and T′ separately, even though that support has been as good as I could wish it to be. Thus my beliefs are about to be put to a more stringent test in this joint application than they have ever been before. (ibid.)

So, scientists *qua* scientists conjoin theories only to test their empirical adequacy under conjunction, not for prognosticative purposes. Nothing more is needed for scientific theory building and evaluation, and constructive empiricism can account for that activity. Thus, neither negative argument for axiological scientific realism holds up. I am aware of no positive argument either, so it would seem that we are not compelled to accept either a realist or an anti-realist account of science's governing axiology.

10) Conclusion

Let us briefly take stock. The formulation of constructive empiricism undercut the realist's *negative* argument for their position, providing a new anti-realist empiricist alternative to scientific realism in both its axiological and epistemological varieties that seems workable (i.e. internally coherent and consistent with the evidence). The soundness of the main *positive* argument for epistemological scientific realism, the NMA, depends on the validity of IBE. The realist interprets this rule as instructing us to treat explanatory power as evidence of truth, and looks to justify it naturalistically by claiming that the scientific method needs to make central use of it. But science only needs to use IBE if it's governed by a realist axiology, i.e. if it aims to identify true theories. The constructive empiricist denies that axiology, however, and seems capable of rationalizing all saliently scientific activities in terms of her own understanding of the aim of science. There is no positive argument for axiological scientific realism, so the epistemological scientific realist's naturalistic justification of IBE appears baldly question begging to the anti-realist empiricist because it's based on the ungrounded assumption of a realist axiology. With no non-question begging argument in favour of axiological scientific realism, the constructive empiricist can reasonably deny any naturalistic justification of IBE, IBE's use in the NMA, the cogency of the NMA itself, and the position the NMA seeks to establish: epistemological scientific realism. And while the constructive empiricist offers a positive argument for her position, it likewise appears question begging and unconvincing to the realist, for it appeals to a value realists don't typically hold: the avoidance of inflationary metaphysics.

Thus, both the scientific realist and the constructive empiricist seem to have staked out rationally permissible positions, but have little hope of convincing their opponent to change their mind. In short, and the debate has been "argued to a stalemate" (Monton 2007, p.3). The apparent intractability of this stalemate leads Fine (1986a) to recommend we reject both positions and avoid investigating their conflict any further, especially since it seems to make no practical difference whether we accept realism or anti-realism. Scientists, for example, seem capable of operating as realists, anti-realists, or with no philosophical interpretation of science at all, so it seems impractical to pour any more effort into arguing about these matters. If we value our own time, Fine suggests, we need to recognize that scientific realism "is well and truly dead, and we have work to get on with, in identifying a suitable successor" (1986a, p.112).

As a philosophical successor to all scientific realisms and anti-realisms Fine proffers the "Natural Ontological Attitude," a non-realist interpretation of science that tows a "homely line" about truth and the reality of entities (ibid., p.128). It goes roughly like this: accept the results of modern science in the same way that you accept the results of perception, but resist any impulse to add something more. Both the realist and the anti-realist, it is supposed, accept theories such as General Relativity and the theory of evolution by natural selection, for example. But the anti-realist wants to add that "truth" means something different when it comes to unobservable entities, that the epistemic standing of unobservable entities is somewhat less than the objects of everyday experience, that "acceptance" of a theory means something very specific, or some other qualification. The realist, similarly, wants to add an account of what it means for entities to be real, what it means for theories to be true, to deny certain anti-realist foils, and perhaps to pound the table insistently a few times to command acceptance of the idea that certain entities "really, really" exist. Fine asks us to stop all of this, to resist both the realist's and the anti-realist's impulse to say something more, to conclude our thoughts with the "core position" that accepts the results of science, and to simply leave it at that. To accept NOA is to refuse to engage any further in the scientific realism debate.

As noted at the outset of this chapter, I share much of Fine's analysis. Nevertheless, I argue that there is still something to be gained by engaging in debate over the relative merits of scientific realism and various anti-realisms. Indeed, I contend that there are even ways we might "convert" would-be realists or anti-realists by giving them reasons to change their minds, to switch sides as it were and adopt a different philosophical outlook on science. Such reasons will even be based in empirical evidence, and will rely for their persuasiveness on people's commitment to rationality and widely shared values. But identifying such reasons will require us to give up a key methodological assumption behind most philosophical arguments: that the way to persuade someone with one position to accept a different one is to show that the prescribed position is the one that *every rational person* would adopt, in *any place* and at *any time*. In giving up the catholic method of converting non-believers, which aims to convince people through appeal to supposedly universal truths, I recommend a more pragmatic and tolerant approach that accounts for and appeals to the practical considerations arising from an individual's particularities. I suggest we look to determine which position a *specific* rational person should adopt, given their idiosyncratic practical contexts, commitments, and values. While it might be discomforting for those analytic philosophers used to the catholic approach, the fact of the matter is that giving someone a compelling reason to adopt a belief, attitude, or commitment does not require this reason to be valid for everyone, or even for that same person at all times and in all places. And with respect to scientific realism, I argue, we can give *individuals* good, practical reasons for adopting or rejecting it, based in rational considerations of empirical evidence, without giving *everyone* good reasons.

These good-but-not-universal reasons, as we will see in the following chapters, will be expressly pragmatic-but-not-epistemic reasons. More plainly: no argument for adopting scientific realism or anti-realism that I suggest may be developed in the following chapters will claim to show that some position is true, correct, or rationally preferable *per se*. Of course, when deciding whether to adopt a philosophical position, it's certainly important to know if there's any reason to think it's true. Such reasons are some of the most edifying reasons that someone committed to being rational and following the evidence could have, because they tell them what they should accept *qua* rational being, interested in finding the truth by following the evidence. But even if we hold truth as our highest value, and consider only epistemic reasons for adopting a position when first considering whether to adopt it, we can move on to consider practical, context-sensitive reasons for adopting it if we find there are no epistemic reasons. This is what I aim to do with the issue of scientific realism. The scientific realism debate may have reached a stalemate as a debate over which position is true, or rationally preferable *per se*, but that stalemate is limited to those standards. If we look for pragmatic reasons to be a realist or an anti-realist, we may still make progress in the scientific realism debate.

Chapter Two: Stances, Stalemates, and Philosophical Progress

1) Introduction

The previous chapter aimed to show why the debate between scientific realists and constructive empiricists has been argued to a stalemate. The aim of this chapter is to show how, even if we accept this stalemate, and conclude that both scientific realism and constructive empiricism are rationally permissible philosophies of science, there's still a way to engage in productive debate about which position we should each accept. To this end I explicate the voluntarist analysis of the scientific realism debate developed by van Fraassen (2002; cf. 1989, 1992, 2000, 2001, 2004, 2005). My focus will be on elaborating the way that, on this analysis, rationality and evidence can inform our evaluation of different philosophical positions. While some have concluded that on a voluntarist analysis there is no way to argue one's opponent out of their position using appeals to rationality or evidence (Chakravartty 2011a, van Fraassen 2002), I show how evidence might be used to give people good, pragmatic reasons to abandon their current position and adopt their opponent's. The scientific realism debate might productively proceed, therefore, by looking to give our opponents such pragmatic reasons to change their mind.

I begin with a discussion of what it might mean to make progress in philosophy once we accept that a debate has reached a stalemate. Following this discussion, I elaborate van Fraassen's voluntarist analysis of his conflict with scientific realists, then specifically address its conception of rationality and the different ways we might try to change an opponent's mind according to this analysis. I then show how it seems feasible to give people pragmatic reasons to be a realist or an anti-realist, which I attempt to do in the fourth chapter. Before moving on to develop a methodological framework for providing such pragmatic reasons in the third chapter, I conclude this chapter by addressing the worry that voluntarism is an intolerable form of epistemic relativism that reduces our philosophical positions to mere matters of taste.

2) Making the Most of our Intellectual Resources: Philosophical Progress and Philosophical Stalemates

Progress in philosophy is not impossible, but it's often limited and hard to come by. Surveys such as the one recently conducted by David Bourget and David Chalmers (2014) show that there's significant disagreement amongst professional philosophers regarding many of the "big questions" of philosophy. Nevertheless, consensus is often reached regarding many smaller issues, e.g. that a particular philosophical thesis implies a contradiction, that one position is more tenable than its rivals given certain assumptions, that certain arguments are invalid or unsound, or that a given thesis does not cohere with some other generally accepted thesis. This suggests that the knowledge produced by philosophical inquiry is "mainly knowledge of the answers to smaller questions, of negative and conditional theses, of frameworks available to answer questions, of connections between ideas, of the way that arguments bear for and against conclusions, and so on" (Chalmers 2015, p.12). But answers to questions like "what is knowledge?" or "is there an external world?", at least when measured by consensus, seem to elude professional philosophers.

Even when it is achieved, philosophical consensus does not always appear progressive, either. Sometimes consensus forms around results that seem to suggest some long hoped-for progress is impossible, e.g. when philosophers come to agree that a debate they've been having for decades (or even centuries) is, in fact, ultimately irresolvable. For instance, epistemologists now generally agree that the debate between skeptics and non-skeptics will never be "resolved," i.e. it has reached a stalemate.[41] Skeptical challenges are based on valid arguments, whose premises cannot be definitively rejected without begging a significant question, so skepticism can't be decisively refuted; and yet, at the same time, most non-skeptics contend they are justified in believing that it is one of their two, real, existing, flesh-and-blood hands that is furiously pounding a desk while debating an incorrigible skeptic. This despite neither side being able to conclusively show their position to be the exclusively rational choice. Anyone that has taught an introductory epistemology course knows how disappointing this stalemate over skepticism can be for students that feel the force of the skeptical challenge and want a cogent argument to

[41] As John Greco (2007, p.210) notes, showing that we are not in a skeptical scenario was never really the point of addressing skeptical arguments in epistemology. The point, rather, is to show how knowledge is possible despite skeptical arguments. The "stalemate" has, in that sense, always been accepted by epistemologists. While this is not a consensus position by any means, I would argue that there is a stalemate on this issue as well: that is, I would argue that both skepticism (knowledge is impossible) and anti-skepticism (knowledge is possible) are coherent and rationally permissible positions.

elide their worries that they're a brain in a vat. The formulation of a theory of knowledge (i.e. a set of necessary and sufficient conditions for knowledge) that defuses the skeptical challenge to the very possibility of knowledge (e.g. by denying epistemic closure) may satisfy the epistemologist by showing how we can have knowledge despite that challenge. But any skeptic can reasonably deny that theory, so a response to skepticism that satisfies the epistemologist doesn't prove the skeptic wrong as (for example) Descartes (1641) hoped to. The "big questions"—do I really know that I'm not a brain-in-a-vat, or that there is an external world?—remain unanswered, and seem unanswerable with any degree of certainty, and those who really worry about them will forever be haunted by the spectre of skepticism.

In cases like this it's unclear what progress has been made by realizing the stalemate, as people holding opposed positions are left to maintain their preferences, but with a kind of epistemological chasm separating them. Perhaps more importantly for the working philosopher, in cases like this it's unclear whether there is any new knowledge, understanding, or potential for conversion of one's opponents to be gained by continuing to research the conflict between such positions. Either one wants to be a skeptic or not, to believe they have knowledge or not, and it seems that further research will never show which view is "correct," so continuing the debate is unlikely to get us anywhere. Perhaps we would all be better off then—as individuals, as an academy, and as a society—if we spent our time and intellectual resources elsewhere. At least the stalemate is now recognized for what it is, but in moving forward perhaps we should just cut our losses and avoid the topic altogether. Perhaps that's the only kind of progress philosophers can hope for after determining that a debate is stalemated in such a way: the progress of moving on.

The sense that most of philosophy is a matter of being locked in perennial, endless, and fruitless debates over topics of little consequence is fairly prevalent these days, leading to its declaration as a bankrupt (Krauss 2012, p.xiv) or dead (Mlodinow and Hawking 2010, p.5) discipline by widely respected scientists operating as public intellectuals. While criticisms such as these may be confused about the facts of the matter when stated universally—philosophy surely achieves some limited forms of progress, and does contribute to our lives in unique and important ways—they are important to keep in mind when thinking about whether (and if so, how) we should continue engaging in philosophical debates that appear to have reached a stalemate. Certainly it's possible to waste intellectual time and talent on fruitless debates, and in a world of limited cognitive resources amidst various social crises we should be careful where we spend our precious intellectual capital. Thus, when we discover that a philosophical debate has reached a stalemate, we should be careful to delineate the extent of the stalemate, determine on what assumptions it subsists, and note what fruits may or may not be borne from continuing to discuss the topic in various ways. This is precisely what I aim to do here with respect to the scientific realism debate.

While I discuss only the stalemate surrounding the debate between scientific realists and constructive empiricists, this will prove an important general point: even if we reach a stalemate in a philosophical debate, this does not mean there is no more progress to be had, that there is no more work to be done, or that continuing to discuss the merits of different positions is a waste of time and thought. Indeed, if I am right, discovering a stalemate between two philosophical positions can be the basis for a renewed, progressive, and socially and existentially important philosophical discussion, capable of providing new information relevant to people's choice between those positions. All that's required is shifting the traditional standards by which analytic philosophers assess different positions in two ways. First, we need to shift away from assessing the rational preferability of opposed positions according to epistemic standards, i.e. trying to determine which position is "true." Instead, we should assess philosophical positions according to pragmatic standards, i.e. trying to determine which position is "useful."[42] For even if we can't determine which position is true, we may still be able to determine which position is most useful; the truth of a position and the benefits of adopting it are certainly different issues, but they may both be relevant when trying to form a rational preference for one position over alternatives.[43] Second, we need to shift away from assessing philosophical positions according

[42] For an example of how attending to the pragmatics of philosophical commitment might move ethical debates beyond a stalemate position, see Legault et al. (2013).

[43] It's worth explicitly pointing out that, in suggesting we pragmatically evaluate scientific realism and constructive empiricism, I am not advocating philosophical pragmatism. Pragmatists tend to see no difference between the usefulness of a claim and its truth conditions; as Rorty (1981) sees it, for instance, "true" is merely an honorific bestowed upon a claim or doctrine by a community when accepting it serves their interests. But as I explain below, it will be important for my approach that we keep truth and practicality distinct, so we can ground our judgements about the latter in judgements about the former. As such, my distinction between "epistemic reasons" and "pragmatic reasons" is closest to that found in discussions of doxastic voluntarism within analytic epistemology (e.g. Reisner (2008, 2009, 2017); cf. Hendry 1995, p.65). For more on pragmatist theories of truth see Peirce (1878)

to catholic, categorical, or universal standards, i.e. trying to determine what's rationally preferable *per se*, that is, rational for *everyone*. Given the diversity of ends and interests people have, different people may find different positions useful for them. I suggest we embrace this result rather than fight it. Figuring out that one position is most likely to help some specific person achieve their ends is still a kind of philosophical progress, I think, even if we also figure out that an opposed position is most likely to help someone else achieve different ends.[44]

This is not the traditional approach to philosophical issues amongst analytic philosophers, where determining the true or universally preferable position is usually the aim. Consider, for example, the opening question of Bertrand Russell's *Problems of Philosophy* (1912): "Is there any knowledge in the world which is so certain that no reasonable man [sic] could doubt it?" (p.2). The part of this sentence worth focusing on here, of course, is the word "reasonable." Russell goes on to discuss our ordinary beliefs about the external world, our usual probabilistic inferences, and several other philosophical issues, aiming to determine which position the "reasonable" person he refers to at the outset should take on these issues. The implicitly central task of philosophy, for Russell, is to determine what a *rational* person should accept, *qua* rational person. A stalemate appears, and appears troubling, when it seems that no position can be established as true or rationally preferable *per se,* as it would seem is the case regarding the conflict between scientific realists and constructive empiricists. Our inability to decide the scientific realism debate according to these standards is assumed in what follows where I ask: assuming we can't determine which position is true, or rationally preferable *per se*, is there any point in continuing the debate, and if so, how should we proceed?

Despite holding conflicting positions in the philosophy of science, at least one eminent scientific realist and one eminent anti-realist seem to accept that their debate has reached a significant

and James (1907), and for pragmatist conceptions of rationality see Niznik and Sanders (1996, esp. p.85), Moutafakis (2007, esp. p.13-14), and Rescher (2014, esp. Ch.10, 12).

[44] I suspect that much of this has already been recognized and more systematically studied by Immanuel Kant, who built his entire philosophical method around recognizing when philosophical debates had reached stalemates (or as he called them, antinomies) and then using practical considerations to break the deadlock. I am addressing issues as they arise in the contemporary scientific realism debate, so it's beyond the scope of the present work to connect, compare, or contrast my approach to the scientific realism debate with Kant's philosophical method in much detail. Investigating the Kantian character of van Fraassen's meta-epistemological voluntarism is a project worth undertaking, but it is not the one undertaken here (cf. van Fraassen 2002, Ch.1).

stalemate of this sort: Anjan Chakravartty and Bas van Fraassen. The stalemate, as I understand it, is exactly as Monton puts it: "Scientific realists cannot conclusively show that belief in the literal truth of scientific theories is epistemically warranted, but constructive empiricists cannot conclusively show that the aim of science is limited in the way they describe" (2007, p.3). That is, Chakravartty has no argument for the truth or rational preferability of his realist epistemology of science that van Fraassen finds compelling, nor does van Fraassen have one for his position that Chakravartty finds compelling. But despite holding conflicting positions regarding the kind of epistemic warrant that scientific inquiry provides, neither needs to see the other as irrational to see themselves as rational. This is made possible by their sharing a meta-epistemological position known as "voluntarism."

In what follows I focus specifically on the stalemate between Chakravartty and van Fraassen, proceeding from within their shared meta-epistemological assumptions. As such, much of what I say will only be of interest to those who find van Fraassen's voluntarist meta-epistemology somewhat appealing, but I think those who do will find that some of the implications and possibilities for philosophical progress within this framework have been overlooked to date.[45] My question can be made more precise as follows: assuming voluntarism, and that neither realism nor anti-realism can be demonstrated as true, is there any possibility that rational arguments and evidence might compel a committed scientific realist or anti-realist empiricist to switch their position? Put differently: if we accept that they've reached a stalemate, could van Fraassen or Chakravartty nevertheless be shown that the rational thing to do would be to abandon their current position and accept the other's? To address this question, we'll first need to get clear about the nature of meta-epistemological voluntarism.

3) Meta-Epistemological Voluntarism: Rationally Agreeing to Disagree

[45] This is not to say that van Fraassen's voluntarism is under-studied. The majority of the papers in Monton (2007) address voluntarism in some manner, *Synthese* put together an entire special issue on Epistemic Stances and Rationality in 2011, and a volume on van Fraassen's account of representation in 2014, with a quarter of the papers focusing specifically on voluntarism in each (in addition to many other excellent treatments of the issues relating to van Fraassen's voluntarism).

On a voluntarist analysis, one's epistemology of science (e.g. scientific realism or constructive empiricism) is not primarily derived from the bottom-up, i.e. from the evidence. Rather, it's primarily derived from the top-down, developed and adopted on the basis of one's "epistemic stance."[46] Epistemic stances operate at a higher level of abstraction than epistemologies of science, and can be understood roughly as a set of epistemic policies for the generation of an epistemology of science (Teller 2004). Once developed and adopted through an epistemic stance, an epistemology then serves as a theory of evidence or justification, and determines how (and which) ground-level, factual beliefs are deemed warranted at any given time, e.g. what is "properly believed" given the evidence provided by scientific inquiry. The epistemologies of science that van Fraassen and Chakravartty each hold can both be seen as rationally permissible, despite their opposition, because the different epistemic stances from which they are developed can both be seen as rationally permissible, despite being irreconcilably opposed.

Not all epistemic stances are rationally permissible (or simply "rational"), on the voluntarist's account. But the conception of rationality that constrains one's choice of stance at the meta-epistemological level is much thinner than the conceptions that philosophers usually appeal to in their arguments. Rationality is often said to compel, determine, require, or demand specific things from us, to show us that one and only one belief, commitment, or action is the correct one (van Fraassen 2000; cf. Kolodny 2005, Bridges 2009). But on the voluntarist's conception, rationality is nothing more than a kind of "bridled irrationality" (van Fraassen 1989, p.172; cf. 2000, p.277), such that an epistemic stance is rational so long as it's not demonstrably irrational. So, whereas rationality is often treated as a guiding force that tells us which view to adopt, regardless of our prior opinions or commitments, the voluntarist sees rationality as only telling us if we need to abandon those prior opinions or commitments because they're irrational. The question for philosophical reflection then becomes not which view to adopt, but whether we need to reject some view we already hold; asking what it means to be rational is no longer a matter of asking why we should be committed in some way, but "why should we not be committed as we are?" (Teller 2011, p.66). Van Fraassen (2000) puts it this way:

[46] Van Fraaseen (1994c and 2002) develops the notion of an epistemic stance as a way of identifying traditions like "empiricism" over time, despite differences in the doctrines of individual empiricists. There he emphasizes the philosophical advantages of seeing different philosophical traditions (like empiricism, materialism, etc.) as different stances, allowing us to see what unites (for example) empiricists like Helmholtz with the Scottish empiricists, logical positivists, and van Fraassen himself. These are also historiographical advantages, as we will see in Ch.4.

> We supply our own opinion, with nothing to ground it, and no method to give us any extra source of knowledge. Only the 'empty' techniques of logic and pure math are available either to refine and improve or expose the defects of this opinion. That is the human condition. But it is enough. (p.279)

The constraints imposed by this conception of rationality on the acceptability of epistemic stances may be permissive and never definitive, but they are not really "empty" or insignificant. They are much like (perhaps even identical to, at least in part) the constraints imposed on our beliefs by logic according to David Miller (2006):

> Logic contents itself with indicating what we should not accept, the evidence being what it is, and what we should not do, and what we should not prefer … that does not imply that we may not go beyond the recommendations of logic. Logic permits what is does not forbid." (p.127)

In van Fraassen's view (2002) the constraints imposed by rationality on the permissibility of any given epistemic stance are not purely formal, they are also conceptual. As the voluntarist sees it, a stance would need to be abandoned as irrational just in case it was discovered to be internally incoherent, e.g. not just if it was shown to contain or imply a contradiction, but also if it relied on some fundamentally nonsensical concepts.[47] But otherwise a stance is, in and of itself, rationally permissible. In this way, the rationality of an epistemic stance is an internal matter; it may contravene the standards of an opposed stance, but what matters is that it doesn't contravene its own.

The conception of rationality operating at the meta-epistemological level of stance evaluation is this thin because it is the closest thing to a form of rationality admitted by *all* epistemic agents, regardless of which epistemic stance they adopt (van Fraassen 2004, p.183). Any more substantial form of rationality (e.g. one that includes IBE, or any other ampliative rule of inference) is based in an epistemology that, as we saw in the last chapter, is often hotly contested. But because epistemologies are developed through epistemic stances, whatever narrower conception of rational inference is given by an epistemology often cannot be used to assess an

[47] Few (if any) real people are likely to hold coherent positions of course, objectively speaking; we all likely hold incoherent beliefs without recognizing that we do so. So, in evaluating the rationality of an epistemic stance, or a person's adoption of it, I think this demand is best interpreted subjectivity. It is not irrational to hold an incoherent stance, but it is irrational to hold a stance one *knows* to be incoherent (cf. Psillos 2007, p.155-6).

opposed stance, e.g. if that stance leads to a different epistemology opposed to that narrower conception of rational inference. This is why the constructive empiricist isn't compelled by the realist's NMA: the rule upon which it's founded (IBE) doesn't have the same status or interpretation in both epistemologies. The rational constrains on the permissibility of any given stance are entirely limited to the demands of internal coherence, leaving us with "epistemic options," as Peter Lipton (2004) put it. Rationality alone doesn't determine a single epistemic stance as uniquely rational, it merely bars some of them because they are internally incoherent. While epistemic stances, in and of themselves, are deemed rational just in case they seem to be internally coherent, one's *holding* of a particular stance over alternatives is judged differently, using factors external to the stance itself. Specifically, one's holding a stance is evaluated pragmatically, in terms of whether that stance is self-defeating in light of that person's values. For example, Chakravartty (2007a, 2007b) holds "the metaphysical stance," whose policies enjoin the search for explanations through speculation about the unobservable things underlying the observable (2007a, p.188). While this sort of metaphysical speculation about the nature of unobservable reality may be epistemically risky, it's the rational stance for Chakravartty to hold because he values the satisfaction of his own curiosity more than the minimization of his epistemic risk. Van Fraassen, by contrast, holds "the empirical stance," which forbids such metaphysical speculation, and it's the rational stance for him because he inversely prioritizes the same values; to him, the epistemic risk of engaging in speculative metaphysics is not worth the potential gain.[48] He clarifies as follows:

> Pragmatic factors, in fact values, do inescapably play a role in how we manage our opinion. For we must inevitably stick our neck our and form beliefs that go beyond our evidence—the extent to which we do so, the risk we take, can only be up to us, there can't be anything in the evidence to dictate *that*. That a risk is worth taking is a value judgement. (2007, p.345)

Chakravartty and van Fraassen each hold an epistemic stance that seems best for them, given their values. In rationalizing their maintenance of that stance, their values play an essential

[48] Van Fraassen (1989, p.172; 2002, p.86) discusses this in terms of William James's idea that we form all opinion by weighing two, conflicting aims: to believe what is true, and to not believe what is false. Each aim could be satisfied fully and easily by either believing everything or nothing, but in balancing them we are forced to make a value judgement "contextually qualified by our interests and values (2002, p.90). My exposition here more closely follows Chakravartty's (2011), which I find simplifies the matter, though each way of putting this point should be considered equivalent (cf. Psillos 2007, p.148-50).

role. For if van Fraassen switched to the metaphysical stance, he would quickly violate his value of epistemic caution by engaging in risky speculative hypothesizing about the nature of unobservable reality. This would be self-sabotage, and therefore an irrational choice for him. And yet, the metaphysical stance is rational in itself because it is internally coherent, and is the rational stance for Chakravartty because it matches his values. And while their epistemologies suggest different beliefs about the world are warranted by the available scientific evidence, as meta-epistemological voluntarists neither has grounds to condemn the other as behaving irrationally, for they are each only accountable to their own values. Someone else's stance may appear irrational according to *my* values, but what matters is that it's rational according to *their* values. Chakravartty and van Fraassen both maintain different commitments, but if their stances are not internally incoherent, and fit their values, the voluntarist sees them as acting rationally in refusing to change their minds.[49]

It might seem that this voluntarist analysis reduces the debate over scientific realism to a mere conflict of values, chosen for idiosyncratic and non-rational reasons before the debate has even begun, constrained only by an extremely thin notion of rationality that permits so many positions it is hardly a constraint at all.[50] Most participants in the scientific realism debate no doubt hope to resist such an understanding of their long-running dispute as just an unrestrained expression of values. One of the first things many academic philosophers are wont to impress on their first-year philosophy classes is that philosophy is expressly *not* just a matter of just stating one's opinion. Philosophical disputes are not supposed to be subjective or relativistic in that sense, heavily dependent on subjective values. Philosophers aim to critically investigate the strength of arguments, not freely express and reinforce their prior opinions. But it seems like the stance-based analysis of the realism debate skirts dangerously close to this way of understanding philosophical discourse, where we are all just in our own Neurath-style boats,

[49] The idea that agent-relative (i.e. subjective) factors play essential roles in our epistemic lives is a central insight of feminist approaches to epistemology (e.g. Code (1991)). While I don't investigate the connections between feminist approaches to epistemology and van Fraassen's meta-epistemological voluntarism here, for sake of simplifying the text, in a future work I intend to expressly show how some of van Fraassen's critiques of traditional epistemology (e.g. in his (2000)) have been anticipated by various feminist critiques of mainstream epistemology.

[50] Alspector-Kelly points out the lack of constraint imposed by rationality according to voluntarism by saying that it is "so wildly permissive that it countenances as rational belief-sets that are obviously completely crazy, including belief-sets which completely disregard all empirical evidence" (2012, p.189)

ungrounded and adrift at sea, pushed along only by the winds of our own antecedently held values (van Fraassen 2002, p.139).

To be sure, voluntarism leads to a kind of meta-epistemological relativism, and many of van Fraassen's interlocutors find this very worrisome (Ladyman 2007, p.59; Psillos 2007, p.145; Chakravartty 2004, 2007b, p.184, 191, 195), as does van Fraassen himself (1989, p.175-80). If one cannot legitimately (i.e. persuasively) criticize someone who holds a coherent stance for contradicting the stance one prefers themselves, or an opponent's choice of stance for not living up to one's own values (Chakravartty 2007b, p.190-6), that certainly sounds like relativism, and like philosophical discourse is just a matter of expression one's opinions. But the relativism of voluntarism isn't absolute, nor especially pernicious. It is not a relativism about truth, importantly, only about the meta-epistemological rationality of certain epistemological positions. Even if Chakravartty's realist epistemology leads him to accept the truth of certain claims about unobservables while van Fraassen's anti-realist epistemology does not, and rationality is unable to condemn either of them given the available evidence, the claims that either of them accepts are either true or false, objectively. Our epistemological positions and factual beliefs are still accountable to an objective notion of truth and falsity on this view (van Fraassen 1989, p.175, 178-80). Voluntarism isn't relativistic in the sense that what's true for one person need not be true for someone else; truth is agent- and value-*independent*, even if what's rational to believe is agent- and value-*dependent*.

Furthermore, within a voluntarist framework, it's possible that someone who adopts the metaphysical stance might convince someone who adopts the empirical stance to change their mind (or vice versa) using rational arguments that appeal to agreed-upon, objective facts. Indeed, there have been several attempts to do so, using two different argumentative strategies. None of the arguments made using either of these strategies seem to have been successful, and the strategies themselves don't seem to be very promising, but the possibility of using arguments and evidence to change people's commitments is certainly there. In section five I argue there's an under-investigated yet promising third strategy, and pursue that strategy in the chapters that follow. I turn now to a discussion of how exactly voluntarism conceives of rationality, as this will be important for getting clear about how, within a voluntarist framework, rational arguments based on evidence can lead people to see that the rational thing to do would be change their stance.

4) The Nature of Rationality According to Voluntarism

Chakravartty (2011) identifies two importantly different ways in which we might come to hold an epistemic stance. First, we may simply be acculturated into a stance, just as we may be acculturated into any set of beliefs, habits, attitudes, or customs. In such cases stance adoption is just a matter of unreflective opinion, "with nothing to ground it," as van Fraassen puts it (2000, p.279). But, Chakravartty notes, we should distinguish between "merely taking a stance, which can be essentially passive, and choosing a stance, which is the outcome of reflection" (2011, p.41; cf. Psillos 2007). Upon reflection, we hope to have *well-informed* judgements about whether it's rational to continue holding our acculturated commitments. Let us deal here only with the latter case, where upon reflection we look to see whether we have any good reason to abandon our prior commitments, e.g. because they are incoherent and therefore irrational in themselves, or because they lead us to violate our values and are therefore irrational for us to maintain.[51]

While the voluntarist's conception of rationality is thin, constraining our commitments rather than compelling them, it's interestingly unified in the sense that it incorporates both theoretical and practical elements. We judge epistemic stances *themselves* using theoretical rationality, based on their internal coherence, and judge people's *holding* of an epistemic stance using practical rationality, based on whether that stance will help them achieve the ends given by their values. For the voluntarist, theoretical and practical rationality are both relevant when we rationalize or evaluate our epistemic stance upon reflection.[52]

[51] For an insightful phenomenological discussion of the difference between a) explicitly taking a stance by self-reflectively evaluating it and b) implicitly taking a stance, without reflection, see Ratcliffe (2011).

[52] In fact, while I have teased apart these two aspects of the voluntarist's notion of rationality here, they are less well differentiated elsewhere, generally seen as just different aspects of the general idea of rationality as coherence (e.g. van Fraassen 2000, 2001, 2002, 2007; Chakravartty 2007a and 2007b). The unity of theoretical and practical rationality is also reflected in much of the recent literature devoted to characterizing each mode of reflection (e.g. Moran 2001, Bratman 1987, Korsgaard 1996).

Theoretical rationality deals with questions bearing on the truth of our positions, e.g. their consistency with the evidence, internal coherence, etc. Kant called this "pure reason" and saw it as the arbiter of truth in all judgements, both formally (analytic judgements) and materially (synthetic judgements). To say that theoretical rationality cannot determine one and only one epistemic stance or epistemology of science as rational is to say that there is more than one stance that is internally coherent and consistent with the empirical evidence. While I do not mean to bind voluntarism to a Kantian perspective, the inability of theoretical rationality to determine which epistemic stance is uniquely rational is consistent with Kant's contention that pure reason is unable to decide an issue that transcends experience. As we saw in Ch.1 (sec. 9), the realism debate is just such an issue, as empirical evidence does not seem capable of deciding it, i.e. theoretical rationality cannot decide the debate by telling us which view is likely true. Theoretical rationality can, however, tell us if a position is incoherent and therefore certainly false, even if it can't tell us that all but one is incoherent.

This vision of philosophical debate and critique, as always a matter of assessing the coherence of a given stance, is explained by Chakravartty (2011) as one where "philosophers investigate various forms of conditional knowledge, premised on and shaped by the stances to which they are committed" (p.45). Showing that anti-realist empiricism can account for scientific revolutions as rational events (van Fraassen 2002, Ch.3), and that scientific realism can identify the portions of scientific theories likely to be maintained through such radical theoretical shifts in non-*post hoc* ways (Chakravartty 2007a, Ch.2), could be considered as forms of progress from within a voluntarist conception of philosophy. One might have thought each of these views was incoherent, given the existence of scientific revolutions, but with some work they can both accommodate the known historical facts of scientific change. But, Chakravartty adds, "so long as rival stances are rational, we are likely to see them perpetuated. And thus, we should *expect* that there will be little or no philosophical progress in the sense of winnowing stances, and thus in the sense of progressing from one to another, or towards any ultimate consensus" (2011, p.44-5, emphasis in original). So, while we can regularly make philosophical progress within the conditionalized confines of an epistemic stance, and in doing so can uncover facts about whether a given stance is truly rational or not, we should be less than sanguine about the prospects of philosophical work helping us eliminate one stance as rationally untenable; dialogue between stances seems to bear little fruit, even if it is strictly speaking possible that it might.

If we assume that we will never demonstrably show either the empirical or metaphysical stance to be irrational, the real anxiety engendered by voluntarism presents itself: perhaps there's no way to change someone's commitments through argument and reflection. That is, voluntarism is worrisome because it might suggest we can't use rational deliberation to change other people's minds, or even worse, our own. Nevertheless, I think this anxiety is misguided. Even if we assume that theoretical rationality alone will not show someone that they should change their stance we have reason to expect that, together with practical rationality, arguments and evidence can still change people's minds.

Practical rationality can be distinguished from theoretical rationality by being concerned not with what's true, but with which action will achieve certain ends. Whether someone's stance fits their values is judged, primarily, using practical considerations about whether it will help them achieve the ends given by those values. If someone maintained an epistemic stance that led them to transgress their values—if someone who valued epistemic caution over satisfaction of curiosity held the metaphysical stance, for instance—they would be acting irrationally, sabotaging themselves by their own lights, even if the stance itself is not irrational *per se*.

Most authors (including Kant) see theoretical and practical rationality as importantly unified. The naturalness of such a unity becomes clear if we understand belief as a kind of action, and "believing what is true" as a kind of end (cf. Foley 1987). In that case the distinction is in terms of the interests they serve, not their innate character; we apply theoretical rationality when our end is believing what is true, and practical rationality when our end is anything else. In either case rationality is used to help us achieve some end. It is an important distinction nevertheless, because it shows that it can be rational to hold or reject a belief or position for reasons that have nothing to do with its truth. If I am in an important meeting, for instance, and see through the window that it's not raining outside, believing it's not raining is rational because it achieves my end of believing what's true; but if there are no windows in the room, and I suddenly remember I left my car's window open, believing it's not raining is rational because it achieves my end of participating in the meeting without being distracted. Those two types of reasons for belief are importantly different: we would call the former kind of reason *epistemic* or *evidential*, as it's a reason to believe something because it's true; we would call the latter kind of reason *pragmatic*, *practical*, or *prudential*, as it's a reason to believe something because it's useful to believe, regardless of whether it's true.

In the present context, maintaining the distinction between theoretical and practical rationality allows us to distinguish epistemic reasons for rejecting a stance from pragmatic reasons for rejecting the same stance; the former only help us achieve the end of accepting what's true, while the latter help us achieve other sorts of ends. The distinction is important, I argue, because there are real prospects for finding rationally compelling, pragmatic reasons to change one's epistemic stance, even if there are no prospects for finding rationally compelling, epistemic reasons.

5) Three Voluntarist Strategies for Challenging Stance Choice

Most of the challenges to realism and anti-realism discussed in the previous chapter—that the pessimistic induction shows realist inferences to be unreliable, that constructive empiricism's notion of "observability" is too vague to bear the needed weight, etc.—can be thought of as based in theoretical rationality: they are challenges to the internal coherence of whatever epistemology is developed through a particular stance. Adequate responses either a) show that the criticism is based on a misunderstanding of the position, or b) formulate a new epistemology that accords with the same epistemic stance but corrects the incoherence identified by the critic. As Chakravartty (2007b) writes, these challenges almost always target the coherence of an epistemology or set of factual beliefs; but since "stances are not identical to the [epistemology or] factual claims with which they may be associated at any given time" (p.192), even if one version of scientific realism were incoherent, this would not make the metaphysical stance itself irrational (cf. van Fraassen 2002, p.62). Hence why responding according to option (b) listed above is an appropriate way to defend one's epistemic stance as rationally permissible; if van Fraassen's constructive empiricism was shown to involve overly metaphysical elements, for example, it would be rationally permissible for him to maintain the empirical stance and work to develop a new anti-realist empiricist epistemology of science. To truly compel a rational agent to abandon a stance by using theoretical rationality to show that it is rationally impermissible, one would need to show that the stance would *inevitably* produce an incoherent epistemology or set of beliefs. So, for example, van Fraassen (1989) once argued that any metaphysical outlook *must* rest on the concept of a law of nature, but that this concept is *necessarily* incoherent. Nevertheless, Chakravartty (2003, 2007) defuses the challenge by developing a conception of laws that seems coherent, at least internally, and is thus able to maintain the rationality of the metaphysical stance. This is a very hard kind of criticism to make stick on one's opponent, and no one seems to have done so yet.

Let's assume that neither the scientific realist nor the anti-realist empiricist will succeed in changing the other's mind by this strategy, because neither the metaphysical stance nor the empirical stance is fundamentally or inevitably incoherent. What do we do then, if we want to show someone it would be rational to abandon their current commitments?

It's the ability of those who adopt the metaphysical and empirical stances to seemingly evade all criticism based on theoretical rationality that makes people like Fine (1986a) feel that continuing to have this debate is a waste of time. But even if arguments based in theoretical rationality are incapable of convincing anyone to change their stance, a voluntarist analysis suggests another way. One might criticize their opponent by arguing not against their stance but against their values; perhaps the empiricist is wrong, objectively, to value epistemic caution so much, or the realist wrong to value the satisfaction of their wonder at all. If one's value judgements were deemed irrational, compelling them to alter those judgements, then the constraints of practical rationality could indicate a need to change their stance as well, if not doing so would be self-defeating by the lights of their new values.

Chakravartty (2011a) investigates this possibility, especially Cartwright's attempt (2007) to argue for the empirical stance by appealing to practical values. He rightly finds this strategy a non-starter, "doomed before we begin" (Chakravartty 2011a, p.47). While there are certainly those who see values as objective and compelled by rationality, it's not clear that they are; at the very least, it's not clear that the kinds of values informing epistemic stance choice are appropriately objective, or compelled by a notion of rationality agreed to by all relevant parties to the debate. The voluntarist sees people's selection and prioritization of their values as an act of will, or an expression of their emotions, not something subject to objective determination (van Fraassen 2002, Ch.5). In short, by introducing human choice and passions into our epistemic lives it seems that voluntarism leaves us unable to condemn people who hold different epistemic stances than the one we hold by attacking the objectivity of their values (Forbes 2016, p.7-9).

Let's also assume that neither the scientific realist nor the anti-realist empiricist will succeed in changing the other's mind by this strategy, because the value judgements informing epistemic stance choice are not subject to objective critique. What do we do then, if we still want to show someone it would be rational to abandon their current commitments?

If attacking a position or set of values as incoherent and irrational exhausted our options for using arguments and evidence to get people to change their epistemic stance, the philosophical debate over scientific realism would be locked in an intractable stalemate. In trying to change people's minds, philosophers are distinguished from rhetoricians, sophists, politicians, and bullies by relying only on rational argument based in evidence and analysis; so, if appeals to rationality couldn't change people's minds on some issue, philosophy couldn't change people's minds on that issue, basically by definition. But empirically investigating the ways a stance can pragmatically fail to serve people's values, I argue, is a third strategy for convincing people that it's irrational to maintain their current stance that holds great potential. We've assumed that we cannot show an epistemic stance itself to be irrational, or someone's choice of values to be irrational or wrong in some objective way. But we might still show that someone's holding *that* stance is irrational given that they hold *those* values, e.g. because it's unlikely to help them achieve the ends given by their values. By focusing on this third way, I argue, the stalemate in the scientific realism debate can be admitted, limited, and moved past.

Here's what Chakravartty (2007) says about the notion of practical rationality, that of irrationality *cum* self-sabotage, used to assess the rationality of someone's stance, given their values:

> [It] is broad enough to include such unfortunate circumstances as believing contradictions and probabilistically incoherent combinations, as one might do on the level of facts, but it may also include circumstances in which the stance one adopts has pragmatic failings, such as consisting in combinations of attitudes or policies that tend to undermine or conflict with one another. (p.191).

Whether a stance has pragmatic failings of this sort, certainly, is a pragmatic, empirical question about whether a given stance helps someone achieve the ends given to them by their values. Rather than arguing that an opponent's stance is incoherent, or that their values are irrational, criticisms of this kind would argue that their stance does not, in fact, serve their values, and therefore transgresses the constraints of practical rationality. While he does not pursue the thought very far, Psillos (2007) brings up this possibility specifically in the context of discussing how the voluntarist might show, on objective grounds, that one epistemic stance is the wrong position for someone, given the goals provided by their values:

> Even if goals are not objective, how one might go about achieving one's goals may well be an objective matter […] relative to a certain goal X, it is a factual issue whether method M is a reliable means to achieve X, or whether method M is more reliable than M' for X. There is, then, a factual way to make comparisons: some methods are better than others. (p.149-50)

It is by making just such factual comparisons that, I suggest, realists and anti-realists can still engage in substantive debate within a voluntarist framework, and potentially show an opponent (or themselves) that, so long as they wish to remain rational, they need to abandon their prior commitments and switch sides. By focusing on this third way we can see that the epistemological chasm between Chakravartty and van Fraassen, and others like them, is not unbridgeable (cf. van Fraassen 2014).

6) Some Assumptions about Values

Before going any further, something should be said about the nature of values assumed here. Value theory is an expansive field of philosophy, but to remain focused on the matter at hand I'll avoid substantial engagement with that literature. My account is closest to that provided by Joseph Raz (1999, esp. Ch.3), and arguments in favour of most of my assumptions can be found there.

Rather than taking values as purely mental attitudes like aims, intentions, or desires, or constituted by specific behaviours, actions, or dispositions, I identify values with things, aspects of the world that provide us with practical reasons for action (Raz 1999, p.1).[53] Specifically, I call anything a value that provides someone with, or constitutes for them, a goal or end because having, maintaining, or producing that thing matters to them in one way or another. Knowledge, spiritual enlightenment, scientific progress, hunger, honour, having big muscles, cheese, justice, and pleasure could all be taken as values in this sense. Because values are identified with objects rather than behaviours or mental states, people can be said to *fail* to serve, fit, meet, accommodate, instantiate, satisfy, maximize, live out, or live up to their values when their choices do not achieve the relevant end. Whatever verb is judged most appropriate to characterize the connection between values, ends, and appropriate action, the key point is that people will be said to *violate* their values—that is, to commit willful self-sabotage, and therefore act irrationally—if they act in a way that they *know* will (likely) fail their values, despite also knowing some other option would not.

One's value sets must, at least sometimes, be ordered by their relative importance to the person that holds them. This allows for more highly ranked values to supersede or trump other values when they demand mutually exclusive ends, i.e. when they are incommensurable.[54] I may value both beer and good times, for example, but if I value moderation, clarity of thought, my job, and my liver more, I may decline a friend's request for one more drink. Then again, my value of that friendship may trump everything else. The prioritization of one's values may also change over time, as may the values one has. And unless one is a consciously principled person, value-rankings may often be indeterminate until one's values come into conflict. Many of our most difficult decisions, I think, are these sorts of situations, where we're not trying to determine the consequences of our actions, but to determine which of our values should get priority in deciding which consequences are best, or least bad. Determining the likely consequences of our actions, however, is always of central importance for rationalizing our choices according to whether they've proven capable of helping us achieve the ends given by our values, or are likely to do so in the future, or instead are likely to lead us to commit self-sabotage. And while our choice and prioritization of different values may be based primarily in subjective judgements, our

[53] For more on the nature of values see Schroeder (2016, sec.1). For more on the nature of reasons, see Raz (1999, Ch.4).

[54] For more on the issue of incommensurable values see Manson (2015) and Hsieh (2016).

evaluation of which action is likely to serve those values should be based primarily in the objective facts, i.e. the evidence.

7) Pragmatic Stance Selection: Ends, Values, and Effective Policymaking

One's choice between the metaphysical and empirical stances is often treated as being informed only by the prioritization of two values: epistemic caution vs. the satisfaction of curiosity (e.g. Chakravartty 2011a, van Fraassen 2002, p.15-7). Considering only these two values, one's choice of epistemic stance seems straightforward. The search for explanations enjoined by the metaphysical stance, for instance, seems like the obvious choice for anyone that values satisfying their curiosity more than maintaining epistemic caution. It's right for them almost by definition. But if an epistemic stance is evaluated according to a more complicated value set, it won't always be so clear which stance will help someone maximize those values or lead to self-sabotage by such lights.

While thinking in terms of only two values may be useful for explicating the voluntarist's picture of how rationality constrains epistemic stance selection, both theoretically and practically, taken as a description of our own lives the idea that we make our choices by only considering two values at a time is a grotesque oversimplification. People exist within a great variety of practical contexts, and have large, complicated, and evolving value sets that can, should, and no doubt do inform their epistemic policymaking. Values like scientific progress, technological development, wealth, joy, safety, community, and a great many other things beyond epistemic caution and the satisfaction of curiosity are no doubt relevant when choosing an epistemic stance. Judged against those values, many of which might rank for some people well above epistemic caution and curiosity satisfaction, it won't be immediately obvious which epistemic stance will be best. Thus, within a voluntarist framework, we can empirically investigate whether different stances are likely to help someone maximize or violate their entire set of values, and to conclude that, if the evidence suggests that maintaining their current commitments will lead to self-sabotage, *the only rational choice is for them to change their commitments*. The consensus view regarding best practices for policymakers, in fact, requires that policies be evaluated and maintained by gathering evidence about how effectively they achieve their intended ends, and that policies should be rejected when they're evidently ineffective;[55] proper epistemic policymaking at the meta-epistemological level should be no different.

Paying attention to the practical aspect of rationality that operates at the meta-epistemological level of epistemic stance selection, then, is the key to seeing philosophical debate as still capable of changing people's minds about scientific realism and anti-realism within voluntarism; that is, it's the key to moving past the stalemate in the scientific realism debate without denying it by providing a way for people to use evidence and argument to convince an opponent that they've chosen the wrong stance, according to that opponent's own lights. Consider the values typical of scientists, educators, and policymakers, for instance: which epistemic stance is most likely to help them achieve their ends of making scientific progress, practicing effective pedagogy, or formulating good science-policy? Well, to be honest, I'm not sure, but there's probably an answer, even if it's that they're equally likely. Finding the answer will take empirical research, however, as it will depend on what the evidence tells us regarding whether people who adopt one stance more regularly achieve certain ends than people who adopt the other. And if the evidence shows that one stance *is* more likely to serve some value, anyone who holds that value would be violating it if they adopted a different stance.

If substantial philosophical debates are those that have the potential to show people that the rational choice would be to change their mind, debates about which epistemic stance best serves someone's entire set of values can certainly be substantial within a voluntarist framework. While the scientific realism debate may be stalemated when restricted to arguments about the irrationality of a stance or set of values, or the truth or universal preferability of some epistemology or set of factual beliefs, there's no reason to think that the debate over which epistemic stance best serves certain values, in certain contexts, is stalemated too. Quite the opposite. As I show in the next two chapters, the kind of evidence needed to provide people holding certain values, and operating in certain contexts, with pragmatic reasons to reject one stance and adopt another can likely be found by taking what I call "a pragmatic, existentialist approach to the scientific realism debate."

---

[55] See, for example, the evidence-based policymaking guide produced by the Pew Charitable Trusts and the MacArthur Foundation (2014, p.4), or basically any literature on evidence-based policymaking.

8) Epistemic Relativism and Practical Rationality

To be sure, the approach I'm advocating does not aim to produce a radical critique of the metaphysical or empirical stance, the kind of argument that might demonstrate that every rational person must adopt one and reject the other. The sorts of evidence-based arguments for or against certain epistemic stances that I aim to construct, through appeals to the constraints of practical rationality, will only be persuasive for people with certain values, in certain contexts. Thus, a certain amount of epistemic relativism remains inevitable within a meta-epistemologically voluntaristic analysis of the conflict between scientific realists and anti-realist empiricists. But this is a relativism we can work with, in the sense that there are still ways to use evidence, analysis, and appeals to rationality to convince our opponents to change their position, or ourselves to change our own, even if we can't change everyone's mind all at once. The idea is that, if presented with sufficient evidence that the epistemic stance they presently accept is less likely to serve their values than an alternative stance, people may be "driven into a corner" (van Fraassen 2002, p.91) to the point that they're forced to admit that changing their minds is the only rational option.

Given that realists and anti-realist empiricists hold different theories of evidence, however, there's a concern that any evidence suggesting a pragmatic reason to abandon one stance and accept the other will not be agreed to by all parties. But as will become clear in what follows, this shouldn't be a problem in any debate between scientific realists and anti-realist empiricists. For those who accept the metaphysical and empirical stances are likely to give the same answers to questions regarding which epistemic stance is most likely to help someone maximize some specified set of values. Empirical demonstrations of the ability of an epistemic stance to help maximize some set of values, therefore, are not relative to one stance, epistemology, or set of values. Not only are they "objective," in that there is a truth of the matter, they are also "inter-subjective," in that those who adopt the metaphysical or empirical stance will judge such truths in the same way. Thus, such evidence, when it can be produced, should be able to inform someone's evaluation of their stance through considerations of practical rationality, regardless of which stance they presently hold and regardless of which stance such evidence was produced through.

A final limitation of this approach should already be apparent, but must be clearly stated. Arguments that someone should reject an epistemic stance based in practical rationality, because evidence shows it's likely to lead that person to violate their values, can be thought of as providing *pragmatic reasons* to reject the stance, its associated epistemology, and the system of beliefs that epistemology warrants. Pragmatic reasons for accepting or rejecting a belief or position relate specifically to its usefulness for achieving an end *other than* believing what is true and not what is false. Thus, the results of this approach to the scientific realism debate will indicate nothing about the truth of realism, anti-realism, or our scientific theories.

Nevertheless, investigating pragmatic reasons for accepting or rejecting different epistemic stances does not mean abandoning all concern with or need for truth and epistemic reasons. For having a pragmatic reason to adopt one stance rather than another will always require having epistemic reasons to believe some claim is true (cf. Mohler 2007, p.213; Raz 2009). In general, for someone whose end is x to have a *pragmatic* reason to take action y in context z, they will need an *epistemic* reason to believe a claim of the form "in z, doing y will most likely result in x." To clearly illustrate this, take an example of someone who, unlike the philosopher or scientist, is not guided by obviously or immediately epistemic ends: someone writing a work of fiction. To say that they have a pragmatic reason, *qua* fiction writer, to reject the empirical stance would be to say that they have an epistemic reason to think that maintaining that stance would likely have a negative impact on their ability to write well. But this need not be because maintaining that stance would likely lead them to hold false beliefs, or less true beliefs than they would otherwise; perhaps they simply know from experience that the limits it places on speculative thought stunt their creative thinking somehow.[56] Practical rationality, in this way, always depends on theoretical rationality, pragmatic reasons for action arise from epistemic reasons for belief, pragmatic arguments are grounded in evidence, and ensuring the practical rationality of our choices will always require determining the truth of some relevant matter. Nevertheless, adopting or rejecting a stance because it will likely help achieve some end (other

[56] At least, not in any straightforward sense. It's an interesting question whether pragmatic reasons for belief are always simultaneously epistemic reasons, e.g. if believing something helps us achieve our ends *because* it leads to more true and/or less false beliefs. I don't address this issue here, and follow the judgements of many epistemologists concerned with voluntarism in more distinctly doxastic matters by assuming that, *prima facie*, genuine pragmatic reasons for accepting an epistemic stance are possible. See Reisner (2008, 2009, 2017), Raz (2009), Vahid (2010), and Whiting (2014) for some suggestive comments on these matters; see Chignell (2017) for a thorough outline of the many issues addressed by doxastic voluntarists, and section 2.1 for this issue in particular.

than believing what is true and not what is false) should not be taken as a reason to think that stance, its associated epistemology, or the factual beliefs to which one is thereby led are true or false; pragmatic reasons and epistemic reasons *for the same* belief or commitment may overlap, but are generally independent.[57]

9) Conclusion

The following chapters will probably not give any philosopher of science a pragmatic reason to choose a different stance than the one they currently do, for I look to provide evidence that some working scientists have pragmatic reasons to choose one philosophical outlook over another, given their professional values. This is an appropriate starting point because there is already some interest in determining the methodological fruitfulness of adopting different philosophical views in the practice of science (e.g. Wray 2015, Hendry 2001), even if van Fraassen (1980, p.93) has rightly separated such issues from the question of how to interpret the axiology and epistemology of science. But I don't expect the evidence I present to be definitive for working scientists either. My ambitions are quite minimal: I aim only to show that, if we accept a voluntarist meta-epistemology, there are still reasons to think we can change people's choice of stance using nothing more than arguments based in evidence. That is, within van Fraassen's voluntarist framework, philosophical reflection based on rational consideration of evidence can still affect our choices at the meta-epistemological level, even if we accept that the traditional realism debate has reached a stalemate.

In the next chapter I sketch out a general methodology for investigating which stance best serves a given set of values, constituted by two assumptions, a model of choice, and an embodied understanding of epistemic stances. The adoption of this methodology need not be given any

[57] The difference is sometimes put this way: a pragmatic reason to hold a belief or position is a reason to "accept" it, not to "believe" it. Acceptance is often thought to be something distinct from and less than belief, a kind of "acting as if" one believed, but I avoid relying on this distinction because it is not at all unproblematic and raises a host of issues beyond both the needs and scope of this project. See van Fraassen (1980, 2002, p.90), Cohen (1989, 1992), and Velleman (2000) for more on this issue. See also van Fraassen (2002, p.89-90), where he leaves this terminology aside because it is unimportant in his efforts to decide the issue at hand, a decision I find appropriate here as well.

justification other than that provided by the success of the studies it aims to support, should it prove effective at identifying non-obvious associations between taking a certain stance and achieving success in certain kinds of practical activities.

For many philosophers of science interested in the scientific realism debate I expect this will look like a worthwhile endeavour, even if only for personal reasons; determining which stance, in fact, best serves a given set of values, in a given context, can help each of us determine which stance is best for us, relative to our own values and contexts. That, I think, will prove to be valuable information. It also presents a way forward for the scientific realism debate in the face of an otherwise intractable stalemate, and does not threaten to conclude thirty-five years of debate over constructive empiricism and scientific realism by declaring it a war between dogmas or sheer acts of the will. At the very least I hope we can all agree that continuing the debate in the manner I'm suggesting would not be a waste of our intellectual resources, even if Fine (1986a) is right that continuing the science realism debate according to the traditional standards of philosophical assessment would be. Perhaps we can't answer the "big question": which position should I adopt because it's true? But with some work we may be able to answer an arguably more important question: which position should I adopt because it will help me achieve my ends?

Chapter Three: Towards a New Approach to the Scientific Realism Debate

1) Introduction

The previous two chapters focused on understanding why the debate over scientific realism seems to have reached a stalemate. The first chapter looked at the various extant arguments for and against scientific realism, as well as those for and against what's currently considered its main rival, constructive empiricism. It then diagnosed the failure of all these arguments to establish either position as true or rationally preferable *per se*. The second chapter placed the failures of those arguments within a meta-epistemologically voluntaristic analysis of the conflict. This analysis sees people's adoption of scientific realism or anti-realism as resulting not from their commitment to rationality and following the evidence *per se*, but more primarily from their holding certain values that lead them to adopt specific epistemic stances, understood as collections of epistemic policies meant to help enact those values in one's epistemic life. Nevertheless, it is widely recognized that this analysis suggests there are still prospects for resolving the debate by showing that adopting one stance either implies a contradiction or is inevitably self-defeating. The chapter concluded with the suggestion that there are still other ways to provide people with evidence-based, context- and value-dependent *pragmatic* reasons to adopt a realist outlook on science rather than an anti-realist one, or *vice versa*. To date, such reasons have not been sufficiently investigated. This chapter is concerned with providing a methodological framework through which these investigations can be conducted.

To conduct these investigations, I first suggest that we explicitly give up the idea that rationality and evidence are the only legitimate grounds for our epistemic commitments. This means accepting that the role ascribed to values in our epistemic life by an epistemologically voluntaristic analysis is legitimate, even if it means that rational individuals need never converge on one epistemic outlook or set of beliefs, given the same set of evidence. The variety of social roles and contexts that humans occupy often requires that different people prioritize or emphasize different, even opposed values, even in cases where as a group they are collectively committed to a common goal. This is not a bad thing: the social good of peace, for example, might require that a nation's president prioritizes the values of diplomatic communication, disarmament, and de-escalation while the soldier prioritizes battle readiness and military might.

In the next section of this chapter I suggest that by evaluating different epistemic stances according to such role-specific value sets we can better understand the differential benefits of each stance for people operating in such roles.

Second, I suggest we accept that sometimes we simply cannot persuade people to change their values, given that we can't condemn those choices as irrational without circularity. And ultimately, I think, there's also nothing wrong with that either, at least insofar as it's simply a reality of human existence. What we value, even if we value concrete objects like family or wine, is simply not determined solely by objective facts about reality (Frondizi, 1971); that is, our decisions about what to value cannot be entirely resolved through "objectifying inquiries," but will ineliminably involve a subject element of choice (cf. Chakravartty 2011a, van Fraassen 2002). While values can be challenged, debated, and sometimes changed, at some point in any argument over what to value we must, as van Fraassen (2000) put it, "supply our own opinion, with nothing to ground it" (p.279). What this means is that our values are, for the most part, ultimately dependent on our own free choices. Here I do not support this claim with any arguments, however, and merely forward its acceptance on methodological grounds, for we need to temper any impulse we might have to try and argue against the values that people choose to hold. What the pragmatic, existential approach aims to do is discover some non-obvious practical consequences of taking one epistemic stance rather than the other, so that those consequences can then be squared with people's values. Thus, a methodological posit that helps us resist the urge to challenge such values is well motivated.

Third, I suggest we specifically accept that pragmatic reasons for epistemic commitments are legitimate. When analytic philosophers are considering whether to accept some position, or believe some proposition, what matters most to them is whether they are warranted in doing so, i.e. whether they have reasons to think that position or proposition is true. Such epistemic considerations are certainly important, for they constitute reasons for belief that are universally valid for all rational, truth-seeking agents, *qua* rational truth-seekers. I agree that there is an important difference between epistemic and pragmatic reasons for holding a position or believing a proposition, and that epistemic reasons should usually take priority (cf. Stroud 2006). All I recommend is that we accept both as legitimate reasons for our doxastic attitudes, at least in certain contexts, for methodological reasons.

Fourth, we'll need a way to understand how we choose philosophical outlooks, one that pays attention to context, values, and pragmatic considerations, not only to considerations of rationality and evidence. This requires modelling such choices in analogy to something that occurs in more ordinary forms of lived experience, rather than in the philosopher's armchair where thought is at its most disembodied. As such, despite a somewhat pedestrian feel, I suggest we model our choice of philosophical positions on the way we choose between dinner options at a restaurant. The considerations of someone sitting in an armchair considering a range of views they might commit to are of a quite different character than the considerations faced by a diner at a restaurant with a server standing in front of them waiting to take their order. I think using the latter as a model for how we might choose between epistemic stances, rather than the former, will help us include an element of "force" to the choices that people sometimes need to make regarding philosophical positions, an element that is entirely absent during the contemplative armchair experience.

And fifth, it will be helpful to understand an epistemic stance as not just an epistemic policy or merely intellectual attitude, but importantly as a collection of "concrete strategies for coping with objects and coordinating with others … a way of holding your body and readying it for action and worldly engagement" (Kukla, forthcoming, p.1) that can have unexpected and unintended consequences. Adopting a philosophical position may be, first and foremost, an intellectual activity, but it is unlikely to be behaviourally inert, for accepting one view rather than an alternative will generally dispose someone towards (inter)acting in certain regular ways they would otherwise not be disposed towards (even if those (inter)actions are entirely restricted to the way one behaves in university classrooms, e.g. by verbally avowing or defending that position). Placing the embodied aspect of epistemic stances front and centre will help us to assess the ways in which opposed stances present differential practical consequences for those that adopt them, especially the more surprising and unintended consequences.

This chapter is occupied with motivating and elaborating these five assumptions, and proceeds through each in order. By the end this will provide a framework through which we can work to uncover empirical facts about the practical consequences of adopting either the metaphysical or empirical stance that might bear on people's reflective assessment of the merits of adopting one over the other, given their idiosyncratic values and context. In the next and final chapter I will use this framework to conduct a case study that exemplifies the sort of research it makes possible. While the discoveries of that study are more suggestive than conclusive, it should be sufficiently illustrative of what this approach makes possible to justify further application of it. I conclude this work by discussing possibilities for further application of this approach.

2) Evaluating the Preferability of Epistemic Stances: Preferable for Who?

There are a wide range of contexts and value sets, at varying levels of generality, against which we might attempt to evaluate the practical consequences (and derivatively, the preferability) of adopting different epistemic stances. I might try to evaluate the respective instrumentality of the metaphysical and empirical stances for myself, in some context, by trying to determine which one is most likely to help me achieve the ends given to me by my own values. I could do the same for a roommate, spouse, or colleague, according to their ends, values, and context. Those would be at the lowest and most concrete level, that of the truly particular. At higher levels of abstractness and generality we might begin to precisely specify an ordered set of values, and a specific context, and see if we can come to a determinate answer about which stance is pragmatically preferable for someone holding that conjured up set of values, in that conjured up context. But the most promising and worthwhile approach, I submit, would be to address *role-specific* sets of values. With respect to the scientific realism debate, for instance, we can ask and attempt to answer questions about which epistemic stance best serves the role-specific values of the working scientist, the science policymaker, the historian of science, the science educator, or the science critic. Such an approach should also provide enough information to help in the more particular and concrete cases, such as the question of which view is more likely to help *that* working scientist achieve their goals, in *that* context; indeed, conducting role- and context-specific investigations of the differential instrumentality of holding various philosophical positions is interesting and useful, I think, primarily for its potential to provide actual people occupying those roles and contexts with edifying information about which position is best for them, pragmatically speaking.

As an example of the kind of study I'm suggesting, the next chapter presents an historical investigation of the way their different philosophical conceptions of science influenced electrodynamics researchers in late 19th century Europe. This case study suggests, as a kind of working hypothesis to be confirmed or disconfirmed by further historical and empirical investigations, that different forms of scientific practice are better motivated and supported by the adoption of different epistemic stances. If further study demonstrates the historical robustness of these motivational and supportive linkages between epistemic stances and certain forms of scientific practice, at different periods but in similar contexts, I suggest we will have achieved our goal: giving individual working scientists a pragmatic justification, on empirical grounds, for adopting one epistemic stance rather than another, assuming they occupy the right practical context and value scientific progress. That is, we'll have shown some scientists which epistemic stance, empirically speaking, seems most likely to support their work, giving them *good reasons* to adopt that stance. Surely such information is worth looking for, even if it won't tell us which stance provides the correct epistemological or axiological theory of science *per se*.

I should be clear that the aim of this work is not to provide such reasons, i.e. to conclusively establish the historical robustness of the connections identified in the final chapter. I do not assume that one case study is sufficient to establish a general rule, and that a working experimental physicist will read my discussion and decide she should adopt a specific philosophical outlook to facilitate her research goals. Rather, I aim only to motivate conducting more of these sorts of investigations (Ch.1 and 2), provide a methodological framework for conducting them (Ch.3), and then produce a compelling example of what such investigations might look like if conducted using historical methods (Ch.4).

3) Making Informed Choices with Groundless Values: A Pragmatic, Existentialist Approach

As a first step towards determining which epistemic stance is best for a specific (type of) person, given their specific context and set of values, I recommend, on methodological grounds, that we simply abandon the idea that values can (or at least need) be ultimately grounded or motivated in anything other than human choice and action. People's values can and regularly are debated, of course, in all kinds of complicated ways, and changed or modified through such debates; but ultimately, I would argue, at some point all value commitments will bottom out in free, individual choices. And given that we're aiming to empirically investigate the way that certain epistemic stances serve specific value sets, rather than investigate which values are to be preferred, I think this assumption will serve us well in focusing on the issues at hand.

This understanding of how we come to value certain things over others accords well with an understanding of values that I generally associate with existentialism, so I will refer to it as an "existentialist approach to the question of value selection," or more briefly "the existentialist assumption." I recognize that the existentialist label will be judged inappropriate by many, for reasons that need not be gone into here, but I can think of nothing better to signify the assumption.[58] For as I take it, the central contention of existentialist thought is that the values we hold and the meanings we supposedly "find in the world" arise, in fact, out of our own choices and practices, whether social or individual, rather than being determined for us by some external authority such as God, Rationality, evolutionary pressures, or an immutable human condition.

Philosophically speaking, taking an existentialist approach to the question of value selection means accepting that we are, ourselves, ultimately responsible for the values we adopt, and that we cannot defer the choice to some other agent. But this existentialist assumption is here understood only methodologically, and methodologically speaking this assumption simply means never trying to argue anyone out of their values, or to argue that everyone must (for whatever reason) accept a single set of values. To be clear, I only suggest that we make such an assumption with respect to the question of how values are selected. This does not entail that choosing an epistemic stance in service of those values is similarly a matter of unrestrained free choice. For even if we consider values to be ungrounded and ungroundable in non-volitional things evidence and rationality, and see them instead as free choices, any downstream adoption of the metaphysical stance (and thereafter some form of scientific realism) or the empirical stance (and thereafter some form of anti-realist empiricism) to serve those values need not be

[58] It should be noted that the idea of working through the scientific realism debate on pragmatic and existentialist considerations was originally put forward, in those terms, by van Fraassen (2000, p. 273, 2002). He notes that, by taking a meta-epistemologically voluntarist perspective on the debate, "[t]he element of personal decision, values, and volition has entered and received a legitimate place in our epistemic life" while also noting that "[by] itself, however, this element is no cure-all" (2002, p. 91). His account is more suggestive than the version I present here in terms of explicit, methodologically grounded assumptions for further inquiry into the connections between epistemic stances and success in certain practical activities.

considered objectively ungrounded and ungroundable. Indeed, there must be some objective, determinable, fact of that matter about which epistemic stance best serves some set of values if my approach to informing stance selection is to work at all.

According to the meta-epistemologically voluntaristic framework we're working within, it's rational to choose the stance which best serves one's values, so as not to be self-defeating by one's own lights; however, I contend, it is not always obvious which stance is more likely to be self-defeating by one's own lights. This is precisely why we need to conduct empirical studies of the practical consequences of stance adoption, to determine which epistemic stance (and resultant philosophy of science) is likely to prove most effective for someone who holds a specific set of values, in a specific context. The existentialist assumption should help us focus on that task by preventing us from arguing over the values themselves. The idea is that if we simply accept that the values informing any individual's epistemic stance choice cannot be given any ground other than that individual's free choice, and that values will therefore likely and legitimately differ between rational epistemic agents, we can then move on to empirically investigate whether there is any evidence that can ground an individual's epistemic stance choice because it seems most likely to help them achieve the ends given to them by their values.

Even as a matter of methodology this may be an uncomfortable assumption for any philosopher used to universalizing argument, which strives to determine which position amongst an array of options should be accepted by anyone and everyone, regardless of their individual circumstances. It might even be objected that this assumption ultimately leads to a pernicious and unpalatable form of relativism, for combining an existentialist approach to the question of value selection with a voluntarist account of epistemic stance selection means nothing less than accepting that different epistemic stances will be rational for different people, or even for the same person in different places and at different times. Such objections are misplaced. Universalizing arguments about the values informing epistemic stance choice have, to date, failed, and seem likely to continue to fail (Chakravartty 2011a). An existentialist approach to the question of value selection is specifically meant to help us avoid that apparently fruitless mode of argument. Furthermore, justifying substantial philosophical assumptions on the basis of their methodological fruitfulness is not without precedence. It is quite common in the historical and sociological study of science (see Latour and Woolgar 1979), for example, where the acceptance of philosophically suspect views on the basis of their methodological fruitfulness has produced some truly fascinating results (Shapin and Schaffer 1985, Arabatzis 2006, etc.). If the existentialist assumption nevertheless proves too uncomfortable, I invite those interested in conducting empirical studies of how philosophical outlooks effect behavior to find some suitably weaker assumptions to work with. But I think it's clear that the existentialist assumption is sufficient for the purpose here intended: preventing people from arguing over the acceptability of certain values, or over which values are "correct."

The second methodological assumption will be to admit as legitimate the influence of pragmatic considerations on one's choice of epistemic stance. Most voluntaristic analyses of the scientific realism debate to date (e.g. Chakravartty 2004, 2007a; van Fraassen 2002) have only investigated the ways that different *epistemic* considerations will impact epistemic stance choice, i.e. the desire to believe what is true and not believe what is false (see van Fraassen 2002, Ch.1). But, of course, real people consider many non-epistemic factors when making important choices: wealth, liberty, equality, community, good governance, joy and happiness, technological progress, etc. Such non-epistemic considerations, I suggest, should be treated as having a legitimate role in our philosophically reflective choice of epistemic stance. This is especially unproblematic when epistemic considerations alone seem incapable of telling us which choice to make, as we've assumed (for good reason) is the case when choosing between the metaphysical and empirical stances.

Cartwright (2007) suggests explicitly that non-epistemic considerations are important for determining our choice of epistemic stance, but she still focuses on grounding or motivating stance and value choice in *universal* considerations, attempting to motivate the universal adoption of the empirical stance by appealing to highly general facts about the practical context of everyday life. Chakravartty (2011a) has shown that her argument does not go through at this level of generality, however, but only holds for anyone who prioritizes (contra Thales) a life of practicality over a life of wonder and curiosity. That is, appealing to non-epistemic considerations does not work in Cartwright's argument specifically because she appeals to values that are not universally held, e.g. by those that value a life of wonder and inquiry over the demands of practicality. But Cartwright is right, I think, in allowing that non-epistemic considerations can properly impinge upon our epistemic stance choice, even if she is wrong in attempting to universally justify a single stance on the basis of the supposedly universal demands of human existence. As long as we stick with the existentialist assumption and don't get caught up trying to argue about which values are "correct" for everyone, the assumption that non-epistemic (i.e. pragmatic) considerations can properly influence our choice of epistemic stance ensures that we are able to address the circumstances of real individuals, rather than abstracted, decontextualized "knowers" who only have epistemic considerations in mind at all times. Such abstracted individuals may be useful posits for the philosopher or epistemologist in certain circumstances, but if we're looking to provide concrete human agents with edifying information about which epistemic stance is best for them, such an abstraction will likely prove pernicious.

In short, these two methodological assumptions will help us avoid any impulse to try and determine which epistemic stance or set of values is "correct," "right for everyone," or "rationally preferable" *per se*, narrowing our focus on investigating which epistemic stance is most likely to help some specific (type of) individual, whether hypothetical or actual, achieve their ends. In addition to these two methodologically motivated commitments it will help to have a simple model for thinking about how people choose an epistemic stance based on their values and context, as such a choice is conceived from within a meta-epistemologically voluntaristic framework. The model I suggest below is useful because it illustrates how, even within this framework, making a good choice of epistemic stance does not entirely reduce to one's choice of values, but depends importantly on empirical information about which stance is most likely to help them effectively achieve the ends given to them by those values.

4) The Menu Model of Stance Selection

To reiterate, it's important to be clear about the aim of approaching the scientific realism debate in the way I'm suggesting. The hope is that, in the end, people will gain access to new information that *they* might find relevant when deciding which epistemic stance to adopt, that *they* might find shows that it's rational to change their position. The sorts of studies these assumptions are meant to facilitate are not meant to determine people's beliefs, i.e. to use the "force of reason," evidence, or anything else to dictate which epistemic stance someone should choose, based on their values and context. No one can ever fully understand or experience other people's practical context or value judgements, so people's judgements about what it's rational for them to do will always be theirs alone, even if those choices can be well- or ill-informed by evidence provided by others. Thus, I don't intend to use any information that may be uncovered by studying the practical consequences of adopting different epistemic stances in various practical contexts to enact some kind of doxastic paternalism, insisting that certain people must choose one stance over another because I know what's best for them. My hope is only that people who are mulling over which epistemic stance is best for them will, in their own judgement, find the information provided by some investigations of the practical consequences typical of adopting each position helpful. In the end, people's judgement regarding which epistemic stance is best for them will always have to be theirs, not mine or anyone else's.[59]

Overall, what I'm suggesting is this: if we seek a firm grounding for our discussion of the conflict between scientific realists and anti-realist empiricists, in the sense of some universal account of the human condition, it should be something like Korsgaard's Sartrean dictum that "human beings are *condemned* to choice and action" (2009, p. 1, emphasis in original). Speaking at the very least from personal experience, this seems to be the best available description of the contemporary situation surrounding the scientific realism debate. When I think about the realism debate in its current stalemated state, I for one—and I'm certain there are others out there—sometimes feel similarly to how I feel ordering off a menu in a restaurant. Do I choose the lobster, or the salad? Speculative metaphysics or anti-metaphysical empiricism? Whether it's easy to make the choice or not, I generally feel the need to make it; it often appears to me as a "forced" choice, in William James's sense of the term (James 1896). I cannot simply defer my decision to the dictates of some other factor such as "rationality" or "my values" when considering whether to make the search for adequate explanations a central aim of my epistemic life, or to forswear such efforts. That would be like saying to a waiter asking for my order, "I'm a determinist, I'll just wait and see what happens." Seated there in the restaurant, my personal context will insist that I make a choice, eventually, whether through the waiter's stink eye, my growing hunger, or even the intervention of the police if I sit there long enough refusing to choose for myself. And just like when a waiter comes to take my order in a restaurant, where the social situation pressures me to make a decision even if I'm still considering my options,

[59] Many thanks to Catherine Elgin for pointedly stressing the need to make this clear.

there are practical contexts in which I feel pressured to make a choice between a metaphysical vision of science and an empiricist one, e.g. during political debates over the validity of certain scientific results, during the critical evaluation of science reporting, during spiritual discussions that touch on cosmological issues, or during philosophical deliberations on other topics such as the rationality of scientific revolutions or what it means to be a meta-philosophical naturalist. But if we accept explicitly that whether we end up as a scientific realist or an anti-realist empiricist is based in part on a free choice at the level of values, just as the choice between having the lobster or salad is based in part on a free choice between certain values, then we can get on with the pragmatic issue of making sure our choices are *well-informed*, i.e. determining which option is most likely to serve our specific set of values.

Call this the "menu model" of epistemic stance selection. It helps clarify a few things. For one, it shows us that epistemic stance selection, like ordering a meal, is not always an easy or straightforward process, as it's not always clear which option best meets our preferences in either case. To make a well-informed choice of meal we need to answer a variety of questions before we know which choice best accords with our system of values and priorities at that moment. We need determine what our values are, for example, and how they rank against each other: Do I value nutrition over flavour? How should I balance cost against my desire for deliciousness, or nutritiousness? Is my craving for cheese more important than my desire to avoid gastrointestinal distress? Are there social values—such as not appearing "cheap" for skipping the tip, or "vulgar" for ordering a grilled cheese—that are more important to me, in this moment, than the cost? And furthermore, should I eat light and try to get some work done after dinner, or can I gorge myself and then safely slip into a post-prandial digestive slumber? Some of these values will be partially grounded in unalterable, objective, yet agent-relative facts about one's allergies, intolerances, and personal finances, while others may result from religious or social commitments, or similarly free choices. Either way, the purpose of taking an existentialist approach to value selection is to help us take such values, once determined and prioritized, as given. For once we have our values determined and prioritized we can focus on answering those empirical questions relevant to determining which menu option best serves them: What is the macronutrient profile of the various options? Do any of the ingredients in any of these dishes give me indigestion, or allergic reactions, or contravene any dietary restrictions I've put on myself for moral, political, or religious reasons? Which meal is likely to be the most filling? What are the relative costs, how much can I afford to spend, are there any specials, and, hey guys, does anyone want to split an appetizer? These are empirical questions, relevant to our decision making in some practical context, and even if we already have an ordered and unquestioned set of values, answering them will require us to collect non-obvious information through additional inquiry (looking at the nutritional information, checking the price, learning our fellow diners' social habits, etc.). Taking a pragmatic, existentialist approach to the scientific realism debate is meant to help us find information that can properly play a similar role in the process of epistemic stance selection, even though gathering such information will likely take more effort than asking whether a restaurant's soup is made with vegetable or beef broth.

So, let's treat the question of which epistemic stance to adopt similarly to how we treat a dining quandary, and see what comes of it. That's my proposal. Let's use empirical methods to study which "epistemic option" is most likely to help people fulfill whatever wants, needs, and values they find themselves having in a specific context so they can make the most well-informed choice possible. Responsible diners make well-informed meal choices by seeking out information about which option is most likely to give them the calories and vitamins they need without giving them indigestion or wreaking havoc on their finances. Likewise, responsible scientists, policymakers, and educators, can make well-informed meta-epistemological choices by seeking out information about which epistemic stance is most likely to support their practical activities. And while there is, of course, reason and room to debate the values informing stance choice, let's use the existentialist assumption to carefully avoid that conversation, just as a polite friend would avoid interjecting to inquire about someone's personal finances while the table discusses shared tapas options, or suppress their impulse to debate the reality of "gluten sensitivity" while someone is asking the server about gluten free options. While such issues are certainly relevant to making good choices, and worth debating at the right time, keeping discussions about what values one should adopt separate from discussion about what choices are most likely to serve certain values should help us more clearly and easily determine which epistemic stance is likely to work best for different (types of) people, given their values and contexts, rather than getting bogged down in discussions of whether people should be holding those values in the first place.

As has already been indicated, the next chapter illustrates how we might provide the sort of information relevant to the choices of one group of people—working scientists—through historical study of the way that philosophical commitments have influenced scientific practice. The menu model is illustrative of how the history of science might prove informative for working scientists when evaluating their philosophical commitments on pragmatic grounds. While a diner might inform their choices by asking for nutritional information, they might also draw on their own past dining experience, or the experience of those around them. One might politely ask someone at a nearby table how their choice is working out for them, or ask the server what they prefer, given their experience. Analogously, if a working scientist is trying to choose between realism and anti-realism, they might look to the history of science for advice and information. If a modern scientist sees that realist scientists have been more productive than anti-realists when operating in practical contexts similar to their own, for example, that would give them a pragmatic reason to follow suit.

To the philosopher most familiar with the catholic mode of philosophy, this may sound strange. But once we accept that we cannot find epistemic reasons to be a realist or an anti-realist (Ch.1), and that pragmatic reasons for philosophical commitments are legitimate reasons (Ch. 2 and 3), it will seem less strange. A scientist investigating what effects different philosophical attitudes have tended to have on the scientific practice of those that came before them, before adopting an attitude herself, need be no stranger than a diner with an allergy asking whether the gai lan has been braised with sesame oil, before taking a bite. Indeed, both methods of informing action seem quite prudent.

As will be discussed in more detail in the conclusion, a pragmatic, existential approach need not be limited to historical studies. Information regarding which epistemic stance is most likely to help people achieve their ends and serve their values, in a given context, can be pursued using a variety of methods—historical, conceptual, statistical, experimental, or whatever else seems promising. An epistemic stance may be rational so long as it is not incoherent or self-defeating by the lights of the values informing it, but *it's not always clear which stances are likely to be self-defeating by which lights*; and while it seems plausible that, with some empirical investigations, clarity on such matters can be achieved, it also seems likely that a variety of methods will need to be used to achieve such clarity.

5) A Pragmatic, Existentialist Approach to the Scientific Realism Debate versus the Traditional Approach

Attempting to determine which epistemic stance is best for a specific (type of) person, using the above model of choice and pair of methodological assumptions, is what I've been referring to as a "pragmatic, existentialist approach to the scientific realism debate." It is pragmatic because it is not concerned with determining which position is true or most rational *per se*, but rather with determining which position is most likely to help someone achieve the ends determined for them by some antecedently, idiosyncratically, and unquestioningly held set of values. It is existentialist because it resists any attempt to question those goals and values by denying that there is any ultimate ground for them to even question, other than people's free choices. These assumptions are to be justified through their methodological effectiveness, but not because they will help us resolve the traditional scientific realism debate, once and for all, according to its catholic standards, i.e. by somehow showing that one stance, philosophy of science, or set of values is correct, true, uniquely rational, or otherwise preferable for anyone and everyone, forever and everywhere. Rather, the pragmatic, existentialist approach will be justified once it helps us uncover information about the practical consequences of adopting one stance over the other that proves relevant for people struggling with the choice between these two rationally permissible options. In short, the approach is justified insofar as it helps people choose which stance is right for them.

Many philosophers engaged in the scientific realism debate have explicitly stated that they are not trying to determine which position is best for specific (types of) people, or most effective in a given practical context. Rather, they want to know which position is "correct" in that universal sense, or at least whether their favoured position is rationally permissible. For example, when developing constructive empiricism, years before articulating his meta-epistemologically voluntaristic framework for understanding the scientific realism debate, van Fraassen (1980) readily admitted that it might make a great deal of difference to the working scientist which philosophy of science she adopts and uses to frame her daily activities (cf. Fine 1986a, Kukla 1998). He attributes to Paul Feyerabend (1964) the idea that "Realism is a philosophy that stimulates scientific inquiry; anti-realism hampers it." As we saw in the first chapter (sec. 9), van Fraassen goes so far as to say that "[w]e might even suggest a loyalty oath [to realism] for scientists, if realism is so efficacious" (1980, p. 93). He immediately clarifies, however, that determining the relative practical efficacy of a realist or an anti-realist interpretation of science for the working scientist is simply not his aim, and that any argument for a realist interpretation of science on this basis raises "a totally false issue [...] for the interpretation of science, and the correct view of its methodology, are two separate topics" (ibid.). Van Fraassen is interested in interpreting science as an activity with a governing aim, telos, or end-in-view, as expressed in the criteria of success and adequacy operant in the process of scientific theory choice and evaluation; and as an anti-realist empiricist, he maintains that this aim is the construction of empirically adequate theories.[60] So even if we show that many working scientists are realists, that scientists as a rule prefer explanatorily powerful theories, and that a realist outlook may even be preferable for working scientists, this may give the working scientist a reason to reject constructive empiricism, but not the philosopher. These are simply orthogonal issues, van Fraassen thinks, so to argue that scientific realism gives the correct interpretation of science's governing axiology and anti-realist empiricism does not because adopting a realist outlook proves methodologically fruitful for working scientists, or because scientists as a matter of fact often search for explanations, would be specious reasoning. We simply cannot draw a philosophical conclusion about the adequacy criteria operant in scientific theory assessment from a social or psychological fact about which assumptions and attitudes work best for practicing scientists.

Van Fraassen is entirely correct here. Determining whether a realist or an anti-realist outlook is more beneficial to working scientists cannot help us determine which of those philosophies of science gives the correct account of the criteria of success governing the assessment of scientific theories. But this does not, of course, imply that such pragmatic questions about the role of philosophy of science in scientific practice are unimportant or unanswerable; they're just different questions. And, perhaps surprisingly, such pragmatic questions have been surprisingly under-investigated, empirically speaking. But since epistemic questions about which position is true have proven to be difficult (and likely impossible) to answer without begging the question, I argue that we should devote some resources to trying to answer pragmatic questions about

[60] For van Fraassen's clearest argument that modern science holds its theories only to the standard of empirical adequacy, see his 2004 and 2008, Ch.12 and 13

which position is most beneficial.

As was discussed in the first chapter (sec.7), Popper, Feyerabend, Plank, Mach, Leplin, Boyd, and others have mounted arguments to the effect that either realism or anti-realism is more pragmatically useful in scientific practice, but none of them conducted significant empirical studies to support their claims. The practical efficacy of different philosophies of science for working scientists, as a matter of empirical, social, and psychological fact, remains unknown but worth investigating, even if such investigations are irrelevant to deciding the traditional (and apparently stalemated) philosophical debate over scientific realism and anti-realist empiricism. While such facts can't tell us which attitude everyone should adopt, they're nevertheless valuable because the can help each of us answer a much more personal and practical question: "which attitude should *I* adopt?" And importantly, given van Fraassen's willingness to admit that realism might be the more pragmatically effective outlook, I can see no reason to expect that investigations conducted by people who accept the empirical stance will turn up significantly different answers than investigations conducted by people who accept the metaphysical stance, making the results of such investigations stance-transcendent in the way they would need to be help inform stance choice.[61]

6) Epistemic Stances as Embodied Policies

When investigating the practical efficacy of different epistemic stances we can't think of them as purely intellectual attitudes. Conceived as collections of policies, stances can be understood as commitments made to actively use certain *strategies* in our epistemic endeavours. Like other types of strategic commitments, Rebecca Kukla thinks that stances "will be embodied; we should take seriously the idea that a stance is, first and foremost, a way of holding your body and readying it for action and worldly engagement" (forthcoming, p.1). This takes seriously the metaphor on which an epistemic stance is built—that of a boxing stance, or a climbing stance—and sees such ways of physically holding one's body as a model for what it means to maintain a philosophical outlook like the empirical or metaphysical stance. Physically speaking, when

[61] For more on this point, see Ch.2, sec.8 and Ch.4, sec.3

boxing or climbing, taking a stance means maintaining "a kind of physical posture that readies our body for some sort of reactions, activities, and perceptions, while making others difficult or impossible" (ibid., p.5). Intellectually speaking, then, taking a stance means maintaining a kind of mental posture that readies our mind for some sort of reactions, activities, and perceptions, while making others difficult or impossible.

Kukla elaborates this view while investigating Daniel Dennett's notion of a philosophical stance, not the kind of epistemic stance pertinent to the scientific realism debate. Nevertheless, there's sufficient similarity between both notions of stance for what she says about one to apply to the other. Dennett thinks of a stance as an attitude through which someone aims to explain and predict the behaviour of different objects, and identifies three different kinds of stance we tend to use: physical, design, and intentional. Here is Dennett (1987) on what it means to take the intentional stance towards some object:

> [F]irst you decide to treat the object whose behavior is to be predicted as a rational agent; then you figure out what beliefs that agent ought to have, given its place in the world and its purpose. Then you figure out what desires it ought to have, on the same considerations, and finally you predict that this rational agent will act to further its goals in the light of its beliefs. A little practical reasoning from the chosen set of beliefs and desires will in most instances yield a decision about what the agent ought to do; that is what you predict the agent will do. (p.17; cf. Kukla forthcoming, p.2)

The intentional stance amounts to a kind of policy for how to proceed when predicting or explaining the behavior of objects, and contrasts with other policies that would have us understand objects teleologically in terms of their being designed for certain functions (the "design stance"), or causally in terms of their being subject to physical forces (the "physical stance"). The key point is that each of these stances can represent a different strategy not just for explaining and predicting the behavior of an object, but also for interacting with those objects.

To stress the fact that "a stance is a kind of *pragmatic* posture of some sort," where the "entities that show up from within a given stance are loci of norm-governed behavior, resistance, and explanatory power," (p.1) Kukla contrasts the intentional and design stances through the example of being in an argument or conflict with one's spouse. For the most part one would hope that, even when disagreements arise, their partner would treat them through the intentional stance. This would mean treating them as a rational agent who has reasons for acting in the ways they do, being prepared to deliberate, discuss, challenge, and otherwise engage with them as a rational agent. But when human beings are under duress, stress, or strain, sometimes it's more appropriate to treat them through the design stance, even if you're their trusted spouse. This would mean seeing them as a malfunctioning machine, explaining (for example) their anger as an effect of low blood sugar rather than their having good reasons to be angry. So, if someone switched mid-argument from the intentional to the design stance, this would not be a merely intellectual move, but would likely result in them treating their partner differently as well, e.g. by offering them food rather than counterarguments. Such a shift it stance wouldn't necessarily be disrespectful or inappropriate, either; in those moments where our blood sugar had dropped precipitously, or circumstances have otherwise caused us to lose control of our rational faculties, an ideal partner would know that it's time to stop treating us under the intentional stance, as someone to be reasoned with, and instead treat us under the design stance, as a broken machine in need of repair, fuel, or maintenance. To my mind, few attributes make for a better partner than knowing when to stop arguing and just offer a snack. That's beside the point, however, which is twofold: first, that taking a stance has practical consequences in terms of how we behave; second, that the appropriateness of different stances can vary with context.

A pragmatic, existentialist approach to the scientific realism debate aims to better understand, in this embodied way, what it means to adopt either the metaphysical or empirical stance, and how their usefulness varies with context. As Teller (2004) notes, "[t]o adopt a policy is to resolve or to commit oneself to acting or making decisions as described by the statement of the policy," (p.166), but the appropriateness and effectiveness of adopting a policy cannot be known *a priori*. This is why best practices guides for evidence-based public policymaking always involve not just the formulation and adoption of policies, but also follow-up evaluations of their effectiveness in bringing about their intended (or unintended) consequences. Only with such non-obvious, empirical information can we properly judge whether the policies we've adopted should be maintained or abandoned.

While policies are often vague and admit of multiple interpretations, clearly stating them can facilitate their proper evaluation. Here's how Chakravartty (2007, p.18-19) characterizes the metaphysical and empirical stances, as sets of epistemic policies:

> The Metaphysical Stance
>
> M1: Accept demands for explanation in terms of things underlying the observable
> M2: Attempt to answer such demands by speculating about the unobservable
>
> The Empirical Stance
>
> E1: Reject demands for explanation in terms of things underlying the observable
> E2: *A fortiori*, reject attempts to answer such demands by speculating about the unobservable
> E3: Follow, as a model of inquiry, the methods of the sciences

Chakrvartty's characterization makes clear that both stances are guides for behaviour, for each component policy is constructed as an injunction. Their statements begin with action words like "reject," "follow," "accept," and "attempt," used in their command form. These are directions for people to act in certain ways and not others, governing their reaction to their epistemic circumstances. Phrased in this way, I think, epistemic stances are properly understood in the embodied way Kukla indicates, as ways of readying oneself to act and respond to stimulus in certain ways rather than others, just as a boxing stance readies one to punch and parry in certain ways rather than others. If empiricism and metaphysics are both understood in this way they will be differentiable not just in terms of the doxastic attitudes they result in, but also in terms of the sorts of actions they readily motivate and prepare us for, as well as their effectiveness in different situations. The pragmatic, existentialist approach to the scientific realism debate aims to investigate these latter types of differentiation in the hopes of providing people with edifying information about which stance is most likely best for them, given their values and the demands of their specific practical circumstances.

7) Conclusion

The following chapter investigates the sorts of actions that the metaphysical and empirical stances differentially motivate and effectively support in the context of scientific practice. It proceeds through a historical case study of the different philosophical outlooks that influenced electrodynamics research in late 19th c. Europe. This case is especially illustrative of what Robin Hendry calls an "historiographical intuition" that realism and anti-realism do not equally well motivate different forms of scientific practice, from the perspective of a working scientist, even if realism and anti-realism can both account for all relevant aspects of scientific practice, from the perspective of the philosopher of science (Hendry 2001, p. S27; cf. Fine 1986a, Kukla 1998, Leplin 1986). During this time period we find several very different approaches to a single field of research existing contemporaneously, neatly divided along philosophical lines, each of which (with the power of hindsight) we know proved instrumental to the advancement of that field in very different ways. As I will argue, the contributions made by each of these research programs are characteristic of the kind of work that comes most naturally to those who hold the philosophical dispositions their leaders did. This case shows, in concrete terms, how adopting different philosophical outlooks on science can differentially motivate and support specific forms of scientific practice over others. If further investigations show this case to be representative of general trends in the history of scientific practice, as I suspect they will, this will show, empirically, that certain philosophical outlooks are best for those involved in certain kinds of scientific activities, *even though no single outlook is best for scientific practice in general*. In that case, working scientists might be better equipped to tailor their philosophical outlook to the specific research context they find themselves in, modelling themselves on a fighter like Roy Jones Jr. who was famously adept at switching stances mid-fight if and when it was appropriate (cf. Einstein 1949).

While the success of such studies might prove pragmatically beneficial for working scientists, no matter how successful they are they will never resolve the philosophical stalemate surrounding the traditional scientific realism debate. Even if it could be shown that every scientific activity is better served by a realist outlook (which, I should say, is not what the historical evidence suggests), van Fraassen is right that this would have no bearing on the issue of what the aim of science truly is. As van Fraassen and Chakravartty note (forthcoming), scientific realism and constructive empiricism are distinctly philosophical positions, so even if all scientists need to be realists to best serve their values *qua* scientists, this implies nothing about what philosophers need to be to best serve theirs *qua* philosophers, nor what policymakers need to be *qua* policymakers, nor what educators need to be *qua* educators, etc. Nevertheless, these studies can "say more" about the "ultimate wellsprings of voluntaristic choice" (Chakravartty 2011a, p.47) than has been said to date, while also providing scientists contemplating their epistemic options with new considerations pertinent to their decisions.

Chapter Four: Philosophy of Science in Scientific Practice

1) Introduction

In previous chapters I argued that the traditional debate over scientific realism has reached a stalemate: insofar as we're aiming to determine whether realism or anti-realism is true, or rationally preferable *per se*, we're likely to fail. In chapter one I showed how both positions seem rationally permissible, i.e. internally coherent and consistent with the available evidence. I also showed how the extant arguments for and against either position seem to beg important questions, and therefore prove unconvincing to the non-believer. I then gave some reasons to think no as-of-yet unformulated argument is likely to fare any better, i.e. that the stalemate won't be overcome. This stalemate was made sense of in the second chapter through van Fraassen's meta-epistemological voluntarism. There I argued that, even if the stalemate can't be overcome, because we can't determine which position is true or rationally preferable *per se*, we may still be able to determine which position is rationally preferable for some individual, pragmatically speaking. I stressed that giving people pragmatic reasons to adopt realism or anti-realism will require conducting empirical investigations of which position tends to help people achieve certain ends, in specific contexts. In the third chapter I developed a framework meant to guide such empirical investigations, and in this chapter I put it to work.[62]

Specifically, I investigate the ways that different philosophical attitudes influenced the scientific practice of electrodynamics researchers in late 19th century Europe. As we'll see, in certain research contexts, certain philosophical outlooks seem to have led to more productive forms of scientific practice than alternative outlooks. The idea in conducting this study (and eventually others like it) is that a modern scientist might judge that, in research contexts similar to her own, people with certain philosophical outlooks are generally more successful. In that case, this study (and others like it) might give her a pragmatic reason to adopt that outlook herself. To be sure,

[62] For economy of speech I abandon some of the nuance found in previous chapters, speaking simply about philosophical "outlooks" or "attitudes" as a way of indicating an epistemic stance along with whichever philosophical attitudes result from it. Being extremely careful about discriminating the tradition characterized by "the empirical stance," say, from the technical details of the anti-realist views it engenders won't be necessary to show how holding such an outlook tends to prime scientists to practice science differently than they would if they held a different outlook.

however, I don't expect one case study to be especially compelling, so this chapter is intended more as a proof of concept than a completed project. My aim here is rather modest; I aim only to show how empirical investigations might give people compelling pragmatic reasons to be a realist or an anti-realist about science by determining the practical consequences of adopting each view, in specific contexts.

Before laying out my approach to studying how the philosophical commitments of late 19th century electrodynamics researchers influenced their scientific practice I begin by further clarifying the aims of this study, and briefly addressing Fine's concerns about its feasibility (Fine, forthcoming).

2) On the Possibility of Pragmatic Reasons for Philosophical Commitment: The Methodological Argument Revisited

At present, we know rather little about the practical consequences of adopting realism or anti-realism in different research contexts, empirically speaking. Fine (1986a) has argued that realism and anti-realism are indifferent with respect to the practice of science, i.e. that there is no practical difference between adopting these views for working scientists. However, Hendry (2001) notes that Fine's argument seems to be based on a confusion between a philosophical outlook *motivating* certain practices and *being consistent with* them, going on to state the following "historiographical intuition":

> Some scientists have sometimes displayed recognizably realist (and indeed instrumentalist) commitments, and their realism (instrumentalism) motivated them to engage in certain projects of theory construction, emendation, and selection … The historiographical intuition implies only that realism (or instrumentalism) was what motivated some scientists to engage in the practices they did engage in, in the sense that they would not have been motivated to engage in those practices had they not been realists (or instrumentalists). Therefore, their stated positions (whether realist or instrumentalist) should be an important part of the historical explanation of why they did what they did. (S27)

So, while all scientific activities are consistent with realist or anti-realist aims, different aims might still motivate certain activities more readily than others.[63] I think there's significant historical evidence that this is the case, i.e. that realists tend to pursue different research agendas than anti-realists, in regular and predictable ways. Going a step further, I hope to eventually show that realists and anti-realists tend to be more successful than each other in different contexts, in regular and predictable ways.

To get clear about what's at issue here, it's worth briefly revisiting Popper's methodological argument for axiological scientific realism, discussed in the first chapter. His question is a generalized, context-insensitive version of the question investigated in this chapter: do realists or anti-realists make better scientists? Popper argues that realists are better scientists, and anti-realist attitudes stifle scientific progress. As Brad Wray (2015) notes, this argument is importantly different than most arguments for scientific realism:

> [I]n this particular argument for realism, the sort of realism that is at issue is a realism that scientists might adopt. Typically, realism and anti-realism are identified as philosophical positions. But the guiding question in this debate is: should scientists be realists or anti-realist? (p.74)

Popper's argument was largely abstract, philosophical, and *a priori*, based in his conception of theory building and testing in science. Here we're looking at the same issue, but from a concrete, historical, *a posteriori* perspective: *empirically speaking*, do realists or anti-realists make better scientists? Wray (2015) further notes, however, that historical evidence suggests no single position seems to be uniquely preferable in the practice of science:

> [T]here is good reason to believe that a research community profits from both realists and anti-realists, and not because either position is correct. Realists and anti-realists may both be needed to ensure that competing theories are developed. (p.78; cf. Hendry 1995 and 2001).

But even if we recognize that science benefits from having a mixture of realist and anti-realist scientists, there may nevertheless be some specific research contexts in which a realist is likely

---

[63] Dan McArthur (2006) is notable as someone who's mounted a similar historical argument against Fine's claim that philosophical debates and opinions don't influence the behavior of working scientists.

to perform better than an anti-realist, or *vice versa*. This is the more fine-grained question that I focus on: if it is best, pragmatically speaking, to have a scientific community made up of both realists and anti-realists, is that because there are specific scientific contexts in which it is beneficial to be a realist, and others where it is beneficial to be an anti-realist?

What follows is only a first step towards establishing a pattern, i.e. towards gathering the kind of information that would give working scientists a compelling pragmatic reason to make a philosophical commitment. Whereas I focus quite narrowly (geographically, temporally, and disciplinarily), identifying whether there are specific contexts in which adopting a realist or anti-realist attitude has regularly been beneficial to working scientists will require multiple case studies across the history of science. As I discussed in the first chapter (sec.1), Fine (forthcoming) remains skeptical that further studies of this type could ever transform Hendry's historiographical intuition into informative historical facts about the ways that different philosophical outlooks tend to benefit scientific practice. Thankfully, pessimism is not an argument for the impossibility of success. For now, my aims are primarily illustrative; I aim only to show that this project *might* succeed. We'll have to carry it out, of course, to see whether it will (cf. Hendry 2001, S36).

3) Linking Philosophical Views with Successful Scientific Practice through History

To show that different philosophical outlooks promoted successful scientific practice in some historical period, three groups of questions need to be answered.

First, how should we understand success in scientific practice? Importantly, success must be defined independently of any philosophical outlook, lest realists and anti-realists disagree about whether some achievement really counts as "success." It must also be defined independently of historical actors' own sense of accomplishment, lest we judge Kepler's Platonic solid model of the solar system a major success, not a quirky bit of neo-Pythagorean metaphysics. I suggest that a *post hoc*, Whiggish notion of scientific success and progress as "helping to bring about modern science" will be useful, unproblematic, and agreeable to all relevant parties, but more importantly it will be a notion of success that's important for modern scientists. Working scientists generally assess the work of their predecessors using the power of hindsight, seeing those scientists whose work retains a lasting significance—Newton, Darwin, Lavoisier, Dalton, etc.—as the successful ones, most worthy of emulation. Insofar as we're aiming to inform scientist's judgements about which philosophical outlook is most likely to help them be successful researchers, it's appropriate to define success and progress in a manner that will matter to them, i.e. Whiggishly. These notions of success and progress, and why they're appropriate for the purpose they're being used, will be elaborated in the next section.

Second, which scientific practices should we expect adopting a given philosophical view to ready someone for? When philosophers adopt philosophies of science, the consequences will be rather academic, a matter of how they understand the activities of scientists, argue with their students and each other, write articles, or give conference presentations. But when a scientist adopts a philosophy of science, she will come to conceive of her own research activities in terms of certain governing aims and potential epistemic outcomes, leading her to think and act in specific ways. So, which activities should we expect a scientist with a realist or an anti-realist self-conception to be most straightforwardly motivated to pursue? That is, which scientific practices does each philosophy of science understand the epistemic action to be centered around? To answer this question, in the fifth section I outline the visions of science given by scientific realism, anti-realist empiricism, and pragmatism, conceived as embodied strategies for conducting science.

Third, and finally, when did the sorts of activities motivated by each outlook tend to produce successful results? Put this way, the question is rather general, but presently, the question I investigate is specific: how did philosophical views motivate successful scientific practice in 19th century electrodynamics? The claim that their adoption of different philosophies of science supported the success of electrodynamics researchers in different and characteristic ways during the late 19th century can be divided into the following three sub-claims, each of which I address in turn through the sixth, seventh, and eighth sections:

1) The leaders of each research program had a specific philosophical outlook on science

2) These individuals practiced science in a way that embodied that outlook

3) Practicing science in that manner, in that context, led to specific types of successful research outcomes

Having answered each of these questions in turn, I conclude this chapter by drawing out some philosophical lessons from this study, and returning to Fine's comparison of philosophical and religious commitments that opened the first chapter (sec.1).

4) A *Post Hoc* Vision of Success in Science

Philosophers interested in determining the practical consequences of adopting some philosophical outlook in the context of scientific practice, in a manner that can help modern scientists judge whether it's appropriate for them to adopt that outlook in their own context, will need to use the standards of success that scientists themselves employ when evaluating past scientific work. Historians generally avoid the use of hindsight, but scientists are typically not acting as historians when telling the history of science. They have different purposes in mind than doing good history, much as a poet would have a different purpose in mind when alluding to Schrödinger's cat than doing good physics.

The adjective "Whiggish" derives from the way that Whig politicians tended to portray the political history of Europe, as a continuous struggle to achieve the liberal democratic political system that they upheld as an attained ideal, defining progress "in terms of steps toward what [are] perceived to be the main components of our modern worldview" (Bowler and Morus 2005, p.7). Whig historians would ignore many of the motivations of many of their historical heroes, instead portraying them as motivated by Whig political ideals, with great anachronism. Whiggish history places the view that the present is superior to the past at the centre of its historical explanations, and in explaining why some historical actors acted as they did, such histories may suggest that they simply saw the correctness of such actions, even when the historical facts show that that person didn't think in anything close to modern terms. For the Whig historian, a historical actor was successful, and their activities constitutive of progress, insofar as they helped bring about the modern world.

Modern historiographers disapprove of Whiggish history because it tends to misunderstand the totality of an historical actor's character, their true motivations for doing what they were doing, and the contingency of historical outcomes. In general, historians want to understand the motivations that historical actors had for doing what they did; they want to understand why those actions seemed right to them, not to us. But the notions of success and progress involved in Whiggish history of science are indifferent to the intentions and evaluative metrics of historical scientists, entirely ignoring their own sense of success, reasons for acting, and overall research agendas. They are, in that way, external criteria of evaluation, and from a historiographical perspective they are far too anachronistic to help us understand the true reasons why historical actors did what they did. For the historian they're not just dangerous, they're pretty useless. But they can be useful for other people, with other purposes. Modern scientists make great use of Whiggish history of science for pedagogical purposes: to communicate the lasting significance of certain laws or experimental results, to derive inspiration, or to provide methodological exemplars (Bowler and Morus 2005, p.2). Kepler may have been proud of how the formulation of his third law of planetary motion gave him Pythagorean insight into the music of the spheres, but modern scientists see it as successful because it aided in the development and proliferation of Copernicanism and (later) the validation of Newtonianism. This is a Whiggish rendering of Kepler's success because it ignores his own motives and evaluations, instead using hindsight to judge his work's significance according to how he helped bring about the modern world.

While Whiggish standards of progress and success will be used to judge the work of historical scientists as successful or unsuccessful in section eight, I'm careful to let them play no historiographical role in sections six and seven. That is, when investigating the activities of historical scientists, and their motives for pursuing them, I look to understand their aims and self-conception in their own terms, through their contemporary context. For we need to understand why the leaders of different research programs practiced science in the way they did, and a historiographically sound approach reveals that much of that practice was a result of their prior philosophical commitments. To establish this, we'll first need to know which sorts of scientific activities we should expect each philosophical outlook to most readily motivate people to undertake, the issue to which I now turn.

5) Embodied Visions of Scientific Practice: Where the Epistemological Action Happens

Richard Feynman (1955) once said that science is valuable for three reasons, roughly coinciding with the divisions of "pragmatism," "realism," and "empiricism" that I outline below in detail. "The first way in which science is of value," he says, "is familiar to everyone. It is that scientific knowledge enables us to do all kinds of things and to make all kinds of things" (p.13). This is the sort value that pragmatists tend to see in science: its ability to help do stuff, to control and recreate nature to our own ends.

Feynman continues:

> Another value of science is the fun called intellectual enjoyment which some people get from reading and learning and thinking about it, and which others get from working in it […] with pleasure and confidence we turn over each new stone to find unimagined strangeness leading on to more wonderful questions and mysteries—certainly a grand adventure!" (p.14)

This is the sort of value that realists tend to see in science, its ability to both reveal and solve some of the deepest mysteries of nature, to help us understand the world and our place within it, and to satisfy our curiosity.

Feynman goes on to identify a third sort of value, one that empiricists tend to see in science, based on the epistemic humility that science enforces upon itself:

> "The scientist has a lot of experience with ignorance and doubt and uncertainty, and this experience is of very great importance, I think [… Science permits] us to question—to doubt, that's all—not to be sure." (p.14)

Here the importance of science is that it doesn't stick its neck out too far, claiming to have decidedly solved all the mysteries of the world.[64] Empiricists add to this an emphasis on observation rather than speculation, but I think the ability of science to showcase our own

[64] As van Fraassen puts it when describing how the scientific spirit is understood within the empiricist tradition in contrast to the metaphysical spirit it characteristically fights against, "in the old myths, to avoid doubt may be piety, in the new it is treason" (1994b, p.132).

ignorance about a great many things, in part by emphasizing the limits of our ability to know, is an especially salient value for anti-realist empiricists.

While the value typical of realism—the satisfaction of wonder—and the value typical of anti-realist empiricism—the enforcement of epistemic humility—are somewhat in tension with one another, Feynman values science for both these reasons, and for its pragmatic value as a tool for mastering nature. Most people, whether scientists or philosophers, will recognize each of these as reasons to pursue scientific research. Whether one ends up as a realist, empiricist, or pragmatist is a matter of how these values are prioritized, such that a realist will see science as incomplete if it doesn't achieve realist aims, even if it perfectly achieves empiricist and pragmatist aims. But holding a realist, pragmatist, or empiricist vision of science doesn't require denying that science is valuable for all three reasons. People's philosophical positions change over time as well, sometimes rapidly; sometimes scientists even hold incoherent philosophical positions. Einstein (1949) wrote that, to the systematic epistemologist, scientists may often appear to be unscrupulous opportunists: realists, idealists, positivists, and Platonists from one moment to the next, or even all at once (p.683-4). No one displayed this epistemological fluidity more than Einstein himself, and perhaps it accounts for some of his early success. It may also account for some of his later lack of success, but the relevance of his point here is this: historical studies of the way that philosophical commitments influenced scientific practice need not assume that people's commitments are set in stone, clearly stated, or mutually exclusive. Kelvin was a pragmatist among pragmatists, generally concerned with getting a working theory capable of supporting industry more than anything else; and yet, he became so obsessed with typically realist questions about the mechanical constitution of the ether than he saw it as a vice, calling it his "ether dipsomania" (Darrigol 2000, p.127). The philosophical commitments of scientists are generally dispositions, sometimes dalliances, and rarely doctrines or dogmas.

Nevertheless, in the course of their scientific training and research, scientists often come to some distinctly philosophical understanding of what the point and product of their research is, which then comes to colour their agenda and style afterwards. History shows that scientists sometimes become entrenched enough in their philosophical attitudes that, at least during some period, they see everything they do in that light. That realists and anti-realists will interpret scientific activities differently is of course not at all a matter of contention; the point is to show that having a self-conception based primarily on one philosophical outlook rather than another makes a *motivational* difference for the working scientist. For this it will be important to know what the embodied practice of science looks like from each philosophical outlook.

Realism portrays science as primarily aimed at discovering the truth about the unobservable and modal aspects of our world, and asserts that a scientific claim is acceptable when it seems to be (approximately) true. But the truth of scientific claims can rarely be determined directly or definitively, especially if they relate to unobservable, counterfactuals, or modal features of reality. Thus, when evaluating theories, scientists work to assess their explanatory strength, e.g. by determining whether they can explain a wide range of phenomena, or whether they fit with other well-established theories. Theory building, according to this image of science, results from the effort to explain phenomena of interest, through which we can demystify their true causes. Experiments play a part in assessing the explanatory merits of theories by allowing scientists to indirectly test the truth of different theories by way of their novel observable consequences. A theory's explanatory power is demonstrated if, in the course of such experiments, its novel predictions are corroborated, and no other extant theory makes those predictions without *ad hoc* modifications. Theory-saving *ad hoc* modifications are to be avoided, of course, for they indicate a theory's explanatory weakness. Not all correction of theory by experiment is illegitimate, however, because measurement work will still be needed to fill in many of the details of a theory, building its basic framework into its strongest form so it can be empirically tested. Accordingly, measurement plays a key role in scientific theory building by providing empirical determinations of various key parameters, constants, and magnitudes that reveal the precise character of the properties possessed by the unobservable entities a theory postulates. Such modifications to theory are not to be considered *ad hoc*; indeed, performing measurements of this sort will be the primary preoccupation of laboratory work, as conducting crucial experiments that can decide between competing theories will be a relative rarity, only possible once significant measurement work has helped develop a theory into its strongest form. This measurement work can also help scientists discover new unobservable entities, sometimes accidentally and sometimes through a directed effort, such as when J. J. Thomson's experimental work measuring the properties of cathode rays suggested the existence of electrons. Through the formulation and testing of further hypotheses regarding these unobservable entities, as the realist sees it, much was learned about these unobservable "electrons," eventually allowing Millikan to measure the magnitude of their individual charge. Through such work, as the realist sees it, experimental apparatus and scientific instruments are used to peer into the unobservable world and detect the existence and properties of unobservable entities (cf. van Fraassen 2008, Ch.4).

Such an understanding of the aim and epistemology of more fine-grained aspects of scientific practice results from the realist's axiology, the idea that revealing the truth is the ultimate goal of science. Realists are forced to assume that greater explanatory power indicates a greater probability of truth, for such abductions are sometimes the only way to assess the relative truth of competing scientific theories, i.e. to achieve what they see as the aim of science. If this is how the realist understands and interprets scientific practice, then we should expect that a working scientist that accepts realism will only accept a theory when it seems to be the best available explanation in a given domain. So, when the postulation of certain unobservable entities seems to explain a variety of phenomena, a realist scientist will begin to believe that those entities likely exist. But they will also be motivated to corroborate such beliefs, and will characteristically try to support a theory they tentatively accept in two ways: first, by making it more detailed and precise, using scientific instruments to detect and measure the properties, behavior, and make-up of those unobservable entities; second, by showing that it's uniquely able to account for phenomena in ever wider domains, beyond its original domain of application.

So, as the realist sees it, the epistemological action happens in the evaluation of theories according to their explanatory breadth and power, and in the development of theories through measuring theoretically important constants and parameters. As such, we should expect realists to be motivated to pursue these activities over others, developing favoured theories rather than competitors, measuring constants and parameters rather than blindly searching for novel phenomena before deducing them from theory, and generally trying to build a theory into its most explanatorily broad form without immediate concern for practical applicability. These are the activities most directly related to achieving the aim they see as central to science: discovering the reality behind the appearances.

Anti-realist empiricism portrays science differently, as an activity aimed towards the production of empirically adequate theories, and asserts that a theory is acceptable when it seems capable of modeling all phenomena in its domain. Theory building, according to this image of science, results primarily from an effort to save known phenomena, rather than an effort to explain them. Theories are evaluated primarily in terms of their capturing the truth about observables, but there can still be pragmatic reasons to accept a theory that appears explanatorily strong over one that doesn't: for one, explanatory strength requires empirical adequacy, at least to a degree (cf. Cartwright 1983, Ess. 6); for two, an explanatory theory may be easier to work with, conceptually speaking, than one that is merely "phenomenal." Nevertheless, such pragmatic considerations are decidedly subsidiary to the epistemic considerations of empirical adequacy. Experiments play a role in the evaluation of theories by expanding the empirical basis upon which they can be evaluated, discovering new phenomena and giving more precise and exhaustive accounts of known phenomena to which theories can be held empirically accountable. A theory's superior empirical adequacy is demonstrated when it proves capable of accommodating all the known phenomena, regardless of whether its models are causal or explanatory. And whereas having a maximally prohibitive theory—that is, one that bars many phenomena from occurring—may be important from the realist perspective (Popper 1963, p.235), a preferable theory for an empiricist might be one that is more flexible than alternatives, capable of being refined in light of new experimental results in ways that realists would view as *ad hoc* modifications. Measurement work helps to build and refine theories in this way, by filling in empirical details that are needed to more accurately and precisely model phenomena; but whereas realists would see Thomson and Millikan's work as constituting the discovery a new unobservable entity and its properties, using their instruments to extend their perceptual faculties into the unobservable world, the anti-realist empiricist would see them as simply producing new phenomena of interest that must be saved by any theory in that domain (cf. van Fraassen 1980, p.75-77). So, whereas realists see experimental apparatus and scientific instruments as windows into the unobservable world, anti-realist empiricists see them as "engines of creation" that produce new phenomena that candidate theories will henceforth be required to save (cf. van Fraassen 2008, Ch.4).

Again, this understanding of the aim and epistemology of more fine-grained scientific practices arises from the empiricist's understanding of the central, governing aim of science: the development of empirically adequate theories. Inference to the best explanation need not be given any epistemological legitimacy in any stage of scientific inquiry, being as it is an instance of a logical fallacy.[65] If this is how the anti-realist empiricist understands and interprets scientific practice, then a working scientist who adopts this outlook will only accept a theory when it seems to be empirically adequate in a given domain. They may consider pragmatic factors such as simplicity and explanatory power as indications of which theoretical framework is likely to support future efforts to accommodate as many phenomena as possible, for a simple or explanatory framework is often easier to work with, cognitively. Nevertheless, their epistemic evaluation of a theory (i.e. their evaluation of its truth content) will be restricted to assessing its empirical merits, i.e. whether it can, in the end, accommodate the well-established facts about observables. This might make a significant practical difference for the working scientist in how they evaluate theories. For example, a non-causal version of quantum mechanics might prove satisfactory to the anti-realist empiricist, not necessarily in need of any further development (cf. van Fraassen 1989, 2004, 2008, ch.12, 13), whereas a realist would find it incomplete and unsatisfactory (cf. Einstein, Podolosky, Rosen 1935). It might also make a difference in how they build theories: postulating certain unobservable entities might be worthwhile, for the anti-realist empiricist, but sometimes simply having a flexible theory that permits the free exploration of phenomena in nature or the laboratory is all that's required. In fact, a theory that sticks to the observable facts and doesn't posit any unobservable causes might even be preferable for the anti-realist empiricist in those circumstances, just in case some new class of phenomena needs to be explored in the lab and ontological assumptions might prove restrictive in such investigations. As such, and in contrast to the realist (e.g. Popper 1959, 1963), we would expect an anti-realist empiricist to work to support a theory they accept by maintaining a certain level of phenomenal flexible (i.e. making sure it will be able model to a wide range of phenomena, regardless of how they turn out after further empirical study), by altering their theories to accommodate new observational and experimental results as they are established (regardless of whether this is to be considered *ad hoc* or not), and by showing that some phenomena cannot be modelled by their more restrictive rival theories.

So, as the anti-realist empiricist sees it, the epistemological action happens in the discovery and modelling of various phenomena, not in the extension of theories to wide domains or in the measuring of theoretical parameters that might narrow a theory's applicability. Certainly,

[65] affirming the consequent, see Ch.1, Sec. 3

measurement might be important in the effort to better characterize phenomena, allowing perturbations and other confounding factors to be eliminated. But the most epistemologically important task will be the actual observation of phenomena, even in a manner undirected by theory, so as to build a theory best able to represent them. While all phenomena should be understood quantitatively, they will not need to be integrated into a single theory immediately; the need for highly unified theories covering wide domains of application will only arise when we end up trying to model systems involving multiple types of phenomena (van Fraassen 1980, p.88). As long as we have a theoretical model capable of saving a phenomenon, to whatever degree of precision we desire, the central task of science is achieved; whether that model satisfies our wonder or allows us to control nature is a different question.

Fine (1986a) has established that both realism and anti-realist empiricism can give accounts of why all saliently scientific activities are rational to pursue, even if they give different accounts because they understand science in terms of different aims. What this means is, first and foremost, that the observed facts about how science is practiced, by actual scientists, cannot speak to the correctness of either philosophy of science. But also, abstractly speaking, we can see that any activity realists would have reason to pursue, anti-realist empiricists would also have reason to pursue it, and *vice versa*. The only difference would be in what they see as the most important activities, the ones they'd be most focused on pursuing because it's where the epistemological action happens, as they see it. Thus, as Hendry (2001) makes clear, the differences in how scientists who accept each of these outlooks conceptualizes their own activities are likely to make a difference in terms of the kinds of work they're motivated to pursue, how they pursue it, and the conclusions they draw from the results of their research. So, Fine is right that realism and anti-realism may be *explanatorily indifferent* with respect to scientific practice in general, but that doesn't mean they're *motivationally indifferent* with respect to the scientific practice of individual scientists. Because they are explanatorily indifferent, the traditional scientific realism debate ends in a stalemate, i.e. we cannot identify one aim as the true governing aim of science. But, I argue, because they are not motivationally indifferent, the debate over the methodological fruitfulness of each philosophy of science need not end in a stalemate. Through the next three sections we'll see that adopting different philosophies of science does seem to motivate scientists to pursue different research agendas, at least in the context of 19th century electrodynamics research. First it will be important to characterize a third philosophical outlook on science that is different from both scientific realism and anti-realist empiricism: pragmatism.

Whereas realists value science primarily for its ability to satisfy their curiosity, and empiricists value science primarily for its ability to avoid metaphysics, pragmatists value science primarily for its ability to inform action. As pragmatists see it, a scientific theory is just "an instrument: it is designed to achieve a purpose—to facilitate action or increase understanding" (James 1907, p.33), and should be judged accordingly, i.e. not in terms of its truth or empirical adequacy, but in terms of its usefulness. Peirce (1878) goes so far as to assert that the very meaning of a proposition can be cashed out in terms of the practical consequences of adopting it. On a pragmatist's view, "no theory is absolutely a transcript of reality, but […] any of them may from some point of view be useful" (ibid.), meaning that inconsistent theories or models may be simultaneously acceptable, e.g. if they are useful for different purposes, in different contexts. In general, a useful theory will be one that is capable of precisely predicting the phenomena of nature, in all their richness, in a way that can successfully inform people's intervention into the natural world to bring about preferred states of affairs. Whereas realism is the outlook on science characteristic of those curious people willing to take epistemic risks in order to satisfy cosmic levels of wonder, and anti-realist empiricism is the outlook on science characteristic of those unwilling to take such risks, pragmatism is the outlook on science characteristic of the practically-minded engineer, technologist, or industrialist. Scientific realism and anti-realist empiricism are often thought of as lying along a spectrum, with unbridled metaphysical speculation on one end and absolute skepticism on the other. But because it lies outside this spectrum, pragmatism is often seen as a form of "irrealism," less concerned with matters of epistemology than with matters of practicality. To some extent this is a semantic issue, but realists tend to see pragmatists as opponents because they are unwilling to make the epistemic jumps characteristic of realism, from the explanatory strength of a theory to its truth. Anti-realist empiricists, as well, tend to oppose the pragmatic outlook on science, with its emphasis on controlling phenomena over fully characterizing them, and its willingness to engage in hypothetical speculations so long as it leads to predictive models. Both anti-realist empiricists and realists also tend to oppose pragmatist philosophies because they see scientific activities as continuous with practical (e.g. industrial and design) activities, with Duhem claiming this taints science with the interests of the factory floor (Duhem 1914, p.70-1), and Popper claiming it makes scientists out to be nothing more than "glorified plumbers" (Popper 1983, p.122-3). Classist connotations aside, pragmatism appears to both scientific realists and anti-realist empiricists to make science into a purely practical endeavor, whereas the greatest value they see in science is more intellectual than practical.

So, simply put, pragmatism portrays science as an activity primarily aimed towards the production of useful theories. A theory is acceptable for the pragmatist when it seems capable of accurately predicting the phenomena in its domain, however it manages to do so, whether through causal or phenomenological models, narrowly applicable models or widely applicable theoretical frameworks, *ad hoc* modifications or bold metaphysical hypotheses. Theory building, according to this image of science, results primarily from an effort to predict and manipulate the world, and theories are evaluated in such terms. Being true, explanatorily strong, or empirical adequate may make a theory more useful for prediction and intervention, but such considerations are decidedly subsidiary to the pragmatic considerations of which theory or model proves most useful. Experiments play a role in the evaluation of theories by determining whether theories accurately predict phenomena, and thereby inform the construction of devices that produce desirable states of affairs. A theory or model's preferability is demonstrated when it proves capable of predicting the phenomena of practical interest, in detail, and informing the creation of favorable states of affairs in a way that others are incapable of. From this perspective, detailed measurement work is useful only insofar as it helps to refine the predictive accuracy of a theory to the degree required by some practical task, like an industrial project. Whether those measurements are understood as determining the empirical parameters of observable phenomena or the properties of unobservable entities doesn't matter; what matters is whether they enable more precise predictions and interventions. So, whereas realists value Thomson and Millikan's work as grounding a belief in the existence of electrons, and anti-realist empiricists value it as producing new phenomena of interest that must be saved by any theory in that domain, pragmatists would value it as a means towards the prediction and manipulation of nature. Realists look at experimental and measurement work in terms of their role in discovering new unobservable entities and their properties, using their instruments to extend their perceptual faculties to peer into the unobservable world; anti-realist empiricists look at such work in terms of the discovery of new phenomena to which theories can be held accountable; pragmatists look at such work as helping to build better predictive tools. So, rather than seeing experimental apparatus and scientific instruments as windows into the unobservable world, or engines of creation that produce phenomena, pragmatists see such objects as "tools for sharpening predictions," helping a theory better anticipate the progression of events, given a specified experimental set-up, or demonstrating its current capacity for accurate anticipation.

If this is how the pragmatist understands and interprets scientific practice, then a working scientist who adopts this outlook will only accept a theory when it is demonstrably predictive in a given domain, and will pursue the development of those theoretical frameworks which seem most readily applicable to predictive tasks, whether because of their breadth or simply because of their mathematical simplicity. Considerations of explanatory power will not matter much in theory evaluation, and considerations of empirical adequacy will only matter insofar as getting the observable facts right relates to predictive accuracy. Again, this can make a significant practical difference, as a pragmatist might be more willing to work with two models that conflict both ontologically and empirically if one is predictive in one domain while the other is predictive in another. They might also work to develop an entirely new framework, even if there already exists one that seems both empirically adequate and explanatorily powerful, just because the new methods promise to be mathematically easier to work with, despite promising no increase (or even promising a loss) in explanatory power or empirical adequacy. Postulating certain unobservable entities might be heuristically valuable for modelling the dynamics of a system, but it might also be worthwhile to stick to phenomenal models; really, whatever helps build predictively powerful theories and models will be seen as appropriate. Whether a theory provides ontological clues about the ultimate make-up of a system will be seen as largely beside the point, so measurement will be used mainly to improve predictive accuracy rather than seek knowledge of the properties of unobservables. Theories and models will be validated primarily through laboratory demonstrations of their predictive power and through their successful application to technological and industrial design, not through crucial experiments that seek to test their explanatory power or empirical adequacy. In general, the pragmatist scientist will be less inclined to evaluate their favoured theories against competitors, so long as they have reason to expect their favourites to prove more (or at least equally) useful, even when they are empirically and ontologically distinct.

So, as the pragmatist sees it, the epistemological action happens in the process of design, and in the development of the theories that can be applied to such purposes. The most important task will be to develop theories while paying close attention to industrial requirements, making sure that such theories are elegant and easy to use. Devices will be built not so much to test theories as to demonstrate their practical potential, and the ultimate aims of science will be achieved when a theory, like any tool, has been well enough built that it can do everything we want it to do. Whether that theory it especially broad, gives us ontological insight, or fully and uniquely characterizes the phenomena will be seen as generally incidental to its practical utility.

With an understanding of how each these three philosophies of science sees scientific practice, and where they locate the epistemological action, I now turn to historical matters. I begin by showing how various 19th c. electrodynamics researchers came to hold distinct philosophical outlooks before moving on to show how those outlooks influenced their scientific practice.

6) The Philosophical Origins of Three Electrodynamics Research Traditions

During the late 19th century there were (at least) three distinct approaches to electrodynamics research in Europe, all operating contemporaneously: Wilhelm Weber's action-at-a-distance approach; James Clerk Maxwell's field-theoretic approach; and Hermann von Helmholtz's interaction-potential approach. It was an interesting time for electrodynamics research, as ontological questions about the nature of electricity and magnetism, as well as methodological questions about how best to study and represent them, were by no means as settled as they are now. Accordingly, these three programs of electrodynamics research varied widely in terms of their ontological and methodological assumptions, the mathematical frameworks they used to represent phenomena, the sorts of long term research agendas they set for themselves, and the day-to-day activities they were typically engaged in. But they also, we will see, varied in terms of the philosophical conceptions of the nature of science that informed the development and acceptance of those ontologies, methodologies, frameworks, agendas, and daily activities. Paying attention to these philosophical divisions, the way they affected scientific practice, and the contributions that each school made to the advancement of electrodynamics suggests some very specific hypotheses about when and how adopting different philosophical outlooks tends to prove fruitful.

I begin with Helmholtz's development of an empiricist understanding of the aims and epistemology of science, focusing on the way that his early studies of the physiology of perception led him to place the science of human perception at the centre of his epistemology. Following that I look at how some of Weber's early work in electrodynamics led him to take a realist attitude towards certain unobservable posits, shaping his subsequent research in ways that are most noticeable when contrasted with his contemporaries, collaborators, and predecessors. Providing this contrast helps show how many of the major electrical researchers at the time (beside the three main characters I focus on) were motivated towards different forms of practice by their philosophical outlooks and research context. This section concludes by zooming out to a wider scale—socially, geographically, and temporally—to explain why the tradition of mathematical physics at Cambridge that Maxwell was trained within was so unique. With its focus on analogical model building, analytic problem solving, and a greater concern with the problems faced by industry, the understanding of the aims and epistemology of science embodied in Cambridge research culture are neither realist nor empiricist, and are instead best understood as a form of pragmatism. Throughout this section I avoid discussing the practical activities of each research tradition as much as possible, focusing only on establishing the reasons for and character of their distinctive philosophical outlooks. Only once it's made clear how each of our main characters understood the aims and epistemology of research do I go on, in the following section, to detail how that understanding influenced their scientific practice; and only once that task is complete do I go on to use the Whiggish conception of scientific success outlined above to discuss the kinds of success each approach led to. This ordering and separation of tasks will ensure that I have reached safe waters before evaluating the achievements of these historical actors using historiographically pernicious standards, having concluded all properly historical matters without understanding the activities and motivations of these characters in anachronistic terms.

There is no record of what Helmholtz's first spoken words were as a child. It's unlikely that they were a declaration of empiricist scruples, but it's hardly an exaggeration to say, as one biographer has, that "[a]ll his life, Helmholtz proclaimed himself a confirmed empiricist"

(Meulders 2010, p. 197).[66] The germ of his anti-metaphysical perspective, by all accounts, was implanted during his toddler years. The young Helmholtz was bedridden for most of his early childhood, and this gave him ample time to listen in on his father's discussions of German natural philosophy with his colleagues, who regularly gathered in his home. Helmholtz later recalled being frustrated with the "scholastic" topics usually discussed in his home, being much more concerned with mathematics, geometry, and properly empirical matters such as physics, making him averse to metaphysical speculation from an early age (Königsberger 1906).

Helmholtz's father pushed him to study medicine rather than physics, as the family's finances only permitted him to study under a scholarship, which the government provided for medical students in exchange for military service. As a result, he was trained originally in medicine rather than physics, though even during his years of military service after graduation Helmholtz was preoccupied with physiological research rather than medical practice. His physiological studies were especially innovative for the time, grounded from the outset in physical methods and standards. Nevertheless, Helmholtz would not dedicate himself to purely physical studies until much later in life.[67]

His opposition to metaphysical speculation was expressed and strengthened through every stage of his physiological research. His early work aimed to undercut the vitalist school of physiology, upheld by contemporaries such as his mentor Johannes Müller, which posited a kind of inexplicable force as the cause of all organic processes, immaterial and imperceptible yet inexhaustible, present, and active in all living things. Helmholtz recognized the physical absurdity of such a force, given its inability to be measured and the possibility of perpetual motion that it presented. Together with several of Müller's students Helmholtz formed the Berlin Physical Society, which dedicated itself to the promotion of an "organic physics" that modelled biological phenomena using only the measurable forces and properties of Newtonian physics. His work within this physiological research program focused on the exploration, in the

---

[66] To contrast his views with metaphysical idealism Helmholtz adopts the forceful label of "realism" after 1870 (Schiemann 2009, p.70), but if we were to contrast his attitudes with those of Weber or modern scientific realists, Meulders (2010) is right that Helmholtz is properly considered an empiricist.

[67] For more biographical details about Helmholtz, including his personal life, see the expansive and definitive biography written by his friend, the mathematician Leo Königsberger (1906).

laboratory, of a variety of phenomena relevant to the vitalism dispute. Specifically, he conducted meticulous investigations of fermentation and putrefaction that challenged vitalist theories of spontaneous generation (1843), and later investigated the way that metabolism and muscle contraction produce heat to show how all the forces involved in physiological action could be accounted for without the need for occult vital forces (1845).

To truly undercut vitalism Helmholtz needed to develop and defend the idea that physics should be based around the conservation of energy (or "force" as he then called it). All physical theorizing, he argued, should be constrained by the assumption that "all our machinery and apparatus generate no force, but simply yield up the power communicated to them by natural forces, falling water, moving wind, or by the muscles of men and animals" (1854, p.23-4). While forces like heat, electricity, and light were not considered properly mechanical at the time, many attempts were made throughout the 19th century to integrate them into the mechanical frameworks of the times. In any such attempt, Helmholtz argued, the impossibility of perpetual motion should be assumed as a starting point from which one might determine the interrelation between the forces and more traditional mechanical forces. This is the only way, he argued, that all natural phenomena could be adequately accounted for; only by equating (what we now call) potential and kinetic energy, and using these quantities to represent the force created by momentum, heat transfer, and electrical attraction and repulsion, could we ensure that no vital forces would be needed, that physics could one day become a complete science; and biology made consistent with physics. Helmholtz published an elaboration and defence of this idea in an 1847 pamphlet that brought him immediate respect within the scientific community, throughout Europe. The principle of energy conservation was widely accepted even before he formulated it, however, so his work was considered less of a theoretical innovation and more of a well-articulated philosophical defence of anti-vitalism, i.e. an injunction to practice physics empirically, without pernicious metaphysical hypotheses, which fell upon many sympathetic ears.

Helmholtz continued his physiological studies after 1847 with a focus on perception, focusing on physiological acoustics, nerve signaling, and optics. In general, he focused on questions regarding the cognitive and physiological processes by which we gain knowledge through the senses, empirically investigating the structure of our sense organs and the nervous system to which they are connected. In the process he developed the ophthalmoscope, barely different in structure from the device still used to non-invasively observe a living person's retina through the pupil. Already known as a leading proponent of the new energy physics because of his 1847 pamphlet, this work on instrument design and human perception brought him even more notoriety, especially within Germany. His investigations showed him not only the fixed limits of the human sensory system, but that human physiology alone seemed unable to account for our ability to perceive external objects. Thus, he argued, perception is not a physiological phenomenon, but a psychological one; as Helmholtz saw it, many of the actions that result in perceptions (e.g. binocular focus, or the association of points on each retina with each other) are learned skills that operate subliminally in adult humans, unconscious habits rather than pre-determined physiological necessities. Helmholtz could not find any physiological structures determining many of our key perceptual practices, so he drew the conclusion that all concepts and perceptual judgements, including our geometrical conception of space and our perception of spatial relations, are founded in experience not inborn aptitudes. This "empiricist theory of vision" saw people's perceptual abilities as the result of their attempts, from an early age, to see which experiences are the result of their own will and which are outside of their control. This influential and expressly empiricist approach to psychology had a massive influence on Wilhelm Wundt, the so-called "father of experimental psychology."[68]

The foil for Helmholtz's empiricist approach to the science of perception was what he called "nativism." Nativism, at base, assumes that we gain knowledge of the external world through a pre-established harmony between our senses, mind, and reality. Some proponents of nativist theories readily admitted that the supposition of such harmony was a point of metaphysics, having no basis in observation or evidence, but argued that there was no way we could gain epistemic access to reality otherwise. According to Helmholtz, however, humans develop their understanding of external objects in much the same way as scientists develop their theories: through experiment. Whereas scientists conduct experiments in laboratories, individuals conduct experiments from the moment they're born, in the process of everyday practical action,

[68] See Meyering (1989, Ch.7) for details on Helmholtz's empiricist approach to psychology. For details on his empiricist approach to geometry, see Richards (1977). For the way that Helmholtz's empiricist views of spatial perception were misused by Zöllner to ground his belief in a "spiritual" dimension, and the negative impact this had on empiricist approaches in both the science of perception and in psychology, for decades after, see Stromberg (1989).

leading them each to form conceptions of the objective powers that oppose them in such activities. "This concept of a power opposing us," he later writes in *The Facts of Perception*, "is directly conditioned by the ways and means that our simplest perceptions occur. From the very start, the changes which we ourselves make by our acts of will are separated off from those which are not made by our will and which cannot be overcome by our will" (1878, p.361). Learning to see is just like learning to walk, he thought, and we know we are doing it right when we achieve the desired result: capably conceiving, perceiving, anticipating, and interacting with external objects (Lenoir 1993). On Helmholtz's empiricist view, every person capable of perception learned this skill empirically and subjectively, through personal experiment, just as scientists discover phenomena empirically and intersubjectively through collaborative experiment. Writing of both perception and scientific experiment, again in *The Facts of Perception* (1878), Helmholtz emphasizes the need for experiment over passive observation as follows:

> Each of our voluntary motions by which we modify the manner of appearance of objects is to be considered as an experiment by which we test whether we have correctly conceived the lawful behavior of the phenomenon in question, that is, its presumed existence in a definite spatial order.
>
> The convincing force of every experiment is, however, in general so much greater than that of the observation of a process occurring without our involvement, because in the experiment the causal chain runs throughout our self-awareness (p.358; cf. 1862, p.89).

While Helmholtz focused on sight in developing and explicating his views on the physiology of perception, this "empiricist theory of vision" was extended to every form of sense-perception. Through such views, physiology became the centerpiece of his epistemology, which upheld a scientific basis for the claim that our knowledge of reality is limited to the macroscopic objects that we discover by continually bumping up against them in our subjective experience. Scientific experiments may lead us, by similar procedures, to construct new objects like atoms or electric particles to account for the phenomena, but this does not give us license to accept their reality, for unlike our account of macroscopic objects, multiple accounts always seem possible regarding the microscopic causes behind the phenomena (Helmholtz 1869, p.207).

His commitment to this view of perception and scientific experiment led to an incredibly vociferous debate with the physiologist Ewald Hering throughout the 1860s. After several lower scale engagements, Helmholtz lambasted Hering in his *Handbook of Physiological Optics* (1867) for eschewing empiricist theories in favour of nativist ones. Hering's responses after this point were tart and acerbic. For example, Helmholtz had insisted that binocular vision was largely a learned procedure, as no physiological reason had ever been found that prevented up from moving each eye independently. Hering denied it was possible to move each eye independently, and argued that this was for physiological reasons. He hadn't found the physiological structures imposing such a constraint, but argued they must exist because no one (including infants) could ever move their eyes independently. When Helmholtz justified his claim by stating that he had once moved his own eyes independently, having been relaxed enough to forget his unconscious habits, Hering mockingly suggested that the dozing Helmholtz had simply gotten confused. The back and forth between these two rivals involved several personal attacks of this sort, but Helmholtz doggedly stuck to his empiricist opinions and was largely seen to have carried the day by their contemporaries (though it's worth noting that modern physiologists uphold a hybrid of their opinions). Through such debates, Helmholtz's empiricist epistemology not only developed but became more entrenched (cf. Turner 1993).

Heidelberger (1993) and Moulines (1981) go so far as to argue that it was Helmholtz's early work in physiology that made modern empiricism possible, a belief shared by Helmholtz himself. While this may be an overstatement, his role in the development of empiricism is generally understated, likely because so much of what he discovered through his physiological studies quickly became taken for granted common knowledge. Even the arch-empiricist Vienna Circle's "manifesto" *The Scientific Conception of the World* (1929) neglects to acknowledge him as an inspiration specifically for their empiricism. While the manifesto authors note (several times) how they took inspiration from Helmholtz's work on geometry, they also explicitly distance themselves from Mach's distinctly Helmholtzian views about the philosophical significance of physiological studies of perception. But despite Hans Hahn, Otto Neurath, and Rudolph Carnap's failure to acknowledge Helmholtz as a key figure in empiricism's philosophical development, it would be wrong to deny the importance of his historical influence on the empiricist movement in general, and the logical positivists in particular (even if only genealogically). Helmholtz had an undeniably massive impact on attitudes towards science in 19th century Germany, on Mach's phenomenalist empiricism, and on the logical positivists' views about the synthetic (i.e. partly empirical) nature of geometrical truths. In paying attention to the taken for granted nature of physiological facts that Helmholtz established, and the way that such empirical facts nevertheless figure necessarily in Carnap's *Aufbau* and van Fraassen's philosophy of science, it seems right to say that "[h]istorically as well as methodologically, Helmholtz can be regarded as the crucial figure in the evolution of [this] program of basing a theory of knowledge upon the physiology of the senses" (Moulines 1981, p. 66; cf. Turner 1977). In his own life, his physiological studies calcified his empiricist view of science into a more mature form, which he carried forward into his electrodynamics research. In his considered opinion, no matter how crafty an experiment, it could never extend our epistemic reach enough to reveal some hidden truth about unobservable reality, for "[i]n immediate experience we find only extended diversely configured and composite bodies; only on these can we make our observations and perform experiments" (Helmholtz 1871, p. 17).

While Helmholtz was certainly anti-metaphysical from a young age, it's worth stressing how his epistemology of science matured over time into a more consistently empiricist form. Take his early paper on the conservation of energy, for example. Dealing as it does with very rarified issues of the proper constraints to be imposed on physical theorizing, he begins with a distinctly philosophical introduction. There we find a broadly empiricist account of the aim of theorizing in natural science in terms of its ultimate end in view: "Its task will be completed when the reduction of phenomena to simple forces is completed, and when it can at the same time be proved that the reduction given is the only one possible which the phenomena will permit" (1847). The idea is roughly that the models of natural science, including biology, are successful insofar as they are empirically adequate, and uniquely so. But this is at best a nascent empiricism for two reasons. First, he assumes the truth of the atomic hypothesis throughout the rest of the paper, an assumption of questionable merit even to some of his more metaphysically inclined contemporaries (1847, p.22). Second, he also suggests that after experimental science has adequately characterized the phenomena, theoretical science seeks "to evolve the unknown causes of the processes from the visible actions which they present; it seeks to comprehend these processes according to the laws of causality" (ibid.). Even by the standards of the time, the effort to discover the hidden causal processes of nature, and the suggestion that the world is composed of atoms, did not accord with an empiricist image of science.

To understand the evolution of his empiricist scruples it's important to note that in the years that followed Helmholtz rarely engaged in atomic modelling himself, preferring to base his theories around the behavior of macroscopic objects he could directly perceive instead. Furthermore, as his empiricist commitments developed through his physiological studies of perception he grew more explicit about the need to reject physical models based on speculative hypotheses about unobservable entities, given the epistemic limits of the human condition. By 1871 he was promoting a new method for physics that he saw as replicating the sort of anti-realist attitudes of Gauss, Neumann, Faraday and several others (Buchwald 1994, p.400). While it remained agnostic rather than opposed to the existence of atoms, it stood "against the endeavor to deduce the principles of theoretical physics from purely hypothetical assumptions as to the atomic structure of bodies" (Helmholtz 1871, p.17). Rather than basing theories in "disparate and homogenous" entities like atoms, Helmholtz argued that the "elementary volumes" of a physical theory should be "continuous and heterogeneous" like the entities of everyday experience. He goes on to express his empiricist epistemology, and its conviction that theory should be based on experience rather than speculative hypothesizing, as follows:

> The characteristic properties of the elementary volumes of different bodies are to be found experimentally. It is thus admitted that mathematical physics only investigates the laws of action of the elements of a body independently of the accidents of form, in a purely empirical manner, and is therefore just as much under the control of experience as what are called experimental physics. In principle they are not at all different, and the former only continues the function of the latter, in order to arrive at still simpler and still more general laws of phenomena. (1881b, p.19)

The mature Helmholtz does recognize a role for hypotheses about unobservable objects in effective scientific theorizing, to be sure, but only as a "preliminary stage to a law" (as quoted in Schiemann 2009, p.165). Rather than being claims whose truth should be tested and supported through experiment, hypothetical postulates like atoms are used heuristically, to help suggest phenomenal laws that can be tested. Thus, he writes, "Every legitimate hypothesis is an attempt to establish a new, more general law that covers more facts than have been observed. Verifying it then means that we try to develop all consequences it will have, particularly those that can be compared to observable facts" (1874, p.416). While we might want science to uncover the true, unobservable, and unchanging cause of our observations of change, science itself has shown us its limits, and our inability to achieve that realist ambition. For the basic physiological facts of human perception show us that we will never be able to know reality as it is, in itself. Hypothesizing can lead to our positing and testing of laws, but he cautions that it is "unworthy of a thinker wanting to be scientific if he forgets the hypothetical origin of his principles" (1878, p.360). Helmholtz, when he did discuss it, certainly never forgot the hypothetical nature of the atomic hypothesis, and eventually abandoned any suggestion that he believed in atoms, and cautioned about the vanity of any scientific effort to establish one's favoured hypothesis as *more* than a hypothesis. Hypotheses about unobservable entities were not to be proven by experiment, for we "are never justified in making an unconditional claim of this type" (Helmholtz 1878, p.362 ft. 623), and should use hypotheses only to guide the formulation of new laws and discovery of new phenomena, the true aim of science. While an assertion of an empirical law can also never be fully certain, and "never thus attains unconditional truth," testing of a law's empirical robustness can confirm it with "such a high degree of probability that it is practically equal to certainty" (1877, p.226).

More problematic from the empiricist perspective is the fact that, throughout his life, Helmholtz retained his commitment to the essence of his 1847 characterization of "the final goal of the theoretical natural sciences" as the discovery of "the ultimate invariable causes of natural phenomena" (p.4). Causality, for Helmholtz, was an "a priori given … transcendental law" and a "condition of conceivability" for any scientific theory (Helmholtz 1878, p.363). Part of his mature philosophy of science was that the search for causal laws was a fundamental part of scientific practice, a "regulative principle of our thinking" because the only conceivable world is one that obeys causal laws (ibid.). By contrast, today's empiricists (e.g. van Fraassen 1989) tend to be skeptical of metaphysically-laden notions like "cause" and "law of nature," so it might seem like Helmholtz does not fit in the empiricist tradition, in either our modern terms or those of his contemporaries. An understanding of what Helmholtz means by "causal law" clarifies his empiricism, however, rather than undercutting it.

According to Helmholtz, individual causal laws are determined only through experiment and observation, and never transcend the phenomenal level. A causal connection is something to infer when, through exhaustive laboratory investigations, we find ourselves unable to alter the phenomena in specific ways. His commitment to providing causal accounts of the world simply mandates that we stridently attempt to alter phenomena, and accept an observed regularity as governed by a causal law only if we are convinced that things will occur in that manner no matter how we attempt to prevent them from doing so. This conception of causality is metaphysically thin, amounting to nothing more than the immutable operation of law-like regularities at the level of observables, and Helmholtz does not develop a law-independent notion of causality based on casual powers, dispositions, or unobservable mechanisms as realists tend to (e.g. Chakravartty 2007, Woodward 2003). This places his approach to causality squarely within the common empiricist approach of, for example, David Hume (1738, Section VII) and the post-Kantian approach of Bas van Fraassen (1989; cf. 2002, ch.1), where the use of exceptionless laws to describe the world can be endorsed because it is a condition for the possibility of scientific understanding. Suitably defined, causality is an innocuous notion for empiricists; it is when we begin to speak of hidden causal powers, unobservable properties, and invisible processes that talk of causation becomes problematic for empiricists. While the indeterministic nature of quantum mechanics may provide a partial counterexample to Helmholtz's picture of causality as a regulative constraint on scientific theorizing, the theory obviously antedates Helmholtz's thinking on the subject by several decades, so the possibility of such a probabilistic account of reality was understandably not anticipated (cf. van Fraassen 2004).

His commitment to characterizing phenomena in terms of system energies and forces should also not be confused with a commitment to casual powers in some metaphysically robust sense, for it is nothing more than an appreciation that our experience of objects is fundamentally dynamical, not purely kinematic. According to his empiricist theory of perception, force and matter are constructed together in the process of learning to perceive, so our scientific accounts cannot dispense with either without dispensing of both (1847, p.4). Thus, even in his commitment to causality and forces as regulative constraints on scientific theorizing, Helmholtz embodies an empiricist outlook, for he is upholding nothing more than a methodological injunction to search for laws and regular connections between perceivable objects that appear "independent of our thought and will" (Helmholtz 1869, p.209). His mature understanding of "reality" is a Fichtean one, where knowable reality is purely noumenal, just the sum of all resistance effected by an unknowable "thing-in-itself" on our free actions (Heidelberger 1993, 495; cf. Schiemann 2009, p.194). Through the regulative ideal of the law of causality, which informs scientists' construction of phenomenological laws through laboratory experiments in the same way as it informs our individual learning of perceptual skills through practical experiment, we can learn much about the phenomena of nature; but, Helmholtz stresses, we can have no knowledge "about the actual real which underlies the phenomena; all opinions which we may otherwise harbor in this regard are only to be considered as more or less probable hypotheses" (1878, p.377).[69]

Thus, when Helmholtz moved to Berlin in 1871, his empiricist vision of science was central to shaping his rather unique approach to electrodynamics research, which I discuss in the next section. Before moving on to discuss the development of Weber's realism and Maxwell's pragmatism, it's important to note that while Helmholtz was most clearly opposed to realism he was also opposed to pragmatism. In "The Relation of the Natural Sciences to Science in General" (1862) Helmholtz states that "Whosoever in the pursuit of science, seeks after immediate practical utility, may generally rest assured that he will seek in vain" even if ultimately "the governments and people of Europe" support science because it might help to "establish the supremacy of intelligence over the world." Helmholtz, like all working scientists, was conscious of why scientific research is supported by non-scientists, given its potential to inform action; but insofar as he saw his own mandate, as a scientist, it was simply to discover and save phenomena. While the force with which material objects and phenomena resist our practical actions are what allow us to learn how to perceive and represent them in empirical laws, using such knowledge to inform action was, properly speaking, not the ultimate aim of scientific inquiry.[70]

In contrast to Helmholtz's doggedly empiricist approach, Weber approached electrodynamic research from a manifestly realist perspective. For a few reasons, there is less to say about Weber's philosophical development than either Helmholtz's or the Cambridge physicist like Maxwell. For one, Weber was not a philosophically minded scientist, and did not employ overtly philosophical arguments as Helmholtz did (e.g. in his (1847) and (1867)). Nevertheless, while not explicitly contemplated, challenged, or defended, his realist attitude towards his electrodynamic theory is quite clear in his writing and actions. But also, unlike Maxwell, Weber

[69] For an excellent and extensive account of the evolution of Helmholtz's philosophy of science, see Schiemann (2009, Ch.7).

[70] Later in life (1887), as the most eminent living physicist in Germany, Helmholtz was made the first director of the German "Bureau of Standards" (*Physikalisch-Technische Reichsanstalt*), whose mandate was (in part) explicitly to support practical, technological development. Nevertheless, his work up until then displayed relatively little concern with practical matters (when compared with Kelvin, Maxwell, or even Weber for example).

was not inculcated with any clear method or vision of science's ultimate aim, for like most continental physicists of his era he gained his technical skills mainly through family connections and personal study rather than formal training (Warwick 2003, p.33). German and French scientists held diverse opinions about the aims and epistemology of scientific research at the time, so Weber existed within a far more philosophically and methodologically inhomogenous community than Maxwell, the latter having worked within the distinctive tradition of mathematical physics that had developed at Cambridge throughout the 18th and early 19th centuries. Thus, Weber was largely left to his own devices in developing a vision of science to guide his scientific practice, observing and imitating the approaches of some of his contemporaries and immediate predecessors while departing from others. As such, it will help to place Weber in context by discussing the work of several contemporary physicists working on the science of electrically-charged, current-carrying, and magnetized bodies in motions (i.e. electrodynamics), not just in terms of their theories, practices, and results but also in terms of the aims and outlooks that led to their distinctive approaches. In the end, we will see in this section and the next, Weber followed André-Marie Ampère's lead in theory, practice, and philosophical disposition, coming to embody a distinctly realist approach to electrodynamic inquiry. So, whereas Helmholtz came to electrodynamics research as a committed empiricist, and Maxwell was imbued with a pragmatic outlook by the culture at Cambridge, Weber's realism developed through his experimental work in this field, specifically his development and testing of Ampère's hypothesis that all electrical and magnetic action results from the presence or flow of two types of electrical corpuscles. Thus, I confine myself in this section to a discussion of Weber's early work testing Ampère's electrodynamics, which grounded his realist attitude towards (one interpretation of) that theory that guided the subsequent work that I discuss in the next section.

While his work in electrodynamics began through a metrological collaboration with Carl Friedrich Gauss, a dogged empiricist, Weber parted ways with Gauss when it came to how he practiced science in his most productive period. Guided by empiricist scruples, Gauss consistently shied away from making or affirming any hypotheses about the nature of magnetism. Contradistinctively, Weber did not (Darrigol 2000, p.56). Their metrological collaboration was nevertheless incredibly fruitful, through which Weber learned Gauss's techniques of determining absolute units of measure for physical quantities. At the time, an absolute unit for measuring the magnitude of some type of physical quantity was one defined entirely in terms of time, distance, and mass (or acceleration).[71] One would define a basic unit of electric charge, for example, as that which could move a specified mass a specified distance in a specified time. The familiar SI units of the coulomb, ampere, ohm, etc. are all absolute in this sense, and the practice of defining our electromagnetic units in this way is primarily the result of Gauss and Weber's work, both collaboratively and separately. Absolute units contrast with relative units, which measure a quantity relative to another instance of that same quantity. For example, one might define a basic unit of electric charge relatively as the amount of charge produced by one full turn of the winch on their own electrostatic generator, such that the magnitude of other charges would be measured as some multiple or fraction of that unit. The use of relative units by various researchers led to a great deal of imprecision and a lack of standardization, making communication and comparison of results more difficult than it would have been with absolute units. Some of the lasting significance of this metrological work by Gauss and Weber, and later Weber alone, will become clear through the next two sections, but even at the time it was widely understood as extremely important work, most especially by Gauss and Weber themselves. Together their central ambition was to provide a new standard of metrological units for the practice of physics, but as a starting point the majority of their work focused on establishing an absolute unit for measuring magnetic magnitudes alone. Once they'd achieved this goal they set about putting it to work, founding the "Magnetic Club" in Göttingen to promote and aid in the mapping of Earth's local force of terrestrial magnetism in many locations around the world, using mathematical techniques that Gauss had previously devised to simplify the complex task.

Gauss's empiricism lent itself to precision measurement work rather well, allowing him to ignore the debates about the underlying cause of magnetism that occupied many of his peers, focusing instead on devising unit measures and using whatever mathematical tools he needed to calculate the strength and vectors of terrestrial magnetism in such units. Whereas many at the time felt the need to devise some story about what was responsible for the observed phenomena of nature, Gauss took a distinctly phenomenological approach similar to Helmholtz's statements

[71] Weber would later strive to define absolute units in terms of distance alone, by taking his own electrodynamic force law and Newton's law of gravitation as fundamental laws of nature capable of defining time and mass (respectively) in terms of distance.

above: "By explanation *[Erklären]*, the *Naturforscher* means nothing but the reduction to the smallest possible number of simple fundamental laws; he knows nothing beyond these laws [...] but he derives the phenomena from them exhaustively and with full necessity" (Gauss 1836, p.315-6, as quoted in Darrigol 2000, p.51). Again like Helmholtz, Gauss expressly rejected the idea that scientists were motivated by pragmatic values. While he recognized the practical utility of scientific research was what brought him State support, for a scientist like himself, he said, "the search for the laws of natural phenomena has an end and value in itself, and peculiar charm accompanies the discovery of measure and harmony in the apparently ruleless" (1837, p.11, as quoted in Darrigol 2000, p.49). But to properly characterize the phenomena of nature, Gauss recognized he would first need the new system of absolute units and the local measures of geomagnetism that he and Weber were working towards. The units would be needed for experimental precision across different phenomena and different laboratories, while the local measures of terrestrial magnetism would be needed so that experimenters could properly account for its influence on observed phenomena. Only then would the phenomena be properly identifiable, and only then would Gauss and others be able to find a harmonious and simple set of laws capable of accommodating them all. Thus, it's important to recognize the empiricist character of Gauss's measurement work, aimed at the discovery of phenomena and their harmonious representation, rather than the kind of measurement work that Weber later pursued to support his realism about electrical corpuscles (as discussed in the next section).

The empiricism of Gauss and Weber's contemporary Franz Neumann also served him well, with considerations of observability consciously shaping the mathematical theory he developed to treat "Volta-induction" (i.e. induction by varying currents) (Darrigol 2000, p.46). Like Gauss, Neumann took great care to eliminate the hypothetical elements from the theories he used when conducting measurements in the physics of crystals and heat, opting to work whenever possible only with differential equations, observable quantities, and the means to measure them (ibid., p.44-5). As we'll see, Helmholtz found Neumann's use of potentials to represent electrodynamic interactions especially suited to his own empiricist approach, given its ontological minimalism. Unlike Helmholtz, however, Neumann and Gauss's empiricism did not primarily motivate them to try to reveal new effects, either in the laboratory or even as predictions derived from theory. Instead, Gauss looked to provide the technical and empirical foundations needed to quantitatively characterize laboratory phenomena, while Neumann set himself the task of seeing if an already well-known phenomenon—Volta-induction—could be given a quantitative description using Ampère's law. Before becoming aware of Neumann's work Weber pursued the same goal, but their philosophical differences became embodied in different aims, approaches, conclusions, and subsequent research. From the start Neumann carefully investigated whether Ampère's law for the interaction between two currents could be used to derive the empirical laws of known phenomena, and in the early stages "limited the scope of his theory to cases for which the empirical basis was unquestionable: linear conductors, closed circuits, and slowly variable currents" (ibid., p.48). Weber, by contrast, approached the problem of accounting for Volta-induction as a means of testing the truth of a hypothesis suggested by Ampère: that the underlying cause of those electrodynamic actions expressed in his law was the presence and fluid movement of two-types of charged corpuscles. While Neumann eventually extended his treatment beyond linear and closed circuits affected by slowly variable currents to accommodate open and highly variable currents in 1845, he was extremely cautious in his claims, never suggesting that Ampère's theory could account for those cases of induction before proving it. By contrast, when his effort to accommodate these previously unaccounted-for phenomena in Ampère's scheme proved successful, Weber reached a much less bounded conclusion than Neumann.

Unconvinced by Ampère's claim that he had derived his theory directly from experiment (D'Agostino 2000, p.20), Weber would take an interest in testing and elaborating Ampère's basic theory after his collaboration with Gauss was abruptly ended in 1837. Dismissed for political reasons from the Göttingen Institute where they worked together, Weber remained in the city without a post for several years, eventually accepting a position in Leipzig in 1843 before returning to Göttingen in 1849 where he would remain. Beginning in Leipzig he set himself a new research agenda, working to place Ampère's electrodynamics on a more secure empirical footing through experimental corroboration and filling in its details by measuring various constants and parameters. To accomplish this task, he developed a new kind of measuring device: the electrodynamometer. He described this device as an "organ" for investigating electrodynamic phenomena, capable of giving the "soul" of Ampère's electrodynamic theory the connection to empirical facts that it needed to flourish. "Be it for firmer foundation and fructification or for refutation," he wrote, the electrodynamometer provided "a more accomplished technique of observation that enables us to enter more specific discussions of the comparison between theory and experiment and thus to equip the soul of the theory with an appropriate organ of observation, without which the soul's forces cannot unfold" (Weber 1848, p.216, as quoted in Darrigol 2000, p.56-57).

Using this device, in combination with existing instruments, Weber verified several predictions of Ampère's law, carefully eliminating possible perturbations and describing his procedures in detail (Darrigol 2000, p.59). To his delight, the crucial experiments he conducted regarding torque effects were in complete agreement with the predictions of theory. He had conducted no experiments regarding Volta-induction, however, and thus like Neumann prior to 1845 had not fully demonstrated that Ampère's approach was empirically adequate to such inductive phenomena. Nevertheless, Weber was so impressed with the experimental corroboration of those predictions of Ampère's theory that he had tested that he drew a conclusion well beyond what had been established: "This complete agreement between the values calculated by Ampère's formula and the observed ones [...] is, considering the diversity of the circumstances under which this agreement holds, a complete proof of Ampère's fundamental law" (Weber 1846, p.50, as quoted in Darrigol 2000, p.59). In short, despite not having tested anything near to the empirical consequences of that theory, Weber displayed the epistemic hubris of a realist impressed by several promising results, unlike Neumann's display of epistemic humility characteristic of an empiricist, withholding judgement until a conclusion had been firmly established through observation or mathematical demonstration.

Following his experimental tests of Ampère's law, Weber began to think about how to build a more complete electrodynamic theory on Ampère's foundations. Like Neumann, his attention was quickly drawn to Volta-induction, an important phenomenon that had not been quantitatively accounted for by any electrodynamic theory he was aware of (he only became aware Neumann's work later), and therefore could not be related to other electrodynamic forces except through Lenz's purely empirical and qualitative rule (Darrigol 2000, p.61). It seemed a natural target on which to extend Ampère's approach. To do this he went beyond the pure mathematics of Ampère's theory and looked to investigate the fruitfulness of his favoured hypothesis regarding the underlying mechanical cause of electrodynamic action: that all electrical and magnetic phenomena resulted from the presence or flow of two types of electrical fluids, each composed of corpuscles with opposite charges, all acting on one another from a distance.[72] This approach had already been explored by Gustav Fechner, Weber's predecessor at Leipzig, which is why the resultant theory is often referred to as the "Ampère-Fechner-Weber" theory. While Fechner had struggled with the mathematics, Weber did not, and showed that a theory based on Ampère's hypothesis could indeed provide an electrodynamic account of the known empirical laws of Volta-induction. It's at this point that Weber became convinced not only that Ampère's law was in agreement with all electrodynamic phenomena, but that Ampère's hypothesized electrical corpuscles were real. His grounds for reifying them were expressly because they seemed able to explain such a wide range of electrodynamic phenomena, including magnetism and induction (ibid., p.56); that is, in characteristically realist fashion, the truth of the hypothesis was inferred from its explanatory power.

In this way, Weber was the true successor to Ampère. When Ampère observed the replication of Oerstead's experiments demonstrating a relation between electrical and magnetic effects, he almost immediately attempted a bold ontological and theoretical reduction of the latter to the former, supposing that magnetism was the product of closed helical current flows inside the molecules of magnetic materials. As he saw it, the existence of electrical current flows was all that was needed to explain electrostatic, electrodynamic, and electromotive phenomena, and despite not being able to observe such flows inside magnets their existence could be inferred from their explanatory power. In 1820 he provided what he took to be "definitive proof" of the electrical nature of magnets by showing that flat helical currents were not only attracted to each other but were affected by a bar magnet. Ampère was committed to the idea of a simple underlying order to reality, and this bold hypothesis of helical currents provided such a picture by eliminating the need for magnetic fluids, with its parsimonious explanatory power evidencing (as he saw it) its likely truth (ibid., p.7). It was his conviction in the reality of this reduction that resulted in the field being generally known as "*electro*dynamics," rather than, say, "electro*magneto*dynamics," for as Ampère understood it magnetic attraction and repulsion was just a special form of electrical attraction and repulsion. In coining the term for the new field, Ampère (1822) is explicit:

[72] While the question of these particles' volume and mass would become an interest of Weber and others later, he initially conceived of them as point-particles of zero mass.

> The term *electromagnetic action*, that I use here only to conform to custom, can no longer be appropriate to designate this kind of action. I think that it must be called *electrodynamic action*. This term expresses the idea that the phenomena of attraction and repulsion that characterize it are produced by electricity moving in conductors. (p.200)

Like Weber, the vast majority of Ampère's experimental work was shaped by his commitment to the reality of electrical currents and his claim that they were the source of magnetism. "In general," Darrigol writes, "he knew the results of his experiments in advance" because his "experiments were planned according to preconceived theoretical ideas. Only the very first experiments had an exploratory value" (2000, p.13, 12). As such, his experimental work was aimed at supporting his theory, not developing it. He tried to evangelize for his theory amongst his French contemporaries by portraying it as independent of any particular picture of the electric current, claiming it was derived directly from observation, and kept his speculations on underlying causes mostly to himself. Nevertheless he did, in characteristically realist fashion, tend to forget the hypothetical character of his electrical conception of magnets, so in discussing his experiments he often conflated the experimental facts about how magnets interacted with electrical phenomena with his theory's interpretation of magnetic action as resulting from naturally occurring helical electric currents (Darrigol 2000, p.13-4). He regularly held appointments with contemporaries to conduct experimental demonstrations he took to be conclusive, hoping to convince them that his picture was correct, but they were often unimpressed. Laplace and Oersted were particularly unconvinced, reporting that his clumsy experiments rarely succeeded, that he would often use his hand to push his apparatus to produce the predicted effect when it didn't perform, and that he was unskilled as a debater (ibid., p.14). These are the actions we would expect of someone convinced of the truth of their theory, growing frustrated with uncooperative instruments meant to provide "definitive proof" of its truth.

As Darrigol (ibid.) stresses, the hypothetico-deductively framed character of Ampère's experimental work, always aimed at corroborating novel predictions to try and convince others that magnetism was *really* electricity flows, can be seen in the way he built his apparatus:

> The more definitive devices were a direct expression of his theoretical beliefs within material constraints such as the compatibility of mobility with current feeding. They served a unique function and could not be transformed to answer new questions. (p.12-3)

We'll see something similar in the material character of Weber's experimental apparatus in the next section, for like Ampère his realist convictions motivated certain forms of experimental practice, including the construction of rigid rather than malleable forms of apparatus. These styles of instrument building and experimental work can be contrasted with the much more exploratory work of Michael Faraday (and later Helmholtz). Faraday carefully maintained a typically empiricist agnosticism about the ultimate causes of electrical and magnetic phenomena. Addressing Ampère's hypothesis in the context of his methodology explicitly, Faraday (1822) clarified his position as follows:

> I have not intended to adopt any theory of the cause of magnetism, nor to oppose any. It appears very probable that in the regular bar magnet, the steel, or iron is in the same state as the copper wire of the helix magnet; and perhaps, as M. Ampère supports in his theory, by the same means, namely currents of electricity; but still other proofs are wanting of the presence of a power like electricity, than the magnetic effects only. (1822)

Like Helmholtz's empiricist approach to laboratory work outlined in the next section, Faraday patiently investigated electrical and magnetic interactions in varied and extensive detail, iteratively varying his apparatus slightly by shifting a wire or other contrivance in as many ways as he could imagine, trying to elicit novel effects. Faraday worked largely qualitatively, and when a new effect was discovered he would investigate its different manifestations and refine his apparatus until the effect was as apparent as possible. Whereas Ampère was aiming to justify his speculations about the unobservable mechanisms responsible for electrodynamic effects, conducting public demonstrations of different effects suggested by his theory, Faraday worked simply to explore the phenomena directly, thinking without any firm theory and only "in terms of vaguely defined powers and concretely imaginable actions" (Darrigol 2000, p.21). He told Ampère directly that he was "naturally skeptical in the matter of theories," and it was this distinctly empiricist attitude that guided his entire approach to experimentation. Because his experimental practice was not committed to any substantial theory, Faraday eventually discovered a phenomenon that Ampère had never imagined and therefore never sought to produce, even though it was implied by the mathematics of his theory: continuous electromagnetic rotations of current-carrying wires. Ampère had built his theory largely in analogy to the theory of gravitation, and for material objects governed by standard mechanical laws the effects of friction meant that a continuous circular motion could never be produced in any system. So, even while the angular dependence of electrodynamic forces in Ampère's theory produced a dynamics that allowed for such motion, he did not foresee it as a consequence of his theory (ibid.). Faraday, by contrast, was not building his devices solely to demonstrate noted consequences of an already favoured theory, and was able to freely explore a wide variety of effects until, as he reports it, he happened upon the idea of such rotations while investigating the action of a vertical current-carrying wire on a compass.[73] The possibility of circular actions in general had been suggested previously, but drove those who understood it to immediately speculate about its cause, rather than to demonstrate or explore it in the lab (ibid., p.23). In contrast, Faraday did not aim to speculate about the underlying mechanical cause of macroscopic phenomenon like electromagnetic rotations, either before or after he had demonstrated their existence. Thus, leading up to his demonstration, unguided and unconstrained by theory, Faraday was able to freely modify his device until the effect was as clear as he could get it. Once its existence had been experimentally established, he did not infer that Ampère's theory was true, and instead simply took magnetic powers and the poles from which they appeared to emanate as primitive facts about the nature of magnets.[74]

Hearing of Faraday's discoveries reinvigorated Ampère's experimental inquiries, as he had become weary of such work and occupied with more speculative concerns. Writing to a friend he expressed his change of concern in this way: "Metaphysics was filling my head. However, since Faraday's memoir has appeared, all my dreams are about electric currents" (as cited in Darrigol 2000, p.23). He was unphased in his commitment to his view of magnetism, however, so unlike Faraday, Ampère refused to accept a unique and macroscopic magnetic dipole in addition to currents. The multiplication of entities not only failed to create the kind of simple

[73] Darrigol (2000) stresses that Faraday worked hard to focus his attention on exploring electrodynamic phenomena rather than getting caught up in practical application or theoretical explanation, a sensible approach for any empiricist who sees the discovery of novel phenomena as the centre of the epistemic action. As Darrigol put it:

> Faraday avoided two ways of blocking the exploratory function of experiments.
>
> First, he did not divert his energies into developing practical applications. He was 'rather desirous of discovering new facts and new relations than of exalting the force of those already obtained.' He was satisfied as soon as the new effects were clear and easily reproduced (eventually in the classroom), and left to others the conception of efficient electric motors and dynamos. Second, Faraday did not let theory invade his researches. Although theoretical prejudices, such as the existence of induced currents, the electro-tonic state, or the vortices in Arago's disk, played a role in orienting his research, they were easily correctible. Faraday was proud and eager of this flexibility, and denounced the sterility of closed mathematical theories. (p.38)

[74] A similar story can be told about the reasons why their differing philosophical outlooks led Ampère away from investigating the phenomena that would reveal electromagnetic induction, yet led Faraday towards them. For brevity I do not tell that story here, though its details can be found in (Darrigol 2000, p.31-39).

and unified ontology Ampère was convinced governed both electric and magnetic phenomena, but Faraday had not even provided a mathematical theory to accompany his theory of poles and powers, never mind a mechanical one. In publishing a systematic account of his mature theory in 1826, Ampère portrayed his claim that electrical currents were the source magnetism as undeniable, claiming that it was the only way to reductively unify all the phenomena in a single theory. He also continued to ape Newton's defence of gravity by saying that the electrical nature of magnets was simply manifest in the empirical facts. He nevertheless took care to express his theory mathematically rather than mechanically, though by this time he felt it was appropriate to share his speculations about the cause of electrical currents after the theory had be exposited. Here he suggested not only the action-at-a-distance approach to accounting for electrodynamic action that Weber would eventually develop, but also the possibility of giving an ether-based account of currents. Weber's realist convictions developed because of his success in establishing the explanatory power of the former approach for previously untreated phenomena like Volta-induction, and like Ampère's convictions they came to shape everything he did afterwards, as we'll see in the next section.

It's worth noting that Weber's metaphysical orientation, as found both in his writings and his actions, is distinct from the metaphysical orientation of German idealists or the mystifying romanticism of *Naturphilosophie*. His predecessor Fechner freely spoke of *Geist* or spirit as the order governing electrodynamic phenomena and physics generally (Buchwald 1994, p.395), while Weber's most ardent proponent Friedrich Zöllner believed that a mind in harmony with nature could reveal reality's secrets through intuition alone. The latter's *a priori* approach to physics, based on taking Weber's electrodynamics and Newton's physics as fundamental principles of the natural order in no need of empirical support, led him to attack Helmholtz's focus on building his electrodynamic theory through experimental inquiry, for Zöllner denied that laboratory experiments were capable of revealing anything of fundamental significance (Buchwald 1994, p.402). Despite lacing his criticisms with anti-Semitic suggestions that Helmholtz was some sort of Jewish patsy, Helmholtz responded quite calmly, saying that such personal attacks were natural for anyone that took metaphysical positions as articles of faith, an obvious effort "to conceal from themselves and from the world the weakness of their own position" (Helmholtz 1874). Weber certainly speculated about the inner workings of nature more than would have been allowed within the positive science of his French contemporaries, who viewed *Naturphilosophie* and idealist metaphysics as patently ridiculous, but he didn't share these outlooks. Weber was only a "metaphysical" thinker in the manner characteristic of a scientific realist, believing as he did that explanatory power counts as evidence of a theory's truth and that scientific experiments and instruments can (and should aim to) reveal facts about unobservable reality.

To finish this section, I'll need to establish that Maxwell is best thought of as neither a realist nor an empiricist, but as a pragmatist. To do this I provide a social history of Cambridge mathematical physics that explains how, by the middle of the 19^{th} century, a distinctive approach to physical research had developed there that saw scientific theorizing as tethered to practical matters in a manner uncharacteristic of both realism and empiricism.

The tradition of mathematical physics that Maxwell was reared within arose at Cambridge during the 17^{th}, 18^{th}, and 19^{th} centuries, developing quite differently from physics on the European continent. The story of how that happened begins with the different ways these communities received Newton's new system of mechanics. The 1687 publication of Newton's *Mathematical Principles of Natural Philosophy* (aka *Philosophiæ Naturalis Principia Mathematica*, or "*Principia*" for short) is often seen as the culmination of "the gradual abandonment of the search for the causes of physical phenomena in favor of the mathematical certainty of what was known from the early seventeenth century as 'physico-mathematics.'" (Warwick 2003, p.28). Despite meeting some initial resistance from Newton's local rival Robert Hooke and his supporters, Newton's physical picture, modelling techniques, and the inductive methodology he suggested led him to develop his law of universal gravitation quickly became the pride of Britain. As Newton's physical picture was gradually accepted on the continent (despite Cartesian resistance) a divide persisted in the way mathematical physics was taught, applied, motivated, and practiced by British physicists and by their continental counterparts throughout the 18^{th} century, mainly because the former focused on developing and using Newton's method of calculus and the latter on Leibniz's.

The divide was by no means absolute, however, as both British and continental physicists remained interested in each other's work. Along with Newtonianism, traditions of experimental philosophy had become well established throughout Europe. Novel experiments were conducted on a great variety of previously under-studied phenomena, with Newton's work serving as an exemplar for how to represent the results. Modeling many of these phenomena could be made easier by employing one of the Leibnizian reformulations of Newtonian mechanics developed by the Swiss mathematician Leonhard Euler, the French polymath Pierre-Simon Laplace, or the Italian physicist Joseph-Louis Lagrange in the 18th century. These methods of "analytical mechanics" constituted a collection of innovative and powerful mathematical techniques for quantitatively and mechanically modeling the physical world using algebraic methods alone, doing away with the geometrical (and therefore synthetic) character of Newton's methods. Their physical assumptions were Newtonian, and logically they were equivalent, but they were mathematically more elegant, making it easier to solve a variety of mechanical problems that would be more difficult using Newton's calculus (Schaffner 1972, p.41). While the earliest developments of analytical mechanics on the continent found parallels in British developments of Newton's methods, by the mid-18th century Britain had failed to keep up. Unfamiliar with continental mathematics in general, and having no local experts sufficiently capable of assisting in their familiarization, it proved increasingly difficult for British mathematicians to understand and adapt the new techniques to their own system.

Whereas the novelty and technical complexity of Newton's physics had slowed its translation and acceptance on the continent, even once Cartesian objections were overcome, the new techniques of analytical mechanics now faced a geographically inverted version of the same problem (Warwick 2003, p.34). This led to a relative decline in productive physical research in Britain during the last half of the 18th century, sometimes leading to the impression that Newton's legacy of excellence in mathematical physics had been squandered by Cambridge. But at the same time, through a mixture of design and circumstance, a distinctive pedagogical culture was developing at Cambridge that would allow British physics to rise to prominence again throughout the 19th century through a unique focus on analytic problem solving.

To understand how this happened it helps to focus on the way undergraduate exams were structured and evolved over time at Cambridge. Final examinations at Cambridge conducted by the Senate House (known as the "Tripos" for etymologically mysterious reasons) were conducted over the course of a grueling few days, and the results were used to give a linear ranking of every graduate, from the highest scoring "senior wrangler" to the lowest scoring "wooden spoon." With the industrial revolution in full swing in Britain, the increased potential for social mobility in mid-Georgian England meant young men were increasingly motivated to not only attend university, but also to achieve a high rank that could earn them an award, a well-paid fellowship, or public appointment after graduation. Traditionally Tripos examinations had been entirely oral, but it gradually became clear that "mathematics was especially well suited to an examination system that sought to discriminate between the performances of large numbers of well-prepared students" (ibid., p.57-58). It was relatively straightforward to simply increase the difficulty of the "mixed mathematics" problems posed over several days of examination, allowing examiners to meritocratically rank students' according to their relative mastery of such methods. Testing for mathematical knowledge orally was rather awkward and time consuming, however, and enrollment at Cambridge was growing, so paper portions were eventually added to the series of Tripos examinations. Over time these portions were gradually expanded, and Cambridge's traditional role as primarily a seminary institution, along with its traditional ideals of liberal education, began to wane, giving way to a Tripos, curriculum, and pedagogy that increasingly focused on testing, teaching, and producing mathematical problem-solving ability.

When paper records of past exams were eventually made available for students to study, a dramatic change in the kind of mathematical skills prized at Cambridge was made apparent, as developing an ability to solve Tripos-like problems became the central aim of undergraduate studies (ibid., p.144, 155). The skills many students developed for solving such problems led to a kind of bootstrapping effect in the university's pedagogical culture and examination standards over time, as examiners needed to make the exams more difficult each year to get a proper linear ranking of the increasingly well-prepared students. This, in turn, made the mathematics portions of the examination lengthier, so by the end of the 18th century mathematics made up a significant portion of Tripos examinations, dominating the Bachelor of Arts exams "to the almost complete exclusion of all other subjects" (ibid., p.37; cf. Ch.2).

Thus, by the end of the 18th century private tutors became quite commonplace, employed even by the less ambitious students (provided they could afford them) to gain some advantage in the increasingly difficult later portions of the exams. Having no affiliation with the university or colleges these tutors were free to develop their own methods of instruction. They quickly found it advantageous to group their students into classes by their ability, facilitating a rapid pace and efficiency of instruction.[75] When the new continental methods of analytical mechanics were eventually picked up at Cambridge during the 1820s, leading to what is often called the "analytical revolution," it was not because the professors or examiners decided they would be adopting continental approaches; rather, these techniques came to Cambridge "from below," through the efforts of tutors seeking to provide instruction in the most useful and effective mathematical techniques, combined with the interests and initiatives of the student body (such as the formation of the famed Analytical Society). Only once these techniques were commonplace in tutorials did colleges begin instruction in them, and examiners begin testing student's skills at using them (ibid., p.66-84). While many Cambridge examiners were opposed to the use of Leibniz's d-notational calculus, within which the new analytic methods were all framed, the adoption of it by undergraduates who would eventually become examiners themselves gradually led to its wide acceptance.

Pressure on examiners to pose ever more difficult problems in the later portions of the mathematical Tripos grew continuously. Some of the questions became so advanced that they effectively involved cutting-edge methods, leading tutors and students to pay close attention to the current state of research. This meant that a top-ranking Cambridge graduate could conduct original research immediately following graduation (and was often invited to do so at Cambridge through the awarding of a college fellowship).[76] In this way, the new pedagogical techniques of private tutors and the Tripos's ever increasing reliance on paper examinations resulted in "what contemporaries characterized as the industrialization of the learning process" at Cambridge (Warwick 2003, p.x). By the time Maxwell was undertaking his studies there in the 1850s, the university had been transformed from an ordinary seminary training ground organized in the scholastic style of the Medieval university into a factory capable of manufacturing dozens of brilliant, adept, and productive mathematical physicists every year

[75] In the 1830s these tutors were given the nickname "coaches," from which our modern use of the word derives. The nickname came from then-modern and extremely rapid method of transportation by stage coach, denoting the tutor's ability to "drive" and "steer" the learning of their pupils, inculcating advanced mathematical skills with a speed that lectures and private study alone rarely could (Warwick 2003, p.xx).

[76] William Thomson (later Lord Kelvin) and John William Strutt (Lord Rayleigh), in fact, both reported being inspired to produce novel theoretical work during the Tripos exam itself (ibid., p.158), and in the latter half of the 19th century excellent undergraduate students such as R. L. Ellis, Arthur Cayley, and Thomson published original results well before even receiving their degrees (Smith and Wise 1989, p.176).

using only pen, paper, classrooms, and young men with prior classical educations as raw materials.

Upon graduation, senior wranglers at Cambridge were masterful mathematicians, but what was unique about their education was how well practiced graduates were at applying their mathematical skills to the modelling of various physical systems. As Warwick (ibid.) notes:

> Mastering mixed mathematics was not just a matter of learning the appropriate notation and writing out proofs and theorems until they could be reproduced from memory—though these activities were important—but of learning through the very act of repeated and closely supervised rehearsal on paper to manipulate mathematical symbols according to the operations of, say, algebra or calculus, to apply general laws and principles to numerous physical systems, and to visualize, sketch, and analyze a wide range of geometrical and physical problems" (p.171-2).

Upon graduation, the Cambridge-educated found themselves amongst a large community of extremely capable peers with whom they could productively collaborate and communicate, all of whom shared a tacit understanding of what could be taken for granted in theoretical proofs and problem solutions. Together with its wealth and public nature, Cambridge became an institution unique in its time for the scale of mathematical training it provided, and proficiency it produced (ibid., p.171).[77]

These changes did not occur without resistance, however, with the ultimate purpose of education becoming a topic of explicit debate at Cambridge and throughout Britain. William Whewell, for his part, objected to these changes based on a liberal educational ideal that wanted undergraduate education to be about more than teaching the technical skills of rational thought (i.e. it should also involve the inculcation of moral and spiritual sensibilities). If students were eager to conduct their own research immediately following graduation, he thought, this was a problem, "a worrying sign that they had failed to be convinced by the explanations of their

[77] Similar pedagogical shifts towards repetitively drilling students in small groups to impart proficiency in the analytical techniques of mixed mathematics took place in some continental institutions as well. While professors at Cambridge continued to give lectures to large numbers of students, professors at the École Polytechnique in Paris began to drill and instruct small batches of students organized by comparative aptitude, much as Cambridge tutors did outside of college lectures (Warwick 2003, p.174). But as a smaller institution, acting as a military academy at the time, the pressures, culture, and career paths of École graduates were different than those who went through the Cambridge system.

tutors" (ibid, p.51). The contentious cultural shifts at Cambridge (and to a lesser degree Oxford) did not go unnoticed by Parliament, to whom universities were ultimately answerable through their chancellor. In the early 1850s the debate over the aims of education was effectively arbitrated by a group of parliamentary commissioners charged with investigating teaching at the universities to determine appropriate reforms. They largely sided with the opinions of William Hopkins, the first person to make an entire career out of being a private tutor (ibid., p.269).[78]

Hopkins especially opposed Whewell's standards of liberal education, and saw the kinds of dedication, clear-thinking, and hard work needed to develop mathematical adeptness as the very kind of moral virtue the university should be aiming to produce in young men. He held a "utilitarian view of mathematical studies" (ibid, p.52), and "argued forcefully that the skills of the mathematical physicist should be turned to the commercial and industrial needs of Britain and her Empire" (ibid). Such a practical and technocratic vision of science no doubt resonated with the parliamentary commissioners' Victorian social ideals. Parliamentary decree was effectively an endorsement of the status quo, however, as the aim of education at Cambridge had already become the production of high quality physical researchers capable of using their skills to serve the interests of Queen and country by applying mathematics to the sorts of mechanical problems faced by industrialists.[79] Such ideals were internalized by many graduates, and Cambridge researchers in general quickly gained a reputation amongst many of their Scottish and continental counterparts as scientists tainted by the concerns of the factory. While continental positivists such as Pierre Duhem objected to physical thinking that was so focused around practical matters of application, Thomson and Maxwell saw it as a virtue of their approach that theorizing was kept close to concrete objects and engineering concerns (Smith and Wise 1989, p.xix; Darrigol 2000, p.188).

---

[78] For specific details of the reforms, and their influence on maintaining the distinctly *applied* research ethos at Cambridge, see Warwick (2003, p.264-280).

[79] While reforms ensured students would be instructed in a manner that made clear that the goal of their education was applied problem solving, this emphasis on practical problem solving was also promulgated by Hopkins himself, who served as the undergraduate tutor of several eminent researchers including Sir George Stokes, Francis Galton, Peter Tait, Thomson, and Maxwell (Warwick 2003).

During their undergraduate training at Cambridge the physicists who helped develop British optical ether models, the theoretical foundations of steam power, and Maxwell's theory of electromagnetism were trained much more systematically than Weber, Helmholtz, and the other electrodynamists on the continent. While they now shared the same basic methods of analysis with their continental peers, and thus had little difficulty understanding their work, Cambridge researchers found themselves in a large community that shared a distinctive research culture, focused on analytically solving problems, much like those found on their final examinations. Conducting physical research obviously wasn't a simple matter of solving pre-set problems, but as researchers Cambridge-trained physicists mainly tried to model new physical phenomena by reducing their dynamics, analogically, to a type of Tripos-like problem whose solution they were familiar with (Warwick 2003, p.279). Thus, research at Cambridge consisted primarily in reinterpreting the mathematical descriptions of already well understood systems as applying to other systems, and was considered successful insofar as it able to solve whatever problem it was meant to.

To be clear, this application of models and methods established in one domain to a new domain was different from the kind of theory extensions Weber was doing. As we'll see in more detail in the next section, Weber consistently wanted to establish the reality of his electrical corpuscles by showing that their supposition could be used to explain an incredibly wide range of physical phenomena. Cambridge physicists, on the other hand, were simply borrowing familiar mathematics from one domain and seeing if it could be used to model phenomena in another by reinterpreting the referents of its terms. It would be ridiculous to think that a successful reapplication of a formalism developed in one domain (say, hydrodynamics) to another domain (say, electrodynamics) meant that the two fields had been unified, or that the former had somehow become better supported by the empirical evidence, and it was very rare for Cambridge physicists to draw any ontological conclusions from the applicability of an analogical model.

By applying familiar problem-solutions to new systems in this way, Cambridge researchers made good on the vision of science that Hopkins sold to the parliamentary commissioners. During the 19th century his students and their peers constructed mathematically elegant and predictive models of many disparate phenomena, often focusing on practically relevant problems arising directly out of the design ambitions of Victorian industrialists and government infrastructure projects. And unlike many continental researchers outside the positivist school, Cambridge graduates generally displayed a striking lack of concern with understanding the unobservable causes of phenomena unless it somehow facilitated their analysis. They displayed a preference for Joseph Fourier's "non-hypothetical, macroscopic, geometrical, and practical" (Smith and Wise 1989, p.161) methods of analysis over Laplace's, for example, given the former's not being concerned with identifying the ultimate parts of a machine but with "the gross features of the machine and what work it could do" (ibid, p.162). To the British, Laplace's commitment to the reduction of all phenomena to inverse square laws operating between ponderable and imponderable fluid particles appeared frivolous at best. What mattered to the British was that they could analyze a system, manipulate some symbols on paper, and accurately predict the system's evolution over time, preferably in a manner that let them control and direct its behavior to achieve practical ends.

Even as they came to accept Augustin-Jean Fresnel's wave theory of light, British physicists tended to understand the theory using Fourier's methods, again because they shared the latter's instrumentalist vision of science. In advocating for the wave theory of light, Siméon Poisson's realist outlook led him to emphasize the importance of experimental testing for validating theoretical models, and tended to justify the theory and all its ontological baggage through a hypothetico-deductive comparison of it against Newton's particle theory using experimental demonstrations. Since Newton's theory couldn't account for phenomena like the Poisson spot, while Fresnel's had predicted it, Poisson argued that light must be a wave in some luminiferous medium, i.e. the wave theory must be true. Fourier, on the other hand, was generally content with predictively useful theories, regardless of whether they seemed to identify the true mechanical constitution of things. When applying his innovative method of summing simple trigonometric functions to the study of heat transfer, for example, Smith and Wise (1989) write that Fourier:

> relied on macroscopic mathematical forms themselves as direct expressions of observable reality. By analogy of mathematical form, heat conduction was heat flow. That identification was all one needed for intellectual satisfaction and practical application. As Fourier put it, 'the theory of heat will always attract the attention of mathematicians, by the rigorous exactness of its elements and the analytical difficulties peculiar to it, and above all by the extent and usefulness of its applications; for all its consequences concern at the

same time general physics, the operations of the arts, domestic uses and civil economy'. (p.161).

The British appreciation of the optical wave theory that developed in the early 19th century was based in a similar kind of instrumentalist judgement of its superiority, taking as canonical the distinctly phenomenological presentations of it given by Fresnel in his more epistemically modest moments. For many British, the wave theory was justified formally and phenomenologically by its demonstrable predictive power, not hypothetico-deductively because of its apparently superlative explanatory strength (ibid., p.163). While some early British appreciators of this undulatory theory of light followed Fresnel, Poisson, and Laplace in exploring the possibility of a mechanical reduction of the theory to the movement and interaction of point particles, in 1831 Cambridge physicist George Airy stressed that the virtues of the optical wave theory were restricted to its status as a highly predictive mathematical description of optical phenomena. Airy plainly took a pragmatic position on scientific method (Cantor 1975, p.114), and specified that the "geometrical" and "mechanical" portions of the wave theory were to be carefully distinguished. The predictive strength of the theory, he thought, was entirely captured by the geometrical portion, so the theory's predictive strength, in his judgement, did not validate the ontologically loaded hypotheses about a mechanically constituted luminiferous ether in which the wave theory was often framed (Smith and Wise 1989, p.163). When David Brewster, a Scotsman schooled in the empiricist tradition of Thomas Reid and contemporary detractor of the wave theory, objected that the wave theory had also been contradicted in certain respects by experiment, and thus could not be the true description of light's physical nature, Airy responded: "I imagine that any theory must be defective in this point" (as quoted in Cantor 1975, p.114).

Such an anti-realist refusal to validate theories and their ontological implications by framing and testing hypotheses was dominant throughout Cambridge physical research during the 19th century. Nevertheless, British physicists, especially the Cambridge-trained, recognized an instrumental role for speculative hypotheses in scientific investigations, in opposition to the Scottish empiricism promoted by Thomas Reid and his students that counselled the scientist to "treat with just contempt hypotheses in every branch of philosophy, and to despair of ever advancing real knowledge in that way." (Reid 1852, p.14).[80] The function Cambridge physicists saw for hypothesizing in scientific practice, however, had little to do with the epistemic conclusions to be drawn from successful experimental tests of some hypothesis or model, as a predictive model was rarely taken to reveal the true causes underlying the phenomena; when mechanical models constructed through hypothesizing were tested, a validation of their predictions was generally taken to justify only the dynamical analysis that resulted from the use of those hypotheses, not the hypotheses themselves.[81] For the Cambridge-trained, speculative hypotheses generally served a pragmatic purpose in the process of theory building.

Their pragmatic attitude was the reason that, along with Fourier's methods of analysis, the Cambridge mathematical physicists of the early to mid-19th century came to favour Lagrangian methods and appeals to William Hamilton's principle of least action, "precisely because, although dynamical in origin, they could be applied to numerous physical phenomena while making only very general assumptions regarding the mechanical nature of the micro-processes involved" (Warwick 2003, p.278). While Airy and others accepted the existence of some luminiferous medium because all waves apparently required a medium, and encouraged the construction of hypothetical models of its underlying mechanics, his careful distinction between "geometrical" and "mechanical aspects" of the wave theory allowed him to maintain his position that we would forever remain ignorant of this medium's true mechanical constitution, *and that that was not a problem*. Such attitudes permitted the judicious use of mechanical model building in physical theorizing, such that by 1849 Robert Moon could count fifteen different models of the luminiferous ether that had been seriously discussed by his fellows at Cambridge.

---

[80] The Scottish empiricists opposing the optical wave theory, in fact, were generally more concerned than their pragmatically minded Cambridge counterparts with experimentally testing it, as a way to argue against it. Both Richard Potter and Brewster conducted crucial experiments to test the predictions of the wave theory, and rejected it because of several anomalous results. With consistency, they rejected Newton's corpuscular theory of light for the same reasons, declaring themselves "rienistes," ontological agnostics in typical empiricist fashion. Rather than trying to use theory to predict and control light, or discover its true nature through abductive inference, the Scottish empiricists "concentrated on describing optical phenomena per se and delineating those phenomena that were not adequately explained by the wave theory" (Cantor 1975, p.119). Pragmatically minded supporters of the wave theory at Cambridge, by contrast, were relatively unconcerned with experimental anomalies, praising the wave theory for its predictive power in those respects that such power had been demonstrated. Given that power, they maintained, it was an appropriate starting point in the effort to accommodate presently anomalous results through slight modifications (ibid. p.120).

[81] Whewell and Herschel being notable realist exceptions to the dominant pragmatist tradition at Cambridge (Cantor 1975, p.114).

Each of these models was constructed through analogy to some already well understood system (an elastic solid or viscous fluid, for example) and used as a stepping stone towards a more abstract analysis. But as Moon stressed, whatever method of inference might lead someone to claim one of those models as a true picture of the underlying mechanics of wave propagation through the ether (as John Herschel did, for example), simply because it "may" be true, the proliferation of such models should serve "to stigmatise the whole system of investigation" (1949, p.viii) that resulted in such a claim. Concluding on the truth of any one model was, to Moon, an "intellectual sin" (ibid., p.xi), for the point of providing a mechanical model of light's unique (transversal) wave properties was not to identify the true mechanisms, but only to "render the conception of transversal vibration as familiar to the mind as that of the vibration which occurs in sound" (ibid., p.xiii). Whether that model is true, he thought, is rather beside the point; all that matters is that it makes intelligible all the dynamics of the phenomena being modelled.

In this way, producing analytical problem-solutions remained the sole criterion of success governing scientific inquiry at Cambridge, rather than discovering new phenomena or validating hypotheses. This is properly considered a pragmatist conception of the ultimate aim, the telos, the end-in-view for science, distinct from both empiricist and realist conceptions. Success in the pragmatic sense can be pursued by whatever means prove most effective, whether through the empiricists' inductive methods or the more realistic methods of physical conjecture. But again, even when the latter method proved successful, the empirical validation of a resulting Lagrangian dynamical analysis, for instance, was rarely taken by Cambridge-trained physicists to validate the more detailed mechanical hypothesis employed to derive the dynamics. When this pragmatic approach to theory and model building eventually led Maxwell to develop the basis of a highly unified field-theoretic account of electrodynamics between 1855 and 1862, apparently capable of unifying electromagnetism with optics, Morrison (1992) notes how at odds Maxwell's inferences were with how a realist would have reacted:

> Not only was unifying power not seen as evidence for the theory itself, but the specific unifying parameter, electric displacement current, was not given a realistic interpretation. The aether model was seen by Maxwell as purely fictitious, a way of illustrating how the phenomena could possibly be constructed. In later versions of the theory published in 1865 and 1873 Maxwell wanted to establish a more secure foundation, relying only on what he took to be firmly established empirical facts, together with the abstract mathematical

> structure provided by Lagrangian mechanics. That structure, unlike the mechanical model, provided no explanatory account of how electromagnetic waves were propagated through space nor any understanding of the nature of electric charge. The new unified theory based on that abstract dynamics entailed no ontological commitment to the existence of forces or structures that could be seen as the source of electromagnetic phenomena. Instead, 'energy' functioned as the fundamental (ontological) basis for the theory. The displacement current was retained as a basic feature of the theory (one of the equations), but no mechanical hypothesis was put forward regarding its nature (p.63).

Thus, not only in his methodology and attention to industrial concerns, but also in his cognition and ontological commitments, Maxwell, like his fellows reared in the Cambridge tradition, embodied neither the realist outlook of Weber, Ampère, Whewell, or Herschel, nor the empiricist outlook of Faraday, Helmholtz, Gauss, Neumann, Brewster, or Potter. Rather, his actions embodied his distinctly pragmatic understanding of the aims and epistemology of science, as we will see in more detail in the next section.

7) Philosophies of Science in Scientific Practice

Having discussed the character and origins of the distinct philosophical outlooks of the three major schools of electrodynamics research operating in late 19th century Europe, I now turn to a discussion of the way those outlooks motivated each school to focus on conducting different research activities. As we will see, the day-to-day work of Weber and his students was primarily oriented towards measuring constants, parameters, and generally using experiment to refine their representations of the electrical corpuscles postulated by their theory. Weber himself also aimed to develop and further establish the truth of his theory by extending it into new domains, hoping to explain the empirical laws of chemistry and gravitation through the interaction of his electrical corpuscles. Cambridge physicists working on electrodynamics, in contrast, were primarily preoccupied with the kind of abstract puzzle solving efforts they'd been trained in, pen-and-paper work aimed at capturing the dynamical forces exerted by magnetic, electrically charged, and current-carrying objects, often focusing on applying their work to the improvement of Victorian electrical devices such as telegraphs. In contrast, Helmholtz was not primarily concerned with refining theoretical representations, nor with mathematically analyzing and predicting the behaviour of various abstractly defined electromagnetic systems; instead, those scientists working with Helmholtz were primarily concerned with using experimental apparatuses to reveal novel electrodynamic effects in through exploratory laboratory investigations conspicuously unencumbered by the expectations of any restrictive theory.

Having dabbled in electrodynamic research throughout his career, electrodynamic phenomena were interesting to Helmholtz for a variety of scientific reasons. They had clear implications for fields like neural physiology and thermodynamics, for example, two areas he had contributed greatly to in his early career. His physiological studies of nerve conduction necessarily involved some understanding of electrical effects, and about a third of his paper on energy conservation had been dedicated to electricity and magnetism, rapidly developing areas of physics where, he wanted to be clear, theory could only be properly advanced if energy conservation was assumed. Thus, when fame and the financial support that came with it gave Helmholtz the opportunity to run a research laboratory in Berlin in 1870, he dedicated himself to a thoroughgoing study of electrodynamic phenomena. He proceeded in a highly exploratory manner reminiscent of Faraday, motivated by the empiricist epistemology that he was actively promoting (see his 1871). He made a conscious effort, from the outset, to avoid letting any hypotheses unduly influence his thinking and experimenting, aiming to maintain as much ontological neutrality as possible. Thus, his laboratory in Berlin served, first and foremost, as a place for researchers to freely try to create, then systematically explore, *new* electrodynamic phenomena, rather than a place to develop or test certain theories. When Arthur Schuster visited his laboratory in 1874, having already visited Weber's which seemed rather "orthodox" in its focus on testing predictions and measuring properties or constants, he remarked on the uniqueness of the exciting work being done under Helmholtz's direction. This work clearly went far beyond simple measurements and tests, he said, for in Berlin "no efforts were made to push numerical measurement beyond its legitimate limits, and though most of the work done was quantitative in character, qualitative experiments were not discouraged" (Schuster 1911, p.15-6, as quoted in Buchwald 1994, p.56). Weber's laboratory, like most of the other laboratories in Germany at the time, was a space dedicated to the service of theory, "a generator for constants" as Buchwald puts it. Helmholtz, by contrast, abjured the constraints of theory, operating his laboratory as "an engine for discovery" (Buchwald 1994, p.57).

Helmholtz did develop a kind of theoretical understanding of electrodynamic phenomena, but it was a theory meant, as it were, to bypass the influence of hypotheses on laboratory work (Buchwald 1994, p.28). His "interaction potential" approach to electrodynamics was based on a generalization of Neumann's work developing Ampère's theory. In this approach, one obtains the force between two electrically charged or current-carrying objects through a potential energy function defined for the entire system, taking the observable charge magnitudes and current intensities present in macroscopic bodies to be theoretical primitives. What this means is that, from the point of view of the Neumann-Helmholtz theory, electrodynamic action occurs when different objects take on specific states or conditions (e.g. charged or current-bearing) that make them interact with each other *directly*. Weber's development of Ampère's theory, by contrast, obtains the force between two objects by first calculating the force between the collections of electrical particles contained within or flowing through those objects, which is then transferred to the object itself to produce the force effects of charges and currents observed in the laboratory. While Helmholtz's interaction potential can be calculated in Weber's system by summing the interaction potentials of all the particles and assuming their transference to the laboratory objects that contain them, this would be a secondary representation of no fundamental physical (i.e. energetic) significance (ibid., p.186). This makes the attractive or repulsive forces between the bodies only apparent, as the real force occurs between the particles, not the bodies. In Helmholtz's system, the states of macroscopic bodies have physical significance in and of themselves, with the interaction potentials they determine not being reduced to the potentials of any other objects. Thus, the world according to Weber's theory was made up of observable bodies that can contain unobservable electrical particles; according to Helmholtz's, merely observable bodies in different observable states of energetic potential. Whereas Weber's approach fundamentally involves a hypothesis about the nature of charge and currents, Neumann's empiricist concern with eliminating such hypotheses from his mathematical analyses meant that his potential functions "in no way at all specify what either charge or the current might be" (ibid., p.8). This was important because the main role of theory in Helmholtz's research was basically as a record-keeper for experimental work, not an explanatory scheme or phenomena predictor. Neumann's potentials allowed the outcome of experiments to be described macroscopically, eliminating the theory-ladenness of observations as much as possible by refusing to take on any unnecessary ontological baggage.

In its most general version, Helmholtz's theory acts as a kind of constraint upon any model of the underlying mechanics responsible for charges, currents, and their associated forces. Over and above those of his competitors, Helmholtz's representational practices were pluralistic and open-ended in that they could easily have "saved" a wide variety of qualitatively different electromotive effects. Helmholtz saw this as a virtue of his scheme, despite its relative predictive impotence and ontological non-commitment, for it guaranteed that it could be effectively deployed to characterize even the most unexpected experimental outcomes. As such, Helmholtz expected it would be capable of modelling the greatest variety of electrodynamic phenomena that he could imagine at the time, including phenomena that might not be possible according to either Weber or Maxwell's theories. Additionally, like energy conservation itself, his potential functions seemed so general that they would "survive extreme changes in the higher-order principles of electrodynamics" (ibid. p.9). Thus, if the Webereans or the Maxwellians encountered anomalous electrodynamic phenomena, incapable of being "saved" by their representational schemes, Helmholtz's could (it was thought) survive. And given the state of electrodynamics research at the time, the prospects of such "higher-order" turbulence understandably seemed plausible, if not expectable. Because of this, Helmholtz tended to portray his account as a kind of neutral or universal account of contemporary electrodynamics, encompassing both Weber and Maxwell's theories without favouring either. In principle it could treat hypothesized electrical corpuscles and electromagnetic fields straightforwardly, as just another type of object, though in the end trying to integrate fields to account for polarization created a plethora of problems with no parallel in Maxwell's original theory, proving fatal for Helmholtz's potential-based approach.

But before such problems arose, the basic idea was to represent all the macroscopic objects one might observe in an electrodynamical system as a collection of interacting pairs. When objects took on electrical states (i.e. became charged or current-bearing) this resulted in a potential for interacting with each of their partners in specific ways. The sum of these "interaction potentials" defined a "system energy," representing the totality of forces every object would be subject to through its interaction with every other object in the system, given their relative distance, orientation, and electrical magnitudes. Because the potential energy states of these macroscopic pairs were taken as primitives, fundamental quantities whose electrodynamic effects were not represented in terms of their containing corpuscles acting on each other from a distance, or through the mediation of energy-bearing fields in the electromagnetic ether, in Helmholtz's scheme electrodynamic interactions were seen as mediated by nothing in particular. So, whereas Helmholtz was able to represent electrodynamic and electrostatic forces without committing to the existence of anything other than the familiar objects of everyday perception, the representational practices of Weber and Maxwell necessarily presumed the existence of certain theoretical objects ("electric particles" and "ethereal fields," respectively). Thus, as Buchwald (ibid.) puts it:

> Helmholtz created a theory that differs radically from both [Weber and Maxwell's theories] in refusing to abstract from the laboratory objects in the fashion of Fechner-Weber and yet also in refusing to introduce something entirely different in nature from them in the manner of Faraday-Maxwell. He substituted instead what one might call a *taxonomy of interactions* for the unitary forces of Fechner-Weber and for Faraday-Maxwell's duality between field and object. (p. 11)

In addition to his anti-realist effort to deliberately circumvent ontological issues and to focus on the elicitation of novel, subtle electrical phenomena in the laboratory, Helmholtz's empiricist vision of science was embodied in his theory by how difficult it would be to use it to inform industrial design. An object's capacity for electrodynamic interaction (i.e. its interaction potential) was generally assigned *a posteriori*, based on what had been observed in a given experimental set-up. Because every interaction was treated as unique, unlike Maxwell's theory (in particular), Helmholtz's methods couldn't be easily used to anticipate a complex system's behavior *a priori*. But predicting a system's behavior was, of course, not the point for Helmholtz when he developed his framework. His representational practices were developed specifically to avoid the need for a predictive (and therefore rigid) theory, lest they prove incapable of recording some novel phenomena elicited in the laboratory. As such, there was nothing comparable to the Cambridge style of producing predictive dynamical analyses in Helmholtz's theory and practice, for it "was actually designed from the beginning to avoid precisely the sorts of questions that powerfully gripped Maxwellians, and that gave them the opportunity to produce satisfying paper work" (ibid., p. 327). With such a flexible method of recording experiments in hand Helmholtz proceeded to construct a wide variety of experimental set-ups, charging or inducing currents in different objects, placing them in novel arrangements, watching to see what happened with minimal expectations and using his theory to quantitatively record what happened.

Neumann's potentials seemed perfect for such purposes. Without constraining his thinking too much they provided a means of representing, in exacting detail, all the various phenomena he, his colleagues, or someone else might one day produce in a laboratory, all while remaining independent of any hypothesis regarding the true nature of charge, current, and magnetism. This makes sense for an empiricist in search of novel phenomena, for the best theory would be the one capable of saving the greatest possible variety of phenomena. After all, you never know what you'll find. Thus, Helmholtz's method of representing electrodynamic phenomena was itself "deeply empirical" (ibid., p.15) in nature, and quite dissimilar from the methods used by Weber and Maxwell. It was, as Buchwald puts it, "an apotheosis of instrumentalism because it seems not to go beyond the laboratory objects and their unmediated interactions with one another" (ibid., p.14). This is exactly why Gauss had developed and used a similar mathematics of potentials in his work, allowing him to treat the Earth as a kind of black box and measure geomagnetism "independent of any particular assumption on the repartition of magnetic fluids in the Earth" (Gauss 1838 p.125, as quoted in Darrigol 2000, p.50).

But Buchwald also stresses the point outlined above, that "there is a heavy price to pay for instrumentalism of this kind because it requires in effect for many situations an a posteriori specification of energies that in the other two theories can be derived a priori" (1994, p.14). In its most general form, for example, Helmholtz's theory saw no necessary interactions between charged and current-bearing bodies: that such interactions did occur was derived from experiment, not from theory, and the forces involved there bore no necessary relation to the forces involved in charge-charge or current-current interactions. For the same reason, his theory left open the possibility that electrostatic states (like a terminating current) might display electrodynamic effects (like a force on a magnet), along with an infinity of other possible state-state interactions (ibid., p.13). Laboratory work conducted through his theory, then, was not guided by theory in the way we will see Weber's work was. His instruments were not seen as allowing us to peer into some other, unobservable world and see the true causes of things, providing the soul of his theory with a sensory organ, for on his empiricist view experiment cannot transcend the observable level of the laboratory and everyday experience. Rigid theories and speculative hypotheses risk blinding our investigations by making us think some possible phenomena are, in fact, impossible, or by constraining our thought in a way that prevents us from realizing the possibility of effects even our theory countenances as possible. We saw this latter form of theory-blindness in the way that Ampere failed to realize the possibility of continuous electromagnetic rotations (and electromagnetic induction), while Faraday was able to elicit both through laboratory tinkering. In this way, the privileging of sense-perception as a mode of knowledge formation led Helmholtz, like Faraday, to freely tinker with the laboratory objects of immediate experience, unconcerned with any "unobservable" hypotheticals like fields and corpuscles that might ultimately be causing these effects. Somewhat sadly, however, while Faraday proved quite successful in eliciting novel effects, Helmholtz's efforts were plagued by failures.

These failures must have been quite unexpected for him, as he'd had success using similar representational practices previously, especially in his attempts to demonstrate the faults of vitalism in physiology. His experimental studies of nerve signaling, for instance, also used a potential energy function to represent nerve conduction. There they'd proved advantageous not just for their ontological neutrality but also because they were guaranteed to not violate energy conservation. This allowed him to show how vitalist theories necessarily violated energy conservation, predicting effects that could be demonstrated not occur. His successful attacks on vitalism were based, essentially, on experimental disconfirmation of such predictions. At the time he began his electrodynamics research Helmholtz had long suspected (incorrectly) that Weber's force law (discussed below) likewise violated energy conservation through the use of velocity-dependent forces. Nevertheless, his early experiments were not aimed at choosing between theories, but rather aimed "to elicit hidden novelties that every one of these theories could in principle accommodate, but that none had ever identified" (ibid., p.28). A critical exchange between him and Weber over the issue of energy conservation dragged on through the early 1870s, however, leading Helmholtz to focus on identifying the differences between the two systems. As it turns out, Helmholtz's theory was not without empirical departures from Weber's, and through a series of highly exploratory investigations (e.g. of the possibility that electrostatic devices might generate electrodynamic effects) he began a series of attempts to discriminate the two theories experimentally. That is, he looked more and more to use experiments as a way of demonstrating the weakness of Weber's system, hoping to disconfirm predictions that depended on Weber's velocity-dependent forces. Because Weber's system did not in fact violate energy conservation, and because Helmholtz's system permitted effects that do not actually occur, Helmholtz failed to elicit the effects he went looking for. As he modified his theory from its most general version to a more restrictive version, it also became unable to accommodate new phenomena, such as current polarization, causing him to eventually abandon his more empirically permissive theory and focus on reinterpreting Maxwell's theory within his potential-based approach.[82]

In short, throughout his electrodynamics research, Helmholtz was always preoccupied, first and foremost, with producing novel phenomena. While he was not initially aiming to do anything other than discover new effects, he increasingly aimed to demonstrate effects that could not be "saved" by one or more of the competing electrodynamic schemes. Helmholtz's motivating idea at this later stage of his research was that he could challenge Maxwell's and Weber's schemes by producing novel, reproducible phenomena in the laboratory that could not be "saved" by their schemes but could be "saved" by his own. This was the context in which Helmholtz's student, Heinrich Hertz, imbibed Helmholtz's empiricist ethos and learned his exploratory style of experimental inquiry. Hertz's eventual "fabrication" of electromagnetic radiation faithfully replicated this aspect of Helmholtz's laboratory practice—as did Hertz's (largely unsuccessful) efforts to create qualitatively novel phenomena involving elasticity, evaporation, and cathode rays—and was conducted for the most part while maintaining the same ontological neutrality that Helmholtz brought to his own work. For Hertz, as for Helmholtz, the predominant concern of laboratory work was the production of novel effects, to which theory could then be held accountable. While Hertz is often interpreted as a follower of Maxwell, whose work was an attempt to corroborate field theory, the historical facts speak against such a simple understanding. In 1884, for example, he attempted to construct field equations without appeal to the ether, operating within an essentially Helmholtzian framework by describing field energies as an infinite series of interaction potentials (ibid. p.96, 325-6). It is an unintentional accident of history that following Helmholtz's empiricist approach eventually led Hertz to a technique for producing novel phenomena—electric waves—that could only be easily "saved" by Maxwell's scheme, and not by Helmholtz's or Weber's. Nevertheless, as an empiricist Helmholtz would clearly have seen the production of such a novel type of phenomena, regardless of whose scheme was corroborated or invalidated, as precisely the kind

[82] see Buchwald (1994, Ch.1-2) for a detailed account of Helmholtz's representational practices, and (ibid., Ch.3) for an account of the experiments that led him to reformulate his potential-theory into a version of Maxwellian field theory that severely constrained subsequent laboratory work.

of success towards which scientists should aim, for it's what he himself had been aiming for. It was because Hertz was a student of this mode of research that he was able to eventually produce and study in detail the novel phenomena of electromagnetic radiation, through exploratory experiments unguided by theory, as will be detailed when discussing Hertz's successes in the next section.

Like the Helmholtzians, and unlike those working at Cambridge, the Webereans had a robust laboratory component to their work. As previously noted, Weber also shared with Helmholtz a theoretical understanding that saw electrodynamic action as *interactive*, the result of a bipartite relationship between two objects rather than the effect of one agent on another, mediated or otherwise. Nevertheless, there were significant differences between these two German schools that gave their theories very different relationships to laboratory work. While some indication of these points has been given above, Buchwald (1994) gives an extremely clear explanation of the similarities and differences between Weber's and Helmholtz's systems, their philosophical character, and the different relationships they saw between theory and experiment. Having explained the character of Helmholtz's theory and practice in some detail, it's worth quoting Buchwald at length to further clarify the metaphysical character of Weber's approach:

> From the metaphysical point of view Weberean force reflects the irreducible and immediate character of bipartite interactions among atoms. Force is not an action by one otherwise dead object on another but a physical symbol of their interconnection. In such a world the ultimate physical objects are in themselves eternally the same, and indeed each is bound to every other ultimate object in a bipartite connection. Consequently, the relational character of Weberean electrodynamics does not extend to the objects with which we have to do in the empirical world, for such things are built up out of a myriad of atomic points, and the relation subsists between the points, not the bodies. Helmholtz's electrodynamics accepts, indeed it is explicitly founded on, the axiom that bodies interact with one another through irreducible bipartite relationships. In the Helmholtzian world, as in the Weberean, things do indeed interact in pairs; they do not, properly speaking, act. But Helmholtz's electrodynamics goes much further in this vein than Weber's ever did, and with radically different implications.
>
> It is entirely possible to dispense with a relational understanding of force and yet still retain the analytical and physical structure of Weberean theory, including his universal model. Whether point atoms are thought to create and to be guided by independent forces, or whether the forces are symbols of an unbreakable unity between points, does not in any way whatsoever affect the theory's empirically testable consequences. Helmholtzian theory is utterly different. It rejects atoms in Weber's sense; it makes no direct use of forces. Instead of building bodies out of invariant and inaccessible entities (Weber's metaphysically pregnant atoms), it builds them out of small pieces that are entirely similar to the bodies

> proper-it takes them, that is, essentially as they are in the empirical world. Instead of having forces link atoms (or even energy determined by atom pairs), Helmholtz has the energy of a *system* consisting of a pair of volume elements in given states and at a given separation. A relational understanding of the actions between objects lies at the very core of this enterprise precisely because the relationships are not determined by the invariant natures of the bodies, and this has direct empirical consequences.
>
> The atoms in a Weberean pair are ever joined in the same family. A pair of Helmholtzian objects can have a potentially infinite number of different relationships with one another depending on their states. Only the laboratory can determine what kinds of states bodies may have and what system energies bodies in given states and at given separations together determine. Here, then, the interactive character of electrodynamics does not need to be inserted into the scheme: it is irretrievably there from the very beginning. And its necessary presence carries a very different message from that of Idealist metaphysics. Since Helmholtz's interactions cannot be known a priori, whereas Weber's are, his relational electrodynamics remains directly tied to the laboratory, to the world of empirical practice." (1994, p.396-7)

Thus, Helmholtz's empiricist leanings produced a laboratory that was "a much more flexible, manipulative place than a Weberean laboratory could possibly be," where instead "one would concentrate for the most part on measuring constants using devices with given, unaltered structures. These pieces of *measuring equipment* would rarely, if ever, be used in conjunction with other devices whose behavior could not be thoroughly calculated *from theory* beforehand. Whereas Helmholtzian laboratories were places for seeking out unknown phenomena, Weberean laboratories were places for measuring unknown constants" (ibid., p.19). The primary reason for this difference is that Weber had developed a realist attachment to Ampere's hypothesized corpuscles, and his laboratory work became driven by his attempts to either a) establish their existence by creating phenomena suggested by their supposition, or b) more fully determine their properties through measurement work.

Weber's theoretical work was also driven by his realist convictions, ever since his success in accounting for Volta-induction using Ampère's hypothesis of corpuscular electrical fluids. Having conducted what he took to be sufficient empirical tests of Ampère's Law, Weber used the hypothesis of electrical corpuscles to extrapolate a more general law that would serve as the fundamental force law of his own system of electrodynamics, aiming to combine Ampère's law governing the interaction of currents with Coulomb's law governing the interaction of charges. He saw this as "a generalisation of that previously erected by Ampère, which in effect represented the special case of four electrical particles simultaneously involved, when current elements are assumed constant and fixed" (as cited in D'Agostino 2000, p.25). In the process of developing this law from Ampère's, Weber tested it both with new experiments and through "synthetic deductions" to show that it cohered with the known empirical laws of electrodynamics. Like he had in testing Ampère's law, however, around 1848 Weber made a characteristically realist epistemic leap and judged that his law was universally valid, despite not having empirically tested it exhaustively but only where it had been especially straightforward to do so (ibid., p.25-6). Over the next several years Weber set himself to various measurement tasks meant to flesh out the empirical details of his theory in a way that would fully integrate Coulomb's more macroscopic electrostatics into his microphysical picture of the source of electrical forces. Most notably he sought to establish absolute units for current intensity, electromotive force, and resistance in the years leading up to 1851. The central aim was to combine electrodynamics and electrostatics into a single force law, validating his accepted ontology by showing that the assumption of electrical corpuscles was capable of explaining both sets of phenomena in a unified way.

Through these efforts he gradually became fixed on a missing constant that seemed to be required to truly complete his aims of unification. The need for this constant in his law only appeared because, unlike Ampère, Weber employed absolute units. His analysis showed that it would be based on a velocity value, likely of massive magnitude, deriving from a comparison of the electrostatic to the electrodynamic force exerted by charges (i.e. the difference between the force exerted by a charge at rest and the force exerted by a charge in a current). Absolute units for these forces had yet to be determined, and it was clear that it would be difficult to determine them with the kind of numerical precision needed to determine the constant needed in his theory. But, he realized, if he wanted to establish a complete set of electrical measures and the explanatory power of his system (and he did) he could not do without an accurate numerical value for that constant (ibid., p.27). He would spend nearly five years just getting a preliminary value measured, with the help of his friend Rudolf Kohlrausch, without knowing that it would provide Maxwell with the empirical basis upon which he could identify the luminiferous and electromagnetic ethers, a theoretical unification so broad, natural, and unexpected that it would lead to the abandonment of Weber's carefully crafted unification of electrostatics and electrodynamics.

The constant that Weber and Kohlrausch measured in 1856 is what we still call *c*, as they did, but also recognize as the speed of light, as they did not. The importance of *c* in completing Weber's fully quantified unification of electrodynamics and electrostatics can hardly be understated, given that its massive value and place in his fundamental law showed why the strength of an electrodynamic interaction stated in absolute units "always appears to be infinitesimally small in comparison with the electrostatic interaction" (ibid., p.31). The speed of light in air had been empirically measured by this time, and Weber noted when reporting his results the similarity between that value and the value *c* he'd just measured; but for him their similarity was worth noting simply as an illustrative comparison, establishing that *c* was not an actual velocity of any body because the only velocity he knew of "that approaches c, namely that of the propagation of light, is not a velocity with which bodies in effect move relative to one another" (as quoted in D'Agostino 2000, p.32). But, he stressed, this similarly large value *c* had a very different physical meaning than the speed of light, so there was no reason to think they should be identified simply because they were numerically close. The comparison was qualitative, to illustrate the magnitude of *c*, and explicitly not ontological.

It's understandable that Weber did not further investigate the numerical similarity of *c* and the speed of light. Upon determining a value for *c*, to Weber's mind, it had never been so well established that all forms of electrodynamic and electrostatic action were the result of his posited corpuscles. As a realist, flying higher than ever, finally demonstrating the ability of his two-fluid ontology to explain not only electrodynamic but also electrostatic forces, there would have been no theoretical reason to even momentarily entertain the idea that these physical quantities were identical. A more enticing thought was the possibility of conducting a radical reformulation of his system of absolute measurement, as taking Newton's law of gravitation and his own electrodynamic force law to be fundamental laws of nature would permit the reduction of units for both mass and time to that of space (D'Agostino 2000, p.33). Determining a value for a new fundamental constant was one of Weber's crowning achievements, not just to him and his contemporaries but also from today's perspective, as we'll see in the next section. It took years of determined effort to make happen, throughout which he was driven by the promise of a completely unified electrical theory, showing the explanatory power (and therefore likely truth) of his hypothesized corpuscles.

Weber was also driven by his realist commitments to perform a lot of other, more mundane measurement work. As mentioned briefly above, the way that electrical action was accounted for in his system meant that Weber could only conduct laboratory experiments with apparatus whose parts were well understood from the point view of his theory, e.g. parts made of materials whose capacities for "storing" or "conducting" electric particles were well-known. Such a restriction on laboratory practice was avoided by the Helmholtzian scheme, for Helmholtzians could seek out new, unknown phenomena in their laboratories without necessarily conducting any prior study of the apparatus' electric properties. Whereas tweaking, rearranging, fiddling with, and inventing new purposes for scientific instruments until they produced novel effects characterized Helmholtzian laboratory practice, Webereans inevitably found themselves preoccupied by the need to refine their measurements of various theoretical parameters, constants, material peculiarities, and property magnitudes using ever more precise but (preferably) familiar instruments. More often than not this work made some use of Weber's electrodynamometer, the so-called "sensory organ" of his theory, which was well understood from the theoretical perspective. In this way, the Webereans' reification of Ampère's corpuscles was embodied not just in his grander measurement achievements, but also in his day-to-day laboratory practices. He was aided, of course, by the metrological techniques he learned during his earlier work with Gauss, but he put them to a new use: establishing the nature and existence of electrical corpuscles.

Weber's theoretical work after completing his unification of electrodynamics and electrostatics was also influenced by his ontological convictions. With the measurement of c he had shown that the existence of two corpuscular fluids of opposite charge could generate the current forces by flowing inversely through wires, produce charges by resting inside objects, and form magnets by flowing helically in closed circuits inside molecules and atoms. This account of magnetism suggested that these corpuscles had something to do with atomic and molecular structure, and Weber pursued this line of thought in an effort to extend his theory's explanatory power even further. Thus, in 1870 he (somewhat) anticipated Rutherford by developing a planetary model of the atom, which had his positive particles being orbited by his negative ones, though to be sure they should not be confused with the modern electron and proton. Nevertheless, these electrical particles were so foundational to his physical thinking that an "electrical theory of matter" seemed natural enough, given the electrical nature of chemical affinities being uncovered at the time. The wave propagation of light, too, seemed like a natural candidate for explanation through electrical particles, with Weber conjecturing as early as 1846 that light might be some kind of current oscillating at a high frequency, later suggesting that the optical ether might be a lattice of positive electric particles (Darrigol 2000, p.61, 73). If they were capable of explaining every phenomenon of electricity and magnetism, it made sense that they might also be able to explain atomic structure (and therein the whole of contemporary chemistry) and even gravity (and therein the whole of contemporary physics).[83] His ambitious efforts to extend Ampère's hypothesis to the explanation of phenomena in wholly separate domains of physics again embodied his realist outlook, according to which ontological commitments can be justified by demonstrating their superlative explanatory strength and breadth.

While the explanatory ambitions embodying his realist attitude bore little fruit for Weber after 1856, as we will see in the next section he is well remembered for the excellence of his measurement work that was shaped by the same outlook. Its significance, however, mainly derives from the way it aided the development, validation, and acceptance of Maxwell's electromagnetic field theory. That was never Weber's aim, obviously, but it is an important contribution to the progress of science nevertheless. Weber saw his laboratory studies as providing information about the true nature of the unobservable entities his theory posited, the capacities of different materials for storing or conducting them, and the values of theoretical constants needed to unify different domains. Such unification work was motivated by his realist outlook, for the explanatory breadth of his hypothesized corpuscles were understood in characteristically realist fashion, as evidence of their reality.

Weber also embodied his realist attitude in his understanding of scientific instruments, calling his electrodynamometer as sensory "organ" capable of supporting the "soul" of his theory and writing that "[a] finer technique of electrodynamic observations is not only significant and important for the proof of the fundamental principle of electrodynamics, but also because it will be the source of new investigations, which otherwise could not be done at all" (1846, p.9-10).

[83] See Wise (1981) or Woodruff (1962) for brief but excellently detailed discussions of Weber's program, in the context of contemporary approaches. For a shorter account, see (Buchwald 1994, Ch. 1). For an extensive account of his model of the atom, including a discussion of its relation to gravitational effects, see Assis et al. (2011).

Indeed, the electrodynamometer was the instrumental basis not only of his later measurements of electromagnetic magnitudes, but also of his earlier studies of Volta-induction that initially convinced him of the truth of Ampère's corpuscular hypothesis, generating the realist commitments that would guide his subsequent work. But in making precision measurement and experimental corroboration of theory the cornerstone of his laboratory practice, Weber neglected the more exploratory uses of experiment characteristic of Helmholtz and Hertz (Darrigol 2000, 75). While nothing concrete or cognitive would have barred Weber from engaging in more freely exploratory studies of electrodynamic phenomena, his neglect of such practices can be seen as a natural result of his realism and the kinds of investigations it suggested to him as important to carry out. To understand why, recall that Helmholtz's empiricist vision of experiment meant that instruments and apparatus were incapable of providing epistemic access to the ontological details of unobservable reality. In contrast, a realist sees scientific instruments such as microscopes, particle accelerators, and the double-slit apparatus as means of detecting and measuring the nature and properties of unobservable entities like microbes, electrons, and light waves, effectively extending their senses into the unobservable realm. It was just such a realist vision of laboratory experiments and scientific instruments as providing a "window into an invisible world" (van Fraassen 2008, Ch.4), or a sensory organ serving the soul of his theory, that motivated Weber to pursue his refined measurement activities with such zeal to the neglect of exploratory experimentation. Without such an understanding, Helmholtz the empiricist remained focused on producing novel phenomena, while Maxwell worked pragmatically, first constructing a predictively powerful theory, largely unconcerned with doing the laboratory work needed to precisify or test it before sorting out the details of its basic formal structure.

Like Helmholtz's, Weber's electrodynamics has largely been discarded by history in favour of Maxwell's field-theoretic approach. Nevertheless, the incredibly refined and precise measurements he made trying to learn the true nature of his electrical particles were stated as absolute magnitudes, which allowed them to be directly incorporated into other electrodynamic theories, such as Maxwell's. Maxwell was also able to use Weber's absolute magnitudes to show that the speed of propagation of electromagnetic waves in the ether was extremely close to the recently measured speed of light, suggesting that light could be identified with electromagnetic radiation. But despite his use of Weber's measurements, Maxwell was clear that his approach was fundamentally distinct from the corpuscularian, action-at-a-distance approach to electrodynamics. As explained in Maxwell's *A Treatise on Electricity and Magnetism* (1873, Vol.2, Ch. XXIII), Maxwell's framework gave primacy to the ether, while Weber's gave primacy to his corpuscles; and while both systems seemed equally consistent with what we now call "Maxwell's Equations" (as it was also thought of Helmholtz's scheme at the time), these two theories differed irreconcilably in terms of the mechanisms of action they proffered for electrodynamic phenomena. Where Weber's theory represented all electrodynamic and electrostatic effects between bodies as the result of charged particles contained or flowing within them exerting central forces on each other at a distance, Maxwell represented these effects as mediated by fields of potential energy distributed throughout an all pervading electromagnetic ether.

At the outset of his *Treatise* Maxwell acknowledges the advances made in electrodynamics through the action-at-a-distance approach, but counsels his readers not to accept Weber's account of the causes of electrodynamic action on the basis of such successes. Despite Maxwell's failure to discriminate between the approaches and conclusions of several continental electrodynamists, it is telling enough of the ontological conclusions he believed were warranted from a theory's success that it's worth quoting at length:

> Great progress has been made in electrical science, chiefly in Germany, by cultivators of the theory of action at a distance. The valuable electrical measurements of W. Weber are interpreted by him according to this theory, and the electromagnetic speculation which was originated by Gauss, and carried on by Weber, Riemann, J. and C. Neumann, Lorenz, &c. is founded on the theory of action at a distance, but depending either directly on the relative velocity of the particles, or on the gradual propagation of something, whether potential or force, from the one particle to the other. The great success which these eminent men have attained in the application of mathematics to electrical phenomena gives, as is natural, additional weight to their theoretical speculations, so that those who, as students of electricity, turn to them as the greatest authorities in mathematical electricity, would probably imbibe, along with their mathematical methods, their physical hypotheses.
>
> These physical hypotheses, however, are entirely alien from the way of looking at things which I adopt, and one object which I have in view is that some of those who wish to study electricity may, by reading this treatise, come to see that there is another way of treating the subject, which is no less fitting to explain the phenomena, and which, though in some parts it may appear less definite, corresponds, as I think more faithfully with our actual knowledge, both in what it affirms and in what it leaves undecided. (1873, p.xi-xii)

His pragmatic outlook, however, seems to have prevented him from suggesting that his own system warrants acceptance as the truth about the causes responsible for electrodynamic phenomena either. He continues as follows:

> In a philosophical point of view, moreover, it is exceedingly important that two methods should be compared, both of which have succeeded in explaining the principal electromagnetic phenomena, and both of which have attempted to explain the propagation of light as an electromagnetic phenomenon, and have actually calculated its velocity, while at the same time the fundamental conceptions of what actually takes place, as well as most of the secondary conceptions of the quantities concerned, are radically different.
>
> I have therefore taken the part of an advocate rather than that of a judge, and have rather exemplified one method than attempted to give an impartial description of both. I have no doubt that the method which I have called the German one will also find its supporters, and will be expounded with a skill worthy of its ingenuity. (p.xii-xiii)

Maxwell's aim, as he states it, is not to establish the truth of his theory over Weber's but rather to show how his framework "may be applied to the calculation of phenomena" (p.vi; cf.1856). While one might question the need for an alternative to "the German one" Maxwell motivates the development of field theory by appealing to its elegance, its intuitiveness, and ease of use in application to problems of navigation and telegraphy. The consequences of a widespread demand for electrical knowledge, given its potential for practical application, "have already been very great," he notes, "both in stimulating the energies of advanced electricians, and in diffusing among practical men a degree of accurate knowledge which is likely to conduce the general scientific progress of the whole engineering profession" (p.viii). Here we see ontological considerations made subservient to practical aims in exactly the manner one would expect of someone with a pragmatic understanding of the driving purpose behind scientific research. The reason for studying electricity and magnetism is not to determine their true nature, but to support engineers, and in advocating for his theory Maxwell only looks to show that it can do the latter.

This pragmatic outlook can be seen not only in Maxwell's presentation of his theory, but also through the manner in which he and his colleagues approached their studies of optics and electrodynamics leading up to the publication of the *Treatise*. As discussed in the previous section, unlike their continental contemporaries, Cambridge-trained mathematical physicists tended to theorize unfamiliar systems by constructing models based on formal analogies to more familiar systems. This was common practice because such models often proved quite heuristically useful in the attempt to develop a dynamical analysis, where the analysis developed from consideration of such models was often expressed in a way that implied nothing about whatever collection of interacting component mechanisms produced the dynamics (e.g. as a Lagrangian system energy-function). Once such an analysis had been obtained, the models from which it had been derived were generally given up rather than upheld as ontological insights (Psillos 1999, p.130-145; Morrison 1992). In the case of optical and electromagnetic theories the models were of the mechanical nature of some imponderable but omnipresent ether, but the Lagrangian description of the forces exerted by that ether referred only to field energies distributed throughout space, without saying anything about its underlying ontology. As such, any commitment to the existence of that ether was metaphysically quite minimal, amounting to little more than a belief that the analysis could accurately capture the forces governing a system.

Optical research at Cambridge provides an excellent illustration of this point. Amongst Laplacian physicists on the continent, punctiform (i.e. molecular) models of the ether such as Weber's and Augustin-Louis Cauchy's were fairly popular during the early 19th century. In Britain, however, they were generally frowned upon because they made substantial use of entities that were in principle unobservable, yet provided no apparent advantage in the effort to produce a mathematical analysis. British physicists tended to prefer continuous (e.g. elastic solid) models because they easily facilitated the development of ontologically neutral analytic treatments, which only relied on macroscopic assumptions (Buchwald 1980, p.247). George Green (1838), for example, in seeking to develop an elastic solid model of the luminiferous ether, upheld the superiority of a Lagrangian treatment over more mechanically detailed models because:

> We are so perfectly ignorant of the mode of action of the elements of the luminiferous ether on each other, that it would seem a safer method to take some physical principle as the basis of our reasoning, rather than to assume certain modes of action, which after all, may be widely different from the mechanism employed by nature. (p.245, as quoted in Psillos 1999, p.132)

Green's model allowed for longitudinal waves, which light does not display. This feature could be neutralized, though only through an inelegant and highly *ad hoc* assumption that made the propagation velocity of longitudinal waves infinite. He and his peers quickly judged that the energy function he derived from this model was therefore unsatisfactory, even if it did display some of the observed dynamics, and optical researchers learned valuable lessons about the positive and negative analogies between the mathematical description of elastic solid dynamics and any adequate description of light.

James McCullagh similarly employed a mechanical model in an attempt to derive a Lagrangian potential energy-function for the propagation of light, himself supposing a rotational model of the ether. But he too recognized that this model should not be judged as a true description of the underlying causal processes, and should be discarded once an adequate energy-function was derived from it: “Having arrived at the value of [the energy-function], we may now take it for the starting point of our theory, and dismiss the assumptions by which we were conducted to it” (1839, p.156, as quoted in Psillos 1999, p.134). George Stokes, in turn, attempted to model the ether as an elastic jelly, but cautioned his readers that his model “is advanced, not so much as explaining the real nature of the ether, as for the sake of offering a plausible mode of conceiving how the apparently opposite properties of solidity and fluidity which we must attribute to the ether may be reconciled” and called for a “suspension of judgement” regarding the true constitution of light and its medium (1848, p.12, as quoted in Psillos, 1999, p.136-7). And while the usually practically-minded and metaphysically minimalistic Thomson became, in comparison to his fellow Cambridge researchers and his work in other areas, uncharacteristically obsessed with uncovering the true mechanical constitution of the ether, as previously mentioned he recognized this as an obsession, an irresistible and probably pernicious vice, referring to it as his “ether dipsomania” (Darrigol 2000, p.127).

Maxwell himself expressed the ontological caution typical of Cambridge physicists when developing his electromagnetic field theory through the method of physical analogy, first when modelling Faraday’s lines of force using an imponderable and incompressible fluid passing through a resisting medium (1856), then when using Thomson’s “imaginary” vortex model (1861), and later when investigating the dynamics suggested by models based on the motion of wheels, gears, etc. In his earliest presentation of his approach to modelling electrodynamic interactions as mediated rather than direct, Maxwell did seem to think that further research might reveal the mechanical constitution of that medium, but recognized his fluid-flow model should not be judged as "even the shadow of a true physical theory" (1856). He used the method of physical analogy, he said, not because it might generate ontological insight, but because it was "a method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is never drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favorite hypothesis" (ibid.) Having used a fluid-flow model to aid in his task of rendering Faraday's way of seeing things within a field-theoretic mathematics, he was explicit that his work had yielded a theory with only the pragmatic benefits of intuitive conceptions and mathematical elegance: "I can do no more than simply state the mathematical methods by which I believe that electrical phenomena can be best comprehended and reduced to calculation" (ibid.). Even once his field theory was fully developed, Maxwell cautioned against judging any mechanical model of the ether to be the true account of its material constitution, and felt satisfied with the bare supposition of a medium characterized only as capable of displacement, with no further mechanical details specified even regarding what, exactly, was being displaced. In many ways field theory was an appealing approach for Maxwell precisely because it "could be still considered mechanical, but without all the scaffolding by aid of which it had been first erected. In other words, one should still assume the existence of an underlying medium, able to transmit the actions from one place to another by some kind of mechanical properties, but without specifying the physical nature of these properties and the structure of the medium" (Bucci 2006, p.208; cf. Maxwell 1856, 1861, 1873).

This preference for adequate dynamical accounts over and above ontological insight expressed itself throughout Maxwell's writings and throughout the *Treatise*. For example:

> We know enough about electric currents to recognize, in a system of material conductors carrying currents, a dynamical system which is the seat of energy, part of which may be kinetic and part potential. The nature of the connexions of the parts of this system is unknown to us, but as we have dynamical methods of investigation which do not require a knowledge of the mechanism of the system, we shall apply them to this case. (1873, p.213)

The role of experiment at Cambridge during the development of Maxwell's theory was minimal. Unlike Helmholtz and Weber, Maxwell and his peers at Cambridge were, relatively speaking, noticeably unconcerned with precision measurement, and entirely unconcerned with the exploratory elicitation of novel phenomena. The Cavendish Laboratory, of which Maxwell was the first director and which would soon become a centre for the nascent Victorian metrological tradition, was not even founded until 1871, a mere eight years before Maxwell's death. This is not to say that the British in general or Maxwell in particular saw empirical tests as irrelevant to evaluating their analyses, of course, just that they did not regularly make the effort to test their theories in this way, as such validation would inevitably be provided (or denied) through their successful (or unsuccessful) application in engineering contexts. For example, upon completing the computation that would ground his 1861 conjecture that the luminiferous and electromagnetic ethers were identical, Maxwell wrote excitedly to Faraday saying: "The conception I have hit on has lead, when worked out mathematically, to some very interesting results, capable of testing my theory, and exhibiting numerical relations between optical, electric and electromagnetic phenomena, which I hope soon to verify more completely" (as quoted in Bucci 2006, p.202). While this is sometimes taken as an indication that he hoped to conduct experiments to validate his theory hypothetico-deductively, it's not clear that he's thinking of conducting experiments himself or even what he might have imagined such experiments to be. He certainly never suggested anything like Hertz's device (though Hertz believed it could not have been predicted from theory), or any other method of producing electromagnetic waves to measure their velocity. He didn't even derive the laws of reflection and refraction from his wave equation to show that his theory was able to reproduce the known empirical laws of light propagation. Rather than working out a clear test case, or deriving known optical laws, it's likely that he felt the application of his theory to telegraphy would eventually reveal something about the velocity of electromagnetic wave propagation (for reasons discussed below); indeed, in the years following his death, those looking to develop his theory focused on the possibility of electrically producing waves in wires, not free-standing waves (Darrigol 2000, p.202-5; cf. Yeang 2014).

At the very least, Maxwell didn't conduct experimental tests of his theory. Writing in the *Treatise*, twelve years after his excited letter to Faraday, he was still remarking about the possibility of such tests, rather than bothering to actually carry them out:

> If it should be found that the velocity of propagation of electromagnetic disturbances is the same as the velocity of light, and this not only in air, but in other transparent medium then we have strong reasons for believing that light is an electromagnetic phenomenon, and the combination of the optical with the electrical evidence will produce a conviction of the reality of the medium similar to that which we obtain, in the case of other kinds of matter, from the combined evidence of the senses. (1873, p.383)

But despite thinking that such experimental results might ground ontological conclusions, Maxwell was not especially motivated to obtain them. For the point of scientific work was ultimately to solve problems, to apply theory to practical matters, not satisfy metaphysical curiosity. His pragmatic outlook makes sense not only of his lack of concern with experimental validation of an ether-based ontology, but also of his lack of concern with precision measurement until it became an issue of practical import. Only after publishing the *Treatise* did Maxwell begin to focus his efforts as director of the Cavendish laboratory on precision measurement, stimulated primarily by practical considerations arising out of industry, engineering, and the need for international communication between scientists to be further standardized. Even then, throughout his limited time directing Cavendish, Maxwell struggled to engage students in much of the experimental work the new laboratory aimed to produce, which is perhaps unsurprising given how the disciplined collaboration such work required would have clashed with the culture of competition promoted through the Tripos, and how unfamiliar laboratory procedures would have been for people trained primarily in pen-and-paper problem solving. It would be left to Maxwell's successor, Lord Rayleigh, to properly transform Cavendish into a productive standards laboratory (Schaffer 1995).

The turn towards precision measurement in Victorian Britain was, in fact, largely a result of Maxwell and Thomson's successes in building up the new energy physics, part of a national effort to put theory to use. The potential for practical applications of dynamical theories of heat and electricity were becoming widely known through the 1860s, so British scientists and parliamentarians alike began to place greater value on precision measurements that could increase the predictive power of those theories, as more precise values for a variety of quantities, constants, and parameters would help make Victorians' industrial fantasies a reality. And so, during the later decades of the 19th century, accurate measurement work became a central part of British science for the first time, spearheaded for practical reasons by the most practically-minded of the practically-minded British physicists, William Thomson. Thomson's work on absolute electrometry in the 1860s involved, in part, a (failed) effort to obtain a more precise value for *c*, undertaken primarily because of its practical (rather than ontological) significance (Darrigol 2000, p.122). Following Thomson's lead, and relying on the techniques of Gauss and Weber, in the 1870s the British Association for the Advancement of Science began an effort to standardize and refine a system of absolute units for all the fundamental physical quantities. Maxwell participated by attempting to establish an absolute unit for electrical resistance, again for practical reasons with incidental ontological significance, as an absolute unit of resistance would be important for applying field theory in telegraph projects like the British marine cable, which Maxwell was also involved in. Nevertheless, the theoretical importance of such a measure was fairly clear (Sibum 2002, p.219), for an accurate absolute unit value for electrical resistance would permit the development of a more exact galvanometer, which would in turn help determine more accurate values for the electrostatic and electrodynamic units Weber and Kohlrausch had measured and Maxwell had used to calculate the propagation speed of electromagnetic waves. Thus, precision measurement had the potential to provide even greater evidence for his suggestion that the luminiferous and electromagnetic ethers could be identified and treated by a single theory (namely his). But even here, the path to supporting the identity of these ethers was indirect, proceeding first through application and calculation, not through direct efforts to predict novel phenomena to design crucial experiments, or to establish the precise numerical equivalency of *c* and the speed of light. It's clear that British metrological work was not aimed directly towards the ontological task of establishing the electromagnetic nature of light because they made no effort to increase the precision of Léon Foucault's 1862 measurement of the speed of light, which would have been just as important as obtaining more precise values for the electrostatic and electrodynamic units. For the practically minded Thomson and Maxwell, ontological insight, we might say, was considered an industrial byproduct.

Early British efforts to conduct precision measurements ended up plagued by difficulties and failures, so Maxwell was never able to satisfactorily conduct the more precise measurement of electrical resistance he had hoped to. Their overwhelming focus on mathematical analysis up until that point meant that, unlike the Germans, the British had no established tradition of excellence in metrology. This was especially true at Cambridge, but the situation was similar elsewhere in Britain. When touring Europe to explore the material culture of European laboratories, hoping to learn the best experimental methods so he could employ them at a new physical research laboratory being created at John Hopkins University, the American engineer Henry Rowland was not at all impressed with what he found in Britain during this time:

> I was considerably disappointed with the apparatus [held at King's College, London] as the whole collection had more the appearance of a museum of antiquities than of a working cabinet of instruments, but still there were some instruments of value . . . as I wandered around [the Oxford physical laboratory] the same old feeling of disappointment came over me. For as usual the architect had got the best of the physicist. There are some fine pieces of apparatus but as a whole they might far better have increased their stock than have spent as they did 10,000 pounds for useless ornamentation. (as quoted in Sibum 2002, p.221)

Maxwell's failure to complete much valuable metrological work, then, was not entirely his own fault, being in large part the result of a research culture in Britain that had been narrowing focused on what had been of immediate practical concern up until then: understanding the general dynamics at play in systems of practical interest. Only once the interplay of forces had been given a parsimonious mathematical rendering would it make sense to fill in the precise empirical details. Weber's realism about electric corpuscles, together with the skills he had learned while working with Gauss, prompted him to pursue similar work much earlier. By the time Maxwell began such work he knew precision measurement was both practically and theoretically important, given the current state of his theory, but his inexperience seemed to cause trepidation. Thus, when Joule produced results that seemed to call into question the accuracy of Maxwell's preliminary resistance measures, Maxwell asked his colleagues to investigate the matter further rather than pursuing it himself (Sibum 2002, p.220). While he might have gone back to such work later in life, as those around him took a greater interest as well, Maxwell's attention at that time became focused on producing a second edition of the *Treatise*. He passed away prematurely a few years later, never to be remembered for any precision measurement work.

A pragmatic view of science suggests a limited role for physical experiments in the development of theory, preferring to validate theories through successful application, and we can see this view embodied in the way that Maxwell approached and reacted to those experiments he did conduct. Empiricism led Helmholtz to focus on eliciting new phenomena through experiment, while realism led Weber to focus on experiment as a way of corroborating his theory and better understanding the properties of his corpuscles. By contrast, while developing the ideas and techniques contained in the *Treatise*, Maxwell primarily drew upon the experimental work of others (especially the early exploratory work of Faraday and the measurements of Weber). When he did conduct experiments, it was usually either to provide concrete illustrations of his abstract analyses for pedagogical purposes, or to see which aspects of a mechanical model should be included in a dynamical analysis.

As examples of experiments used for illustration one can point to Maxwell's use of the colour top to illustrate the trichromatic theory of colour, his use of a zoetrope to illustrate the motion of smoke rings traveling in the same direction, and his plaster model of Gibb's thermodynamic surface for water. In his early years at Cavendish he also had his students reproduce historical experiments to help illustrate the theories they supported. Such experiments were by no means treated as tests of theory, however, for as Sir David Gill once recollected, in this context "his experiments always failed" (Forbes 1916, p.17). As any student of modern science will know, however, the failure to reproduce an established effect in an educational setting is a failure of the experimenter, not the theory they hoped to illustrate.

On at least one occasion Maxwell attempted to detect an effect predicted by a mechanical model. While the use of experiments to try and reproduce the observational consequences of mechanical models might at first appear to be a implementation of the realist's hypothetico-deductive approach to assessing the truth of those models, that Maxwell was not attempting to establish the truth of a model can be seen in the way he reacted when his experiment failed to produce the predicted effect. After he failed to observe an effect that the ether would have on a freely rotating magnet, if it was mechanically constituted according to Thomson's molecular vortex model, Maxwell continued to use this model to help develop the mathematics of his field-theoretic electrodynamics in *On the Physical Lines of Force*, simply noting the absence of such effect as a disanalogy that should not be included in a proper dynamical account (1861; cf. Sarkar et al. 2006, p.199). Thus, it would be wrong to suggest that Maxwell's aim in conducting experiments was ever really to hypothetico-deductively test any speculative mechanical hypotheses. Rather, such experiments were used, when they were used at all, simply to inform the dynamical analyses being developed from mechanical hypotheses through the method of physical analogy. An experimental test of a mechanical model's predictions shows, for the pragmatist, which aspects of a physical analogy are positive (and therefore worth capturing in the final analysis) and which are negative (and therefore worth leaving out).

To be sure, the motivations for experimentally testing a hypothesis's predictions, as embodied by a pragmatist like Maxwell, can be hard to distinguish in practice from the motivations embodied by realists like Weber, or by empiricists like Helmholtz and Brewster (cf. Cantor 1975, p.119). Regardless of their philosophies or motivations, scientists always seem to move from hypothesis to prediction to testing in the same way, one might think, casting doubt on the idea that philosophical dispositions make any practical difference. But as we've seen, the difference between pragmatists on the one hand and realists and empiricists on the other can be seen through the conclusions drawn once testing is complete. While realists, empiricists, and pragmatists alike may be motivated to experimentally evaluate a theory's novel consequences, to the realist concerned with unobservable truth or the empiricist concerned with empirical adequacy, an experimental failure like the one Maxwell encountered while investigating the vortex model would likely have resulted in its total abandonment. But for someone whose scientific practice embodies a pragmatic outlook, noting the negative analogy and continuing to work with the model makes perfect sense.

Despite their geographical separation, their linguistic barriers, their distinctive research cultures, their opposed theoretical systems, their contrasting research focuses, and their philosophical differences, 19th century researchers engaged in each of school of electrodynamics found some significance in the results of their competitors, though generally after some reinterpretation. Helmholtz, for instance, wrote prolifically on Weberean theory, carefully investigating its empirical adequacy in his laboratory and criticizing his use of velocity-dependent quantities as violating energy conservation. He followed Maxwell's work as well, and in 1870 was the first to find the boundary conditions required to derive the laws of reflection and refraction from Maxwell's equations (Sarkar et al. 2006a, p.17). Helmholtz read Maxwell's *Treatise* early on, and while he seems to have struggled with it (as most did, even in Britain) he expressed his astonishment at the number of general principles encapsulated by Faraday's approach once it was given a mathematical expression (Helmholtz 1881a). But despite this mutual interest and understanding, each of our three main actors continued to judge his own approach preferable, even when their competitors made significant progress or produced important results. While their local research culture would have presented some resistance to any would-be convert, none of them made a significant attempt to adopt a competitor's approach beyond Helmholtz's radical reinterpretation of Maxwell's theory in terms of interaction potentials and the continental concept of polarization. Nor did any of these characters move to pursue the different research activities that were being fruitfully pursued by others, despite each of them developing and deploying various strategies for communicating and promoting their approach to competitors. Thus, when Helmholtz saw the need to accept fields to accommodate polarization phenomena, he didn't abandon his theory or experimental agenda, he simply introduced a polarizable dielectric medium as an additional entity in his potential-based scheme, expanded his taxonomy of interactions accordingly, and began the arduous task of investigating the interaction potentials it could form with other objects in the laboratory. In short, despite being engaged with each other, often through direct communication and debate, throughout all the developments in electrodynamics in the 1870s, Helmholtz remained focused on eliciting and exploring novel phenomena, Weber on measurement and expanding his theory's explanatory reach, and Maxwell on developing his dynamical analyses.

Their lack of interest in changing their approach is perhaps not surprising. As Hendry notes, "Scientific practices, after all, can be arduous and time-consuming things to engage in, and can draw one's efforts away from other important activities like childcare, politics, and engaging in other scientific practices" (2001, S32). But it is the reason *why* different electrodynamists steadily pursued some practices rather than others that is of interest here. Given their familiarity with each other's work, and the possibility of transferring certain methods and results between each community (as occurred in Britain through the "analytical revolution," the integration of Weber's metrological results into Maxwell's scheme, or the introduction of fields into Helmholtz's system), any explanation of the continued differences in their theories and practices based merely on a kind of intellectual path-dependence, the resilience and influence of local research cultures, or personal pride would surely be incomplete.

The foregoing discussion helps to complete our understanding of the resilience of their theories and practices, as their embodiment of distinct philosophical conceptions of the primary aims, purpose, and value of science helps explain why these electrodynamists remained motivated to practice science in certain ways rather than others, and to react to new results in different ways. Having internalized different philosophical outlooks over the course of their careers, each of them was led to develop and work within different kinds of theories, to focus on resolving different issues in different ways, to conduct different kinds of experiments, and to draw different conclusions from new developments. At this point, then, it would appear we have made good on Hendry's "historiographical intuition" that a scientist's philosophical commitments "should be an important part of the historical explanation of why they did what they did" (ibid., S.27). By itself this is historically interesting, but we have yet to make good on the full aim of this chapter and work as a whole: to show how studying the way previous scientists were motivated by their philosophical outlooks to practice science in peculiar ways can provide pragmatic, context sensitive reasons for today's scientists to adopt one philosophical outlook rather than an alternative. For this we will need to show how the activities that 19th century electrodynamists were philosophically motivated to engage in proved successful, not as such activities would have been judged at the time, but as they are judged today in terms of their lasting contributions to the progress of science.

8) Scientific Progress and Philosophical Motivations

Having established the influence of their philosophical attitudes on the theories, practices, and conclusions of Helmholtz, Weber, and Maxwell, we can now safely employ the power of hindsight to identify the lasting successes of each approach. The potential pitfalls of employing a Whiggish approach to understanding history are behind us, and thankfully so, for we will need a modern vantage to understand the contributions that each tradition of electrodynamics made to the advancement of science.

Maxwell's legacy is certainly the most well-known of our three main characters. His theory formed the basis of classical electromagnetic theory, a highly unified and powerfully predictive approach to modelling both the statics and dynamics of electrical, magnetic, and optical phenomena that truly made the modern world possible, both technologically and theoretically. Once its virtues were recognized it rapidly became widely accepted, even outside of Britain, before being subsumed (or replaced, depending on how you look at it) by special then general relativity and a quantum approach to electrodynamics. The Newtonian assumptions of absolute space and time that Maxwell took for granted have been purged from today's electromagnetic theories, along with the supposition that light must have a medium; even the ether proved to be a merely heuristic assumption, as many of the puzzles about how a medium capable of propagating waves could allow matter to freely pass through it were resolved simply by deeming the existence of a medium a negative analogy viz. other types of waves. And so it was that the mathematics of Maxwell's theory eventually came to be interpreted as describing the spatial distributions of electrical and magnetic energy emanating from sources, with electromagnetic waves propagating themselves through space without need for any medium. While it is difficult to overstate the significance of Maxwell's accomplishments, Feynman certainly gave it his best shot when he wrote:

> From a long view of the history of mankind—seen from, say, ten thousand years from now—there can be little doubt that the most significant event of the 19th century will be judged as Maxwell's discovery of the laws of electrodynamics. The American Civil War will pale into provincial insignificance in comparison with this important scientific event of the same decade. (Feynman et al., 1965)

For well over a century now scientists looking back at Maxwell have been presented with the image of a man who significance ranks alongside the likes of Newton, Shakespeare, and Napoleon. The Maxwell of the scientist's historical imagination was a brilliant physicist who gave to us an incredibly powerful and insightful new theory, its beauty and elegance born largely from his individual genius, destined to serve as the basis of a revolutionary way of looking at the world that would quickly conquer the globe.

Given the clarity with which modern instructors can present Maxwell's theory, however, it's easy to get the wrong impression. Today we identify Maxwell's Theory mainly with four concise theoretical principles, usually stated rather elegantly in vector form, which we call "Maxwell's Equations." These principles allow us to analyze the dynamics of any (classical) electromagnetic system in terms of fields of electromagnetic energy potential extended in space. Their significance is often framed by their ability to unify optical and electrodynamic phenomena, showing that light rays are just one segment of wavelengths within a much wider spectrum of electromagnetic waves. This point can be impressed upon students by using Maxwell's equations to derive the laws of reflection and refraction that constituted Fresnel's wave theory of light, much as Newton's theory is often taught through the derivation of the elliptical orbits implied by Kepler's laws. The thing being, of course, that Maxwell did not give us Maxwell's equations, at least not in a tidy presentation or anything like their current form, having presented a long list of equations in the *Treatise* rather than the more compact versions that now bear his name; nor did his work firmly establish the electromagnetic nature of light, or suggest how this might be experimentally established; nor did he even derive the laws of reflection and refraction from his wave equation (Hunt 1991, Ch.1). Maxwell died just six years after publishing his *Treatise* while still preparing a second edition, and nowhere in his published works did he leave his peers a clear, concise, and convincing statement of electromagnetic field theory.

Any contemporary who read Maxwell's *Treatise* would have seen there was something important and innovative there, but it was initially intended as a reference book so that problems surrounding the mathematical treatment of electricity and magnetism could be included in the Mathematical Tripos (Darrigol 2000, p.166). As a result, his expansive two-volume tome had the character of a comprehensive tour through every aspect of electrical science rather than a systematic exposition and demonstration of his distinctive approach. It was clearly written by someone who saw the aim of science as solving various practical problems through mathematical analysis, presenting a massive collection of exemplary solutions to the sorts of mixed mathematics problems one might find on the Tripos exam. So, despite having developed a new and unfamiliar approach, Maxwell did not frame the *Treatise* around a careful exposition of its methods, novelty, or advantages. Even more so than the presentation of those theoretical germs that gave birth to the traditions of Aristotelianism, Cartesianism, Newtonianism, Darwinism, and Einsteinianism, the foundational document of Maxwellianism presented a theoretical perspective in desperate need of distillation, organization, elaboration, corroboration, and promotion before its true value and insight could be widely recognized. The careful exposition of his novel methods, along with the reworking of his large collection of principles into the four equations that today bear his name and give a proper theoretical order to things, was left in the hand of a small group of British men who recognized the latent novelty and potential of Maxwell's methods and sought to make them patent (Hunt 1991, p.2).

Despite having access to his prior publications and personal guidance, even the most sympathetic of Maxwell's Cambridge peers found the *Treatise* hard to read and digest in the years immediately following its publication. Maxwell's problem-solving approach to theory construction had produced a heterogeneous collection of techniques, proofs, and demonstrations which cluttered his exposition and obscured the essence of his theory. It wasn't until Oliver Heaviside used vector calculus to redress the theory in terms of a few fundamental equations that its essential features became obvious. As George FitzGerald put it: "Maxwell's treatise is cumbered with the debris of his brilliant lines of assault, of his entrenched camps, of his battles. Oliver Heaviside has cleared these away, has opened up a direct route, has made a broad road, and has explored a considerable trace of country" (as quoted in Sarkar et al. 2006b, p.18). But even with the efforts of these "Maxwellians," experimental support of the claim that light is a form of electromagnetic radiation was not forthcoming, as Maxwell's presentation of his theory was in no way oriented towards the goal of experimentally supporting that claim.[84]

While history would come to judge field theory as the correct approach to representing optical and electromagnetic phenomena in a unified manner, was corroborated by subsequent experiments, this was not the manner in which Maxwell sought to promote his theory. Weber's approach was already well developed, widely accepted, and quite powerful by the time Maxwell started giving a mathematical gloss to Faraday's physical insights, and with some effort it seemed like it would also prove capable of unifying optics and electrodynamics. Instead, Maxwell promoted his electrodynamics based on fields rather than corpuscles pragmatically, emphasizing its mathematical elegance and conceptual intuitiveness. As a collection of equations, demonstrated through application to a massive variety of technical engineering problems, with no effort to establish its empirical virtues over (or even departures from) its rivals, as we saw in the last section Maxwell's *Treatise* proffered field theory mainly as tool with promising prospects for industrial application, given that conceptual and mathematical simplicity. The promise of a system that was easier to apply to the technological projects of Victorian industrialists was sufficient reason for someone like Maxwell to put the effort into developing an entirely new way of representing electrodynamic phenomena in the first place,

[84] While Maxwell "basically remained silent on the topic" (Yeang 2014, p.212) of artificially producing electromagnetic radiation following the publication of the *Treatise* and until his untimely death six years later, several "Maxwellians" recognized the possibility of producing non-optical electromagnetic waves through artificial means. Nevertheless, they were unable to do so.

so once it was developed, he promoted it as such: as an easier system to work with, not as the "true" system, or even as more "empirically adequate."

It's unlikely that someone with a less pragmatic outlook on the purpose of science would have taken the time to develop a new approach simply because it would likely be easier to work with, mathematically and conceptually. It's also unlikely that someone with a less pragmatic outlook would have deprioritized the experimental testing of the electromagnetic wave hypothesis in the way that Maxwell did. An empiricist would have been eager to explore the possibility of any new phenomena suggested by a new theory, drawing out its empirical novelties for elicitation through experiment; indeed, as we will see, Helmholtz offer a prize to his students in Berlin for just such an experimental test of Maxwell's theory. A realist might have done the same, but also looked to establish its superlative explanatory power by unifying it with (or using it to explain) phenomena in still other domains, such as gravitation, chemical affinity, or matter theory; at the very least, they would likely have made the effort to derive the empirical laws of optics from the wave equation, as Weber had done with his "synthetic deductions" while developing his fundamental force law. Maxwell, by contrast, was content to simply demonstrate how his methods could be used in a wide variety of exemplary cases, leaving the choice between his approach up to the judgement of open-minded and assiduous readers.

Some of his pragmatically-minded peers at Cambridge were convinced of the magnitude of Maxwell's accomplishments relatively early on, based just on his disordered presentation of his field-theoretic analysis's application to various problems. But those who sought to understand the true underlying causes of electrodynamic phenomena, like many continental adherents to the action-at-a-distance theory, tended to remain unconvinced, even when they encountered one of the more systematic expositions provided by people like Heaviside. The empirically minded Helmholtz was more immediately intrigued than realists like Weber, and while his attempt to integrate a dielectric medium into his potential-based approach ultimately led to the collapse of his entire system, thankfully the empiricist ethos with which he ran his laboratory lived on in his most successful student. For it was only through the empirical corroboration of Maxwell's theory provided by that student, Heinrich Hertz, that the majority of physicists outside of Cambridge came to see the virtues of field theory against all alternatives as undeniable. Pragmatism may have motivated the practices that produced the theory that served as the progenitor of the modern scientific worldview, even while there already existed a well-established alternative that seemed to have great explanatory power (and therefore, to the realist, seemed very likely true). Nevertheless, empiricism motivated the practices that allowed most people to see why this new approach was preferable.

Helmholtz himself contributed little to the progress of science through his laboratory work on electrodynamics. In a decade or more of trying to do little else he repeatedly failing to produce phenomena of any significant consequence. When his experimental work suggested anything, it was that he needed to include new types of interaction energies in his own theory to account for phenomena easily understood by the theories of his competitors (Buchwald 1994, p.41). Whereas Faraday had taken a similarly exploratory approach with great success, guided as well by a kind of ontological agnosticism, Helmholtz was conducting his exploratory studies at a time when many of the electrodynamic phenomena that would prove most theoretically interesting had already been discovered. He of course had no reason to suspect that there were not a great many phenomena that had not been predicted through existing theory, or even some that could not be predicted through existing theory at all. Nevertheless, few novel electrodynamic effects remained to be found using 19th century laboratory apparatus, and Helmholtz was unable to find those that were. Thus, Helmholtz's experimental work in electrodynamics is not, itself, viewed as very successful from the modern vantage.

Despite his empirical focus Helmholtz was also an adept and attentive theoretician, and was probably more familiar with all three of the major approaches to representing electrodynamic phenomena than anyone else at the time. He was not without his mistakes in derivations, but he made several contributions to the minutiae and general aspects of electrodynamic theory, mainly as matters of clarification and attacks on Weber's theory. And while it was eventually abandoned because it had become clearly hamstrung by a plethora of interaction types and the acceptance of a dielectric medium equivalent to Maxwell's ether, Helmholtz's reinterpretation of Maxwell's theory within his more familiar potential-based system was widely influential, helping continental physicists better understand what Maxwell was doing. Additionally, while not decisive, seeing such an icon of German science like Helmholtz continually attacking Weber's theory, both abstractly and experimentally, prevented continental physicists from having total confidence in Weber's theory, thereby ensuring that not everyone who favoured

Weber's theory would immediately dismiss field theory. These contributions were real, but relatively minor. Especially in physics, subsequent scientists often recognize the contributions of their predecessors by naming concepts after them. Whereas the fundamental equations of electrodynamics bear Maxwell's name, the unit of magnetic flux bears Weber's, and the unit of frequency bears Hertz's, Helmholtz did not contribute anything of special significance to electrodynamics to warrant such canonization. He has received well-deserved eponymous honours in the nomenclature of thermodynamics, vector calculus, and acoustics, but none of the key concepts in electrodynamics bear his name, and understandably so.

While I have used it throughout this chapter to help explicate the character and empiricist motivations behind Helmholtz's approach to electrodynamics, Buchwald's *The Creation of Scientific Effects* (1994) is actually a book about Hertz. In painstaking detail, Buchwald works to "take the reader into Hertz's world, eventually into his laboratory, in a way that does not permit one to stand apart from it in understanding" (p.xiv). In doing so he provides a rich picture of Hertz's work that is starkly opposed to an all too common understanding of it as simply an early German convert's attempt to validate Maxwell's theory experimentally. When we see Hertz's work as he himself did, we see that it was the result of "his complete absorption of the peculiar ethos that characterized the Helmholtz-centered physics community of the 1870s and 1880s" (p.2). By looking at the training Hertz received in Berlin, Buchwald writes, "in the environment established by a man whose complex influence on Hertz ran so deep that it can scarcely be exaggerated, we will find the inmost sources of Hertz's approach to physics and of the motivations that drove him so intensely throughout the 1880s to seek novelty, to try repeatedly to fabricate new effects in the laboratory" (ibid.). In short, Hertz's production and investigation of electromagnetic radiation proceeded in the characteristically empiricist style he had learned from working with Helmholtz, so much so that when he first produced the novel effect that would come to be seen as a confirmation of Maxwell's theory, it was not "at first altogether clear to Hertz just what he had produced" (ibid.). Rather than resulting from a realist (or any other sort of) commitment to Maxwell's theory, Hertz's work was unguided by any theory in particular. In his introduction to *Electric Waves* (1893), in fact, Hertz asserts that the effect he produced that would come to be interpreted as electromagnetic radiation "could not be foreseen by any theory" (p.3; cf. p.217, 231). So, while Helmholtz himself did not achieve much success, the empiricist attitude that drove him to seek novelty through exploratory experiments did motivate others to undertake activities that proved instrumental to the advancement of science. Hertz began his work in electrodynamics at a time when Weber and Helmholtz's disagreement over the thermodynamic plausibility of Weber's force law had taken on a life of its own, percolating up into the politics of academic administration at their universities in Berlin and Göttingen, with lasting consequences. Over a decade later, for instance, Max Planck was awarded second place in an essay competition on "The Nature of Energy" held by the Philosophy Faculty at Göttingen. He was awarded second place even though no first place was awarded, he surmised, simply because his essay had sided with Helmholtz against Weber (ibid., p.57). Despite this work generating little interest amongst his contemporaries, Planck was later invited to fill a position in Berlin, with the Faculty report praising him "for carrying through 'the strong consequences of thermodynamics without interference from other hypotheses'" (Jungnickel and McCormmach 1986, vol. 2, p.52, as quoted in Buchwald 1994, p.58). Clearly this was meant as praise for his rejection of Weber's corpuscles in favour of Helmholtz's potentials. It was into such a charged climate that Hertz, perhaps not perceiving the full extent of the divide, chose to work with Helmholtz and was baptized into his unique understanding of the purpose of laboratory work, to the exclusion of Weber's understanding. Indeed, the first experiment Hertz undertook in Berlin was in response to a prize offered by the Berlin Academy, set by Helmholtz, for any experimental answer to the question of whether electric currents possess mass. Still suitably general, Helmholtz's theory said nothing about whether currents had mass, though he suspected they did not. Weber's theory, on the other hand, necessarily required them to by this time in order to account for resistance in circuits, so the goal for Helmholtz became to attack Weber's theory by producing an effect showing their absence. While Weber could save his theory by simply asserting that their mass was smaller than could be detected, if a delicate experiment could show that their mass must be vanishingly small doubt could be cast on Weber's central hypothesis (and thereby his entire theory) by pointing out its empirical invulnerability (see Buchwald 1994, Ch.5 for more detail).

Using experiment to set a maximally small upper limit for current mass was a formidable task for someone undertaking experimental work of this sort for the first time, requiring Hertz to first design and then tweak, balance, isolate, stabilize, and iteratively mutate an intricate apparatus ever so slightly, pushing the instruments and techniques of the time to their furthest limits. Of special interest is that he focused on amplifying rather than measuring the effect that a current's mass would display, concentrating first on demonstrating the effect qualitatively before assigning it a numerical value. For whereas for the Webereans "numbers are the very essence, the goal, of experiment," to Hertz, "as a disciple of Helmholtz numbers were not in and of themselves very interesting. Certainly measurement was important in order to determine the magnitude of something known to occur. But it was more important to show *that* something occurs than to measure it precisely" (ibid., p.89). Note that the focus here is on demonstrating the (non-)existence of some effect at the observable level, not the (non-)existence of some hypothetical entity.

With significant guidance and assistance from Helmholtz, through his efforts to win the prize Hertz was inculcated with his mentor's characteristic understanding of the aims of research, and learned a great deal about how to serve those aims. First and foremost he learned that, unlike the rigid apparatuses of a laboratory used as a "generator for constants" like Weber's, apparatuses in a laboratory used as an "engine for discovery" needed to be fluid so they could be freely modified over time, not just to eliminate external sources of perturbation but also to more exhaustively investigate the phenomena of interest. He also learned how to estimate error, amplify effects, and various ways to reduce the former to bring out the latter, techniques that were essential to his subsequent experiments (ibid., p.74). Hertz would carry forward the empiricist spirit motivating Helmholtz's laboratory work as well, finding intellectual satisfaction only in the attempt to elicit novel effects. After leaving Berlin, for instance, he first accepted a position in Kiel, but he became frightfully depressed by the lack of access to experimental apparatus his position there provided him, fearing his career could not advance without it (ibid. 218). When arrived at Karlsruhe and gained access to a laboratory, the moody Hertz began to find his drive and passion for physics again, experimenting with a wide variety of phenomena in an attempt to produce novel effects, using the same theory-neutral methods and technical skills he had learned from Helmholtz. During this time Hertz "had little directly to do with industry (with one partial exception), was uninterested in metrology (though not in units), was impatient with mathematical rigor or undue abstraction, and strongly disliked testing things thought up by other people unless there was the chance of finding novelty. From the moment he left Helmholtz's physical presence, Hertz moved from topic to topic, from technique to technique, seeking always to make something new come into being" (ibid., p.2). In a word, he remained a practicing empiricist.

His work in Karlsruhe covered a wide spectrum of topics of physical interest at the time, from hardness and elasticity to evaporation, coupled circuits, and cathode rays. In each instance Hertz was directed towards the same purpose—the discovery and further investigation of novel effects— and proceeded always with a typically Helmholtzian aversion to the constraints imposed on the mind by speculative hypotheses and refined theories. The activities that led him by 1888 to develop his spark-gap transmitter and receiver apparatus—which would eventually aid in the rapid spread of field theory as it came to be seen as a fabricator of electromagnetic radiation, an experimental verification of Maxwellianism—did not in any way begin with any effort to support field theory, though the heirs of Maxwell in Britain were by this time hard at work promoting it. In fact, Hertz's laboratory notes and publications make clear how undirected by any theory he had been. Rather, Hertz's most acclaimed contribution to the progress of science began with a typical Helmholtzian activity: freely altering the conditions of an experimental set-up to elicit and investigate novel effects.

It all started when Hertz found a pair of Riess (or Knochenhauer) spirals amongst the equipment in Karlsruhe. A specific kind of induction coil, this device was designed primarily for use in the classroom to qualitatively demonstrate electromagnetic induction phenomena. It consists of a pair of tightly wound coils oriented in a parallel plane, with each coil's curcuit bridged by a small gap so that when a current was put through one coil and induced in the other, each current would visibly spark across the gap. In 1886 Hertz began to play around with this device, discharging Leyden jars and other induction-coils into them rather than connecting them to a battery, as was customary for demonstrations. He soon noticed a puzzling effect: there was a strict correlation between the absence or presence of a spark in the induced coil and the absence or presence of a spark in the inducing coil. Even when he unwound the inducing coil into a linear circuit, a spark would follow in the secondary circuit so long as there was a spark in the first. At first, he expected that the spark somehow increased the rate of change of the current in the inducing circuit, and thereby the strength of the induced current, but this was unexplored territory in any case, so he continued to investigate.

Without bothering to try and provide a theoretical account of this effect, Hertz proceeded to experimentally probe the gap's inducing power by modify the coils further, using Helmholtz's potentials to represent his findings. In the process of trying to investigate the primary gap's inducing power he found that some of his required measuring devices were also producing sympathetic sparks. This was unexpected and initially frustrating, but when these additional sparks proved ineliminable they eventually became interesting in their own right. His attention turned away from trying to establish that a spark gap increased the rate of change in a current, and focused instead on determining the conditions under which sympathetic sparks would or would not be produced. He added circuits and varied their length and orientation, and the results suggested that effects occurred because the spark gap made the state of a circuit inhomogenous in the moments following the spark, as the current propagated throughout the circuit with finite speed. Up until this point the empirical consequences of inhomogenous currents in small devices like this had never been investigated, or even considered very significant, generally being treated as effectively homogenous (i.e. currents were treated as propagating instantaneously). This was understandable given that Hertz had only now, largely by accident, constructed a device capable of detecting the effects that finite propagation speeds generated (he initially thought) via induced sparks.[85]

Hertz made many subsequent modifications to his induction apparatus, and eventually invented an apparatus known as a linear, spark-switched oscillator. He also modified the secondary circuit into a suitable resonator for this new oscillator, all through a piecemeal and delicate investigation of curious and subtle features of these new effects. Only after significant experimentation and mutation did Hertz arrive at a device with a specific construction that produced effects so peculiar that he decided he needed to give it a theoretical treatment to better understand what was happening in it. But even then, he saw the effect as a species of simple induction, albeit a quite novel one, and continued to freely experiment with it, often absent theoretical considerations. In the process he discovered what we now understand as the photoelectric effect, soon after which he rushed a publication into print on the subject. He rushed this publication not only because it gave him a definite claim to novelty, but also because it was less puzzling than whatever was going on with the spark-gap generator and receiver (Buchwald 1994, p.244). For by this point he had begun to suspect that his device was his ticket to winning another experimental prize posed by the Berlin Academy, this time looking for the

[85] For a more detailed and complete account of how Hertz ended up with his final device, see Buchwald (1994, Ch.14) and Hertz (1893).

experimental production of induction through dielectrics (of which Maxwell's ether was one).[86] But it was only gradually, over extended and highly exploratory investigations conducted in the Helmholtzian style, driven by Helmholtzian motives, that Hertz came to see the effects he was producing and investigating as electrical waves in the electromagnetic ether. His subsequent laboratory investigations of these waves proceeded similarly. Not satisfied simply with their production and derivation from theory, which would have been the most significant accomplishment for someone aiming simply to support and promote Maxwellianism, Hertz did not see this alone as worth publishing. He proceeded to alter his apparatus systematically to explore the entirety of the phenomena's various features, measuring the speed of wave propagation, probing the extent of their polarizability, testing the variability of their wavelength, and investigating their possibility of reflection and refraction. He did this largely without the assistance of derivations from theory, and never with an aim to simply corroborate Maxwell. In fact, his publication outlining his results ended with the claim that he had discovered aspects of the phenomena that Maxwell's theory was "unable to account for" (see Buchwald 1994, Ch. 17, Sec.4-5). From Hertz's perspective, this was Helmholtzian exploratory work, not Maxwellian promotional work.

The empiricist approach to laboratory research, focused on the use of apparatus as engines of creation rather than windows into an unobservable world or tools for sharpening predictions, guided Hertz throughout his work, motivating him to focus on specific sorts of activities like demonstrating effects rather than determining numerical values for the forces involved in such effects, and fully characterizing the phenomena rather than testing theories or corroborating ontological claims. That is, he was not guided by a desire to support Maxwell's theory, either with empirical confirmations or by extending its explanatory scope. Nor was he guided by a desire to apply theory to practical ends. Hertz, like Helmholtz, desired merely to elicit novel phenomena that any theory would then need to save to be empirically adequate. Understanding the Helmholtzian (and therein empiricist) motivations embodied in Hertz's work is important because without it we are left, as Buchwald (ibid.) puts it, with:

> a story in which Hertz's experiments on (among other things) hardness and elasticity,

[86] From the perspective of Helmholtz's theory, at least. From the perspective of Maxwell's theory, the ether is the only dielectric.

> evaporation, cathode rays, coupled circuits of all sorts, spheres rotating in the earth's magnetic field, not to mention his otherwise odd (1884) attempt to generate propagation without an ether or the sequence through which he eventually fabricated electric waves, would all appear to be distinct, essentially unrelated events. Further, I would no longer be able to explain the character of Hertz's practice, and indeed the motivation behind many of the specific things that he did. It is precisely the unexpressed aspects of Helmholtz's physics that strikingly unify the young Hertz's work. (p.325-6)

The successes resulting from this empiricist approach to scientific research are quite significant. By Hertz's own account, it would not have been possible "to arrive at a knowledge of these phenomena by the aid of theory alone. For their appearance upon the scene of our experiments depends not only upon their theoretical possibility, but also upon a special and surprising property of the electric spark which could not be foreseen by any theory" (Hertz 1893, p. 3, as quoted in Buchwald 1994, p.217). But once such phenomena were well established and readily interpreted as the production of electromagnetic radiation in space, the identification of the luminiferous and electromagnetic ethers suggested by Maxwell's theory was a natural conclusion to draw. So, for those who agreed that there must be some sort of medium supporting the propagation of light waves, yet were also committed to Weber's electrodynamics based around corpuscles rather than fields which had yet to produce any non-*ad hoc* account of the ether and light's electromagnetic character, Hertz's work would have had immediate and revolutionary theoretical significance. The spread of Maxwell's theory, freshly glossed in Heaviside's vector form, was rapid after this point, given that the majority of physicists at the time, wherever they called home, had accepted the wave theory of light in one form or another, and thereby the existence of an optical ether (whatever its mechanical constitution). By artificially generating, detecting, refracting, reflecting, polarizing, and superimposing the sort of electromagnetic radiation which Maxwell's theory implied visible light was but a special instance of, Hertz's work seemed to bind anyone who accepted the existence of a luminiferous ether to an acceptance of Maxwell's electromagnetic ether as well, for they were evidently identical. Near universal acceptance was all but inevitable, by physicists in Britain, on the European continent, and beyond.

While they should be clear by now, a few remarks are in order regarding the contributions resulting from those research practices motivated by Weber's realist attitude towards his theory. Weber's most lasting, memorable, and indispensable contributions to the progress of science was his work in metrology and measurement. Without Weber's efforts to establish a system of absolute units, to understand the abilities of various materials to store electrical corpuscles, to measure the unit values of the electrostatic and electrodynamic force and obtain a value for *c*, and to generally (as he saw it) provide a sensory organ to enrich the soul of his theory, Maxwell's theory would not have had much of the empirical content it did as quickly as it did (unless someone else had done a lot of measurement work that no one was pursuing with Weber's degree of zeal). Perhaps most importantly, Maxwell would not have been able to subsume optical theory within electromagnetic by identifying the luminiferous ether with the electromagnetic. That identification was very important for converting action-at-a-distance electrodynamists to field theory through their acceptance of wave optics, and without the ontological identification of the two ethers that Weber's measurement of *c* suggested it's not clear that Hertz's device would have been interpreted as a generator of electromagnetic radiation. It's not even clear that it would have even been seen as a corroboration of field theory, so it probably would not have led to its rapid and widespread adoption as it did. We will of course never know, but it's clear that Weber's efforts to support his corpuscular fluid hypothesis made significant contributors to the advancement of science, even though it ultimately led the scientific community to reject that hypothesis.

While I've accepted a historiographically suspect notion of scientific success in this section, judging it risk-free now that the historical narrative has been properly laid out, and necessary for helping us understand what sorts of success arose from different philosophical outlooks, it remains prudent to avoid relying on too many counterfactual historical hypotheses to make my point. Weber was such a towering figure in 19th century electrodynamics that it's utterly unclear what would have occurred had he taken a different course by, say, developing Ampere's ether hypothesis about the underlying cause of electrodynamics action rather than his corpuscularian hypothesis, or by following Gauss and Neumann in adopting an empiricist attitude that sought to eliminate hypotheses all together. Sticking to the factual, Weber's work is to this day widely seen as instrumental to the initial development of Maxwell's theory, both in his measuring of *c* and in his determining in absolute terms a great many electromagnetic units, along with his measurements of various magnitudes using those units. While metrology was a stable German tradition at the time, the steady dedication with which Weber applied its methods to electrodynamics was manifestly the result of his realist convictions, part of an effort to provide empirical flesh and evidential support for his theory's basic ontological assumptions. While his two-fluid theory was largely (though by no means entirely) rejected by the turn of the century, Weber's unique concentration on promoting his own theory realistically served to fill in many of the gaps of Maxwell's work in his effort to mathematize Faraday's physical picture, and helped make possible the rapid shift in attitudes and research following Hertz's work of 1888. These are clear contributions to scientific progress that should not be understated, nor forgotten, even in the shadow of Maxwell who, according the Feynman, will eclipse every significant event of the 19th century. Indeed, the lasting success of Weber's electrical measurements, as the means through which empirical detail could be given to Maxwell's theory and optics could be integrated with electromagnetism, is valued and acknowledged not only by modern physicists, but was also acknowledged by Maxwell himself, calling Weber's establishment of a system of absolute units for electromagnetic magnitudes "one of the most important steps in the progress of science" (1873, Art. 545, p.193).

While Weber's metrological work stands out, as a great success in retrospect his attempts to extend his theory to explain phenomena like chemical affinity and gravitation proved unsuccessful (or at least incomplete), as did his attempt to represent the ether using his corpuscles. Nevertheless, it's worth noting some of the subtle ways in which the followers of Maxwell returned to portions of Weber's system and tried to use them to develop Maxwell's further. Several experiments in the late 19th century, in particular with cathode rays, suggested that there must still be an electrical particle of some sort, roughly akin to one (though not both) of Weber's corpuscles. Despite Hertz winning the first Berlin prize for experimentally problematizing the assumption, it also became apparent that currents had some degree of mass. It accordingly became recognized that some discrete and material source of electrical attraction would need to be added to Maxwell's theory, and those familiar with Weber's approach (such as Lorentz and Weber's student and successor Eduard Riecke) attempted to develop the concept of an electron by taking inspiration from Weber's microphysical explanations of phenomena like resistance and currents. In 1886, albeit before the widespread adoption of Maxwellianism, Ludwig Boltzmann noted that with some modification Weber's representation of currents as corpuscular fluid flows could account for the puzzling Hall effect (Kaiser 2001, p.256). Even by the end of the century, with Maxwell's theory forming the basis of most electrodynamics research throughout Europe, Weber's microphysical picture was not entirely dismissed as utterly mistaken. In 1902, for instance, Ambrose Fleming ascribed value to J. J. Thomson's detections of electrons as a means of relating the work and insights of Faraday, Helmholtz, Weber and others (Gooday 2001, p.117). While Ampère's corpuscular flow hypothesis by no means formed the fundamental ontology of those electromagnetic theories that modern theories count as their predecessors, Weber's realism did lead to some theoretical tools that proved an inspiration to his successors. Nevertheless, to be sure, our modern conception of the electron did not develop out of any such inspirations, leaving the bulk of Weber's actual (rather than potential) successes, as motivated by his realism, to be measured by his metrological work.

9) Conclusion

So, what does this study of the philosophical motivations behind, and successful outcomes of, late 19th century European electrodynamics research practices tell us? What lessons can be found here for contemporary scientists who find themselves on the fence viz. different philosophical outlooks? And what lessons can be found for philosophers of science concerned with issues concerning scientific realism?

It must be stressed that one thing this case study, or any study like it, can't be taken to demonstrate is that some philosophical outlook encapsulates the true aims and epistemology of science. If we characterize our observations of scientists in axiologically-neutral ways, as groups of people moving some objects around or manipulating some symbols, there are no scientific practices that a realist, empiricist, or pragmatist scientist would be conceptually barred from seeing as well-motivated. While they would have understood their actions differently, Helmholtz, Weber, and Maxwell could easily have done just what the other was doing without contradiction. Helmholtz aimed to demonstrate the empirical inadequacy of Weber's theory through experiment, for instance, while Weber sought to support its truth through similar activities. Buchwald (1994) draws out the way that Weber's disciples might have engaged in precisely the same activities that Helmholtz's did as follows:

> Consider again the [first Berlin] prize competition. Here, we saw, Hertz's results could easily be dismissed by Webereans because they merely provided a numerical value, an upper bound for electric mass. Webereans had only to say that the mass was lower than

> that. If we invert historical reality for a moment, then we can uncover something quite interesting about this. Suppose that a contra-Hertz working for the aged Weber in, say, the late 1870s had decided to *measure* electric mass. The contra-Hertz does not question the existence of the mass, or at least of something that behaves like mass; he wishes, however, to know its magnitude. So he performs an experiment like Hertz's (unlikely though this would have been, albeit for reasons having to do with experimental technique), and he obtains some upper bound. "Excellent," he concludes, "this is a useful, though hardly definitive, number to use for electric mass, at least until experimental delicacy improves sufficiently to directly detect its effect." Within a community that takes electric mass for granted, a Hertz-like experiment would be the beginning of a program of measurement, not the end of a research tradition. (p.89)

Similar accounts could be drawn up to show how any one of the three main electrodynamics research programs might have conducted the kind of the work being done by a competitor. Weber might have looked to develop a field theory, for instance, in an attempt to better establish his own theory's superiority, or else discover the *true* source of electrodynamic action. Maxwell might have dedicated himself to eliciting novel effects in the laboratory, hoping to guide him in the development of a more powerful field theory. Helmholtz, in turn, might have recognized that Weber's theory could be held more empirically accountable with a value for c, and worked hard to provide one (albeit only so he could more definitively prove that theory's inability to save the phenomena, because it violated energy conservation). Whether someone is a realist, empiricist, or pragmatist, they will surely agree that theory must be evaluated through experiment, that promising theories should be extended to new domains, that a unified theory is preferable to a disunified one, that discovering new phenomena is a good thing, that constants and parameters will need to be accurately measured, and that predictively powerful theories need to be built up (even through the use of hypotheses) in a manner that ultimately permits their application to concrete systems. Each of these practices is, regardless of one's philosophical outlook, an important part of scientific practice, even if these activities are rationalized through different aims, as warranting different conclusions given certain outcomes, or as more or less likely to prove fruitful. Thus, we must conclude, there are no truly realism- or antirealism-laden scientific activities, and therefore no identifiable aim that uniquely motivates and governs all scientific practice.

But while they would not have been doing anything *inconsistent* with their philosophical outlooks had the Cambridge researchers, Helmholtzians, or Webereans chosen to do what their competitors were doing, the point is that they were less *motivated* to do what their competitors were doing because of those outlooks. For notice how Buchwald says that Webereans would have been unlikely to conduct an experiment like Hertz's, given the differences in their experimental techniques which, as we saw, were aimed primarily towards giving support to the central hypothesis of Weber's theory. It was already clear that current mass must be very small, and conducting any experiment that merely set an upper bound for such a small value (rather than an upper and lower bound) would clearly be interpreted by the Helmholtzians as counting against Weber's theory. Webereans simply didn't conduct such experiments, because their techniques were developed to build support, not problematize it. Furthermore, even if a Weberean had conducted this experiment, Buchwald says, we'd expect them to react differently, both cognitively and practically, going on to refine their measurement of current mass rather than accepting a prize for having shown the likely absence of current mass and moving on to other work. So, while our case study doesn't show that the methods of science are laden with philosophical commitments, one thing it does show, at least in this case, is that philosophical commitments tend to affect the way that scientists practice their craft in characteristic ways, suggesting to them that different activities are most worth pursuing at a given state of research.

This study has shown, then, that Hendry (2001) is right. Contra Fine (1986a, 1986b), the fact that no scientific practices are realism- or antirealism-laden does not mean that realists and anti-realists don't practice science differently. For through this detailed study (as well as the more cursory examples found in Hendry (2001) and Wray (2015)) we can see that, historically (i.e. *empirically*) speaking, different philosophical commitments result in very different forms of research. Their philosophical commitments are not idle in practice, either, for the success of each of the three main electrodynamics research programs can't be accounted for by their practices alone, e.g. by saying that Hertz discovered new phenomena because he was looking for them. For the researchers' philosophical perspective played an important role in stabilizing their research around a narrow set of activities rather than others to fulfill what they saw as the ultimate aim of research. Historiographically, this is an important point: philosophical commitments need to play a role in our historical explanations of why scientists did what they did.

Fine's main point is philosophical, however, and he's right: the absence of any realism- or antirealism-laden practices means no *epistemic* reasons can be found for accepting one philosophical account of science's governing aims and epistemic outcomes as the true account. Nor can we say that, pragmatically speaking, there's some uniquely preferable philosophical outlook that all working scientists should adopt. Quite the opposite, in fact, for my history of this period has suggested more than anything else that a diversity of philosophical commitments amongst working scientists greatly benefits the advancement of science. As we've seen, realists, empiricists, and pragmatists were each motivated to undertake practices that those with different philosophical attitudes were less motivated to undertake, and having different groups of researchers narrowly focused on some of those practices rather than others proved instrumental to the progress of electrodynamics through this period. It doesn't take significant counterfactual thinking to see that, had the scientific community been entirely made up of people with the same philosophical outlook, progress would have been harder to come by. Thus, there is nothing in this study that can help guide a working scientist's choice between philosophical outlooks by showing them the universal pragmatic superiority of one view over alternatives. While I've focused on informing the free choice of individuals it's probably worth noting that, from a policy or managerial perspective, this study also gives us no reason to promote anything other than philosophical pluralism within our scientific communities. Thus, we must agree with Wray (2015) that, contra Popper, Feyerabend, Planck, Mach, and even Fine, philosophical diversity within the scientific community, at least in this case but I suspect in all cases, seems to be a good thing so far as the progress of science is concerned (cf. Longino 2001).

But we can draw more fine-grained and informative conclusions. It would seem that, at least in this case, working within different philosophical outlooks led to *specific forms of successful practice* for those holding each outlook. This is significant, I assert, because knowing when and how philosophical commitments led to successful research in the past can be of significant practical value in the present, helping working scientists make better-informed decisions about which philosophical outlook to adopt themselves. To see why, recall the menu model of epistemic stance selection presented in the previous chapter, which models the way people choose their philosophical commitments on the way people order food off a menu. Now imagine you're a working scientist, aware of your own research context and the state of the field, perhaps already involved in a large research project or simply aware of the kinds of work getting praise and grants these days, wondering about which philosophical account of science you should accept. Might not the historical illustration presented above, or ideally a collection of studies showing that such patterns of influence are historically robust, begin to shape your understanding of which philosophical outlook is likely to be the best choice for you, given your specific practical context? Looking at the successes arising in the past from different philosophical outlooks, in different contexts, perhaps you'll notice that, in contexts similar to your own, one of those outlooks tends to promote successful practice more than the others. Practically speaking, then, this could provide a reason to adopt that outlook yourself. A scientist using historical studies of how philosophical attitudes led to successful scientific practices to inform their choice of philosophical outlook in this way, I suggest, is akin to being in a crowded restaurant, looking at those who've already eaten off the menu we're being asked to select from, noticing that someone is especially delighted by their choice for reasons we suspect we'd also be delighted by, and then saying to the waiter: "I'll have what she's having."

This is the kind of assistance that historical studies like the one I've just presented can offer to scientists contemplating a choice between philosophical outlooks, helping them better understand the likely benefits and dangers of choosing between different "epistemic options" (Lipton 2004). But whereas our observations of the satisfaction of other diners helps us answer rather mundane questions about what we want to eat, our observations of how philosophical choices influenced the actions of other people can help us answer questions that I think many of us would judge to be much more important. For knowing how philosophical commitments tend to influence practical action, and when such actions tend to be successful, can help us judge which position is likely to help us achieve our goals and maximize our values. By showing that scientists who accept realism tend to prove especially adept at measuring constants or extending theories into new domains, for instance, scientists who seek to accomplish these tasks could find a pragmatic reason to accept realism themselves to help guide their thought and action. By showing that empiricists tend to be successful at discovering new phenomena, and pragmatists tend to be good at building successful theories and informing design, similar pragmatic reasons may be found for those scientists looking to accomplish those tasks to accept one view rather than another. But let me be clear: one historical case study is unlikely to prove persuasive, so I don't think I've provided strong enough evidence to inform scientists' practical reasoning about philosophical commitments yet. Pragmatic reasons are still grounded in evidence, and we will need more data to conclude that realism, empiricism, and pragmatism are so regularly effective in the contexts they appear to have been amongst electrodynamists in late 19th century Europe that they will probably be effective for today's scientists in similar contexts as well. But at least we have a working hypothesis to test, probe, elaborate, and corroborate through further historical studies, and can see how it's possible to find evidence bearing on some people's pragmatic reasons for making philosophical commitments.

So, while historical studies like the one provided here cannot provide anyone with *epistemic* reasons to adopt a philosophical position, they can provide working scientists with *pragmatic* reasons, if only relative to certain practical contexts. Providing pragmatic reasons for scientists to adopt a philosophical outlook is of potential significance to scientists, of course, but there are methodological lessons for philosophers here as well, specifically regarding how we might proceed when we judge that a philosophical debate inevitably ends in a draw. It seems to me undeniable that van Fraassen has argued his realist opponents into a permanent stalemate, such that both the constructive empiricist and the scientific realist should be seen as taking rationally permissible positions. But this stalemate has always generated a thinly veiled anxiety amongst those who accept its existence, which I think strikes at the heart of the analytic philosopher's ethos. The worry, I think, is that this might mean there's no way to influence people's philosophical commitments viz. scientific realism by appealing to their rationality with arguments and evidence. *But that's what philosophers do*, at least in the analytic tradition: they argue about what it's rational to think, given the available evidence. If there's no way of influencing people's adoption or rejection of scientific realism with arguments and evidence then Fine is right, "realism is dead" (1986a), insofar as he means that debating the issues any further is a waste of time and intellectual resources. This that case, trying to change people's commitments is futile. Realists like Chakravartty and anti-realist empiricists like van Fraassen might want to spend their time developing their positions into their most coherent form, but the prospects for opinion-altering dialogue or debate effectively vanishes, leaving us only with the baroque task of building an invulnerable philosophical system that some people will find pleasing but most people won't care about at all. Fine calls such systems "castles in the air" (1986a, p.116, n.4) to indicate that, while beautiful, they don't matter to anyone who's not inside them, and serve only to further the intellectual isolation of those who are. The anxiety intensifies if we start to consider how many philosophical debates seem stalemated in this way, where there is no prospect of coaxing our philosophical opponents out of their impenetrable flying fortresses using rational arguments based on evidence. If we accept there are no epistemic reasons to be found for accepting realism or anti-realism, but still accept that both positions are rationally permissible, and then find that this is the case for many opposed philosophical views, we might then conclude that philosophical inquiry is unable to change people's minds regarding a great many philosophical issues. But then philosophy is either apologetics, intellectual decorating, or wild goose chases looking for epistemic reasons that will never be found. Put bluntly, if philosophers can't change each other's minds, yet still choose to spend their days ornamenting dogmas, insulating their opinions until they're indestructible, and isolating themselves from those with opposed positions, what claim can they have to be lovers of wisdom?

What I think this study shows, however, is that granting rational status to opposed positions locked in a stalemate does not devolve philosophy in this way. For there's a way around such stalemates that still uses rational arguments based on evidence to inform our philosophical views and influence those of others. The key is abandoning the traditional ambitions and standards of philosophical argument: looking for that position that no rational person could deny. For even if we can't find epistemic reasons that show every rational person should adopt some philosophical position, we may well find pragmatic reasons for this or that person to adopt it. So, realism is not dead, nor need we worry that argument and evidence are unable to change people's minds on stalemated issues, even if we can't change everyone's mind with the same arguments or evidence, or change people's minds by making them think that one view is the "right" view *per se*.

I suspect this may be of little consolation for many analytic philosophers, as the search for epistemic reasons, and the need to identify the "right" view, seems to me a deeply ingrained part of their method. Furthermore, the study of how philosophical commitments influence scientific practice may provide pragmatic reasons for *scientists* to make certain philosophical commitments in specific contexts, but this has no bearing on the commitments of *philosophers*. Admittedly, anti-realists like van Fraassen and realists like Chakravartty will find nothing in my account of 19th century electrodynamics that will give them the slightest reason (pragmatic or otherwise) to consider moving to a different castle. Indeed, given how history indicates that a healthy and productive scientific community seems to be made up of scientists with a plurality of philosophical positions, van Fraassen and Chakravartty might appeal to the case study presented here, and the similar studies of other historical periods I hope to conduct in the future, as a way of rejecting Fine's claim that further developing their views is a waste of intellectual resources. For if an ideal scientific community is philosophically pluralistic, philosophers who value scientific progress had better make sure that each of these philosophical outlooks is developed into a maximally coherent form. That way, when scientists come looking for a philosophical framework that history suggests will be most capable of motivating successful forms of research, given their practical context, they'll find a clear and well-manicured version of it, devoid of the kinds of internal incoherence that might otherwise trouble their thinking.

It would please me greatly if realist and anti-realist philosophers of science saw historical case studies such as the one detailed here as providing a pragmatic justification for continuing to develop their positions, despite not finding in these studies any reason to alter their own commitments. And in creating the possibility for something like the kind of dialogue about realism which Fine suggests would be more fruitful, which admits that "realism [and anti-realism] involves a profound leap of faith, not at all dissimilar from the faith that animates deep religious convictions" (ibid., p.116, n.4), I think realist and anti-realist philosophers might eventually find ways to engage each other in rational, mind-changing debate again too. For while historical studies of how philosophical commitments influence scientific practice won't provide philosophers with any pragmatic reasons to accept or reject realism or anti-realism, given that they aren't scientists, it remains entirely possible that philosophers might find pragmatic reasons that do apply to them as philosophers, educators, citizens, or one of their other identities, roles, or practical contexts. Fine is wrong in thinking that seeing realist and anti-realist commitments as partially based in leaps of faith requires that "realists [and anti-realists] finally stop pretending to a rational support for their faith, which they do not have" (ibid.). For pragmatic reasons for philosophical commitments are reasons, based in evidence. They may not apply to everyone, of course, and they may not be reasons to believe that a position is true, but that's just how practical reason operates outside of categorical modes. Goals and values factor in necessarily in hypothetical imperatives, and different people have different goals and values, holding true beliefs and opinions being but one goal amongst many. But accepting a philosophical position for pragmatic reasons is still about doing what rationality suggests is most warranted, not abandoning rationality altogether. In fact, the comparison that Fine makes between philosophical and religious commitments speaks to this very point.

As I noted at the outset of this work (Ch.1, sec.1), in comparing realists to theists Fine cites Pascal's claim that religious commitment generally arises from "reasons of the heart." These are reasons that, strictly speaking, are not reasons. Rather, they are impulses fueled by emotion. And yet, Pascal also gives an *argument* for committing oneself to theism. Framed as a wager, we would call this a decision-theoretic argument, though Pascal was writing before the formulation of a proper probability calculus. But the appeal to probability seems to necessarily require the engagement of our rational faculties. So, it would seem that in weighing out the potential benefits and losses of theistic commitment against those of atheism, Pascal is saying that theism is the rational choice, for pragmatic *reasons*. This suggests there are reasons for theism, and for scientific realism, that are reasons after all.

Pascal's conception of rationality, however, is different than the one analytic philosophers tend to find most intuitive, and tend to regularly appeal to in their arguments. The traditional conception of rationality is the one that Russell employs when he aims to find positions that are so certain that no rational person could doubt them (1912). In this sense, of course, we can provide no reasons for theism, nor for scientific realism, as there are no considerations which would compel every rational person to choose one position rather than an alternative. In my second chapter I outlined and adopted van Fraassen's meta-epistemologically voluntaristic framework for understanding the scientific realism debate as a debate between opposed epistemic stances, where stances are thought of not as factual claims *per se* but more immediately as epistemic policies for the generation of epistemologies and factual beliefs. On this account, we adopt epistemic stances because they cohere with our values, not because rational consideration of the evidence dictates that we do so. Rationality still factors into our choice, just not determinately so. The notion of rationality constraining our choice of stance is rather thin and permissive, such that one's choice of epistemic policies is deemed rational so long as it is not internally incoherent or self-defeating in light of their values. It should not be surprising that van Fraassen develops this conception of rationality as "bridled irrationality" in part through an investigation of Pascal's wager, focusing on "the conceptions of reason and rationality in this text" (2002, p.96).

Van Fraassen finds Pascal admitting that we find no reasons be a theist, but only according to the traditional conception of rationality that allows for no sensitivity to personal context. Pascal's argument only holds sway over those who see theism as a live option (ibid., p.99), so even if it is persuasive for some people it will not be for all people, and the traditional conception of rationality does not allow for this kind of subjectivity. Even for those who see theism as a live option, however, van Fraassen writes that Pascal's argument may not be rationally compelling, for "[e]xpected gain is not known gain" (ibid., p.100). He elaborates:

> The probabilistic reasoning presented as exactly the pattern of reasoning proper to the human situation is not, indeed is never, of the same sort as demonstrations that purport to provide rationally compelling grounds. Their relation to rationality is more subtle. First of all, they are relevant only in a context in which such personal factors are present as which options we accept as live options for us, and how we value things—not to mention how we personally assess the various probabilities and what we set as our personal risk quotient. Second they make sense only on a conception of what is rational or rationally endorsable that is entirely at odds with the traditional "compelled by reason" conception. (ibid., p.101)

So, if we accept this non-traditional understanding of rationality, as something that permits (or maybe suggests) rather than compels, and where reasons prove persuasive only if we make certain subjective judgements about probabilities and risk aversion, then pragmatic reasons to adopt philosophical positions can be considered real reasons (as I assumed for methodological reasons in the previous chapter). But for philosophers like van Fraassen or Chakravartty to ever find persuasive pragmatic reasons for changing their philosophical commitments, a few things will need to happen. First, they'll need to accept this conception of rationality, which differs from the one philosophers generally hold themselves accountable to these days, which requires rationality to be determinate for all rational agents. This condition is easily met in this case, as van Fraassen and Chakravartty both explicitly accept that this notion of rationality as bridled irrationality is all that constrains their choice of epistemic stance at the meta-epistemological level. Second, they'll need to see each other's position as live, in some sense, and given their continued engagement with each other's views, I suspect this condition is also met. Third, someone will need to find evidence that making certain philosophical commitments has practical consequences they would care about, evidence that specifically suggests that changing their commitments might better serve their personal values, given their practical context. This condition, so far as I can tell, has not been met, but I see no principled reason to think it can't be, someday, somehow. But even if this third condition is met, a shift in their philosophical commitments for pragmatic reasons would require a fourth and final condition be fulfilled by each of them individually: they would need to accept principles of risk management, and assign subjective probabilities to the claim that switching their commitments will have the beneficial outcomes, in a way that makes the likelihood of obtaining the potential benefits outweigh the likelihood of suffering the potential losses. In this way, personal choice will still factor into their philosophical commitments. To be sure, the same holds for those scientists who might use historical case studies to pragmatically inform their philosophical commitments through rational consideration of the evidence. In either case, as the Bayesian epistemologists maintain, human will an *a priori* judgement always factor into our epistemic life through our idiosyncratic strategies of risk management and the subjective assignment of prior probabilities. Nevertheless, unless we start assigning nulls and unities to prior probabilities in ways that would themselves be irrational (e.g. for claims other than tautologies and logical contradictions), we must admit the possibility that pragmatic reasons might be found to persuade a philosopher committed to the metaphysical stance (and thereby scientific realism) or the empirical stance (and thereby constructive empiricism) to change their mind.

And that, I think, is what philosophy aims to do: change people's minds, including our own. Perhaps that is not the universally accepted or true axiology of philosophy, and perhaps it is something that I accept as a matter of faith rather than evidence, with no reasons to ground it, pragmatic or otherwise. Nevertheless, that is an issue for a separate discussion. As it stands this is the meta-philosophical axiology that captures the unique value I have found and continue to find in philosophical modes of thought and discourse, and the value I fear would be lost by accepting the reality of too many apparent philosophical stalemates. So while I know that a full frontal assault, organized around the charge that anti-realism is irrational *per se*, will never break down the walls of the floating stronghold that has been built by a committed anti-realist like van Fraassen, and that the same holds *mutatis mutandis* for a committed realist like Chakravartty, it renews my faith in the power of philosophical inquiry to have shown that it might somehow be possible to coax them out of their encampment by providing pragmatic reasons that appeal to nothing other than reason, evidence, and each of their freely chosen values. While the probability of this happening may be small, it is not infinitesimal, and that alone should be enough to relieve the anxiety generated by admitting that the scientific realism debate has reached an intractable stalemate according to the traditional standards of philosophical debate. At the very least, it relieves my own.

Fine has looked at this stalemate and chosen to give up on the debate. I argue that we should give up the standards that give rise to the stalemate instead, and that shifting our focus towards pragmatic rather than epistemic reasons for our philosophical commitments is especially warranted within the meta-epistemologically voluntaristic framework that van Fraassen and Chakravartty use to understand the scientific realism debate (cf. Cartwright 2007). After all, once we start thinking of our commitments to scientific realism or anti-realist empiricism as resulting from the epistemic policies we adopt in order to serve our values, rather than as claims derived from the evidence, we will need to check whether the policies we adopt, in fact, serve their intended purposes. And those will, necessarily, be pragmatic standards of evaluation, for we want to know whether the policies get the job done, not whether they're "true," or rationally preferable *per se*. Indeed, it's not ever clear what it would mean for a policy to be true, or preferable absent any reference to practical aims. If scientific realism and anti-realist empiricism are best understood as epistemological institutions formed through the adoption of epistemic policies in order to serve our personal values, it would be irresponsible, potentially self-defeating by our own lights, to not empirically evaluate the performance of these policies and institutions. I submit that it is not as obvious as has generally been assumed which epistemic stance does, in fact, best serve which values. Perhaps it's obvious that the metaphysical stance is best for the curious, and the empirical stance for the cautious, but we are all guided by far more than one or the other of those values. If we want to serve all our values, evaluating the practical effectiveness of our epistemic policymaking will be required, both in anticipation of and subsequent to our choosing one set of policies over alternatives. Refusing to do so is the path to true dogmatism, epistemic policymaking that apes the social policymaking of ideologues who silence media, gut libraries, defund research, and ignore statistics. Even the rulers of flying castles are not infallible.

More generally, seeing pragmatic reasons to adopt a philosophical position as legitimate reasons opens up a vast wilderness of argumentative territory for analytic philosophers to explore. I conclude by partially mapping this territory's geography, proposing some strategies for navigating it, and otherwise speculating about how we might explore it in more detail by applying a pragmatic, existential approach in other ways, and to other issues. As we'll see, the appeal to pragmatic reasons for making philosophical commitments is not at all unprecedented, though it's often viewed as a kind of impropriety by many analytic philosophers, and are thus often offered alongside epistemic reasons. But I suspect that seeing pragmatic reasons for philosophical commitments as legitimate all on their own will permit a kind of argumentative wanderlust I find quite appealing. While I'm hopeful that others will too, I'm certain there will be many philosophers to whom such explorations of the pragmatics of philosophical commitment will not appeal, or even appear foolish. This is fine, of course, as there is plenty of important philosophical work to be done on more familiar ground, and in the sky. Indeed, if there's one thing I think this study has shown that no rational person could deny, it's that we shouldn't all be doing the same thing, or thinking the same way, whether we are scientists or philosophers. As it is in our political communities, so it is in our epistemic communities: diversity is our strength.

Conclusion: Philosophy and Volition

I've argued that, for those who accept the voluntarist analysis of the scientific realism debate, the best way to move past the apparent stalemate is to begin looking for pragmatic reasons to adopt one view over its rivals. It seems unlikely that we'll ever have a non-question begging argument that establishes scientific realism, constructive empiricism, or the epistemic stances through which such views are developed as the true or universally preferable position. And it seems likely to stay that way, no matter what new evidence comes in or what new arguments are formulated. Fine (1986a) may be right that continuing to argue about which view is correct is a waste of time and intellectual labour, and that the scientific realism debate is in that sense "dead." Nevertheless, it still seems possible to establish, on common ground, that one position is more useful than the others for someone operating in a specific context, motivated by certain values. I hope I've shown that it's worth investigating which view is most useful in this sense, and that the scientific realism debate is in that sense alive and well.

There are a variety of ways we might give people pragmatic reasons to adopt one philosophical position over another. In general, my approach looks to show that someone is more likely to achieve specific ends in a specific context if they adopt one position rather than another. Minimally, this means doing three things:

1) determine that several different people, operating in similar practical contexts and aiming for a similar end, hold different philosophical outlooks

2) determine whether and when people with one outlook tend to successfully achieve that end, in that context, relative to those with different outlooks

3) provide an account of how such success is the result of holding that outlook rather than an alternative, and test this account (i.e. show that this is not a mere correlation)[87]

[87] Having some expectation of what one might expect from the outset is advisable, so that any hypothesized mechanism is not simply an *ad hoc* just-so story. At the very least, one must be sure to have a plausible account of how certain commitments lead to certain forms of success, and to not commit the sin of statistic methodology known as p-hacking. This is especially important given that the ultimate aim is to help people make good choices.

I've chosen historical methods to do all three, but we might also use (individually or in conjunction) the methods of anthropology, cultural studies, sociology, psychology, experimental philosophy, economics, statistics, political science, or scientometrics. Here's an example of how that might go: experimental philosophers often see people's answers to trolley problems as implicit tests of their philosophical attitudes regarding the nature of morality. So, if philosophers of science could construct a suitable "trolley problem" to assess people's tacit realism or anti-realism, we could "detect" people's philosophical outlook on science (that's (1)). After testing a group of people, other disciplines could take over and see whether people with different test results tend to be successful according to various measures of success, e.g. publication rates, departmental reviews, teaching evaluations, salaries, promotions, number of patents, policy effectiveness, perceived standing amongst their peers or even (I suppose) their contributions to GDP, the size of their congregation, their number of followers on Twitter, or whatever else is of interest and we have some reason to conjecture might be influenced by people's philosophical views (that's (2)). Once an account of how holding the detected view helps people achieve the measured success has been provided, social and psychological experiments might further corroborate or disconfirm this hypothesis by working to vary or hold fixed different variables or parameters, testing the robustness of the association in different conditions (that's (3)). Another way to test for the robustness of an association would be to conduct steps (1) and (2) amongst people in a similar practical context but in other countries or cultures, showing that philosophical views promote certain kinds of success across demographics; this is similar to the approach I will take, in subsequent work, to testing the robustness of the associations between philosophical views and successful forms of scientific practice suggested by studying 19th century electrodynamists. There are many other ways to proceed through steps 1-3. I've focused on using historical methods because those are the ones I'm familiar with, but there's no reason to see a pragmatic, existentialist approach to the scientific realism debate as fundamentally limited by my competencies. Pragmatic reasons, like epistemic reasons, come in a variety of forms, and can be produced by a variety of methods.

While I've focused on gathering evidence that might provide working scientists with pragmatic reasons to adopt one position rather than another, we might also try to assist with other people's decision making. Science educators, science writers, science policymakers, and many other groups with a professional interest in the interpretation of science could also benefit from knowing whether a metaphysical or empiricist outlook is most likely to help them achieve their ends. To be sure, several authors have proffered pragmatic arguments for accepting either realism or anti-realism in certain contexts to help achieve role-specific ends; however, these arguments are generally based on abstract analysis, not empirical research. For example, Park (2016) argues that scientific realism is more appropriate than anti-realist empiricism in science education; Godfrey and Hill (1995) argue that scientific realism is preferable to positivism in strategic management research; and Roy Bhaskar made a career out of arguing for the preferability of his own form of scientific realism in emancipation-oriented sociology (e.g. 1975, 1987). Yet none of them looked to conduct empirical studies to corroborate what their analyses predicted: that scientific realists tend to be more successful in certain practical contexts than anti-realists, making realism rationally preferable for people in those contexts, for pragmatic reasons. I would argue that, in such down-to-earth contexts, we should prefer down-to-earth arguments: if we want to know which philosophy of science is best for people like scientists, science writers, or science educators, we should study them, not analyze our ideals of them.

Abstract analysis would probably be most appropriate if we aimed to give pragmatic grounding to the commitments of philosophers themselves. For some analytic philosophers, there's a sense of impropriety to a philosopher defending a position as anything but true or rationally preferable, but pragmatic arguments for adopting a position are by no means without precedent in this field. He is often read as defending the truth of his view because of its explanatory power (e.g. Day and Kincaid 1994, p.273), but I have always read David Lewis's defence of the reality of possible worlds as blatantly pragmatic. Believing in the physical reality of every logically possible world is metaphysical inflation of unparalleled proportions, but so many philosophical problems can be solved with this assumption that's it's hard to resist: the nature of identity, necessity, causality, knowledge, reference, etc. If an assumption can let you solve almost any major problem in metaphysics, epistemology, or the philosophy of language before lunch, in a coherent and non-trivial way, there's a pragmatic reason to make that assumption, even if you know you can never discharge it without losing everything you've gained. As Lewis himself put it: "the benefits in theoretical unity and economy are well worth the entities" (1986, p.4). Much more explicitly, James Woodward develops and defends his 'interventionist' conception of causality because of its usefulness rather than its metaphysical plausibility, its capturing common usage, or its fit with physical theory (2014, p.694). He argues that causes need to be distinguished from correlations, for instance, because this serves certain goals, not because it reveals the truth about the nature of causality (ibid., p.696). In defending his view pragmatically, Woodward also defends the pragmatic standard itself, calling usefulnesss 'the only standard that matters.' Van Fraassen (1980) himself, in some sense, originally offered the aspirant empiricist a pragmatic justification to accept constructive empiricism: given the demise of logical positivism, if your goal is to be an intellectually satisfied empiricist, here's a way to do that (cf. Alspector-Kelly 2001). And while I've only suggested that we should look for pragmatic reasons for our beliefs and philosophical positions when epistemic reasons do not seem forthcoming, some go even further. Sarah Stroud (2006), for instance, defends the view that one of the partial duties we have towards our close friends is to always see them as good people, even in the face of counterevidence. If that's true, virtue or moral duty may sometimes trump rationality and evidence, and the virtuous agent may sometimes prioritize non-epistemic reasons for belief over epistemic ones. Janet Kourany (2016) goes even further, arguing that sometimes pragmatic or political considerations should lead us to forbid certain kinds of epistemic projects, i.e. that we can have non-epistemic reasons to not even seek the truth on certain matters. Either way, if we pay attention we can see that analytic philosophers sometimes defend beliefs or positions because of their usefulness rather than, in addition to, or even contrary to their truth or rational preferability *per se*.

Nevertheless, not all philosophers will accept pragmatic reasons to adopt a philosophical position as legitimate, and not all scientific realists and anti-realists are voluntarists. Many realists would flatly deny that constructive empiricism is a workable philosophy of science, would claim that no anti-realist empiricist philosophy of science is truly workable, and would argue that adopting the empirical stance is not rationally permitted given the evidence. No doubt there are anti-realists who view realism and metaphysical speculation the same way, not just from their own perspective, they think, but from the perspective of any rational person who properly considers the evidence and relevant arguments. Nevertheless, by allowing us to focus on the empirical question of whether adopting some position is, in fact, most likely to help someone achieve their ends, I think the voluntarist understanding of the scientific realism debate presents greater prospects for continuing the realism debate in fruitful ways than a more traditional approach, which seems to me to have reached an impasse even outside of a voluntarist framework.

I see the search for pragmatic reasons to adopt realism or anti-realism as promising because it presents a relatively clear way for philosophers, in the face of a stalemated debate, to still use argument and evidence to change people's minds, including their own. The value of philosophy for me has always been less about its ambitious efforts to find the truth about life's biggest questions and more about its commitment to keeping an open mind and using rational debate rather than things like rhetoric, cajolery, wheedling, seduction, bribery, or violence in any attempt to change someone's mind. Working to provide pragmatic rather than epistemic reasons to adopt a philosophical position may involve calling off the search for whatever important truth that position is supposed to be an answer to, but it won't require giving up what I see as the philosopher's fundamental conviction: that all we really need to change people's minds is a good argument. And as important as the big questions of philosophy are, determining the likely practical consequences of adopting different positions on such matters can help us each answer questions that are arguably more important: questions about how to effectively achieve our ends, live out our values, and be the kind of person we want to be.

Works Cited

Achinstein, P. (2002). "Is There a Valid Experimental Argument for Scientific Realism?" *The Journal of Philosophy*.

Alspector-Kelly, M. (2001). "Should the Empiricist be a Constructive Empiricist?" *Philosophy of Science*.

Alspector-Kelly, M. (2012). "Constructive Empiricism Revisited." *Metascience*.

Ampère, A. (1822). *Recueil d'Observations Electro-Dynamiques.* Paris.

Arabatzis, T. (2006). *Representing Electrons: A Biographical Approach to Theoretical Entities*. Chicago: Chicago University Press.

Aronson, J., R. Harré, and E. Way. (1995). *Realism Rescued: How Scientific Progress is Possible*. Open Court Publishing.

Assis, A., K. Wolfschmidt, and G. Wiederkehr. (2011). *Weber's Planetary Model of the Atom*. Hamburg: Tredition Science.

Baird, D. et al. (1998). *Heinrich Hertz: Classical Physicist, Modern Philosopher*. Dordrecht: Kluwer Academic Publishers.

Baumann, P. (2011). "Empiricism, Stances, and the Problem of Voluntarism." *Synthese*.

Ben-Menahem, Y. (1990). "The Inference to the Best Explanation." *Erkenntnis*.

Bhaskar, R. (1975). *A Realist Theory of Science*. London: Verso.

Bhaskar, R. (1987). *Scientific Realism and Human Emancipation*. London: Verso.

Bourgeois, W. (1987). "On Rejecting Foss's Image of Van Fraassen." *Philosophy of Science*.

Bourget, D. and D. Chalmers. (2014). "What Do Philosophers Believe?" *Philosophical Studies*.

Bourguet, M., C. Licoppe and H. Sibum (eds.). (2002). *Instruments, Travel and Science: Itineraries of Precision from the Seventeenth to the Twentieth Century*. London: Routledge.

Bowler, P. and I. Morus. (2005). *Making Modern Science: A Historical Survey*. Chicago: University of Chicago Press.

Boyd, R. (1981). "Scientific Realism and Naturalistic Epistemology." In P. Asquith and R. Giere (eds.), *PSA 1980, Vol.2.* East Lansing, MI: Philosophy of Science Association.

Boyd, R. (1984). "The Current Status of Scientific Realism." In J. Leplin (ed.), *Scientific Realism*. Berkeley: University of California Press.

Bridges, J. (2009). "Rationality, Normativity, and Transparency." *Mind*.

Bucci, O. (2006). "The Genesis of Maxwell's Equations." In T. Sarkar, et al. (eds.), *History of Wireless.* New Jersey: John Wiley & Sons.

Buchwald, J. (1980). "Optics and the Theory of the Punctiform Ether." *Archive for History of Exact Sciences*.

Buchwald, J. (1993). "Electrodynamics in Context: Object States, Laboratory Practice, and Anti-Romanticism." In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Buchwald, J. (1994). *The Creation of Scientific Effects*. Chicago: University of Chicago Press.

Buchwald, J. and A. Warwick. (eds.) (2001). *Histories of the Electron: The Birth of Microphysics*. Cambridge: MIT Press.

Cahan, D. (ed.) (1993). *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Cantor, G. (1975). "The Reception of the Wave Theory of Light in Britain: A Case Study Illustrating the Role of Methodology in Scientific Debate." *Historical Studies in the Physical Sciences*.

Cartwright, N. (1983). *How the Laws of Physics Lie*. Oxford: Oxford University Press.

Cartwright, N. (2007). "Why be Hanged for even a Lamb?" In B. Monton (ed.), *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas van Fraassen*. Oxford: Oxford University Press.

Chakravartty, A. (2004). "Stance Relativism: Empiricism versus Metaphysics." *Studies in History and Philosophy of Science*.

Chakravartty, A. (2007a). *A Metaphysics for Scientific Realism*. Cambridge: Cambridge University Press.

Chakravartty, A. (2007b). "Six Degrees of speculation: Metaphysics in empirical contexts." In B. Monton (ed.), *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas van Fraassen*. Oxford: Oxford University Press.

Chakravartty, A. (2011a). "A Puzzle about Voluntarism about Rational Epistemic Stances." *Synthese*.

Chakravartty, A. (2011b). "Scientific Realism", *The Stanford Encyclopedia of Philosophy,* (Winter 2016 Edition), E. Zalta (ed.), URL = <https://plato.stanford.edu/archives/win2016/entries/scientific-realism/>.

Chakravartty, A. and B. van Fraassen (forthcoming). "What is Scientific Realism?" *Spontaneous Generations: A Journal for the History and Philosophy of Science.*

Chalmers, D. (2015). "Why isn't there more Progress in Philosophy?" *Philosophy*.

Chesterton, G. (1905). *Heretics*. Chicago: John Lane Company.

Chignell, A. (2017). "The Ethics of Belief", *The Stanford Encyclopedia of Philosophy*, (Spring 2017 Edition), E. Zalta (ed.), URL =<https://plato.stanford.edu/archives/spr2017/entries/ethics-belief/>.

Churchland, P. (1985). "The Ontological Status of Observables: In Praise of the Superempirical Virtues." In P. Churchland and C. Hooker (eds.), *Images of Science: Essays on Realism and Empiricism*, *(with a Reply from Bas C. van Fraassen)*. Chicago: University of Chicago Press.

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

Clendinnen, F. (1989). "Realism and the Underdetermination of Theory", *Synthese*.

Code, L. (1991). *What Can She Know? Feminist Theory and the Construction of Knowledge*. New York: Cornell University Press.

Cohen, L. (1989). "Belief and Acceptance." *Mind*.

Cohen, L. (1992). *An Essay on Belief and Acceptance*. Oxford: Oxford University Press.

Collins, H. and T. Pinch (1998). *The Golem at Large: What you Should Know about Technology*. Cambridge: Cambridge University Press.

Cooper, N. (1964). "The Aims of Science." *The Philosophical Quarterly*.

Copernicus, N. (1543). *De Revolutionibus Orbium Coelestium*.

D'Agostino, S. (2000). *A History of the Ideas of Theoretical Physics: Essays on the Nineteenth and Twentieth Century Physics*. Springer Science & Business Media

Dancy, J. (2000). *Practical Reality*. Oxford: Clarendon Press.

Darrigol, O. (2000). *Electrodynamics from Ampere to Einstein*. Oxford: Oxford University Press.

Darwin, C. (1859). *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*.

Day, T. and H. Kincaid (1994). "Putting Inference to the Best Explanation in its Place." *Synthese*.

Dennett, D. (1987). *The Intentional Stance*. Cambridge: MIT Press

Descartes, R. (1637). *Discourse on the Method of Rightly Conducting One's Reason and of Seeking Truth in the Sciences*.

Descartes, R. (1641). *Meditations on First Philosophy: in which the existence of God and the immortality of the soul are demonstrated*.

DiSalle, R. (1993). "Helmholtz's Empiricist Philosophy of Mathematics: Between Laws of Perception and Laws of Nature." In D. Cahan (ed.) *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Douven, I. (2000). "The Anti-Realist Argument for Underdetermination." *The Philosophical Quarterly*.

Duhem, P. (1914/1954). *The Aim and Structure of Physical Theory*. Princeton: Princeton University Press.

Dummett, M. (1978). *Truth and Other Enigmas*. Cambridge, Cambridge University Press.

Earman, J. (1993). "Underdetermination, Realism, and Reason." *Midwest Studies inPphilosophy*.

Einstein, A. (1949). "Remarks Concerning the Essays Brought together in this Co-operative Volume." In Schilpp, P. A. (ed.) *Albert Einstein: Philosopher-Scientist*. Evanston: The Library of Living Philosophers.

Einstein, A., B. Podolsky, and N. Rosen (1935). "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?" *Physical Review*.

Faraday, M. (1822). "On New Electro-Magnetical Motions, and on the Theory of Magnetism." *The Quarterly Journal of Science, Literature, and Art*.

Feyerabend, P. (1964). "Realism and instrumentalism: comments on the logic of factual support." In M. Bunge (ed.), *The Critical Approach to Science and Philosophy.* New York: Free Press.

Feynman, R. (1955). "The Value of Science." In R. Feynman. (1988), *What Do You Care What Other People Think*. New York: Bantam Books.

Feynman, R., R. Leighton, and M. Sands (1965). *The Feynman Lectures on Physics: Vol. 2: Mainly Electromagnetism and Matter*. Addison-Wesley.

Fine, A. (1986a). *The Shaky Game: Einstein, Realism, and the Quantum Theory*. Chicago: University of Chicago Press.

Fine, A. (1986b). "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science." *Mind*.

Fine, A. (forthcoming). "Motives for Research." *Spontaneous Generations: A Journal for the History and Philosophy of Science*.

Fitzpatrick S. (2013) "Doing Away with the No Miracles Argument." In V. Karakostas and D. Dieks (eds.), *EPSA11: Perspectives and Foundational Problems in Philosophy of Science. The European Philosophy of Science Association Proceedings*.

Foley, R. (1987). *The Theory of Epistemic Rationality*. Cambridge: Harvard University Press.

Forbes, C. (2016). "A Pragmatic, Existentialist Approach to the Scientific Realism Debate." *Synthese*.

Forbes, G. (1916). *Memories of Sir David Gill, K.C.B, H.M. Astronomer (1879-1907) at the Cape of Good Hope*. Ann Arbor: University of Michigan Press.

Foss, J. (1984). "On Accepting van Fraassen's View of Science." *Philosophy of Science*.

Foss, J. (1991). "On Saving the Phenomena and the Mice: A Reply to Bourgeois Concerning van Fraassen's Image of Science." *Philosophy of Science*.

Franklin, A. (1986). *The Neglect of Experiment*. Cambridge: Cambridge University Press.

French, S. and J. Ladyman (2011). "In Defence of Ontic Structural Realism." In A. Bokulich and P. Bokulich (eds.), *Scientific Structuralism*. Springer Netherlands.

Frondizi, R. (1971). "What is Value?" *Philosophy and Phenomenological Research*.

Ganson, D. (2001). *The Explanationist Defense of Scientific Realism*. Taylor & Francis

Gauss, C. (1836). "Erdmagnetismus und Magnetometer." in C. Gauss, *Werke* (1973).

Gauss, C. (1837). "Uber ein neues, zunachst zur unmittelbaren Beobachtung der Veranderungen in der Intensitat des horizontalen Theils des Erdmagnetismus bestimmtes Instrument." in *Resultate aus den Beohachtungen des magnetischen Vereins*.

Gauss, C. (1838). "Allgemeine Theorie des Erdmagnetismus," in *Resultate aus den Beohachtungen des magnetischen Vereins.*

Giere, R. (2006). *Scientific Perspectivism*. Chicago: University of Chicago Press.

Godfrey, P. and C. Hill (1995). "The Problem of Unobservables in Strategic Management Research." *Strategic Management Journal*.

Gonzalez, W. (ed.) (2014). *Bas van Fraassen's Approach to Representation and Models in Science*. Springer Netherlands.

Gooday, G. (2001). "The Questionable Matter of Electricity: The Reception of J. J. Thomson's 'Corpuscle' among Electrical Theorists and Technologists." In J. Buchwald and A. Warwick, *Histories of the Electron: The Birth of Microphysics*. Cambridge: MIT Press.

Greco, J. (2007). *Putting Skeptics in their Place: The Nature of Skeptical Arguments and their Role in Philosophical Inquiry*. Cambridge University Press.

Green, G. (1838). "On the Laws of the Reflexion and Refraction of Light at the Common Surface of Two Non-Crystallized Media," *Transactions of the Cambridge Philosophical Society*; reprinted in N. Ferrers (ed.), *Mathematical Papers*, New York: Chelsea Publishing Company

Hacking, I. (1983) *Representing and Intervening*. Cambridge: Cambridge University Press

Hacking, I. (1999). *The Social Construction of What?* Cambridge: Harvard university press.

Hanh H., O. Neurath, and R. Carnap (1929). *Wissenschaftliche Weltauffassung: Der Wiener Kreis*. Vienna: Arthur Wolf Verlag

Harker, D. (2013). “How to Split a Theory: Scientific Realism and a Defence of Convergence without Proximity.” *British Journal for the Philosophy of Science*.

Harman, G. (1965). “The Inference to the Best Explanation.” *Philosophical Review*.

Hausman, D. (1982). “Constructive Empiricism Contested.” *Pacific Philosophical Quarterly*.

Heidelberger, M. (1993). “Force, Law, and Experiment: The Evolution of Helmholtz’s Philosophy of Science.” In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Helmholtz, H. (1843). “On the Nature of Fermentation and Putrefaction.” *Müller’s Archiv.*

Helmholtz, H. (1845). “On Metabolism during Muscular Activity.” *Müller’s Archiv*.

Helmholtz, H. (1847). “The Conservation of Force: A Physical Memoir.” In R. Kahl. (ed.), *Selected Writings, 1821-1894*. Middletown: Wesleyan University Press.

Helmholtz, H. (1854). “On the Interaction of Natural Forces.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1856, 1862, and 1867). *Handbuch der physiologischen Optik*. Leipzig: Leopold Voss.

Helmholtz, H. (1857). “The Physiological Causes of Harmony in Music.” In R. Kahl. (ed.), *Selected Writings, 1821-1894*. Middletown: Wesleyan University Press.

Helmholtz, H. (1861). “The Application of the Law of the Conservation of Force to Organic Nature.” In R. Kahl. (ed.), *Selected Writings, 1821-1894*. Middletown: Wesleyan University Press.

Helmholtz, H. (1862-63). “On the Conservation of Force.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1862). “The Relation of the Natural Sciences to Science in General.” In R. Kahl. (ed.), *Selected Writings, 1821-1894*. Middletown: Wesleyan University Press.

Helmholtz, H. (1863). *On the Sensations of Tone as a Physiological Basis for the Theory of Music*. Braunschweig: Verlag von F. Vieweg & Sohn.

Helmholtz, H. (1868). “The Recent Progress of the Theory of Vision.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1869). “On the Aim and Progress of Physical Science.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1870). “On the Origin and Significance of Geometrical Axioms.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1871). “Gustav Magnus in Memorandum: Address to the Leibniz Meeting of the Academy of Sciences.” In H. Helmholtz (ed.) (1881a), *Popular Lectures on Scientific Subjects*, E. Atkinson (trans.). London: Longmans, Green, and Co.

Helmholtz, H. (1874). “Induction and Deduction.” In H. Helmholtz (1884): *Vorträge und Reden*. Braunschweig

Helmholtz, H. (1877). “On Thought in Medicine.” In H. Helmholtz (ed.) (1881a), *Popular Lectures on Scientific Subjects*, E. Atkinson (trans.). London: Longmans, Green, and Co.

Helmholtz, H. (1878). “The Facts of Perception.” In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (ed.) (1881a). *Popular Lectures on Scientific Subjects*, E. Atkinson (trans.). London: Longmans, Green, and Co.

Helmholtz, H. (1881b). "The Modern Development of Faraday's Conception of Electricity." In R. Kahl. (ed.), *Selected Writings, 1821-1894*. Middletown: Wesleyan University Press.

Helmholtz, H. (1884). *Vorträge und Reden*. Braunschweig

Helmholtz, H. (1891). "Herman von Helmholtz: An Autobiographical Sketch." In D. Cahan (ed.) (1995), *Science and Culture: Popular and Philosophical Essays*. Chicago: University of Chicago Press.

Helmholtz, H. (1921) *Hermann von Helmholtz. Epistemological Writings. The Paul Hertz/Moritz Schlick Centenary Edition of 1921*, M. Lowe (trans.). R. Cohen and Y. Elkana (eds.), (1977). Dordrecht: D. Reidel Publishing Company.

Helmholtz, H. (1971). *Selected Writings, 1821-1894*. Kahl, R. (ed). Middletown: Wesleyan University Press.

Helmholtz, H. (1995). *Science and Culture: Popular and Philosophical Essays*. Cahan, D (ed.). Chicago: University of Chicago Press.

Hendry, R. (1995). "Realism and Progress: Why Scientists Should be Realists." *Royal Institute of Philosophy Supplements*.

Hendry, R. (2001). "Are Realism and Instrumentalism Methodologically Indifferent?" *Proceedings of the Philosophy of Science Association*.

Hertz, H. (1893). *Electric Waves: Being Researches on the Propagation of Electric Action with Finite Velocity through Space*, D. Jones (trans.). London: MacMillan and Co.

Hertz, P. and M. Schlick (1921). "Foreword" to Helmholtz, H. (1921) *Hermann von Helmholtz. Epistemological Writings. The Paul Hertz/Moritz Schlick Centenary Edition of 1921*, M. Lowe (trans.). R. Cohen and Y. Elkana (eds.) (1977). Dordrecht: D. Reidel Publishing Company.

Hesse, M. (1966). *Models and Analogies in Science*. Notre Dame: University of Notre Dame Press.

Hooker, C. (1985). "Surface Dazzle, Ghostly Depths: An Exposition and Critical Evaluation of van Fraassen's Vindication of Empiricism against Realism." In P. Churchland and C. Hooker (eds.), *Images of Science: Essays on Realism and Empiricism*, *(with a reply from Bas C. van Fraassen)*.

Hsieh, Nien-hê (2016). "Incommensurable Values." *The Stanford Encyclopedia of Philosophy* (Spring 2016 Edition), E. Zalta (ed.), URL = <https://plato.stanford.edu/archives/spr2016/entries/value-incommensurable/>

Hume, D. (1738/2000). *A Treatise of Human Nature*. D. Norton and M. Norton (ed.). New York: Oxford University Press.

Hunt, B. (1991). *The Maxwellians*. Cornell University Press.

Hyder, D. J. (1999). "Helmholtz's Naturalized Conception of Geometry and his Spatial Theory of Signs." *Philosophy of Science*.

James, W. (1896). *The Will to Believe: And Other Essays in Popular Philosophy*. Longmans, Green, and Company.

James, W. (1907/1979) *Pragmatism*. Cambridge: Harvard University Press.

Jungnickel C. and R. McCormmach (1986). *Intellectual Mastery of Nature.* Chicago: University of Chicago Press.

Kaiser, W. (2001). "Electron Gas Theory of Metals: Free Electrons in Bulk Matter," in J. Buchwald and A. Warwick (eds.). *Histories of the Electron: The Birth of Microphysics*. Cambridge: MIT Press.

Khalifa, K. (2010). "Social Constructivism and the Aims of Science." *Social Epistemology*.

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

Kolodny, N. (2005). "Why Be Rational?" *Mind*.

Königsberger, L. (1906). *Hermann Von Helmholtz*. Clarendon Press.

Korsgaard, C. (2009). *Self-Constitution: Agency, Identity, and Integrity*. Oxford: Oxford University Press.

Kourany, J. A. (2016). "Should Some Knowledge be Forbidden? The Case of Cognitive Differences Research." *Philosophy of Science*.

Krauss, L. (2012). *A Universe from Nothing: Why There is Something rather than Nothing*. Simon and Schuster.

Kukla, A. (1994). "Scientific Realism, Scientific Practice, and the Natural Ontological Attitude." *British Journal for the Philosophy of Science*.

Kukla, A. (1995). "The Two Antirealisms on Bas van Fraassen." *Studies in the History and Philosophy of Science*.

Kukla, A. (1996). "Antirealist explanations of the success of science." *Philosophy of Science*.

Kukla, A. (1998). *Studies in Scientific Realism*. New York: Oxford University Press.

Kukla, A. (2000). *Social Constructivism and the Philosophy of Science*. Psychology Press.

Kukla, R. (forthcoming). "Embodied Stances: Realism without Literalism."

Ladyman, J. (1998). "What is Structural Realism?" *Studies in History and Philosophy of Science*.

Ladyman, J., O. Bueno, M. Suárez, and B. van Fraassen (2011). "Scientific Representation: A Long Journey from Pragmatics to Pragmatics." *Metascience*.

Latour, B. and S. Woolgar (1979). *Laboratory Life: The Construction of Scientific Facts*. Beverly Hills: Sage Publications.

Laudan, L. (1981). "A Confutation of Convergent Realism." *Philosophy of Science*.

Laudan, L. (1984). "Explaining the Success of Science: Beyond Epistemic Realism and Relativism." In J. Cushing, C. Delaney, and G. Gutting (eds.), *Science and Reality*. Notre Dame: University of Notre Dame Press.

Laudan, L. (1990). "Demystifying Underdetermination." In C. Savage (ed.), *Scientific Theories*. Minneapolis: University of Minnesota Press.

Laudan, L. and Leplin, J. (1991). "Empirical Equivalence and Underdetermination." *Journal of Philosophy*.

Legault, G., J. Patenaude, J. Béland, M. Parent (2013). "Nanotechnologies and Ethical Argumentation: A Philosophical Stalemate?" *Open Journal of Philosophy*.

Lenoir, T. (1993). "The Eye as Mathematician: Clinical Practice, Instrumentation, and Helmholtz's Construction of an Empiricist Theory of Vision." In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Leplin, J. (1986). "Methodological Realism and Scientific Rationality." *Philosophy of Science*.

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

Lewis, D. (1986). *On the Plurality of Worlds*. Oxford: Blackwell Publishers.

Lipton, P. (1991). *Inference to the Best Explanation*. London: Routledge.

Lipton, P. (2004). “Epistemic Options.” *Philosophical Studies*.

Longino, H. E. (2001). *The Fate of Knowledge*. Princeton University Press.

Lyons, T. (2002). “Scientific Realism and the Pessimistic Meta-Modus Tollens.” In *Recent Themes in the Philosophy of Science*. Springer Netherlands.

Lyons, T. (2003). “Explaining the Success of a Scientific Theory.” *Philosophy of Science*.

Lyons, T. (2005). “Toward a Purely Axiological Scientific Realism.” *Erkenntnis*.

Lyons, T. (2009). “Non-Competitor Conditions in the Scientific Realism Debate.” *International Studies in the Philosophy of Science*.

Lyons, T. (2012). “Axiological Scientific Realism and Methodological Prescription.” In *EPSA Philosophy of Science: Amsterdam 2009*. Springer Netherlands.

Lyons, T. (2013). “A Historically Informed Modus Ponens Against Scientific Realism: Articulation, Critique, and Restoration.” *International Studies in the Philosophy of Science*.

Magnus, P. and C. Callender. (2004). “Realist Ennui and the Base Rate Fallacy.” *Philosophy of Science*.

Mason, E. (2015). “Value Pluralism.” *The Stanford Encyclopedia of Philosophy* (Summer 2015 Edition), E. Zalta (ed.), URL = <https://plato.stanford.edu/archives/sum2015/entries/value-pluralism/>.

Maxwell, J. (1856). “On Faraday’s Lines of Force.” *Transactions of the Cambridge Philosophical Society*.

Maxwell, J. (1861). “On Physical Lines of Force.” *Philosophical Magazine*, Vol. 21 & 23.

Maxwell, J. (1873). *A Treatise on Electricity and Magnetism*. Oxford: Clarendon.

McArthur, D. (2006). “The Anti-Philosophical Stance, the Realism Question and Scientific Practice.” *Foundations of Science*.

McCullagh, J. (1839). “An Essay Towards a Dynamical Theory of Crystalline Reflexion and Refraction.” *Transactions of the Royal Irish Academy*.

McMullin, E. (1984). “A Case for Scientific Realism.” In Leplin (ed.), *Scientific Realism*. Berkley: University of California Press.

McMullin, E. (1985). “Galilean Idealization.” *Studies in History and Philosophy of Science Part A*.

Meulders, M. (2010). *Helmholtz: From Enlightenment to Neuroscience*. Cambridge: MIT Press.

Meyering, T. (1989). *Historical Roots of Cognitive Science: The Rise of a Cognitive Theory of Perception from Antiquity to the Nineteenth Century*. Dordrecht: Kluwer

Miller, D. (1974). “Popper's Qualitative Theory of Verisimilitude.” *British Journal for the Philosophy of Science*.

Miller, D. (2006). *Out of Error: Further Essays on Critical Rationalism*. Ashgate Publishing, Ltd.

Mizrahi, M. (2012). “Why the Ultimate Argument for Scientific Realism Ultimately Fails.” *Studies in History and Philosophy of Science Part A*.

Mlodinow, L. and S. Hawking. (2010). *The Grand Design*. Random House.

Monton, B. (2007). *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas C. van Fraassen*. Oxford: Oxford University Press.

Monton, B. and B. van Fraassen. (2003) “Constructive Empiricism and Modal Nominalism.” *British Journal for the Philosophy of Science*.

Moon, R. (1849). *Fresnel and His Followers: A Criticism: to which are Appended Outlines of Theories of Diffraction and Transversal Vibration*. Macmillan, Barclay, and Macmillan.

Morrison, M. (1992). "A Study in Theory Unification: The Case of Maxwell's Electromagnetic Theory." *Studies in History and Philosophy of Science Part A*.

Moulines, C. (1981). "Hermann von Helmholtz: A Physiological Approach to the Theory of Knowledge." In H. Jahnke and M. Otte (eds.), *Epistemological and Social Problems of the Sciences in the Early Nineteenth Century*. Boston: D. Reidel.

Moutafakis, N. (2007). *Rescher on Rationality, Values, and Social Responsibility*. Frankfurt: Verlag.

Müller, J. (1837). *Handbuch der Physiologie des Menschen für Vorlesungen*. Coblenz: Verlag von J. Hölscher.

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

Musgrave, A. (1988). "The Ultimate Argument for Scientific Realism." In R. Nola (ed.), *Relativism and Realism in Science*. Dordrecht: Kluwer Academic Publishers.

Musgrave, A. (2004). "Popper and Hypothetico-Deductivism." In D. Gabbay, J. Woods, and A. Kanamori (eds.). *Handbook of the History of Logic*. Elsevier

Niiniluoto, I. (1987). *Truthlikeness*. Dordrecht: Reidel.

Niiniluoto, I. (1998). "Verisimilitude: The Third Period." *British Journal for the Philosophy of Science*.

Niiniluoto, I. (1999). *Critical Scientific Realism*. Oxford: Oxford University Press.

Niznik, J. and J. Sanders (1996). *Debating the State of Philosophy: Habermas, Rorty, and Kolakowski*. Praeger.

Okasha, S. (2000). "Van Fraassen's Critique of Inference to the Best Explanation." *Studies in History and Philosophy of Science Part A*.

Olesko, K. and F. Holmes (1993). "Experiment, Quantification, and Discovery: Helmholtz's Early Physiological Researches, 1843-50." In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Park, S. (2016). "Scientific Realism versus Antirealism in Science Education." *Santalka: Filosofija, Komunikacija*.

Peirce, C. (1878). "How to Make our Ideas Clear." *Popular Science Monthly*.

Peters, D. (2012). *How to be a Scientific Realist (if at all): A Study of Partial Realism.* PhD thesis, The London School of Economics and Political Science.

Pew-MacArthur Results First Initiative. (2014). *Evidence-Based Policymaking. A Guide for Effective Government*. Philadelphia, PA: The Pew Charitable Trusts.

Poincaré, H. (1900). "Sur les Rapports de la Physique Expérimentale et de la Physique Mathématique." In *Rapports Présentés au Congrès International de Physique*. Paris: Gauthier-Villars.

Popper, K. (1959/1934). *The Logic of Scientific Discovery*. London: Hutchinson.

Popper, K. (1963). *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge.

Popper, K. (1971). "Conjectural Knowledge: My Solution of the Problem of Induction." *Revue internationale de Philosophie*.

Popper, K. (1983). *Realism and the Aim of Science*. W.W. Bartley III (ed.). London: Hutchinson.

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

Psillos, S. (2000a). "Agnostic Empiricism versus Scientific Realism: Belief in Truth Matters." *International Studies in the Philosophy of Science*.

Psillos, S. (2000b). "The Present State of the Scientific Realism Debate." *British Journal for the Philosophy of Science*.

Psillos, S. (2007). "Putting a Bridle on Irrationality: An Appraisal of van Fraassen's New Epistemology." In B. Monton (ed,), *Images of Empiricism: Essays on Science and Stances, with a reply from Bas van Fraassen*. Oxford: Oxford University Press.

Putnam, H. (1975a). *Mathematics, Matter and Method (Philosophical Papers, Vol. I).* London: Cambridge University Press

Putnam, H. (1975b). "The Meaning of 'Meaning.'" In K. Gunderson (ed.) *Language, Mind, and Knowledge*. Minneapolis: University of Minnesota Press.

Ratcliffe, M. (2011). "Stance, Feeling and Phenomenology." *Synthese*.

Raz, J. (2000). *Engaging Reason: On the Theory of Value and Action*. Oxford: Oxford University Press.

Raz, J. (2009). "Reasons: Practical and Adaptive." In D. Sobel and S. Wall (eds.), *Reasons for Action*. Cambridge: Cambridge University Press

Reid, T. (1852). *Essays on the Intellectual Powers of Man*. Cambridge.

Reisner, A. (2009). "The Possibility of Pragmatic Reasons for Belief and the Wrong Kind of Reasons Problem." *Philosophical Studies*.

Reisner, A. (2017). "Pragmatic Reasons for Belief." In Daniel Star (ed.), *The Oxford Handbook of Reasons and Normativity*. New York: Oxford University Press.

Reisner, A., (2008). "Weighing Pragmatic and Evidential Reasons for Belief." *Philosophical Studies*.

Rescher, N. (2014). *Metaphilosophy: Philosophy in Philosophical Perspective*. Chicago: Lexington Books.

Richards, J. (1977). "The Evolution of Empiricism: Hermann von Helmholtz and the Foundations of Geometry." *The British Journal for the Philosophy of Science*.

Rorty, R. (1981). *Philosophy and the Mirror of Nature*. Princeton: Princeton University Press

Rorty, R. (1999). "Phony Science Wars." *The Atlantic Monthly*.

Rosen, G. (1994). "What is Constructive Empiricism?" *Philosophical Studies*.

Rouse, J. (2002a). "Vampires: Social Constructivism, Realism and Other Philosophical Undead." *History and Theory*.

Rouse, J. (2002b). *How Scientific Practices Matter; Reclaiming Philosophical Naturalism*. Chicago: University of Chicago Press.

Russell, B. (1912). *The Problems of Philosophy*. Home University Library.

Ruttkamp-Bloem, E. (2013). "Re-Enchanting Realism in Debate with Kyle Stanford." *Journal for general philosophy of science*.

Saatsi, J. (2015). "Replacing Recipe Realism." *Synthese*.

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

Sankey, H. (2001). "Scientific Realism: An Elaboration and a Defence." *Theoria*.

Sarkar T. and M. Salazar-Palma, D. Sengupta (2006a). "Introduction" in Sarkar, T. et al. (eds.), *History of Wireless*. New Jersey: John Wiley & Sons

Sarkar, T. and R Mailloux, A. Oliner, M. Salazar-Palma, and D. Sengupta (eds.) (2006b), *History of Wireless*. New Jersey: John Wiley & Sons

Schaffer, S. (1995). "Accurate Measurement is an English Science." In M. Wise (ed.), *Values of Precision*. Princeton: Princeton University Press.

Schaffner, K. (1972). *Nineteenth-Century Aether Theories*. Oxford: Pergamon.

Schiemann, G. (2009). *Hermann von Helmholtz's Mechanism: Loss of Certainty*. Springer.

Schroeder, M. (2016). "Value Theory." *The Stanford Encyclopedia of Philosophy* (Fall 2016 Edition), E. Zalta (ed.), URL = <https://plato.stanford.edu/archives/fall2016/entries/value-theory/>.

Schuster, A. (1911). *The Progress of Physics during* 33 *Years (1875-1908).* Cambridge: Cambridge University Press.

Shapin, S. and S. Schaffer (1985). *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton University Press.

Sibum, H. (2002). "Exploring the Margins of Precision." In Bourguet et al. (eds.), *Instruments, Travel and Science: Itineraries of Precision from the Seventeenth to the Twentieth Century*. Routledge.

Smith, C. and M. Wise (1989). *Energy and Empire: A Biographical Study of Lord Kelvin*. Cambridge: Cambridge University Press.

Solomon, M. (2001). *Social Empiricism.* Cambridge: MIT press.

Stanford, P. (2000). "An Antirealist Explanation of the Success of Science." *Philosophy of Science*.

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

Stanford, P. (2015). "Unconceived Alternatives and Conservatism in Science: The Impact of Professionalization, Peer-Review, and Big Science." *Synthese*.

Stanley, M. (2003). "An Expedition to Heal the Wounds of War: The 1919 Eclipse and Eddington as Quaker Adventurer." *Isis*.

Stokes, G. (1848). "On the Constitution of Luminiferous Ether." *Philosophical Magazine*.

Stromberg, W. (1989). "Helmholtz and Zoellner: Nineteenth-Century Empiricism, Spiritism, and the Theory of Space Perception." *Journal of the History of the Behavioral Sciences*.

Stroud, S. (2006). "Epistemic Partiality in Friendship." *Ethics*.

Stürzbecher, M. (1972). "Zur Berufung Johannes Müllers an die Berliner Universität." *Jahrbuch für die Geschichte Mittel-und Ostdeutschlands*.

Teller, P. (2001). "Whither Constructive Empiricism?" *Philosophical Studies*.

Teller, P. (2004). "Discussion–What is a Stance?" *Philosophical Studies*.

Teller, P. (2011). "Learning to live with Voluntarism." *Synthese*.

Thomson, W. (1891). *Popular Lectures and Addresses*.

Tichý, P. (1974). "On Popper's Definitions of Verisimilitude." *British Journal for the Philosophy of Science*.

Tolstoy, L. (1904). *Essays & Letters*. A. Maud (trans.). New York: Funk &Wagnalls Company.

Tuchman, A. (1993). "Helmholtz and the German Medical Community." In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Tuomela, R. (2000). "Belief versus Acceptance." *Philosophical Explorations*.

Turner, R. S. (1977). "Hermann von Helmholtz and the Empiricist Vision." *Journal of the History of the Behavioral Sciences*.

Turner, R. S. (1993). "Consensus and Controversy: Helmholtz on the Visual Perception of Space." In D. Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*. Berkeley: University of California Press.

Vahid, H. (2010). "Rationalizing Beliefs: Evidential vs. Pragmatic Reasons." *Synthese*.

Van Fraassen, B. (1967). "Meaning Relations among Predicates." *Nous*.

Van Fraassen, B. (1969). "Meaning Relations and Modalities." *Nous*.

Van Fraassen, B. (1977). "The only Necessity is Verbal Necessity." *Journal of Philosophy*.

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

Van Fraassen, B. (1984). "Belief and the Will." *The Journal of Philosophy*.

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

Van Fraassen, B. (1989). *Laws and Symmetry*. Oxford: Oxford University Press.

Van Fraassen, B. (1991). *Quantum Mechanics: An Empiricist View*. Oxford: Oxford University Press.

Van Fraassen, B. (1992). "From Vicious Circle to Infinite Regress, and Back Again." *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*.

Van Fraassen, B. (1994a). "Gideon Rosen on Constructive Empiricism." *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition.*

Van Fraassen, B. (1994b). "The World of Empiricism." In J. Hilgevoord (ed.), *Physics and Our View of the World*. Cambridge: Cambridge University Press.

Van Fraassen, B. (1994c). "Against Transcendental Empiricism." In T. Stapleton (ed.), *The Question of Hermeneutics: Essays in Honor of Joseph K. Kockelmans*. Springer Netherlands.

Van Fraassen, B. (1997). "Structure and Perspective: Philosophical Perplexity and Paradox." In M. Chiara, et al. (ed.), *Logic and Scientific Methods*. Dordrecht: Kluwer.

Van Fraassen, B. (2000). "The False Hopes of Traditional Epistemology." *Philosophy and Phenomenological Research*.

Van Fraassen, B. (2001). "Constructive Empiricism Now." *Philosophical Studies*.

Van Fraassen, B. (2002). *The Empirical Stance*. New Haven: Yale University Press.

Van Fraassen, B. (2003). "On McMullin's Appreciation of Realism Concerning the Sciences." *Philosophy of Science*.

Van Fraassen, B. (2004). "Science as Representation: Flouting the Criteria." *Philosophy of Science*.

Van Fraassen, B. (2005a). "Replies to Discussion on the Empirical Stance." *Philosophical Studies*.

Van Fraassen, B. (2005b). "The Day of the Dolphins: Puzzling over Epistemic Partnership." In K. Peacock and A. Irvine (eds.), *Mistakes of Reason: Essays in Honour of John Woods*. Toronto: University of Toronto Press.

Van Fraassen, B. (2006b). "Representation: The Problem for Structuralism." *Philosophy of Science*.

Van Fraassen, B. (2007). "From a View of Science to a New Empiricism." In B. Monton (ed.), *Images of Empiricism: Essays on Science and Stances, with a reply from Bas C. van Fraassen*. Oxford: Oxford University Press.

Van Fraassen, B. (2008). *Scientific representation: Paradoxes of perspective*. Oxford: Oxford University Press.

Van Fraassen, B. (2014). "Values, Choices, and Epistemic Stances." In Gonzalez, W. (ed.) (2014). *Bas van Fraassen's Approach to Representation and Models in Science*. Springer Netherlands.

Velleman, J. (2000). *The Possibility of Practical reason*, Oxford: Clarendon

Vickers, P. (2016). "Understanding the Selective Realist Defence against the PMI." *Synthese*.

Votsis, I. (2011). "The Prospective Stance in Realism." *Philosophy of Science*.

Warren, R. and R. Warren (1968). *Helmholtz on Perception: its Physiology and Development*. New York: Wiley.

Warwick, A. (2003). *Masters of Theory*. Chicago: University of Chicago Press.

Weber, W. (1846). *Elektrodynamische Maassbestimmungen.* Leipzig.

Weber, W. (1848). "Elektrodynamische Maassbestimmungen." *Annalen der Physik (und der Chemie)*.

Whiting, D. (2014). "Reasons for Belief, Reasons for Action, the Aim of Belief, and the Aim of Action." In C. Littlejohn and J. Turri (eds.) *Epistemic Norms: New Essays on Action, Belief, and Assertion.* Oxford: Oxford University Press

Wise, M. (1981). "German Concepts of Force, Energy, and the Electromagnetic Ether, 1845–1880." In G. Cantor and M. Hodge (eds.), *Conceptions of Ether: Studies in the History of Ether Theories, 1740–1900*. Cambridge: Cambridge University Press.

Woodruff, A. (1962). "Action at a Distance in Nineteenth Century Electrodynamics." *Isis*.

Woodward, J. (2003). *Making Things Happen: A Theory of Causal Explanation*. Oxford: Oxford University Press.

Woodward, J. (2014). "A Functional Account of Causation; or, A Defense of the Legitimacy of Causal Thinking by Reference to the Only Standard That Matters—Usefulness (as Opposed to Metaphysics or Agreement with Intuitive Judgment)." *Philosophy of Science*.

Worrall, J. (1989). "Structural Realism: The Best of Both Worlds?" *Dialectica*.

Worrall, J. (1994). "How to Remain (Reasonably) Optimistic: Scientific Realism and the Luminiferous Ether". In *PSA: Proceedings of the biennial meeting of the Philosophy of Science Association*.

Wray, K. (2007). "A Selectionist Explanation for the Success and Failures of Science." *Erkenntnis*.

Wray, K. (2015). "The Methodological Defense of Realism Scrutinized." *Studies in History and Philosophy of Science Part A*.

Wylie, A. (1986). "Arguments for Scientific Realism: The Ascending Spiral." *American Philosophical Quarterly*.

Yeang, C. (2014). "The Maxwellians: The Reception and Further Development of Maxwell's Electromagnetic Theory." In R. Flood, M. McCartney, and A. Whitaker (eds.), *James Clerk Maxwell: Perspectives on His Life and Works*. Oxford: Oxford University Press.