EVALUATING AND EXPLAINING THE SUCCESS OF SCIENCE: A HISTORICAL PERSPECTIVE

by

Jamie Craig Owen Shaw

A thesis submitted to the Department of Philosophy

In conformity with the requirements for

the degree of Masters of Arts

Queen's University

Kingston, Ontario, Canada

(September, 2014)

Abstract

The recent literature surrounding the realist/anti-realist debates in the philosophy of science has focused its attention towards the role that history plays in explaining why science is successful and thus approximately true. This has been caused, in large part, by the Pessimistic Meta-Induction (PMI), which has challenged attempted explanations by turning our attention towards the large amount of scientific theories that have been abandoned but were still empirically successful. There will be two primary goals of this paper. The first will be to explore the PMI more deeply than it traditionally has been. This will involve demonstrating how the PMI is more malleable than it is normally given credit for as well as how meta-inductions can and have been used in practice. The second goal will be to discuss the implications of this more robust form of the PMI for explanationist realism, or the thesis that scientific realism provides the best answer as to why science is successful. I will argue that explanationist realism, after being cornered by the malleability of the PMI, turns out to be either a trivial or false thesis. Given this demise of explanationist realism as a substantive thesis, I will argue that the strength of scientific realism truly lies in its critical abilities or its ability to normatively assess scientific theories and the way we ought to practice science.

Acknowledgements

First and foremost, I would like to thank Sergio Sismondo for going above and beyond the call of duty for supervising this project, and for providing such thoughtful and helpful commentary and continuous support. I would similarly like to thank Josh Mozersky for his detailed comments and thought-provoking discussions. On top of this, I greatly appreciate the stimulating conversations with Torin Doppelt, Kyley Ewing and Ashley Perna-Capell which were instrumental for clarifying and improving my own thought processes. I am also sincerely grateful for the thorough and attentive comments of David Pitcher and Tim Juvshik, which were extremely valuable for challenging and refining the final content of this project. Finally, I want to thank my parents, John Shaw and Diane Purdie, for their continuous love and support.

Table of Contents

Chapter 1

What is the Pessimistic Meta-Induction?

Preamble

The Pessimistic Meta-Induction (PMI) has been called the most notorious anti-realist argument in the recent literature in the philosophy of science (Ladyman 2009). The PMI posits, in a nutshell, that since past scientific theories that were thought to be true turned out to be false by the lights of our best current scientific theories, our current scientific theories are likely to suffer the same fate. When seen in this light, the PMI seems like both a general argument about all scientific theories and a skeptical argument against the eternal and absolute ‘truths’ of our best current scientific theories. Not only does this discourage an overly strong epistemic interpretation of scientific theories, but it also casts doubt on particular interpretations of how science works. The two standard formulations of the PMI, by Hilary Putnam (1978) and Larry Laudan (1981), both aim to disrupt particular interpretations of this sort. While I will explicate these classical versions of the PMI, my aim in this chapter will be to explore the malleable nature of meta-inductions more broadly. While the PMI is often thought of as a single problem, there are many different forms that meta-inductions can take. Each form would be different in both its construction and its implications. Furthermore, meta-inductions need not be problems for particular interpretations (that is to say they need not be skeptical). The history of science is not only usable as a weapon against interpretations of the scientific enterprise, but also as a tool that can productively bolster interpretations. A discussion of the various roles meta-inductions can play will both expand and nuance their place in the literature of the philosophy of science.

Section 1 of this chapter will explore the existing formulations of the PMI. I will start (1.1) by analyzing the skepticism of Sextus Empiricus, which provides an important historical antecedent for the contemporary literature. I will go on (1.2) to outline the two popular versions of the PMI set out by Putnam and Laudan and finish (1.3) by outlining the realist's *optimistic* meta-induction (OMI), which is used to support realist interpretations of the nature of scientific inquiry. Section 2 will suggest three features of meta-inductions that can potentially be modified. The first feature (2.1) will be particular historical methodologies that meta-inductions are dependent upon. The second feature (2.2) will be the philosophical targets of meta-inductions. The third feature (2.3) will be the scope and timeframe which the meta-induction is investigating. All three of these features, when altered, change the nature and implication of the meta-induction being used. I will conclude this chapter (section 3) with a brief discussion of some other reasons for why meta-inductions are important. While meta-inductions are generally considered important for strictly philosophical reasons, I will explore (3.1) the social and political reasons why meta-inductions are needed and illustrate the significance (3.2) with a case study of the Human Brain Project.

1. Classical Formulations of the PMI

1.1: PMI as Skepticism

The basic intuition behind the PMI is not a new one. Sextus Empiricus advances a similar challenge as a form of skepticism about the possibility of absolute knowledge in general. He writes:

> [W]hen someone propounds to us a theory which we are unable to refute, we say to him in reply, "Just as, before the birth of the founder of the school to which you belong, the theory it holds was as not yet apparent as a sound theory, although it really was in existence, so likewise it is possible that the opposite theory to that which you now propound is already existent, though not apparent to us, so that we ought not as yet to yield assent to this theory which at the moment seems to be valid." (Sextus Empiricus 1990, 26).

For Sextus, we ought to suspend our judgment on the true nature of things for the sake of a state of quietude or *ataraxia* (state of tranquility). Since anything we currently believe may become replaced by something that will become recognizable as a sound theory, we should not commit ourselves to any absolute knowledge of the world. However, Sextus argues, certain things are unavoidable; even the strongest skeptic cannot deny that she is hungry or cold and that eating or finding shelter will relieve them of this discomfort. This allows the skeptic to be involved in common day practices and accepting of their consequences while "reject[ing] of the added belief in the natural badness [or goodness][1] of all conditions" (25).

When applied to scientific knowledge in general, this is often the heart of the PMI: a form of *skepticism* about overly strong epistemic interpretations of scientific theories. This skepticism does not entail the absurd conclusion that we ought to abandon scientific or philosophical inquires, nor does it challenge the claim that scientific theories are, often times, able to successfully predict or explain[2] empirical phenomenon. Skeptics readily accept that scientific theories have allowed us to get to the moon, fly planes and cure diseases.[3] However, the skeptic challenges the added *realist belief* that not only are scientific theories instrumentally reliable,[4] but are also approximately true of the world. The skeptic and the realist are both committed to the view that scientific theories provide what Arthur Fine called 'homely truths' (Fine 1991a, 270), but the skeptic denies that there is some sort of epistemic 'bonus' on top.

---

[1] Natural badness can be understood here as the natural or intrinsic state of things.
[2] To clarify, I am not conflating explanation and prediction here. I am merely suggesting that skeptics can consider scientific theories valuable in either of these respects.
[3] Bloor (2008), for example, argues that particular 'skeptical' philosophies are able to both deny overly-strong epistemic accounts of scientific knowledge while accepting the usefulness of scientific knowledge.
[4] Instrumental reliability is understood here in Fine's terms as "useful for getting things to work for the practical and theoretical purposes for which we might put them to use" (Fine 1991b, 86).

This is one form the PMI may take: a general skeptical argument aimed at undermining our confidence in the approximate truth[5] of scientific theories for the sake of fostering some attitude (though these attitudes are vastly different from *ataraxia*). Sextus' proto-PMI and the two more recent versions by Hilary Putnam (1978) and Larry Laudan (1981), while similar in this basic respect, differ in their precise formulations. Sextus' skepticism is grounded on the *possibility* (which always exists) that future judgments of 'equal strength' may eventually contradict current judgments. Whereas Sextus' skepticism is an on-going and consistent approach in deciding what to believe, the *strength* of the anti-realist's skepticism is grounded on inductions of past scientific theories, making their skepticism contingent. The anti-realist skepticism comes in degrees of urgency, depending on the results of a meta-induction on the history of science, whereas Sextus' state of quietude is irrespective of any historical investigation. Despite these differences, as will be shown, Putnam and Laudan's PMIs both share in the same form as Sextus' general skeptical approach.

1.2: Putnam and Laudan's PMIs

Putnam presents a version of the PMI that challenges our confidence in the successful reference of the theoretical terms postulated by our best current sciences. Putnam writes:

> What if *all* theoretical entities postulated by one generation (molecules, genes, etc., as well as electrons) invariably 'don't exist' from the standpoint of later science?...One reason this is a serious worry is that eventually the following meta-induction becomes overwhelmingly compelling: *just as no term used in science of more than fifty* (or whatever) *years ago referred, so it will turn out that no term used now… refers*. (Putnam, 1978, 24-5; emphasis in original).

[5] Here, approximate truth is understood as an 'added belief' in Sextus' sense. Not only are scientific theories 'true' in the sense that they are reliable instruments in navigating our way around the world, but they are true in a more philosophically substantial sense.

The challenge here is that since many of the theoretical terms of past sciences ended up failing to refer to anything in the real world (e.g. phlogiston, caloric, vital force, etc.), we should have the same skepticism about our current theoretical terms. This is an inductive argument whose strength relies on the purported referential failures of past theoretical terms.[6] This undermines the realist who is committed to the view that unobservable entities can be successfully referred to (29).[7] This makes the "desideratum for [a realist] theory of reference that this meta-induction be blocked" (ibid). The kind of realism that is targeted by Putnam's PMI is one that is an "over-arching empirical hypothesis" (20) of scientific theories in general. That is to say, realism is an interpretative stance of the success of science. This makes realism, for Putnam, a contingent thesis in which the 'truth-tracking record' of scientific theories is not guaranteed in any *a priori* sense.[8] In other words, Putnam's realism is an *a posteriori* claim about how science works.[9] This makes the PMI both an *empirical* challenge and a *general* challenge. It is empirical in the sense that it is a historical claim about scientific theories which opposes the historical claims of the realist and it is general in that it targets the referential success of *any* theoretical term. Generality is shared in Sextus' skepticism, which is aimed at no claim in particular, but at *any* claim that is made. I shall return to this later; for now I want to focus on the priority Putnam places on epistemology in his formulation of the PMI.

Putnam speculates that theoretical entities from one generation "don't exist' from the standpoint of a later science.' This worry is fueled by the potential absence of current theoretical terms, which we now believe refer to entities in the real world, in future scientific theories. If the

---

[6] This skepticism comes from logical empiricism which attempts, in various ways, to cast doubt on the existence of non-observable entities.
[7] This supposes that in order for a scientific theory to be approximately true, its central terms must refer to entities in the real world.
[8] It should be noted that Putnam does not preclude the possibility of having an *a priori* defense of realism (i.e. realism as a necessarily true hypothesis) which is compatible with the PMI. I will not address this kind of realism or its opponents (i.e. forms of idealism) here.

term 'electron' is not *used* in future scientific theories, then there seems to be no secure basis for one to infer the existence of electrons. This ontological worry could be a worry in two senses. It could be an idealist worry (i.e. the external world is somehow constituted by the mind) or a worry stemming from a Quinean strategy of obtaining ontology from the resources of science itself. Since the latter worry seems to be more consistent with Putnam's general approach,[10] the worry seems to be that without knowing whether the referential terms of science are successful, we have no way of populating the ontological landscape of the natural world. If this is the strategy, then the epistemic worry takes precedence. First we have to know that the terms of scientific theories successfully refer before making the ontological claim that the entities to which those terms refer actually exist. Not only does this imply that Putnam's PMI takes epistemic matters to be of primary importance, but also that epistemic claims are 'read off' successful scientific theories. That is to say that we *know* the theoretical terms refer to actual entities due to *their success* in scientific theories. This leaves the philosopher two tasks: (i) to qualify what makes a particular scientific theory worthy of an ontological analysis (i.e. has the requisite virtues)[11] and (ii) to analyze the contents of these scientific theories to see what theoretical terms refer to in order to find out what entities actually exist. The PMI, then, disrupts these tasks by casting doubt on the connection between the virtues of scientific theories and the successful reference of its theoretical terms in general. Whatever result comes from (i) could potentially be shown, through a meta-induction of the history of science, to have no necessary connection to whatever results come from (ii). This means that the realist must block or circumvent the PMI in order to have a working

---

[9] This will be developed more fully in the next chapter.

[10] This characterization seems fair due to Putnam's general approach. He argues that we ought to give the 'benefit of the doubt' that Bohr, for example, was actually referring to real entity since it was behaving how it was predicted to behave. The ontology of electrons, in this case, comes from the term 'electron' which successfully refers due to its essential role in its surrounding scientific theories (Putnam 1978, 22-5).

[11] Putnam's particular virtues come through in his analysis of the progress of science (19-22).

hypothesis as to how their relation to the natural world makes scientific theories approximately true.

While Putnam's PMI requires some unpacking, Laudan's is more fully developed. Like Putnam, Laudan targets a general form of realism. However, rather than focusing on the connection between the theoretical terms used in current scientific theories and their referents in the natural world, Laudan challenges the connection between the empirical success of a scientific theory and its approximate truth. This undermines the widely celebrated 'no miracles argument' (NMA), that scientific realism is the best theory which makes the success of science non-miraculous, which presupposes a connection between the success of science and its approximate truth. Laudan writes:

> Because [most past theories] have been based on what we now believe to be fundamentally mistaken theoretical models and structures, the realist cannot possibly hope to explain the empirical success such theories enjoyed in terms of the truthlikeness of their constituent theoretical claims. (Laudan 1984, 91-2).

This argument has been unpacked as a *reductio* with the following presentation:[12]

(a) Current scientific theories are approximately true because they are empirically successful.
(b) If our current successful scientific theories are approximately true, then past theories cannot be.
(c) These characteristically false theories were, nonetheless, empirically successful (what Laudan refers to as 'the historical gambit').
(d) Therefore, empirical success does not necessarily imply approximate truth.[13]

---

[12] I am modeling this unpacking on Psillos (Psillos 1996).
[13] Psillos merely addresses the concern that empirical success is not *necessary* condition for approximate truth. However, Laudan claims that empirical success is not even a *reliable* condition for approximate truth. He writes "for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring" (Laudan 1981, 35). However, I do not think Laudan is justified in making this claim. I will return to this in chapter 2.

In this presentation, (a) grants by assumption the realist premise that our current theories are approximately true while (b) is made true if past theories are incompatible with current ones. For (c), Laudan provides a list of past theories which are now considered to be false, a list that he argues could be extended *ad nauseam* (a few examples include the caloric theory of heat, the crystalline spheres of ancient and medieval astronomy, the vital force theory of physiology, and so on). Most realists have focused on the historical gambit; trying to make the list appear smaller than Laudan makes it out to be. Before delving into these discussions, I want to briefly shift the focus of the debate. Here, the *ad nauseam* qualification, which is generally overlooked, will be examined more closely to emphasize its significance. This will allow Laudan's PMI to emerge in a new light.

Laudan could be read as compiling a list of challenges or historical counter-examples for the realist. By shortening that list, the force of Laudan's PMI diminishes. This strategy has led to accusations of Laudan's list being 'over-stated' (Psillos 2000) and not taken to be as much of a worry as Laudan makes it out to be. This has been achieved by showing ways in which past scientific theories have been retained in current ones. If past theoretical constituents have been retained in our current scientific image, then they have not been shown to be entirely false. That is to say that we have not fully abandoned past scientific theories, but have progressed from them. However, Laudan's *ad nauseam* qualification, when put front and center, suggests that a different response is needed. On this reading, Laudan is putting forth a *view of the history of science* and not simply a potentially manageable list of counter-examples to the realist thesis.[14] The history of science, for Laudan, is not filled with convergence, agreement, patterns, similarities, and so forth but with discrepancies, contradictions, and deep disagreements. This makes the history of science

[14] This reading is further solidified by Laudan's explicit attempts to avoid a whiggish historical position that "recounts only those past theories which are referentially similar to currently prevailing ones" (Laudan 1981, 34). This aversion to whiggish history is quite similar to Kuhn and Feyerabend's.

a seemingly endless pool of counterexamples to any proposed overriding logic of history (or 'whig history'). While I will not take a stance on either view of history here, the realist responses (Psillos 1994, Nola 2008) that aim to minimize Laudan's list one by one accomplish very little in terms of undermining the force of the PMI when understood as an indefinitely long list of counterexamples. A more persuasive response requires positing a contrasting account of history which shows a historical accumulation of facts with large amounts of convergence, agreement, and continuity.[15] Since Laudan's PMI and its associated view of history is a *general* argument, a response must be general as well.

1.3: Optimistic Meta-Inductions

Both Putnam and Laudan's PMIs are classified as anti-realist. This is because they aim to disrupt realist explanations of how science is successful. Given Putnam's PMI, realists cannot assume that the central theoretical terms must refer for the theory to be approximately true. Given Laudan's PMI, realists cannot help themselves to the NMA, which assures that empirically successful theories are approximately true. Additionally, Laudan's PMI casts doubt on a realist optimism that scientific theories will converge upon a complete description of the natural world. One realist strategy to blunt the force of Laudan's attack has been to respond in a piecemeal fashion. Realists no longer hold to an 'all-or-nothing' realism[16] about *all* scientific theories, but instead limit their realism to particular instances of theories that conform to certain realist conditions. While this strategy will be explored at greater length later on, what is of current importance is the realist attempts to use the history of science to bolster their own interpretations of particular scientific phenomenon. The history of science, in these attempts, is a friend to the

---

[15] This has often existed as a background assumption for realists but is not often substantively argued for explicitly. The important point here is the focus of the debate becomes about historical methodology and a revolutionary-based method comes with the PMI.

realist, not the enemy it was made out to be in Laudan's presentation. Rather than being pessimistic, which is a common characteristic of anti-realist positions, realists use meta-inductions optimistically[17] to show how realist interpretations can be substantiated by the history of science.

Optimistic meta-inductions (OMIs) begin by outlining a particular realist interpretation.[18] While there are many forms of realism, which have importantly different methods of investigating history, realist interpretations usually have a few distinctive features. Psillos sums up the realist positions as three constitutive theses: (i) the *metaphysical thesis* holds that there exists a mind-independent world with a definite structure. (ii) the *semantic thesis* holds that scientific theories are "truth-conditioned descriptions of their intended domain" and that "[t]he theoretical terms featuring in theories have putative factual reference" (Psillos 2000, 706), and (iii) the *epistemic thesis* holds that predictive scientific theories[19] are approximately true and are generally retained across theory-change. While (i) is not provable by investigations into the history of science,[20] both (ii) and (iii) are contingent claims grounded in the historical record, which is to say that the evidence for realism lies in the history of science. While meta-inductions have been traditionally formulated in a skeptical vein that aims to undermine realist interpretations, there is no *a priori* reason why realists need be wrong on *particular instances* within the history of science. As Psillos writes, "Putnam's enduring contribution to the realist cause is his thought that the defense of realism cannot be a work of *a priori* epistemology, but rather be part and parcel of an empirical-naturalistic programme which claims that realism is the

---

[16] This is Psillos' (1996) term.

[17] Psillos (2000) describes realism as a position of epistemic optimism (about convergence and that the 'explanatory virtues' are connected to the approximate truth of scientific theories) where anti-realists are pessimistic about convergence and the ability to produce an overriding logic of the scientific project.

[18] By this I mean an interpretation that could aptly be described as realist (i.e. contains realist notions).

[19] 'Predictive scientific theories' holds as placeholder here for multiple empirical virtues.

best empirical hypothesis of the success of science" (717). While the PMI casts doubt on realism being the best at explaining every instance of successful scientific theories (i.e. realist interpretations of all breeds falter sometimes), it does not cast doubt on the ability of realism (when sufficiently qualified) to be the best interpretation in certain cases. This gives hope to the realist who wants to use meta-inductions to support their claims rather than refute them.

There have already been a few attempts in the literature to use OMIs given particular realist approaches. Psillos (1994), for example, attempts to show how the important features of caloric, which were necessary for its success (laws of calorimetry, adiabatic change and Carnot's theory of the motive power of heat), were conceptually retained in thermodynamics.[21] Other OMIs have been formulated as well (Robert Nola (2008), Sherlyn Roush (2009), John Worrall (1989)) which attempt to reconcile the historical record with a realist interpretation[22] of the success of science. While each particular strategy requires its own attention in regards to its merits and deficiencies, it remains an open possibility that these realist strategies will show instances of convergence in accordance with some realist principles. However, simply because OMIs are successful in particular instances, it does not disrupt the PMI as a global argument unless it is generalizable.[23] In other words, Putnam and Laudan's PMIs discourage global attempts to reconcile the history of science within a particular interpretative framework, but this does not discourage realists from using the history of science productively in isolated meta-inductions. While this realist strategy will be explored at greater length in the next chapter, for

---

[20] Since the anti-realist opponents of realism that are being dealt with in this paper are not denying this thesis, I will not address this issue here.

[21] This interpretation has been challenged by Chang (2003).

[22] Though these are all slightly different forms of realism. Nola and Roush employ a Ramsey-sentence realism while Worrall employs a structural realism.

[23] To clarify, since the PMI is *not* advancing a positive thesis about the history of science but disrupting the *necessary* connection Putnam makes between the reference of a theories central theoretical terms and its approximate truth, a single (or small number of OMIs) only disrupts the PMI for a particular instance and not more generally.

now I merely want to emphasize that meta-inductions need not be skeptical, but can be used to confirm and defend interpretative stances as well.

2. The Malleability of the PMI

I have thus far outlined the traditional way of understanding the PMI. It is a general argument that applies to any scientific theory or theoretical term and it is advanced as a form of skepticism against particular interpretive stances of the success of science. The PMI's opposite, the OMI, is a more specific kind of meta-induction that aims to bolster an interpretative stance on one particular scientific theory. However, there are many other ways in which meta-inductions can be warped such that they pose different challenges and have different implications. In this section I will outline a few suggested ways in which meta-inductions can be modified.

2.1: Historical Methodologies

As seen with Laudan's PMI, meta-inductions are not purely descriptive of the history of science, but are also dependent on a particular historical methodology. That is to say that historical methodologies differ in their manners of reconstructing the development of scientific knowledge.[24] As Imre Lakatos writes, "modern methodologies or 'logics of discovery' consist merely of a set of…rules for the *appraisal*[25] of ready, articulated theories" (Lakatos 1981, 108). Each methodology is not the final word about how science has grown or changed over the years,

---

[24] Lakatos is solely concerned with philosophical (or 'rational') reconstructions of history in this essay, since these strategies bear little resemblance to the methodologies historians themselves typically employ.
[25] The emphasis here is Lakatos', which represents a shift in the normative philosophy of science from "rules for arriving at solutions [to] directions for the appraisal of solutions already there" (Lakatos 1981, 108 fn. 2).

but rather a perspective which includes rules for normatively assessing scientific progress.[26] Lakatos surveys four prominent historical methodologies and demonstrates how each understands the growth of scientific knowledge in different terms. Inductivists look for “hard facts and so-called inductive generalizations”, the conventionalist looks for “factual discoveries and the erection of pigeonhole systems and their replacement by allegedly simpler ones”, the falsificationist “dramatizes bold conjectures, improvements which are said to be *always* content-increasing and, above all triumphant ‘negative crucial experiments’”, and his own methodology of research programmes “emphasizes long-extended theoretical and empirical rivalry of major research programmes, progressive and degenerating problem shifts, and the slowly emerging victory of one programme over the other” (123). Each methodology recreates the history of science with different epistemological standards and sets of emphasis which are themselves normatively loaded. A rational reconstruction of the history of science requires the selection of particular ‘facts’, which is grounded in some theoretical bias, and the praise or blame of particular moves in the history of science.

The four examples Lakatos analyzes are certainly not the only ones. Many realist reconstructions of history (other than Psillos’) exist as well. For example, Phillip Kitcher’s rational reconstruction of science involves delineating ‘presuppositional’ from ‘working’ posits in which the historian aims to show which theoretical terms “apparently have to exist if the instances of the schemata are to be true” (Kitcher 1993, 149); John Worrall provides a rational reconstruction of the history of science in which ‘structure’ (rather than observational content) is consistently retained;[27] and many other instances exist as well (e.g. Newman (2005), Hardin and

---

[26] Lakatos includes the rules that prescribe the rejection of certain theories and the acceptance of others and codes of scientific honesty.
[27] For further clarification on this distinction, see Worrall (1989) 117-121.

Rosenberg (1982))[28] While it should be noted that this is not a free-for-all in which any way of conceiving of history will be welcome, since there are legitimate methodological and conceptual difficulties with some, each methodology can be particularly well suited to certain kinds of investigations. For example, Worrall grounds his structural realism on his interpretation of the transition between Fresnel's theory of wave optics and Maxwell's electromagnetism. Despite the lack of continuity in Fresnel's 'luminiferous aether' as an elastic solid that obeys traditional laws of mechanics, since it was ultimately rejected by Maxwell, formal structural features such as Fresnel's equations describing the reflection and refraction of light were retained in electromagnetism. While this transition appears to fit Worrall's historiography quite nicely, it is more awkward when applied to transitions in the biological sciences (Newman 2005).[29] This provides an example of a methodology which may be particularly well adapted for certain kinds of transitions, but not for others. As long as structural realism is not held in a recklessly general way, this leaves room for other distinct and incompatible formulations of meta-inductions on the history of science.

Here, our choice of being structural realists or not depends on whether the strong emphasis on 'structure' and Worrall's distinction between structure and observational content fit well with the historical period being analyzed. Putting this another way, its plausibility resides in its ability to fit a given area of research. That being said, since multiple methodologies may be employed on the same area of research, the meta-inductions grounded in those methodologies will differ in their character. There may be a convergence between Fresnel and Maxwell on a

---

[28] Newman's Ramsey-sentence realism searches for theoretical constants and seeks to "replace these constants with distinct variables, and then binds these variables by placing an equal number of existential quantifiers in front of the resulting formula" (1378) while Hardin and Rosenberg propose a more liberal notion of reference whereby older theories can be said to be approximately referring (in different degrees) to the same entities as our current best sciences.

[29] This is because mathematics play a substantially different role in biological sciences (Newman 2005, 1377-8).

structural realist account, but not on a Psillosian framework.[30] But there is another difficulty: if our normative preferences change, then our choice of methodology may change as well. That is to say our meta-induction may be grounded on some independent normative considerations. For example, consider Steven Epstein's (1995) analysis of AIDS trials in the U.S. in the 1980s. At the time, there were two kinds of trials for conducting AIDS research. The dominant approach emphasized 'pure' or 'fastidious' trials, in which experiment groups were homogenous[31] in order to allow for more stable and untainted scientific knowledge. However, this had social implications for groups that had little relevant knowledge produced for them since they were not tested on. This led to protests for more pragmatic or 'dirty' trials due to concerns about the welfare of groups outside of the specified demographic. Here, a falsificationist or Platonist meta-induction blocks and denounces such dirty trials as 'pseudoscientific'[32] while a constructivist or empiricist meta-induction are more inclusive and welcoming of differing methodologies. This shows an example in which the choice of historical methodology was be grounded in normative considerations. Depending on our views of social justice, in this example, we may not want to be falsificationists in this instance.

This demonstrates two ways in which historical methodologies change the character of a meta-induction. We may choose a historical methodology on the grounds that it is the most conceptually appropriate for a given area of interest, or because of separate normative considerations about the implications of the meta-induction in question. This provides the first

---

[30] Psillos (1996) expressly criticizes Worrall's use of the Fresnel/Maxwell transition since Fresnel's luminiferous aether was necessary for Fresnel's ability to make novel predictions. The transition for Psillos, then, was not one of full convergence whereas it was for Worrall.

[31] Homogeneity is a multi-faceted notion here. It includes those who were not taking any other drugs (legal or illegal), subjects who did not have other major conditions, and racial homogeneity (see Epstein (1995) 421-3 for more details).

[32] This would be due to their high epistemological standards and account of progress (see Feyerabend's criticism in chapter 15 of *Against Method*. While this analysis may not be accurate, I merely want to use it as an illustrative example here.

feature of a meta-induction which, when tampered with, changes its implications and results. Next, I will address another feature of meta-inductions which is similarly malleable: the targets of meta-inductions.

2.2: Meta-Inductions and their Targets

Putnam's PMI targets a particular form of realism: an over-arching empirical hypothesis that assumes that the central theoretical terms of a theory must approximately refer to entities in the natural world in order for the theory to be empirically successful and thus approximately true. Laudan's PMI, while he explicitly targets convergent forms of realism which rest on the NMA, is presented in a much more flexible manner. Laudan explicitly argues against *any* overriding or 'whig' logic of the history of science (Laudan 1981, 39-45). In a sense, this is quite amenable to Paul Feyerabend's 'epistemological anarchism' outlined in *Against Method*. As Feyerabend has argued, "the idea of a fixed method, or of a fixed theory of rationality, rests on too naïve a view of man and his social surroundings" (Feyerabend 1975, 11). The history of science is too rich and varied to be encompassed by a single interpretation.[33] The attempt to characterize the whole history of science under a single heading (cashed out in terms of theories of rationality, logics of scientific progress, etc.) ends up being unable (or unwilling) to sufficiently explain or justify many individual historical episodes. In this sense, meta-inductions are malleable as to what position they attack or defend.[34] More specifically, not only could a meta-induction disrupt any potential realist position, but it could also attack any of the so-called 'explanatory virtues' or ideals of scientific practices as well.

---

[33] Realists have often attempted to deflate their conception of 'approximate truth' or 'representational accuracy' in order to make them more broadly applicable. However, as Kyle Stanford points out, this dilutes these notion to become extremely thin such that they lack "any real significance in the debate over realism" (Stanford 2003, 566).

Explanatory virtues (EVs), as Adolfas Mackonis (2013, 978) puts it, "play a role in, firstly, generating potential explanations and, secondly, in the choice of the best one out of them." They provide the best possible explanation[35] (or interpretation) as to why science is truth-conductive, pragmatic, or logical.[36] Mackonis' own examples include coherence, breadth, depth, simplicity, and empirical adequacy, the last of which "constitutes the very aim of science…thus it should be the most desirable feature of explanations" (989).[37] EVs have two primary functions: (a) they are *descriptive* of how science works and (b) they provide *ideals* for future scientific inquiry. EVs are provided by investigations into science itself and are chosen as the best possible explanation (i.e. they are an inference to the best explanation). In other words, they are realist in the sense that theories that display particular EVs[38] are more likely to be successful (and therefore approximately true) and will be retained (more often than not) across theory-change. Since they are explanations as to why sciences functions *well* (i.e. why they are *virtues*), they also double as an ideal for how science ought to function.

Since the PMI disrupts the plausibility of providing an *a priori* reason as to why any particular feature of scientific theory must be present to guarantee its success,[39] there is similarly no *a priori* reason why an ideal or normative prescription might produce an aimed result. Take, for example, Mary Hesse's claim that we can't be unrestrained methodological pluralists, since this would imply that we ought to "go back and exploit the objective criticism of modern science

---

[34] This has been shown in section 1.3. Many different formulations of realism can be defended using meta-inductions depending on the particular features of the meta-induction itself.
[35] Since EVs are an inference to the best explanation, they are not *necessary* for a theories approximate truth or explanatory power, but they are the best explanation available.
[36] Mackonis divides the many different explanatory virtues into epistemic, pragmatic, and logical virtues.
[37] Mackonis goes onto argue that empirical adequacy is the pinnacle of the EVs since it is actually a result of the other explanatory virtues.
[38] Different realists emphasize partially different sets of virtues (and to what extent each should be valued) but there is generally significant overlap.

available in Aristotelianism or indeed in Voodoo" (Feyerabend 1975, 28). This is a common enough normative prescription; archaic texts or 'pseudo-sciences' ought not to be sources of inspiration or influence in legitimate scientific practices. As intuitive as this sounds, Feyerabend offers the counterexample of how Aristotelian physics continued to have a productive influence after they had been 'replaced' by early modern astronomy and physics.[40] Putting the point more broadly, Feyerabend argues that "a determined application of the methods of criticism and proof which are said to belong to the context of justification would wipe out science as we know it" (150). The history of science has had many instances of progress[41] by deliberately disobeying normative requirements that philosophers have articulated. Putting it bluntly, "scientists… interpret the evidence so that it fits their fanciful ideas, eliminate difficulties, by *ad hoc* procedures, push them aside, or simply refuse to take them seriously" (ibid). The history of science is so rich, complex, and intertwined that to adopt some methodological principle for the whole of science (or even some large section of scientific inquiry) will interrupt not just a particular notion of progress, but potentially *any* notion of progress.

Coming back to the PMI, it seems as if these ideals of scientific practice or EVs suffer from the same fate as the realisms which Putnam and Laudan attacked. In the same way that science has not been approximately true by virtue of some philosophical consideration (i.e. reference of theoretical terms, empirical success, etc.), it has not progressed due to an unwavering commitment to a single set of ideals or methodological principles. If Einstein had considered empirical adequacy the ultimate EV as Mackonis does, then he would not have felt the need to

---

[39] I am speaking broadly here. I want to leave open the possibility that while particular features may not be necessary to the success scientific theories in general, it may be necessary (in some sense) in a particular example.
[40] See fn. 3 on Feyerabend (1975), 28. He further argues that Voodoo also had a profound impact our knowledge of physiology (30).
[41] Feyerabend does not argue for an explicit criteria for what constitutes genuine progress but rather understands progress "in any of the senses one cares to choose" (11).

criticize the Copenhagen interpretation of quantum mechanics.[42] Furthermore, particular EVs can, at times, conflict with each other. As Kuhn notes, EVs[43] "repeatedly prove to be in conflict with one another; accuracy may, for example, dictate the choice of one theory, scope the choice of its competitor" (Kuhn 1977, 322).[44] As intuitive as any of Mackonis' (or anyone else's) EVs may sound, when seen as objective criterion of theory preference, they result in being (at times) needless handcuffing. Put more generally, the realist (or anyone who posits an explanation of how science works or an ideal of how it ought to work)[45] is essentially playing a game of 'truth or dare' with meta-inductions: the realist claims what the 'true' EV(s) of science are and then dares the critic to render such claims foolish or naïve through a meta-induction on the history of science.

However, that does not entail that the project of constructing and proposing EVs is to be completely terminated. On the contrary, it could be expanded to include virtues which are valued because of independent normative concerns (as seen in the previous section).[46] None of this should recommend that any EV should be taken as a guiding principle for all scientific inquiries at all times. As Feyerabend argues, "whatever we accept we should only do so tentatively, always remembering that we are in possession of, at best, partial truth" (Feyerabend 1970, 216). These EVs, when seen as permanent and persistent pressures on scientists whose force comes irrespective of historical or social positioning, lose their credibility and remain only as "verbal

---

[42] See Chang (2001) for a brief discussion of this. I will be discussed further in the following chapter.
[43] Kuhn is addressing the possibility of an objective criterion for theory choice. I take this to be sufficiently analogous to EVs.
[44] Kuhn illustrates this with the examples of choosing between Ptolemy and Copernicus' astronomical theories, oxygen and phlogiston theories of combustion, and Newtonian and quantum mechanics.
[45] These are two separate sorts of challenges but are address in a similar fashion. By explaining how science works, realists open themselves up to counterexamples of instances of science which succeed (by any definition) without operating in the relevant way. By creating ideals for science, realists open themselves up to episodes in history where genuine progress (however it is understood) was made by disobeying such ideals.

ornament[s] [or] as a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and 'rational' rules" (215). Rather than static principles, EVs ought to be conceived of as recommendations which are taken with the grain of salt provided by the historical and theoretical (and potentially social) situatedness of the inquiry in progress. There are many EVs, which are proposed under different circumstances[47] to produce various results; they all must first pass the test of history to find their limitations. Meta-inductions need not target any one particular position, they may attack any interpretative stance about how science functions or how it ought to function.

Finally, I want to briefly consider other ways in which meta-inductions are malleable in their formulation. In this next section, I will consider the malleability of the scope of meta-inductions both in terms of which scientific discourse is being investigated and what timeframe is being used.

2.3: Scope and Timeframes

Both Putnam and Laudan make extremely broad claims in their use of the PMI. Their targets are broad and general, so the scope and timeframe used by these PMIs is quite large. It makes use of thousands of years of the history of science[48] and is open about which subfields of science are being inducted upon. Here, I want to briefly consider the impact of timeframes and scopes on meta-inductions. More specifically, I will argue that larger timeframe and scopes will lead to thin or borderline trivial results. Conversely, more narrow meta-inductions on limited

---

[46] This would also multiply the amount of conflicts. EVs may be conceived and received differently in Jacobin traditions during the French revolution, or in Fichte's *Wissenschaftslehre*, for example.

[47] Some virtues were chosen due to highly contextual social features which were prevalent at the time (i.e. the humanist trend in academia during the Renaissance for Baconian science, the impact of German Romanticism on the acceptance of energy conservation, the impact of 19th century social politics in Britain on the acceptability of Darwinian struggle for survival, and so forth).

[48] Laudan, for example, uses Aristotelian mechanics as one of his examples.

periods of history entail using particular examples of science and thus provide more substantive and plausible results.

Consider Robert Nola's (2008) historical approach at attempting to show the ontological continuity of the electron across many different theories. Nola argues that a Ramsey-Sentence realism can account for a great deal of continuity across multiple theoretical frameworks. Nola argues that the greenish-yellow glow from Julius Plücker's Geissler tubes is the same 'something' that Robert Millikan saw in the oil-drop experiment and what "the Greeks knew [was the cause] of the attractive powers of amber" (Nola 2008, 194). While the terms used to express what this something was changed over the years, the same 'something' was being referred to. This means that "*despite radically changing theories*, scientists did talk about the very same thing" (emphasis added, 160). This strategy explicitly ignores all theoretical commitments that constitute each theory's understanding of that 'something' and instead insists that we can be realists about the electron due to its retention across many theory-changes (from the ancient Greeks to Quantum Mechanics). This is a weak form of realism. Without discussing the roles all these 'somethings' played in different theories, their ability to explain and predict phenomenon, conceptually fit into different theoretical and metaphysical landscapes, their use in experimentation, and so forth, Nola seems to be missing out on a large part of what all these different descriptions of these 'somethings' amount to. This comes along with the difficulty of isolating individual experiments or 'discoveries' from the contexts in which they have been made. A more detailed analysis of these contexts reveals greater amounts of differences between theories making the continuity that Nola seeks stretch itself thin and thus fade into irrelevancy. Glossing over vast differences between the various descriptions of the same 'something' for the explicit purpose of obtaining some tenuous similarity obscures more than it clarifies. By these standards, future scientific theories which directly compete with contemporary ones (i.e. even ones which do not agree about the properties or the nature of electrons) will *support* Nola's Ramsey-sentence realism simply because 21st century physicists were referring to 'something.'[49]

This suggests that the realist should avoid larger, more sweeping historical timeframes. The larger the timeframe, the more these historical investigations play into the hand of the anti-realist; that is to say that continuity seems more contentious the more ambitiously one wants to push the realist cause. Furthermore, larger timeframes require exploring larger scopes of sub-disciplines. Especially before the age of specialization, during periods in which philosophy, religion, and various other cultural features were intimately tied up with science, it becomes necessary to expand the scope of the investigation in order to account for the details of these interactions. However, more modest meta-inductions with more narrow timeframes and scopes are not subject to the PMI in the same way. Psillos' OMI on caloric is one example of a narrow meta-induction; it is focused on a fairly definite period of time and a particular entity and its role in two different theories. I do not want to oversimplify what it entails to provide an account of even a particular episode in history, or tracing a particular idea; many volumes could be written about caloric, its various theoretical conceptions, its roles in thermodynamics and Lavoisier's phlogiston theory, uses in experimentation, and the history of its development. However, these kinds of analysis pale in comparison to meta-inductions with much larger timeframes and scopes whose complexities and intricacies are drastically more multi-faceted. More broad meta-inductions gloss over large amounts of potentially important minutiae in order to provide a coherent presentation of what happened and what can be learned from a period of history.

[49] In other words, since Nola is explicitly indifferent about the character of particular scientific theories (past, present, or future), future theories could potentially be largely incommensurable with current theories and they would still be referring to the same 'something.' Given the vagueness of this commitment, it seems to boil down simply to the notion that scientists have always been referring to the external world; a point anti-realists do not deny.

I will return to the realist attempts (particularly Psillos') to reconcile the historical record with realism in the following chapter. For now, I simply want to point out that explanations of science that span larger periods of time are susceptible to the PMI in a more straightforward way than narrowly construed explanations. The scope of the meta-induction matters depending on what explanation is being given. This should become more apparent in the final section where I will explore the ways in which meta-inductions and realist or anti-realist explanations of science can be used in practice.

3. Why Do Meta-Inductions Matter?

I've thus far tried to suggest a few different ways in which meta-inductions may be adjusted. But why do we need so many different kinds of meta-inductions at all? I've already suggested that there may be philosophical reasons: they can be used to criticize or support a variety of positions which may be advanced. Theories which seek to explain the success of science must pass tests of history and which test they must past depends on how the theory is meant to be understood. This section will explore both meta-inductions and the theories they permit in practice. This will begin by arguing that realism and anti-realism form different *normative tendencies* depending on their relationship to the history of science. This will be followed by a case study of the Human Brain Project to illustrate both the malleability of meta-inductions and the conflict of differing normative tendencies.

3.1: Philosophy in the Social Sphere

There has been an explicit worry that the realist/anti-realist debate has run its course and is no longer a meaningful discussion.[50] Arthur Fine famously declared that realism was dead and

---

[50] See André Kukla (1994) for a discussion of this.

the future of philosophy of science involved finding a suitable replacement (Fine 1991, 261). I will return to this issue in the following chapter but, for now, I want to follow up on some of the *normative tendencies* of realism and anti-realism I have been discussing in this chapter and show how they may operate in social contexts.

Realists and anti-realists do not disagree about the content of the history of science. That is to say that philosophers do not argue about the 'brute facts' of what actually happened, but they do provide their own little twists or sets of emphases which lead to a characterization of history which can cause disagreements. Put another way, realists and anti-realists *value* different features of scientific practices. In this section, I will briefly skim over a few ways in which realists and anti-realists *tend to* make certain recommendations over others for ways in which science should move forward. The following section will provide an example of this in a bit more depth.

Fine sums up what he calls the 'small handful' argument for realism. He argues that at certain points, there are a few open possibilities for theories (or hypotheses) which will be successors to the current theory in place. Realists, according to Fine, argue that:

> Suppose that the already existing theories are themselves approximately true descriptions of the domain under consideration. Then surely it is reasonable to restrict one's search for successor theories to those whose ontologies and laws resemble what we already have especially where what we already have us well-confirmed. And if these earlier theories were approximately true, then so will be such conservative successors. Hence, such successors will be good predictive instruments; that is, they will be successor in their own right (Fine 1984, 87).

This certainly seems in keeping with realist expectations. We have more reason to be confident in scientific theories that converge across theory-change than those that don't. Future theories that contain electrons will, *ceteris paribus*, be preferable (to the realist) than a theory which makes a more radical departure by abandoning electrons without replacing them with anything similar. This makes conceptual continuity a reliable indicator of success on the realist account since it is what is needed to avoid the PMI. Furthermore, the explanatory virtues that go along with the approximate truth of scientific theories (such as coherency, depth, empirical adequacy, and so on) would also call for realists[51] to adopt or pursue theories that display these virtues better than competing theories (or potentially may display these virtues better). While different realists disagree about exactly which virtues are required for success (and their order of preference), there is still a general consensus that some combination of EVs is required for a scientific theory to be successful.

Anti-realists tend to focus on different values when it comes to accepting certain theories over others. Bas van Fraassen, for example, argues that "because the amount of belief involved in acceptance is typically less according to anti-realists,[52] they will tend to make more of the pragmatic aspects" (van Fraassen 1980, 13). Since, according to van Fraassen, scientists should be agnostic about whether their theories are true or whether they actually are referring to electrons or photons, they should not *fully* believe their own theories (81-2). This gives scientists (and critical outsiders) more wiggle room as to which theories we choose to develop and believe. Because of the differing stance van Fraassen takes on what science is doing (i.e. their aim is empirical adequacy, not approximate truth), his tendency to amplify pragmatic values[53] comes quite readily. Since the constructive empiricist is not concerned with the approximate truth of a scientific theory as a criterion for believing in it, she is freer to search for many different empirically adequate theories to satisfy a variety of pragmatic needs. This makes the epistemic issues of science quasi-pragmatic and not (at least entirely) dependent on the explanatory values

---

[51] This depends on which realist account we are talking about. Different realists tie different EVs to their accounts and place different weights on them. Putnam, for example, puts a strong emphasis on coherency whereas Psillos places a stronger emphasis on the ability to make novel predictions.

[52] This is because, according to van Fraassen, realists require scientific theories to be approximately true on top of empirically adequate.

[53] Van Fraassen details these values in a few different contexts. He, for example, argues that since explanation is not a real explanation of the world but an answer to a question, the value of that explanation rests on the pragmatic presuppositions. Things like the relevance of certain features in explanations are

which supposedly guarantee the approximate truth of a scientific theory. Van Fraassen writes "the epistemic merits a theory may have or must have to figure in good explanations are not *sui generis*; they are just the merits it had in being empirically adequate, of significant empirical strength, and so forth" (88). Epistemic merits[54] are subservient to pragmatic interests insofar as those merits lead to the construction of an empirically adequate theory. Without delving too deeply into van Fraassen's constructive empiricism and the normative implications it emphasizes, I simply want to present an example of how a different way of characterizing science can motivate different kinds of normative impulses.

The contrast between these values and anti-realists values is not a stark one. There is certainly overlap between the two positions and there is plenty of conceptual room for agreement. That is to say, there is no *a priori* reason why a realist couldn't adopt any particular pragmatic virtue and, similarly, there is no *a priori* reason why an anti-realist couldn't value convergence or some explanatory virtue (such as coherency or depth). That being said, there are clear tendencies the realists and anti-realists have towards making different suggestions about how to do science (i.e. choose between competing theories, be optimistic or pessimistic about the prospects of a theories empirical success, etc.)[55] Here, I suggest that there is a distinction between *strong* and *weak* normative preferences where realists tend to, *ceteris paribus*, prefer different courses of action from anti-realists.

A realist or anti-realist has a strong preference for making one recommendation over another if it is clearly motivated by methodological considerations. For example, anti-realists have no strong reason to value convergence. Laudan, for example, sees a lack of convergence

---

determined by the question being asked. This includes things like cultural and moral factors and the general context of the question being asked (van Fraassen 1980, 88-9).

[54] These are equivalent to explanatory virtues.

[55] Psillos (2000) explicitly claims that epistemic optimism is a feature of realism which distinguishes them from anti-realist forms of skepticism.

everywhere[56] and argues that there is no strong inference to be made between converging theories and approximate truth.[57] This also means that a continuity in ontology is not a strong reason to favor one theory over another. On the other hand, realists actively seek convergence in order to salvage their position from the PMI. Convergence, due to the methodologies of realism, is strongly valued by realists since it is more crucial to their position. While anti-realists do not completely discount the value of convergence, they tend to downplay its significance whereas realists highlight and stress its importance. Conversely, anti-realists tend to stress pragmatic virtues more than realists (see van Fraassen above)[58] even though the realist can still agree, in principle, that these are legitimate virtues. Similarly, realists attempt to provide lists of EVs which explain the success of science irrespective of its historical positioning. Anti-realists focus more on historical and contextual features which allowed for the construction of subsequent flourishing of scientific theories since any particular EV can be shown to be not necessary for the success of a scientific theory (as shown in section 2.2). While realists do not deny that these contextual features can be salient and anti-realists do not deny that a theory displaying EVs is, *ceteris paribus*, preferable to one that doesn't, they tend to emphasize different sides of the same coin.

This conflict becomes more apparent when the *ceteris paribus* qualification is removed. While there may be a great amount of agreement that coherence, simplicity, convergence, pragmatic virtues, etc. are all *genuine* virtues, there may be more disagreement when these values

---

[56] Laudan, at one point, claims that "I daresay that for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring" (Laudan 1981, 33).
[57] Chang (2003) advances a similar argument.
[58] Van Fraassen explicitly argues that realists tend to understate the role pragmatic virtues play in theory choice. He argues that "[i]t is a mistake to think that the terms in which a scientific theory is appraised are purely hygienic, and have nothing to do with any other sort of appraisal, or with the persons and circumstances involved" (88). The *priorities* of the realist, according to van Fraassen, are in the wrong place.

conflict and we must stress one over another. If it comes down to choosing a theory that is coherent and simple or a theory that is more pragmatically useful, realists and anti-realists will have methodological tendencies to favor one path over the other especially when the choice between the two alternatives is not clear. I hope to bring this point home in the following section where I argue that the Human Brain Project is an example of this: realist and anti-realist values clash precisely because there is no clear way to choose which path is best.

3.2: A Case Study in Meta-inductions

Meta-inductions are used quite frequently in regular parlance in order to justify the potential value of some scientific proposal (though they often are done quite informally). In this section, I want to provide one such instance in which meta-inductions have actively been employed. By doing this, I hope both to illustrate the malleability of meta-inductions that I have been discussing and show how the realist and anti-realist tendencies discussed in the previous section play out in a social context.

The Human Brain Project (HBP) is an extremely large collaborative project that aims to model approximately 1% of the human brain using supercomputers. After being announced in early 2013, the ten-year program, which incorporates over 80 partner institutions and 256 researchers (Kenall 2014), accrued approximately 1.2 billion euros[59] in funding and explicitly aims to "gain profound insights into what makes us human, develop new treatments for brain disease and build revolutionary new computing technologies" (HBP official website). Not only does the HBP aim to achieve these goals, it also aims to systematize peer-reviewed articles in a uniform manner and, as project figurehead Henry Markram has put it, to create a new paradigm

[59] There are also private revenue streams coming from companies such as Hewlett-Packard, GlaxoSmithKline, Olympus, amongst others.

about the brain. This would integrate all knowledge about the brain from the structures of ion channels in neural cell membranes to the mechanisms underlying conscious decision-making (Waldrop 2012). However, this project has come with a great deal of vocal resistance, with over 280 neuroscientists boycotting HBP funds (Curtis 2014). Neuroscientists[60] have argued that this project will not only be unable to attain the goals it has set out to accomplish, but also that the current state of neuroscience should not be systematized at this point in its development. Here, the computer scientists are the realists, optimistic about the potential fruits of the HBP, and the neuroscientists are the skeptics. Each use meta-inductions and emphasize their own set of virtues to ground their respective attitudes which has resulted in a strong disagreement about whether the HBP will be successful or not.

Computer science has steadily accumulated the ability to effectively represent and model neuron behavior. The HBP explicitly aims to build upon the Blue Brain Project, which successfully modeled approximately 30,000 neurons of the neocortical column. This project stood at the end of a steady and cumulative growth in the ability of model neural behavior. The challenges the HBP expects to meet involve creating larger and more powerful supercomputers to model approximately 4 million neurons and 1 billion synapses.[61] While this would require supercomputers with the ability to run 10^{18} operations per second and the invention of new virtual technologies (e.g. virtual fMRIs), Markram is confident that this increase in computing power is possible by 2023, given the consistent trend of computing power doubling every 18 months (Waldrop 2012).

Markram's optimism is, largely, realist. The cumulative growth of computer modeling provides strong reasons for suspecting that this growth will continue. Markram aims to unite the

---

[60] Not *all* neuroscientists, as some are optimistic about the potential fruits of the HBP.
[61] This would be a modest first step towards modeled the human brain which contains approximately 100 billion neurons and 100 trillion synapses.

methodologically and conceptually fragmented findings in neuroscience under a single paradigm. This is because Markram values the conceptual coherence and depth provided by a paradigm over the disconnected experiments done in contemporary neuroscience. Rather than engaging in these isolated experiments, which cannot plausibly be aggregated to cover the vast complexity of the human brain, we ought to "tease out some underlying principles governing their morphology and architecture" (Honigsbaum 2013). Markram's strategy of reverse-engineering (i.e. using supercomputers to run statistical simulations to predict how neuron clusters will combine and test these simulations against 'real data' from biology) involves various methodological assumptions about the nature of the brain[62] and disease and these assumptions are substantiated by the success of Markram's (and others) projects (e.g. Blue Brain wet labs, Steve Furber's work on asynchronous circuit designs,[63] and Karlheinz Meier's work on the physical grounding of information processing). Markram's theoretical grounding is well-established in particular bodies of literature and his confidence that the HBP will be able to deliver what it promises is justified by a cumulative historical trajectory, with no substantial theoretical 'breaks', and growth in the collection of data[64] and its assimilation into a Markramian framework. Put more philosophically, different theories and data collection has grown cumulatively for developing a method of modeling the brain and explaining its behavior. The HBP expects to continue on the upwards trajectory which computer science has been on in recent history that is there will be conceptual continuity from now until the completion of the project (i.e. the use of the same methods).

Neuroscientists, on the other hand, do not share in this enthusiasm. Rodney Douglas, co-director of the Institute for Neuroinformatics (INI), has argued that neuroscience is not ready to

---

[62] See Maass, Markram, and Natschläger (2002) for a more detailed description of this.
[63] Furber's theoretical commitments and the history of the evidence for his view is detailed in Furber and's *Principles of Asynchronous Circuit Design – A Systems Perspective* (2002).

be unified under one set of paradigmatic assumptions. Douglas argues that contemporary neuroscience *requires* variance; huge amounts of disagreement and multiple competing accounts. At least tentatively, neuroscience should proceed in a pluralistic fashion since there is not enough knowledge to justify the implantation of a particular paradigm over others. Given the immaturity of neuroscience as a discipline, Douglas argues that "we need as many different people expressing as many different ideas as possible" (Waldrop 2012) and this becomes increasingly difficult when funds and intellectual efforts become diverted from methodologically isolated efforts to explore sporadic and remote corners of the brain. Douglas' claims are grounded on the history of contemporary neuroscience. Many contemporary neuroscientists resist Markram's 'bottom-up' approach to understanding the brain (i.e. begin at ion channels and have the gaps in knowledge filled in the incoming data from the HBP) and instead use more approximate and abstract models of basic biological functions to explore higher-level brain functions (ibid).[65] Put in other terms, different methodologies have *not* converged onto a single explanation of the human brain in neuroscience. While this may be troublesome for realists, anti-realists are perfectly comfortable with this phenomenon. It is too soon to choose one method over its competitors since no particular method has stood out enough to warrant such a consensus. The massive complexity of the human brain, for these neuroscientists, cannot be overstated making attempts of constructing a unified theory of the brain premature and unwarranted for the time-being.

Both the computer scientists and neuroscientists use the history of their own subfields to ground their realist optimism and anti-realist pessimism respectively. The growth of computer

---

[64] This is Markram's 'big data' approach which involves both accumulating data about neuron behaviour and so forth as well as collecting and methodologically subsuming data about mental diseases from public hospitals and the proprietary databases of pharmaceutical companies (Honigsbaum 2013).

[65] This is not the only method. Some computational neuroscientists use models of individual neurons to explore higher-level brain functions as well (Waldrop 2012).

modeling has been cumulative, and is therefore likely to continue being cumulative,[66] while the ‘growth’ of knowledge in neuroscience has been non-convergent.[67] While I do not want to side with either position of the debate, I want to demonstrate two primary things: (a) illustrate some of the malleability of meta-inductions that I have been discussing and (b) different emphasis on particular virtues have led to differing opinions on whether to praise or denounce the HBP.[68] The computer scientist meta-induction is optimistic (i.e. it is an OMI) in that it expects the HBP to be successful given the cumulative growth in knowledge in computer modeling while the neuroscientific meta-induction is pessimistic given the pluralistic growth of knowledge in neuroscience. Furthermore, both meta-inductions use very short periods of time: the computer science meta-induction begins after the late 1990s when computer modeling of the brain started to take off and the neuroscientists refer back as early as the 1960s when many contemporary neuroscientific institutions began.[69] Proponents of neither position referred back to Pascal’s calculator or trepanning in ancient Egypt to bolster their historical grounding. If the timeframe of either meta-induction had been expanded to this extent, it would have diluted the force of both arguments.[70] Furthermore, neither meta-induction makes, or needs to make, larger claims about the ontological continuity (or lack thereof) in other branches of science (i.e. physics, chemistry, biology, etc.) of science as a whole. These meta-inductions were still productive since they aided in bolstering their respective optimism and skepticism but they did so without making larger

---

[66] Though it has been cumulative, it has not been convergent since the growth of knowledge in computer modeling has not ‘withstood’ any substantial theory-change.

[67] By this, I simply mean that disparate methodologies in neuroscience have not converged on any single methodology or understanding of the human brain.

[68] This has obvious implications for funding issues, policy matters, degrees of public support, etc. I will not discuss this here.

[69] For example, the International Brain Research Organization was founded in 1960 the International Society for Neurochemistry in 1963, and the European Brain and Behaviour Society in 1968.

[70] This is because there is a great deal of discontinuity throughout the history of computer science and its predecessors which would undermine their optimism and the current importance of methodological

claims about the nature of scientific knowledge. This shows a potential use of meta-inductions outside of a strict philosophical context.

The second purpose of this case study is to show the normative character of both meta-inductions in play. I discussed this briefly in the previous section: realists and anti-realists have different *tendencies* to emphasize certain virtues over others. This comes out a bit more clearly in this case study. Despite the fact that a computer science realist[71] could recognize the need for pluralism in neuroscience, they *emphasize* the cumulative growth of knowledge of computer modeling, as well as the coherence and depth[72] provided by a unified account over the need for pragmatic need for multiple competing methodologies. Computer scientists, like realists, are made more uncomfortable by having methodological and conceptually isolated attempts to understand the same phenomenon than neuroscientists. They would rather have a coherent and deep account which is admittedly incomplete (i.e. they would not be understanding the *entire* brain) than relatively free-floating methodologies with a lack of a definite direction. Neuroscientists, on the other hand, do not have a strong *theoretical* aversion to having a single paradigm of understanding the human brain, but, given the contemporary state of things, there are strong *pragmatic* reasons[73] for not prematurely subsuming all neuroscientific knowledge under a single conceptual framework. This makes Markram's desire for conceptual coherence for these

---

pluralism in neuroscience would be lost since it would lose sight of the current contextual features of neuroscience which dissuade a methodological monism.

[71] Here, I mean to distinguish one who is optimistic about the prospects of the HBP rather from some other kind of realism.

[72] By this I mean that Markram's account aims to uncover the 'fundamental structure of the brain' which is similar by what Mackonis means by 'explanatory depth' (i.e. science uncovers the basic structures that underlie the appearances of objects.

[73] Here, I mean 'pragmatic' in van Fraassen's sense where contextual factors due to social/political/economic etc. circumstances determine the aim of scientific explanations. In this sense, neuroscientists recognize their conceptual constraints given their location in history and this informs their research programs.

methodologically disparate experiments as well as his attempt to provide a deep explanation of the human brain unappealing given the overriding contextual factors.

While the division between computer scientists and neuroscientists is not a strict or rigid divide between realism and anti-realism, there is a difference in attitudes towards whether the HBP will be successful or not which results in a difference of recommendations for action. Slight variations on the emphasis of different virtues of scientific knowledge has resulted in a strong disagreement about what to do given the same set of data. This difference of attitudes takes on some of the semblances of the realist and anti-realist debate even if the whole philosophical package has not been bought by either side.[74]

Concluding Remarks

I have thus far attempted to expand on the PMI by showing how many different forms it may take. While Putnam and Laudan's PMIs share the general form of skepticism as Sextus' proto-PMI, there are many other ways in which the PMI may be conceived. By changing the historical methodology, the target, and the scope and timeframe of the PMI, the implications alter drastically. I then hoped to show why these meta-inductions are valuable outside of a strict philosophical context and illustrate their roles in a social context while elucidating their character in their use in the HBP case study. Throughout this chapter, I have been hinting at what these observations entail for the realist/anti-realist debate. In the next chapter, I will dive into more detail on this issue and use the malleability of meta-inductions to shed some light on the strengths and limitations of the various forms of realism present in the contemporary literature.

[74] For example, neither computer scientists nor neuroscientists make claims about whether their unobservable entities actually exist.

Chapter 2
Explanation, Evaluation, and Scientific Realism

Preamble

The PMI has seen several different kinds of responses since its inception, with some attempting to reformulate realism so that it avoids the PMI altogether.[75] This chapter will begin by chronicling the transitions between three distinct waves of realism. I start (1.1) with an exposition of J.J.C. Smart's realism (1963) and the important conceptual shift towards the explanationist defense of realism (EDR) of Hilary Putnam and Richard Boyd (which will be described in 1.2). I then (1.3) proceed to describe the transition between this EDR to the final wave of realisms, which I call 'selective realisms.' Selective realists are more cautious and modest in their outlook but concede that the PMI has at least some force. This more compromising position allows for the retention of realism but only in limited areas. I will proceed to outline two different kinds of selective realism. The first involves the strategy of methodologically restricting the legitimate inductive base of realism. I detail (2.1) this strategy through an exegesis of Worrall's structural realism. The second kind of selective realism involves being particularistic about when realist principles ought to be applied. This strategy is outlined (2.2) through an exposition of Stathis Psillos' account. The final section of this chapter is critical of selective realism and of explanationist defenses of realism in general. This is because explanationist realism is ultimately susceptible to what I call the "dilution problem", which I will describe in (3.1). I go on (3.2) to address the response of Psillos and argue that explanationist realism turns out to either be trivial or simply false.

---

[75] See Doppelt (2007) for an example of this.

1. Realism: Then and Now

Before beginning, it is worth stressing the fact that realism has been articulated in many different ways; each containing its own strengths and weaknesses. Proposals of realism have been put forth by Clyde Hardin and Alexander Rosenberg (1982), Ernan McMullin (1984), Frederick Suppe (1989), Robert Almeder (1992), Crispin Wright (1992), Philip Kitcher (1993), James Brown (1994), Jarret Leplin (1997), André Kukla (1998), and others.[76] There is minimal epistemic realism, ontic structural realism, Ramsey-sentence realism, entity realism and even Heideggerian scientific realism.[77] These positions partially overlap, retaining particular elements of each other while modifying, recasting, or completely rejecting others. As such, I do not expect to fully address the huge spectrum of kinds of realism. In this section, I merely want to provide a schematic narrative of the development of the explanationist defenses of realism (both general and selective), which remains central to a family of commonly held positions in the contemporary literature. I will begin with a brief overview of Smart's conception of realism, which provides an important historical antecedent for what explanatory forms of realism draw from and reject.

1.1: Smart's Realism or an a priori *Epistemology of Science*

Smart conceives of realism as a part of the larger project of philosophy: outlining a coherent world-view that seeks to understand "what there is in the world and what we ought to do about it" (Smart 1963, 1). That is to say, Smart thinks that distinctly philosophical considerations[78] (which are not themselves amenable to empirical tests) are required in order to

---

[76] A fuller list can be found in Psillos (2000).
[77] This position is outlined in Rouse (1981).
[78] Smart understands philosophy as thinking *both* clearly and comprehensively about the nature of the universe and its principles of conduct. The former aspect "ties up with the prevailing conception of philosophy as linguistic or conceptual analysis, and the latter ties up with another conception of philosophy

provide conceptual clarity for a proper interpretation of science. Smart is not making claims about what science historically has done, but what science *is* and its relationship to the natural world. Smart's argument relies on intuitive judgments as to what scientific inquiries are and must be in order to succeed. Any rational being would be compelled[79] to become a realist through well-reasoned analysis and deliberation. This is ethically required,[80] for Smart, since our decision to be realists is a part of obtaining a synoptic view of the world (i.e. of reaching the truest possible account of the natural world and our place in it). Realism is a part of finding the place of science within a coherent world view and thus requires a philosophical interpretation of what science *does*. This makes realism a product of our rational reflection and therefore a piece of our larger *a priori* view of what science is.

For Smart, there are two primary competing accounts for what science is and how it describes the fundamental features of the natural world: phenomenalism and his own brand of realism. The former argues that unobservable entities, such as electrons and photons, do not exist in an analogous way to chairs and plants and other macro-sized objects, while the latter is grounded primarily on the argument from 'no cosmic coincidences.' This section will outline the phenomenalist position and why Smart believes, because of his own positive argument for realism, that it is mistaken.

Smart argues against phenomenalism or the view that "statements about electrons [and other non-observable terms] are only of instrumental value: they simply enable us to predict phenomena on the level of galvanometers and cloud chambers" (Smart 1963, 39). Smart divides this position into two distinct sub-positions: the 'extreme view' that argues that propositions

---

as the rational reconstruction of language so as to provide a medium for the expression of a total science" (1-2).

[79] Though not logically compelled since any theory can be made logically consistent although it may be implausible.

[80] Smart details this account in chapter 8.

about unobservable entities must be logically tied to propositions about macro-sized object or else they are 'meaningless' (i.e. unverifiable)[81] and the more moderate position that argues that unobservable entities are simply useful fictions for explaining and predicting phenomenon and are not 'real' in the same way that tables and chairs are. The extreme view Smart takes to have been sufficiently addressed by Frank Ramsey (1929) and R.B. Braithwaite (1953); science must be more than empirical facts and generalizations about those facts or else it would not be able to make predictions about future undiscovered facts.[82] The moderate position is more subtle; it does not argue that electrons must be reduced to logical constructions derived from propositions about macro-sized objects but that electrons do not have the same ontological status as them (i.e. they are not 'real' in the same sense). This is not to say that electrons are not real in the same sense as unicorns are not real, but that electrons are to be understood as useful fictions like lines of force.[83] Electrons make sense of observed effects (i.e. in galvanometers and cloud chambers) by the role they play in scientific theories. Put another way, the existence of electrons is "understood solely in terms of microscopic *concepts*" (35). The concept of an electron exists only as a useful fiction; a necessary tool for understanding observable phenomenon. But electrons themselves do not exist the way that seagulls and paper clips do; the concept of 'electron' exists solely because of *theoretical* considerations of scientific theories. Furthermore, these theoretical terms (or concepts) can be *systematically eliminated* by means of Craig's theorem: since it is mathematically possible that theoretical terms can be replaced by observational terms and retain the same set of

[81] This is the view expressed by Carnap. He argues that "there [is] a certain rock bottom of knowledge, the knowledge of the immediately given, which [is] indubitable. Every other kind of knowledge [is to be] firmly supported by this basis and therefore likewise decidable with certainty" (Carnap 1963, 57).
[82] If theoretical terms are just logical constructions they cannot be 'extended' to accommodate future facts. See Smart (1963) p. 28 for a fuller account of this.
[83] Lines of force, for Smart, are simply necessary to provide a 'useful picture' to understand the distribution and directions of forces exerted on electrical charges. These are a 'different kettle of fish' from electrons (34).

consequences, the concepts of 'electrons' and 'photons' could be entirely replaced by the equivalent observational terms.

Phenomenalism opposes Smart's position that the ontological status of unobservable entities is analogous to the ontological status of macro-sized objects. This makes electrons just as real as paper clips. Leaving aside idealism as a plausible theory,[84] Smart argues that phenomenalist views of this sort cannot be right because they rely on 'cosmic coincidences' or some remarkable and accidental fit between our instruments of understanding and the natural world. In order for the phenomenalist account to be true, according to Smart, a whole host of ontologically disconnected unobservable entities only coalesce by means of some overriding instrumental theory. How could electrons, protons, quarks, etc., interact with each other in the way scientific theories say they do on a phenomenalist account since there is no ontological relationship between them? All of these entities *happen to be related* in certain ways such that some purely instrumental theory can effectively navigate them. This requires a gigantic coincidence (or set of coincidences) that this instrumental theory is able to explain all these entities which bear no ontological relationship to one another. In Psillos' words, "[a]ccepting the vast number of purely instrumental connections implied by the Craig-style theory exceeds the limits of tolerance" (Psillos 1999, 72). Realism, on the other hand, does not leave these coincidences unexplained: all these unobservable entities are ontologically connected since they all exist equally. This is a part of Smart's *a priori* approach to realism; realism is more plausible than phenomenalism since it does not require cosmic coincidences.

Smart posits this argument quite quickly in *Philosophy and Scientific Realism*, but he clarifies it in his later writings. He does so by addressing van Fraassen's criticism that the cosmic

---

[84] Smart is explicitly dismissive of idealism. He writes "[s]urely we need not fall back onto Berkeley and suppose that *esse* is *percipi*" (38). On the contrary, Smart argues, following Mill, that matter exists as a "permanent possibility of sensation" (17).

coincidence argument parallels Aquinas' arguments for the existence of God. In the same way the theist posits the existence of God to explain the seemingly designed nature of reality, the realist posits the electron to explain the regularities at the level of observable phenomenon. The two strategies, according to van Fraassen, are analogous; both God and electrons explain the way the world appears to us in experience. Why can't, van Fraassen asks, the philosopher of science rest comfortably with the fact that scientific theories are only empirically adequate without an added explanation, in the same way the atheist is comfortable with the fact that there is no further explanation behind nature as a cause for its appearance? Smart responds that his 'no cosmic coincidences' argument actually finds strength in the refutation of this argument; the posit of 'God' increases the complexity of our ontology while the positing of electrons decreases it. This ties the cosmic coincidence argument into Smart's desire for ontic simplicity, which comes in tandem with his adherence to Occam's razor[85] and his insistence that the laws of physics produce regularities (i.e. describe the laws of the universe).[86] Smart's positive argument for realism, then, is that realism is the most simple and plausible position that makes the success of science sensible. This ties into his earlier arguments since the phenomenalist who posits that ontologically disparate entities happen to coincide to make purely instrumental theories true requires a much more ontologically complicated argument to make sense of the uniformities on the external world.

The naturalistic account of Putnam and Boyd, which I will outline in the next section, incorporates a version of Smart's no cosmic coincidences argument in its defense of realism,

---

[85] Smart explicitly endorses this principle as a "familiar maxim not only of philosophical method but also of scientific method" (11).

[86] Smart argues that "[t]he postulation of "theoretical entities" and of cosmic uniformities (however in a sense "accidental") seems to me to produce a simpler metaphysical picture than does the non-realist's array of observational generalisations or even non-generalisations" (Smart 1985, 280).

while rejecting the *a priori* characterization of the realist position. This departure leads to an importantly distinct account of realism.

1.2: The Explanationist Defense of Realism: The Putnam-Boyd Account

This section will explain two features of Putnam and Boyd's portrayal of realism. First of all, I will explicate the 'no miracles argument' for realism and articulate its similarities and differences from Smart's cosmic coincidences argument. Secondly, while I have already begun to describe Putnam's conception of realism in the previous chapter, I will now delve further into his and Boyd's defense of realism as an *empirical hypothesis* or an *explanation* of the empirical fact that science is successful at describing the observable constituents of the natural world.

Putnam's 'no miracles argument' (NMA) has become one of the more powerful and influential arguments for realism.[87] Putnam argues that one of the "typical arguments against idealism[88] is that it makes the success of science a miracle" (Putnam 1978, 18). Already, we can see a strong resemblance between Putnam's NMA and Smart's 'no cosmic coincidences' arguments. Both suggest that instrumentalist arguments rely on remarkable flukes of fit between the natural world and our theories of it. However, Putnam grounds his NMA quite differently from Smart. Whereas Smart grounds his argument on the value of ontological simplicity, Putnam grounds it in the proven ability to *predict* novel phenomenon. It is quite easy to provide *ad hoc* devices to explain data that have already been gathered; I could posit that the glow emitting from Hittorf's cathodes was actually caused by microscopic and undetectable pirates whose hooks radiate due to decreases in gas pressure. But it is something else entirely to posit these pirates, with particular properties, in order to make various novel predictions of empirical phenomena. It

---

[87] Van Fraassen (1980) even refers to the NMA as the 'ultimate argument' for realism.
[88] The argument is extended to forms of instrumentalism as well.

wouldn't be a miracle if my pirate theory became debunked after being tested in a variety of novel experiments, or if it failed to make sense of empirical phenomenon that have not been discovered yet, but it would be a miracle if something approximately like electrons didn't exist and the many theories which make use of electrons were not approximately describing some feature of reality. This characterization of the NMA has been endorsed by the majority of contemporary realists[89] and is distinct from Smart's conception. Since Smart's 'no cosmic coincidences' argument is grounded in the *a priori* value of ontological simplicity, the historical track-record of science is irrelevant to its force. Putnam's NMA is only forceful because science has actually succeeded in making many true predictions. If science had a poor truth-tracking record, the NMA would lose its strength. However, the similarities are also plain, making the continuity between Smart and Putnam's arguments readily apparent. This makes both positions a similar form of realism. Now, I want to turn to the important differences between Smart and Putnam-Boyd's accounts.

Smart's positon was that realism ought to be construed as an *a priori* thesis; something that explains the place and intelligibility of science. Putnam and Boyd's account, however, is an *a posteriori* thesis. That is to say, it takes the success of science as a given empirical fact that demands explanation. Putnam writes "[t]hat science succeeds in making many true predictions… is an undoubted empirical fact… [i]f realism is an *explanation* of this fact, realism itself must be an over-arching scientific hypothesis" (Putnam 1978, 19). Realism, here, ceases to be an *a priori* position but is instead dependent on the actual success of science and on its ability to explain that success. In Boyd's words, the fact that science is successful is a radically contingent fact. He argues, "the scientific realist must deny that the most basic principles of inductive inference or justification are defensible *a priori*. In a word, the scientific realist must see epistemology as an

[89] Psillos (1999), Worrall (1989), Leplin (1997), to name a few.

empirical science" (Boyd 1983, 71-2). This is the 'explanationist defense of realism' (EDR); the *a posteriori* view that begins with the fact that science is successful and this fact is something which requires explanation. Realism, for Putnam and Boyd, is the best explanation of this fact.[90]

This represents a fundamental shift in the conception of realism. The strength of realism is now completely dependent on whether or not it is the best explanation for the *actual* successes of science. If it weren't for Plücker's experiments with Geissler tubes, Hittorf's Cathode rays, Crooks' 'torrent of charged molecules' theory, and so on we would not be justified in believing that electrons are real. However, there remains a similarity between the EDR and Smart's realism; the EDR is an *over-arching* or *general* thesis about scientific knowledge as a whole. While Putnam explicitly claims that his realism is a general position about science (Putnam 1978, 20-1), this can be seen in a few other ways in how this account has been developed. Boyd defines scientific realism as consisting in four theses:

> (i) "Theoretical terms" in scientific theories (i.e., non-observational terms) should be thought of as putatively referring expressions; scientific theories should be interpreted "realistically".
> (ii) Scientific theories, interpreted realistically, are confirmable *and in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.
> (iii) The historical progress of mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories.
> (iv) The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments (Boyd 1983, 45).

(i) distinguishes realism from instrumentalist views which see theoretical terms as non-referring. (ii) distinguishes Boyd's position from Smart's since it effectively makes realism an empirical hypothesis whereas Smart argues, according to Boyd, that "distinctively philosophical

---

[90] There have been accusations that this strategy, or making an 'inference to the best explanation', is a circular argument since it begs the question (Fine (1986)). I will not address this debate here.

considerations[91] are required, over and above ordinary standards of scientific evidence, in order to justify our acceptance of the theoretical claims of scientific theories" (46). (iii) distinguishes realism from Larry Laudan's anti-realism and makes opposing historical claims about the development of scientific knowledge and (iv) distinguishes realism from Kuhnian positions that see reality as inherently dependent on the lens of a paradigm or world view.[92]

However, in distinguishing realism from these various other general positions, realism itself is portrayed as a *general* thesis about science as a whole. None of these theses are specific about *which particular* scientific theories[93] are confirmed by evidence or which theoretical terms should be interpreted realistically. For Boyd, "particular instances of scientific reasoning are sound just in case the background theories upon which they are based are themselves approximately true and the approximate truth of such theories is itself an *a posteriori* matter" (Boyd 1989, 13). Scientific methodologies and particular applications of them in experimentation, for Boyd, are profoundly theory-dependent. In order for particular claims to be approximately true, the background theories that guide the application of the methodologies that lead to those claims must also be approximately true. This is because particular scientific claims are profoundly dependent upon background scientific methodologies which makes their approximate truth indicative of the approximate truth of the larger framework that claim is embedded in. Background scientific theories which are continuously able to produce approximately true individual claims about the external world, must themselves be latching on to some larger features of reality. This means that the truth of individual claims is only made sensible through the truth of the background theories. This makes realism a thesis about our best background

---

[91] By distinctively philosophical considerations, Smart means non-evidential claims.
[92] Boyd is referring to Kuhn's remarks that scientists literally see the world through their paradigm as a form of gestalt switch (see chapter 10 of *Structures of Scientific Revolutions* for a detailed account of this).
[93] Both Putnam and Boyd do limit realism to mature theories but not to, say, electromagnetism or cell biology, electrons or molecules, etc.

theories and their ability to make predictions. This makes scientific realism a *general* hypothesis as well as an *a posteriori* one.

Despite the fact that EDR is an empirical hypothesis, it does not attempt to reconcile the historical record with realism. Rather, Putnam and Boyd's efforts are devoted to developing the conceptual apparatus of realism and not to showing that the history of science actually meets this particular conception.[94] For example, Boyd writes that "[b]ecause scientific realists hold that progress in mature sciences is a reflection of theoretical as well as instrumental progress and, indeed, that instrumental progress often depends upon theoretical progress, it is essential to the empirical case for realism that historical evidence support the claim that there is the relevant sort of semantic and methodological continuity in the history of mature sciences" (Boyd 1983, 87). This means that *historically* succeeding scientific theories are not incommensurable with their preceding theories.[95] Scientific theories, for Putnam and Boyd, are cumulative, since they grow in instrumental reliability and scope. What made past scientific theories approximately true is retained in succeeding theories as a limiting case.

This ties into Putnam's 'principle of the benefit of the doubt', a vital part of his causal theory of reference, which states that past theories refer to roughly the same entities as do current theories. That is to say that "we can assign a referent to 'gravitational field' in Newtonian theory from the standpoint of relativity theory; a referent to Mendel's 'gene' from the standpoint of present-day molecular biology" (Putnam 1978, 22). Past theoretical terms are seen as constrained versions of contemporary theoretical terms, referring to approximately the same entities, making the ontological continuity unthreatened by theory-change. These are a few of the features of what the realist claims the history of science are like. In other words, "[i]n so far as a realist perspective

---

[94] Both Putnam and Boyd use do make reference to historical examples (Putnam, for instance references the transition from Newton's Universal Gravitation to Einstein's relativity) but they are used only as quick illustrations (not as evidence).

proves fruitful in understanding the history of mature sciences, that would provide further evidence for realism, but the primary role of historical studies in this area is to subject the claims of realists to possible disconfirmation by historical evidence" (Boyd 1983, 88). If Putnam's theory of reference or Boyd's cumulative approach to history turn out to be inaccurate depictions of the history of science, then the Putnam-Boyd thesis fails.

The PMI disrupts the plausibility of this thesis as explaining *all* of science, since there are many historical counterexamples to this thesis. The malleability of the PMI further disrupts the possibility of making slight modifications of the conceptual apparatus of a *tout court* form of realism in order to make it workable. The new face of realism has taken note of this difficulty and responded by making a few concessions to Putnam and Boyd's version of EDR. In this next section, I will explicate what this transition consists in and present the new wave of scientific realism.

1.3: Giving up on Generality: Selective Realism

While some have argued that the PMI is not even a valid argument (see Marc Lange (2002) and Peter Lewis (2001)),[96] many realists take the PMI very seriously; that is to say, it has at least *some* force. The goal of the post-PMI realist project is to retain two features of the Putnam-Boyd formulation of realism: (i) that realism ought to be an empirical hypothesis, or an explanatory project, since the success of science is a contingent fact of the history of science and (ii) some watered down version of the NMA[97] that is not susceptible to the thrust of the PMI. Some have attempted to remove individual examples from Laudan's historical gambit one by one, claiming that they are not genuine counterexamples to the Putnam-Boyd thesis. However, most

---

[95] Assuming that the preceding theories were approximately true.
[96] I take these arguments to have been sufficiently addressed by Saatsi (2005).

have conceded that this project is not going to save realism as *tout court* thesis.[98] At least some of Laudan's counterexamples are genuine. On top of this, the malleability of the PMI outlined in the first chapter amplifies the seemingly endless stream of counterexamples that could potentially be proposed. The predominant approach, which I will detail here and explore more deeply in section 2, has been to accept a partial defeat: realism cannot explain the success of science across the board. Realism, in other words, has given up on being an over-arching argument in the way Putnam and Boyd wanted and must be selective in its commitment.

Rather than assuming that convergent or cumulative growth of scientific knowledge exists through all instances of theory-change in a uniform manner, selective realists now characterize themselves as 'epistemic optimists.' This distinguishes them from van Fraassen's agnostic instrumentalism, which is agnostic about whether scientific theories deliver truth or not, and sceptics or pessimists who do not see science as largely progressive. Laudan's list, on the selective realist view, is not wrong, but its conclusions are overstated.[99] This more modest position allows the realism to retain most of the conceptual apparatus of the Putnam-Boyd thesis while admitting some wiggle room; there may be some exceptions to the thesis, but realism still provides the rule more often than not. The job of the realist then becomes to do some historical dirty work and attempt to reconcile the history of science with the realist interpretation of the success of science.

This downgrade has been primarily caused by two key recognitions: first of all, that Laudan presents some genuine counterexamples to the realist thesis. This makes a *tout court* or overly general form of realism seem like a lost cause. The second recognition is that the realist position is left vulnerable and implausible if there is no historical work done to show how realist

---

[97] Some realists drop the constraint that the central theoretical terms must refer while retained the NMA exclusively in terms of the ability to make novel predictions (Leplin (1997) and Psillos (1999)).

[98] Kitcher (1993) and Psillos (1999) for example.

conceptions of approximate truth, reference, and convergent or cumulative growth actually fit with the realist explanations. This has resulted in realists being more selective as to which aspects of science we ought to be realists about. However, there are two distinct ways about going about this: (a) be selective by adopting some methodology that determines the 'proper' inductive base of which realism is an explanation or (b) be realists about particular theories, theoretical terms, or subfields of scientific inquiry. The next section will explore influential attempts of both (a) in work by John Worrall and (b) in work by Stathis Psillos.

2. Two Kinds of Selective Realism

Selective realism grew out of Putnam-Boyd arguments as a result of the PMI. This marks the beginning of trying to not only provide a philosophical explanation for the success of science, but also recognizing when and where these explanations are limited or even completely inappropriate. This has led to more historical investigations that have attempted to develop and collect examples relevant particular conceptions of realism. A few realists have engaged in these kinds of projects, but for now I want to outline two kinds of strategies advanced by selective realists. While I will use Worrall and Psillos as my representatives of these strategies, there are other philosophers who have developed similar arguments and positions.[100] This section will reconstruct the arguments of Worrall and Psillos and show how both advance distinct kinds of selective realist theses.

[99] Devitt (1984) and Psillos (1999) take this view.

2.1: Worrall's Structural Realism

Worrall's structural realism is an epistemic refinement of traditional scientific realism.[101] He is torn between the intuitive power of the NMA for realism and the equally intuitive PMI for anti-realism. As for the NMA, Worrall agrees with Putnam's contention that science must be basically getting things approximately right in order for it to be as successful as it is. After all, "quantum theory, like the 'Lamb shift', gets it right to…6 or 7 decimal places…only a philosopher, overly impressed with mere logical possibilities, could believe that this is compatible with quantum theory's failing to be a fundamentally correct description of reality" (Worrall 1989, 101). On the other hand, Worrall admits that Laudan's historical gambit (or instances of 'Kuhn-loss') presents genuine problems for the realist thesis. Mature theories have, at times, been radically revised and fundamentally altered in such a way that it would be over-charitable to suggest that there is always substantive realist continuity across theory-change.[102] For Worrall, "a really satisfactory position would need to have both arguments on its side" (Worrall 1989, 101). In this sense, he *accepts* the consequences of the PMI and that "realists' sentiments evaporate… [due to] the phenomenon of scientific revolutions" (103).[103]

Worrall's response to Laudan's historical gambit is quite intriguing; he argues that the legitimate inductive base from which realists ought to confine themselves to is to be established by an *independent criterion*. Putnam and Boyd already have provided such a criterion in terms of

---

[100] Ladyman (1998) and Ladyman and French (2003), for example, have taken Worrall's strategy on board although they have made some conceptual alterations to it (i.e. they have turned Worrall's *epistemic* strategy into an *ontic* one). Kitcher' account is also quite similar to Psillos' (see fn. 34).
[101] I am using Ladyman's (1998) interpretation of Worrall here.
[102] He criticises Hardin and Rosenberg for attempting to retain realism across radial theory-changes of this sort (116-7).
[103] He acknowledges that 'fundamental shifts' have occurred in the history of science. He provides his own example of the transition between Newton and Einstein and in 19th century wave optics (107-9).

‘mature sciences’, a criterion that is, admittedly, left underdeveloped.[104] Furthermore, since maturity is vaguely defined, their criterion “naturally engendered the suspicion that the realist has supplied himself with a very useful *ad hoc* device” (113). The independent criterion should not be used to filter the history of science so that counterexamples are defined away. In light of this, Worrall aims to provide his own independent criterion of ‘structure’ (which is contrasted with observational content) which is consistently retained across theory-change. This allows him to retain the NMA while avoiding the PMI making structural realism ‘the best of both worlds.’ This choice of criterion is grounded in Worrall’s analysis of the transition between Fresnel’s luminiferous aether and Maxwell’s electromagnetism.[105] He argues that while Fresnel misidentified the *nature* of light, he was right in identifying the structure (understood as formal mathematical equations)[106] of optical phenomenon more broadly construed. Following Poincaré, “Fresnel’s object was not to know whether or not there was an ether, if it is or not formed of atoms, if these atoms move in this way or that; his object was to predict optical phenomena” (Poincaré 1905, 162). Despite the radical changes at the theoretical level (i.e. at the level of the description of unobservable entities and the underlying mechanisms behind empirical phenomena) at the level of mathematical structure there is a great deal of continuity, on which Worrall capitalizes to support his thesis.

While the details of Worrall’s definition of structure and what distinguishes it from observational content may be contentious,[107] what is of current importance is the general aim of the strategy: accept the PMI as a refutation of a *tout court* realism and methodologically reduce

---

[104] Worrall himself tries to tie the notion of maturity to the ability to avoid post hoc explanations and make novel predictions (114).
[105] Worrall argues that this position is further supported by the continuity of the ‘correspondence principle’ in the history of physics. He details this account (120) and cites Boyd (1984) and Zahar’s (1983) work to further corroborate this.
[106] Worrall develops this point from 119-21.
[107] See Psillos (1999) for a criticism of this.

the inductive base that realism is meant to explain.[108] This means that in order for Worrall's structural realism to be true there must be retention of a consistent kind of structure across all theory-changes. In a sense, Worrall's restriction of realism is similar to Putnam and Boyd's, when they restrict realism to only mature theories. The goal of this breed of selective realism then, is to find the proper inductive base (i.e. with great deals of continuity), on which realists can comfortably rest. Worrall's realism is 'selective' in that it chooses a preferred methodological base, structure, and then relegates realism to that terrain; Worrall's position might also be seen as a *tout court* realism with a smaller inductive base, a point to which I will return critically in section 3. For now, I want to have highlighted one way in which selective realism operates. In the following section, I will describe the alternative strategy of Psillos.

2.2: Psillosian Selective Realism

Psillos makes similar concessions to the PMI as does Worrall and also retains a similarly narrowed form of realism. Like Worrall, Psillos believes that the NMA provides the strongest argument for realism. He defends the NMA as an inference to the best explanation: scientific theories that refer to unobservable entities that really exist, that have the relevant explanatory virtues and that make novel predictions tend to generate approximate true descriptions of the world. However, Psillos' positive strategy for realism differs from Worrall's in that he does not aim to provide an independent criterion for narrowing the inductive base of realism, but instead, rests his realism on whatever instances happen to confirm the realist framework. These may be structural or not;[109] Psillos is agnostic as to what will be (and has been) retained across theory-

---

[108] To clarify, structural realists must explain why science is successful through the retention of structure across theory-change.

[109] Psillos specifically criticizes Worrall's optimism that structural features are consistently retained. This is due to the fact that even if the formal equations are retained, the physical interpretation of these equations differ drastically (1999, 147). He discusses Worrall at length in chapter 7.

change. Realism, then, is a *result* of historical investigations and it would be “far too optimistic – if at all defensible – to claim that *everything* the theory asserts about the world is thereby vindicated” (Psillos 1999, 80).

This has led Psillos to downgrade his realism to a local thesis held cautiously, and one that “acknowledge[s] the existence of failures” (ibid).[110] Rather than talking about the success of science in general, realists should focus on explaining the successes of particular theories. This means refining the Putnam-Boyd thesis between the approximate truth of a theory and its empirical and predictive success. That is to say that the fact that particular theories are predictive and empirically successful should be explained by their approximate truth. This brand of ‘local realism’ is wedded to Psillos’ *divide et impera* strategy which involves “identify[ing] the theoretical constituents of past genuine successful theories that made essential contributions to their success and show that these constituents… have been retained in subsequent theories of the same domain” (110-1). When scientific theories are rejected they are often not rejected *en bloc* but by piecemeal, realists ought to commit themselves to those constituents that are retained in our current scientific image. This involves distinguishing between the ‘idle’ and ‘essential’ constituents of scientific theories.[111] The essential constituents contribute to the empirical success of a past theory while its idle constituents were abandoned by successor theories as being false and not necessary for their success. This means that “if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of

---

[110] Here, Psillos is referring to the failures of scientific theories to be empirically success for but not approximately true.

[111] Psillos’ distinction, and general strategy, are extremely similar to Philip Kitcher’s distinction between the ‘presuppositional’ and ‘working’ posits of scientific theories. However, Psillos’ distinction is meant to “capture how the successes of a theory can differently support its several theoretical constituents” (111) while Kitcher’s distinction is meant to capture the difference between referring and non-referring terms. This is developed more fully in Psillos (1999, 111-2).

scientific realism can still be defended" (108). If this is not the case, then Psillos is happy to admit that realism would not be warranted in such a circumstance.

Psillos not only articulates his *divide et impera* strategy abstractly, but he also puts it into practice. This can be seen most forcefully in his historical analysis of the transition between the caloric theory of heat and thermodynamics. Here, Psillos carefully delineates the evolving and differentiated degrees in evidential support for various features within the transition (i.e. for heat as a material substance, law of propagation of sound, the Carnot cycle, and so forth).[112] This has rested on the premise that it is "both in principle and practice possible to *localize* the relations of evidential support, and show which parts of a theory are supported by the evidence at hand, or at any rate, which parts are better supported than others" (Psillos 1994, 179). While this characterization may seem somewhat ad hoc, Psillos also shows how scientists at the time (specifically Antoine Lavoisier, Joseph Black, and John Davy, to name a few) also viewed the transition this way without the advantage of hindsight: "[M]ost of the eminent theorists of the caloric theory had precisely this differentiated attitude towards the several claim of the theory, and a very cautious attitude towards the hypotheses that did not enjoy strong evidential support" (180). The features that enjoyed strong evidential support, and played important roles in the predictive power of the caloric theory (the laws of calorimetry, adiabatic change, and Carnot's motive power of heat) were retained in thermodynamics and our current scientific views. Despite the fact that caloric as a fluid, of inappreciable tenuity, whose particles are endowed with indefinite ido-repulsive powers,[113] did not exist and therefore the theoretical term 'caloric' did not properly refer, there still were elements of the caloric theory of heat about which we can be

---

[112] This can be seen in Psillos (1994) p. 162-87. It can be seen in a visual layout in his flow charts (see Schemas 1-3).

[113] This is from the definition in Ure's (1820) *Dictionary of Chemistry*, see p. 251.

realist, since they were retained in thermodynamics. Taking a closer look at caloric shows that this is not a straightforward counterexample to the realist thesis.[114]

This presentation shows Psillos' commitment to a local and detailed formulation of realism. While realism may be plausible in the cases of caloric and the luminiferous aether (the two cases for which he provides a historical analysis), it may not necessarily be similarly or equally plausible in other areas of science. Put another way, the *divide et impera* strategy produces results for the realist and itself does not make any grand claims about the nature of scientific progress as a whole. Rather than pursuing an indiscriminate realist picture of science as a whole (or specific slices of science, as does Worrall), Psillos restricts his realism to what the historical record supports. He even goes so far as to discriminate between different *degrees* of realist belief in different entities. He wholeheartedly agrees with Alan Musgrave's contention that "[w]e should be more confident about atoms and molecules than we are about electrons, and more confident about electrons than we are about quarks and gluons" (quoted in Psillos 2003, 4).[115] This leaves Psillos standing firmly against generalized accounts of realism, accounts that stand independent of the messy historical and conceptual work that comes along with selecting those scientific theories about which we can plausibly be realist. However, rather than selecting a proper terrain for realism, like Worrall, Psillos is selecting any aspect of scientific practices that can support a realist thesis.

Psillos' view does not come without its share of interpretive difficulties. For instance, from time to time, he makes general and somewhat sweeping remarks that science is "by and large true" (Psillos 2011, 18) or makes generalized claims about the 'rationality (or methodology)

[114] Laudan includes caloric in his historical gambit.
[115] This 'guarded realism', for Musgrave (and Psillos) involves making such differentiations due to the fact that "some of them are better supported by evidence than others [or] some of them play an indispensable explanatory role, while others do not [or] some contribute to the success of theories, while others do not" Psillos 2003, 4). These criteria can further be subdivided into degrees of indispensability, contribution, etc.

of science'[116] which stem from his commitment to developing a *general* philosophy of science.[117] These remarks do not seem in keeping with his tendencies to favor more local conceptions of realism. But these difficulties can be left aside for the time being. More importantly, the details of Psillos' selective realism and his *divide et impera* strategy should be apparent. In the next section, I will turn a critical eye towards both kinds of selective realism explored in this section, and to explanationist defenses of realism in general.

3. The Dilution Problem and Explanationist Realism

I have thus far chronicled the transitions between the various forms of realism while showing what they have in common. Realism began as an *a priori* thesis and became an explanationist defense. It began as a universal claim, but selective realism gave up on the generality of the Putnam-Boyd flavor of EDR, retaining only the strategy of providing an explanation for individual successes in science. This section will turn a critical eye towards selective realism and explanationist defenses of realism more generally.

3.1: The Dilution Problem and Triviality

The development of selective realisms says something strong about the prospects of an explanationist defense: true explanations of the history of science seem highly implausible, since the complexity and richness of history is able to produce counterexamples to any position. In this section, I will articulate what I call the 'dilution problem', which states that *any* explanationist

[116] See Psillos (2012a) for his particular views on this topic.
[117] In Psillos (2012b), he argues that "there must be something like *science in general*" (93) which has its own distinctive epistemic features and depiction of reality. While he is sensitive to micro-level issues in philosophy of science (or philosophy of particular sciences) which may themselves not directly fall from a macro-level view of science, he remains adamant that 'science' still has some essence and commits himself to elucidating this essence in order to "cover - or be relevant to – the various individual sciences and to inform the particular scientific endeavours" (103).

defense of realism will either be trivial and plausible, or substantive and implausible. Such considerations should be enough to discourage further developments of the explanationist project.

Kyle Stanford writes "recently influential efforts to defend realism from the historical record turn out to dilute approximate truth[118] in ways that are…self-defeating" (Stanford 2003, 566). Approximate truth, as a notion that is supposed to explain how science develops, must be watered down or deflated in order to account for the various degrees and kinds of success that abandoned theories have enjoyed. Put another way, when notions of approximate truth are given counterexamples, they bend accordingly. After a long series of counterexamples, the notion continues to bend until (ideally) there are no more counterexamples. This dilutes approximate truth down to a fairly deflated notion given the difficulty of identifying something consistently retained across all theory-changes. While a diluted explanation has the advantage of being very inclusive, it has the disadvantage of being "too meager to render the prospect of this inferential entitlement of any real significance in the debate over realism" (ibid).

Diluted realisms approximate Fine's 'Natural Ontological Attitude' (NOA): our scientific claims and methodologies are typically well justified and thus 'true' in a benign and homely sense. For Fine, there is already a notion of truth that is quite familiar and already at play in ordinary scientific practice. The proposition "Electrons really exist" is true insofar we have lots of evidence that they exist. If new sufficiently strong evidence suggested otherwise, neither realists nor anti-realists would assent to that proposition.[119] Since realists and anti-realists agree on this

---

[118] Since approximate truth is used here as the primary explanatory tool of the realist, I take it to be analogous to explanationist attempts in general.

[119] Ramsey-sentence realists, like Nola, may argue that whatever 'replaced' electrons would be referring to the same 'something', and thus this new evidence does not dissuade our confidence that 'somethings' that we now call electrons exist. I address this argument in the first chapter, but it is worth noting here that this does not address the triviality objection. The proposition "Electrons really exist" is actually "Somethings (which we now call electrons) really exist" which essentially states that science is referring to an external world. Since anti-realists like Laudan, van Fraassen and Stanford do not deny this (i.e. they are not idealists) this strategy cannot save the realist either.

much, Fine calls this the 'core position', a position that is uncontroversial and thus trivial. Since, according to Fine, realists add nothing to the core position other than 'a desk-thumping, foot-stomping shout of 'Really!'', the debate over realism becomes an exercise in futility. He illustrates this with a thought experiment: suppose we had a machine that pumps out premises to valid arguments which generate instrumentally reliable conclusions over and over again. This tempts the realist to say 'Well these premises must be true!' However, Fine argues, this analogy does not guarantee realism since the premises are only 'true' insofar as they are reliable (i.e. the instrumentalist conception of truth) (Fine 1986, 153). Put another way:

> In the context of the explanationist defense, the realist offers the truth of a theoretical story in order to explain its success at a certain range of tasks. If this offering is any good at all, the realist must then allow for some intermediate connection between the truth of the theory and success in its practice. The intermediary here is precisely the pragmatist's[120] reliability (154).

Here, the realist notion of 'truth' is not doing any explanatory work above and beyond the pragmatist conception of truth already in play. Realists and anti-realists agree on this conception of truth but realists attempt to distinguish themselves by trying to say something more.[121]

The selective realist response to the PMI exemplifies the gradual transition towards this deflated notion of truth. Since there are many different ways in which science has been successful, whatever concept of truth explains all (or even most) of these instances must be thin enough to incorporate them all. The ultimate explanation, on this trajectory, is simply describing all of the minutiae that went into each instance of the success of science. This is just the history of science. When seen through this lens, Psillos' selective realist approach, granting that it is

[120] Fine equates instrumentalism with Dewey's pragmatism here.

[121] Since anti-realists (at least the kind Fine is concerned with) do not deny the existence of the external world, nor deny that scientists are, at least partially, talking about the external world, realists cannot expand their conception of truth in this way.

historically accurate,[122] seems undeniable on philosophical grounds. Of course scientists routinely judge possibilities on the basis of the evidence available to them. Of course these judgments come in differing degrees. Of course we will choose new theories that explain and predict more than the older ones did. Psillos' attempts to reconcile his position with history of science actually ends up bringing him closer to NOA.[123] That is to say, assuming he did his history well, his position simply describes agreed-upon history of science. We all agree in the 'check, double-check, triple-check' attitude of science[124] and all Psillos' analysis shows is that it appears to be working healthily in the case of caloric. Rather than being substantive, Psillos' realism ends up leaving no room for disagreement since his positon is just a description of the history of science that anti-realists do not dispute. Since Psillos does not seek to expand his explanation globally, the philosophical debate between him and the anti-realist seems to be at the level of 'elevator words' (Hacking 1999, 23): metaphysical and epistemological jargon that has obscured itself in overly abstract philosophical dialogues and not about how science succeeds.[125]

This is the first horn of the dilution problem: explanationist realist accounts, when narrowed down enough in order to fit the history of science, become trivial since they tell us what was already assumed about the history of science (i.e. the 'facts' of what happened). Realism is not adding anything to the history of science by saying that it is 'approximately true' since it is only approximately true by virtue of its ordinary practices. The lessons learned from Psillos' (and others) historical excursions are lessons that could have been taught by a non-realist historian or

---

[122] Chang (2003) criticizes Psillos' reconstruction of the transition. However, his criticism is based on Psillos' neglecting of relevant and important historical facts.
[123] This is more apparent given the stress Psillos places on reconciling his *divide et impera* strategy with the way in which scientists already actively engage in scientific inquiries.
[124] This is Fine's characterization of how science works.
[125] Ian Hacking (1999) describes how elevator words, such as 'objectivity', 'truth', 'reality', and so on undergo substantial transformations through their usages under many different frameworks leaving their meanings unstable and non-transparent (23). As such, we need to be "wary of arguments in which they are

any anti-realist with a penchant for history. Put another way, since the goal of explanationist realism is to explain the history of science that is taken for granted by both realists and anti-realists, explanationist realism can only amount to a trivial thesis.

I will now to turn to the other horn of the dilution problem: robust formulations of explanationist realism. Selective realists already have recognized that a *tout court* realism is of this sort; a realist thesis whose robustness lies in its characterization of large portions of the history of science. These forms of realism seem robust since they are making strong claims about how science is successful. This has strong implications since it is able to produce the necessary conditions for successful sciences and thereby would be able to make recommendations for how to make future sciences successful. However, the boldness of these claims opens themselves up to an *ad nauseam* list of counterexamples. The stronger and more robust the claim (i.e. the more 'comprehensive' it is),[126] the more counterexamples it potentially opens itself up to. The more narrow the focus, the fewer counterexamples and the more plausible the position becomes. Worrall's structural realism may not be as ambitious as a *tout court* realism since it is agnostic about the retention of observational content across theory-change, but still it remains ambitious nonetheless. It would be remarkable if a single kind of structure was consistently found in all instances of successful science. Newman (2005) shows that the idea of a consistent kind of structure retained across theory-changes from Aristotle to Einstein, from biology to psychology,[127] is subject to many counterexamples.[128] Furthermore, the more precisely structure is defined, the less likely it is that that particular kind of structure has been consistently retained. This is because it would exclude other similar forms of structure that don't exactly fit the

used, especially when we are asked to glide from one to the other without noticing how thin is the ice over which we are skating" (ibid).

[126] By this, I mean robust forms of realism cover large amounts of examples of successful sciences.

[127] This is the malleability of the PMI discussed in chapter 1.

provided criterion. If structure is left vaguely defined, then it is difficult to see what value the position holds since it seems to be more of a retrospective *ad hoc* tool for explaining sciences that are successful. Worrall's structural realism is, then, just another *tout court* realism but about a certain kind structure. This selective strategy does not just apply to Worrall, but any similar brand of selective realism. Whatever your inductive base is: mature theories, mathematical structure, predictive theories, etc., it will eventually suffer the same fate. The malleability of the target of the PMI knows no bounds, it can disrupt any substantive thesis that professes to have finally provided the final explanation as to how science works. Put another way, the malleability of the PMI allows for a wide variety of counterexamples to any explanationist defense that makes robust claims about how science works. Robust forms of explanationism are thus implausible in the sense that they invariably, with enough historical research, end up becoming false. Attempts to refine explanations in order to accommodate counterexamples slowly dwindle these substantive explanations back down to trivial forms of realism.

Explanationist defenses of realism cannot advance a position that is both substantive and plausible. The next section will address in more detail the issue of what realism must be in order to be substantive. While I understand being 'trivial' as being equivalent to NOA (i.e. it cannot account for more than can NOA), realism must add something to this thesis. The next section will address Psillos' defense of his own position against Fine's analysis. I intend to show how this defense fails to save an explanationist thesis and thus that Psillos cannot meaningfully distinguish his own position from NOA.

---

[128] See Rivadulla (2010) p. 11-12 for a discussion of how Bohr's hydrogen atomic model and in Schrödinger's wave mechanics serve as counterexamples to the structural realist thesis.

3.2: Substantive Realism and Explanation

Psillos argues that his position is substantive, as it importantly departs from NOA. This section will outline and evaluate this argument and posit that Psillos is only able to retain a substantive thesis by abandoning his explanationist roots in favor of a critical stance. I hope to show, more generally, that explanationist defenses cannot offer anything on top of NOA.

Psillos advances two distinguishable but interwoven arguments that are meant to demonstrate how realism and anti-realism differ from NOA.[129] The first argument goes like this: realists and anti-realists have substantive debates over differing *concepts of truth* while Fine's 'homely truths' (or the deflated notion of truth already at use in scientific practices) is not a concept at all. Realist conceptions of truth, although diverse and multi-faceted, "should be understood as non-epistemic claims since the concept of truth employed in them is not conceptually linked to irreducibly epistemic notions such as 'conclusive verification', or 'warranted assertibility', or 'idealised justification."' (Psillos 1999, 231). This is because realists take the notion that truths are made true by their relation to a mind-independent world (i.e. the mind-independent world is the 'truth-maker') seriously. Anti-realism, on the other hand, typically understands truth as epistemically constrained. In order for something to be true, it must be known to be true or we have some recognizable means for arriving at this truth.[130] This makes the truth-maker for anti-realists quite different from the realist; they are pragmatic standards, conceptual conventions, ideal assertibility conditions, etc.[131] While there is much more to these positions, Psillos merely wants to show that the concepts of truths actually do differ in substantial

[129] Although, curiously, he cites Devitt (1991) and van Fraassen's (1985) claims that NOA does not differ from their own positions (although van Fraassen's constructive empiricism would require some 'minor modifications') (Psillos 1999, 228). He further argues that Fine's explicit adoption of Tarski-Davidsonian referential semantics "just [pays] a compliment to the realist account of truth" (231).
[130] This is Dummett's (1982) position.
[131] To clarify, anti-realists of this sort do not deny the existence of the external world nor that scientists refer to it, they only deny that the external world is the final arbiter of truth.

ways. This is what all the fighting is about and if Fine want to label this as pointless and thus worth abandoning, he is really just giving up in the middle of an unresolved debate.

Fine's core position, where homely truths are all that there are, is not a concept of truth at all, according to Psillos. He writes "[i]f you look at the 'core position', as formulated by Fine, it becomes clear that it is *not* a position about what truth *is*. [This is because] two parties may take exactly the same set of assertions to be true and also agree that all types of statement within a discourse are subject to the very same standards of truth-evaluation, and yet disagree as to what exactly each attributes to a statement when they say that it is true" (237). In other words, realists and anti-realists can agree about what sets of scientific theories and propositions are true (and to what extent they are true), but can disagree about what it is that makes them true. So general relativity is 'true' for realists since it approximately describes the real structure of the world whereas it is 'true' for anti-realists since it is empirically successful. For Psillos, the dispute is about the concept of truth, whereas the core position simply circumvents the whole debate. If homely notions of truth are actually a concept of truth, then they can be criticized by either realists or anti-realists.

Even if this analysis is correct, it does not salvage a substantive realist account that Fine criticizes in the first place. This is because Psillos and Fine are thinking of 'substantive' in different ways. Psillos understands the realist concept of truth as substantive because it differentiates realisms from a variety of anti-realist stances. There is a legitimate debate about what makes scientific propositions and theories true. Fine, on the other hand, sees neither realist nor anti-realist accounts of science as substantive since neither denies anything that the NOA cannot readily accommodate. Putting this another way, both 'concepts of truth' end up at the same place when at issue is accepting scientific truths. That is to say that both concepts of truth ideally portray the history of science having the exact same 'truth-tracking record.' Explanationist realists and anti-realists attempt to create definitions of approximate truth (or empirical adequacy or whatever) that fit the history of science; if it doesn't, they abandon the definition. This turns into a debate about what to label scientific theories which, by itself, is not an interesting debate since both labels account for the exact same set of historical data (or at least they try their best to).

This also implies that realists and anti-realists would agree on *new* truths as being genuinely true. Both concepts of truth will have to accommodate any new evidence or 'homely truths' that come along, even if this means bending and modifying the concept. If what we are doing is modifying our explanatory concepts to explain what we already know, what is the point of the concept? Put another way, these concepts of truth do not even *imply* anything different about what we would recognize as true. This means that explanationist realism (and anti-realism) aren't substantive, since they do not challenge the ordinary deflated notion of truth we all already commonly ascribe to successful scientific theories. This point is particularly clear for selective realism. Selective realists choose to be realists in certain areas and anti-realists in others on the basis of historical evidence. Such historical evidence is entirely explainable by way of a deflated notion of approximate truth. Their conceptual apparatuses are isomorphic to each other (in the sense that the realist notion of 'approximate truth' is equivalent to the deflated notion of truth) leading to only a mere 'verbal dispute.'[132] Since the goal of explanationist realism is to explain a set of data that is uncontentious, and it cannot do so in a robust sense, the debate does not seem to amount to anything substantial. In other words, since more robust concepts of truth have more counterexamples, the least robust concept of truth (i.e. the one already in use) will have no counterexamples and thus cannot be challenged.

[132] I am using Chalmers' notion of a verbal dispute here. He argues when "we are concerned with a first-order domain, not with the usage of words, and in such a way that nothing crucial to the domain turns on the usage of words." (Chalmers 2011, 516).

Psillos launches a second criticism of Fine, which I think may be partially responsible for these differing notions of what counts as a 'substantive' theory. Psillos sees the debate as substantial at an abstract 'philosophical' level whereas Fine denies that it is substantial since it does not, at the end of the day, change our pre-philosophical views on what science, or the history of science, is. However, Psillos *does* think realism changes our attitudes towards science. This was explored in the previous chapter: realists are *optimistic* as opposed to *skeptical*. But this isn't explanation anymore, but a form of criticism (i.e. of a particular attitude). Rather than taking the history of science as granted, realists and anti-realists *reconstruct* the history of science leading to different normative implications (this was explored in the previous chapter as well). If Psillos is arguing that the debate between realists and anti-realists is substantive because we treat science differently, adopt different attitudes on it, or see it in a different light, then the debate is only substantive because of its *normative* implications (i.e. it changes the way he behave towards or view particular instances of scientific practices).[133] If realism emphasizes certain features over others, whose presence either guarantees or is strongly conducive to approximate truth, and anti-realism makes different recommendations is the same circumstance, then we have a disagreement.

This leads Psillos to abandon, without realizing it, an explanationist defense of realism in favor of a *critical* project. This is seen more clearly in Psillos' contentions against the 'hands-off' attitude of the NOA. He argues that Fine wants to eliminate the need for a *normative* epistemology of science; that is to "pinpoint, examine, and *possibly remove* the ambiguities of the ordinary scientific practice" (emphasis added, 250). More specifically, he thinks the debates between realists and anti-realists matter for which scientific theories we choose and which ones

---

[133] This could also be about our attitudes about science as whole.

we develop.[134] This all sounds quite uplifting; the debate between realists and anti-realists matters after all! However, this involves an important shift between purely *explaining* science and *critically evaluating* various elements, arguments and episodes within science.[135] Rather than accepting Boyd's naturalistic account (which Psillos explicitly does)[136] where realism explains the success of science through particular conceptions of approximate truth, realists became critical commentators on the scientific project (or individual instances within science). This stance certainly seems substantial in that it can potentially *resist* the practices of science[137] rather than taking them on board as an explanandum. This also explains Psillos' frustration with Fine's contention that debates about realism are insubstantial; they *are* substantive, on Psillos' account, precisely because they are critical and because realists and anti-realists take different critical stances or attitudes. This is all well and good, but it is not explanationist realism anymore. Particular theory-led successes become either praised as *genuine* successes or they are denied such a status rather than accepting them as successes and trying to explain them. This is not explaining how science works, but a normative assessment of it. Rather than arguing that realism *is* the best explanation for the success of science, realism *should be* the explanation which is used to understand the success of science.

This can be put in another way. When understanding realism as a critical project, the debate that Psillos sees as substantive (i.e. the debate over concepts of truth) is actually debating which lens we *should* view science through. When thought about as purely a take on what science

---

[134] Psillos writes "one of the central concerns of modern epistemology of science has been to characterize what *should be involved* in accepting a scientific theory. Scientific realists suggest that acceptance should be equated with the belief that theory is approximately true" (emphasis added, Psillos 1999, 249).
[135] To clarify, critically evaluating science involves, at some point, explaining various features of science and is thus not *purely* critical. However, explanationist realism is meant to be *purely* explanation.
[136] See p. 78-81.
[137] This could involve appraising certain scientific theories over others, supporting further research in one avenue rather than another, criticizing an acceptance of a certain theory within the scientific community, etc.

is, realism and anti-realism cannot add to what we already knew through NOA. This can be seen in the fact that the metaphysics (or lack thereof) of each account, if meant to simply explain the history of science, must constantly bend to accommodate whatever the history of science tells us. This should be clearer if we return to electrons: realists claim the theoretical term approximately refers to an entity that exists and anti-realists says that we don't know whether the term refers to anything or not. This debate seems substantive even though both agree on the epistemic merits of the scientific theories which posit electrons. Now let's posit that one hundred years from now, new theories of physics come to be recognized as largely 'true' but make no use of electrons (or anything remotely similar).[138] Realists will have to slightly change their tune: the physicists of the early 21st century were still referring to 'something' and the continuity between the two theories of physics is explainable by providing the details of the historical transition between the two theories. Anti-realists will be in complete agreement with this. Of course the physicists of the early 21st century were referring to 'something' in the external world (after all the external world exists and scientists are dealing with it in *some* capacity), but it wasn't an electron in 21st century-physicist sense. The 'approximate' qualification on realist notions of truth and correspondence becomes stretched to accommodate the difference between two seemingly radically different theories of physics. Both 21st and 22nd century physicists were *approximately* referring to the same thing. Since the 'same something' is vague enough to essentially mean that both groups were referring to some features of the external world, there is nothing for the anti-realist to disagree with. The two concepts of truth constantly agree with each other every step along the way insofar as they aren't speaking to each other in elevator words. They both agree about the science of the early 21st century and 22nd century, the history that led to the differences between

---

[138] By this I mean a comparison between the future physics and our current physics would be similar to the comparison between today's physics and, say, Aristotelian physics.

the two, and that physicists were referring to 'something' which allowed for a certain degree of success.[139] But if realists and anti-realists began to *recommend different attitudes* towards 22nd century physics (since it was non-convergent in the case of electrons), then we would have a debate.

The distinction between explaining and criticizing science can be seen more explicitly with the example of Einstein's refusal to accept the Copenhagen interpretation (CI) of quantum mechanics as approximately true *despite the fact that it was empirically successful*. Here, Einstein's metaphysical commitments (to a deterministic world with determinate properties) led him to withhold the label of 'approximate truth' to the CI. This stands in stark contrast to the instrumentalist views of Bohr[140] and Heisenberg,[141] which accepted the CI as approximately true *because it was empirically successful*.[142] The counter-intuitive ontological conclusions of the CI (i.e. that particles do not actually have definite properties and that laws are probabilistic rather than deterministic) must be accepted at face value and we have to simply 'suck it up', as Seth Lloyd has put it (Wolchover 2014). While I will not explore the Einstein-Bohr debates in too much depth here, it should be noted why Einstein's realism is substantial while, say, Psillos' is not: when Einstein argues that scientific theories are approximately true because they refer to the external world, he has a *specific* notion of what this means whereas Psillos does not. Einstein is

---

[139] If realists were to get specific about what is being referred to (i.e. electrons with x, y, and z properties), then an anti-realists would object. But this specification moves right back towards the second horn of the dilution problem since this specific conception has not been retained throughout all of history.

[140] While Bohr's epistemological arguments are not advanced in a traditional philosophical vein, he makes several instrumentalist arguments such as experimenters being ultimately suspended in language which is culturally conditioned (i.e. unable to know anything apart from language), the irreducibility and incommensurability of quantum theory, relativistic quantum mechanics, quantum field theory, and nuclear physics. A fuller description can be seen in MacKinnon (1982) 349-76.

[141] Heisenberg's epistemology was primarily grounded on the uncertainty principle leading to a philosophy of '*anschaulich*' roughly translated as what is perceptual, intelligible, and intuitive. He also advances arguments about the meaning of a term being specified to a particular experiment leading to his 'measurement is meaning' thesis (Hilgevoord 2006).

[142] A more detailed analysis of the philosophical views of Bohr, Heisenberg and Schrödinger can be found in (Plotnitsky 2009).

metaphysically committed a world that is deterministic and knowable, in principle, with certainty. Since the CI does not refer to this kind of world, it cannot be the whole story and thus approximately true. Psillos, on the hand, does not have a robust notion of what it means to refer to the external world. This is because if he put one forward, it would be robust and it would be susceptible to the PMI. If explanationist realists bit the bullet and argued that certain scientific theories were empirically successful but *not* approximately true, then we would know what realists add to NOA. This kind of realism would suggest there is something *more than the history of science* (i.e. it is incomplete in some sense). Against Psillos' claim that "metaphysics should be in the service of science and should be constrained by it" (Psillos 2013, 29), the metaphysics of realism can actually be used to evaluate and potentially criticize science as it is practiced. The metaphysics of science becomes, instead of a blanket aimed to perfectly cover the given history of science, an external standard of assessment.

So Psillos' account may not be trivial after all, but it is trivial if it is understood as an explanationist thesis. Despite his explicit adherence to a local form of EDR, a critical form of realism begins to emerge as the real potential strength of his position. While I do not want to discuss its prospects here, it should at least be noted that this is a different kind of project from trying to explain how historical successes of science. Explanationist defenses of realism can only learn from the history of science, but critical forms of realism can change science.

Concluding Thoughts

The long and winding road of realism has had many turns and twists in its attempts to remain the best possible explanation for why science is successful. Throughout this journey, it has slowly dwindled down into a much more deflated theory than it was made out to be by Smart.

First, realism was a grand *a priori* theory that made sense of the scientific project in its broadest form. It became less bold by becoming an empirical hypothesis, an explanation for the 'success of science.' The PMI has restricted the thesis even further, forcing realists to be more cautious about which particular theories they choose to be realists about. With the gradual dwindling down of the realist position, the substantive nature of its character began to come under scrutiny. This, when combined with the malleability of the PMI, brings into question the need for an explanation at all! The stronger and more robust the explanation, the more the history of science seems to prove the explanation inadequate. Conversely, the weaker the explanation becomes, the more tenable it seems. Why would we want either a weak explanation which tells us nothing that the history of science has not already taught us or a strong explanation which cannot account for large portions of that history?

I argued here that this problem comes part and parcel with an explanationist thesis. Since the explanationist realist takes the content of the history of science as a premise, content that is not disputed by the opponents of realism, then explanationist realism is at its strongest when it makes no substantive claims. But there does seem to be a way out. Realism has built up quite an arsenal over the years and while this toolkit may be in vain when trying to give the history of science with a shiny new gloss, it certainly has provided some historical perspective on how science operates from time to time. This perspective seems enormously useful in constructing *normative* tools for appraisal and dismissal of scientific theories, guiding principles for theory choice, or, in short, a *world-view* that can try to help science along. While this may seem like a radical departure from the realist project, the seeds of this have already been planted. Realists have often shown, perhaps unintentionally, their normative impulses. Smart sees the adopting of realism as an ethical decision, Psillos claims that a realist stance is necessary for understanding science in the first place, and Kitcher claims that realism helps organize our priorities for scientific activities. The upshot of EDR, then, may not be its explanatory power, but its normative implications.

Work has already begun in this vein. Hasok Chang (2001) has made a persuasive case for the practical applicability and theoretical importance of what he calls "ontological principles": basic ideas which can be used, at times, to challenge scientific theories and demand a basic level of metaphysical clarity. Theories that cannot meet this demand are deemed 'ontologically implausible' and may give us a reason (even if only temporarily) to withhold our full-fledged support. Einstein's aforementioned concerns about the CI is like this; the CI is ontologically implausible since it denies that objects have definite properties.[143] While the CI remains dominant, it has been subject to recurring challenges. For example, recent empirical evidence (e.g. Couder and Fort 2006) has suggested that Einstein's realism may not have been metaphysical quackery, but that he may have been right after all! This is not the place to explore the connection between metaphysics and science more generally; but it certainly seems like it could be a potentially fruitful path. Metaphysics and science have had a close relationship throughout history and this seems to give the philosopher a role as an active contributor to the growth of scientific knowledge. The leap from explanation to evaluation is not a large one; in fact, it may have been its hidden strength all along.

[143] This is Chang's 'principle of value-definiteness.'

Bibliography

Almeder, Robert. 1992. *Blind Realism.* New Jersey: Rowman & Littlefield.

Bloor, David. 2008. "Relativism at 30,000 feet." In *Knowledge as Social Order*, by Massimo Mazzotti, 13-34. Hampshire: Ashgate Publishing Ltd.

Boyd, Richard. 1983. "On the Current Status of the Issue of Scientific Realism." *Methodology, Epistemology, and Philosophy of Science* 45-90.

Boyd, Richard. 1989. "What Realism Implies and What It Does Not." *Dialectica* 5-29.

Braithwaite, R. B. 1953. *Scientific Explanation.* Cambridge: Cambridge University Press.

Brown, James R. 1994. *Smoke and Mirrors.* London: Routledge.

Carnap, Rudolf. 1963. *The Philosophy of Rudolf Carnap.* Chicago: Open Court Publishing Company.

Chalmers, David. 2011. "Verbal Disputes." *The Philosophical Review* 515-566 .

Chang, Hasok. 2001. "How To Take Realism Beyond Foot-Stomping." *Philosophy* 5-30.

Chang, Hasok. 2003. "Preservative Realism and its Discontents: Revisiting Caloric." *Philosophy of Science* 902-912.

Couder, Yves, and Emmanuel Fort. 2006. "Single-Particle Diffraction and Interference at a Macroscopic Scale." *Physical Review Letters* 1-4.

Curtis, Malcolm. 2014. *Scientists Threaten Boycott of Brain Project.* July 8. Accessed 2014. http://www.thelocal.ch/20140708/scientists-threaten-boycott-of-brain-project.

Devitt, Michael. 1991. "Abberrations of the Realism Debate." *Philosophical Studies* 43-63.

Devitt, Michael. 1984. *Realism and Truth.* Oxford: Blackwell.

Doppelt, Gerald. 2007. "Reconstructing Scientific Realism to Rebut the Pessmistic Meta-Induction." *Philosophy of Science* 96-118.

Dummett, Michael. 1982. "Realism." *Synthese* 55-112.

Empiricus, Sextus. 1990. *Outlines of Pyrrhonism.* Prometheus Books: Amherst.

Epstein, Steven. 1995. "The Construction of Lay Expertise: AIDS Activism and the Forging of Credibility in the Reform of Clinical Trials." *Science, Technology, & Human Values* 408-37.

Feyerabend, Paul. 1975. *Against Method.* London: Verso Books.

Feyerabend, Paul. 1970. "Consolations for the Specialist." In *Criticism and the Growth of Knowledge*, by Imre Lakatos and Alan Musgrave, 197-230. Cambridge: Cambridge University Press.

Fine, Arthur. 1991. "Piecemeal Realism." *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition* 79-96.

Fine, Arthur. 1991. "The Natural Ontological Attitude." In *The Philosophy of Science*, by R Boyd, P Gasper and J. Trout, 261-277. Cambridge: MIT Press.

Fine, Arthur. 1986. "Unnatural Attitudes: Reaist and Instrumentalist Attachments to Science." *Mind* 149-179.

Furber, Steve, and Jens. Sparsø. 2002. *Principles of Asynchronous Circuit Design - A Systems Perspective.* Boston: Kluwer Academic Publishers.

Hacking, Ian. 1999. *The Social Construction of What?* Cambridge: Harvard University Press.

Hempel, Carl. 1958. "The Theoretician's Dilemna: A Study in the Logic of Theory Construction." In *Concepts, Theories, and the Mind-Body Problem*, by H Feigl, G Maxwell and M Scriven, 37-98. Minneapolis: University of Minnesota Press.

Hilgevoord, Jan. 2006. "The Uncertainty Principle." *Stanford Encyclopedia of Philosophy.* http://plato.stanford.edu/entries/qt-uncertainty/.

Honigsbaum, Mark. 2013. "Human Brain Project: Henry Markram plans to spend €1bn building a perfect model of the human brain." *The Observer*. October 13. Accessed 2014. http://www.theguardian.com/science/2013/oct/15/human-brain-project-henry-markram.

Horrigan, David. 2013. *The Human Brain Project Official Webpage*. Accessed 2014. https://www.humanbrainproject.eu/hbp-summit-2013-overview.

Kenall, Amye. 2014. *Building the Brain: The Human Brain Project and the New Supercomputer*. July 8. Accessed 2014. http://blogs.biomedcentral.com/bmcblog/2014/07/08/building-the-brain-the-human-brain-project-and-the-new-supercomputer/.

Kitcher, Philip. 1993. *The Advancement of Science.* New York City: Oxford University Press.

Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Kuhn, Thomas. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change.* London: University of Chicago Press.

Kukla, André. 1994. "Scientific Realism, Scientific Practice, and the Natural Ontological Attitude." *British Journal for the Philosophy of Science* 955-975.

Kukla, André. 1998. *Studies in Scientific Realism.* Oxford: Oxford University Press.

Ladyman, James. 1998. "What is Structural Realism?" *Studies in History and Philosophy of Science* 409-424.

Ladyman, James, and Steven French. 2003. "Remodelling Structural Realism: Quantum Physics and the Metaphysics of Structure." *Synthese* 31-56.

Lakatos, Imre. 1981. "History of Science and its Rational Reconstructions." In *Scientific Revolutions,* by Ian Hacking, 107-127. New York City: Oxford University Press.

Lange, Marc. 2002. "Baseball, Pessmistic Inductions and the Turnover Fallacy." *Analysis* 281-285.

Laudan, Larry. 1981. "A Confutation of Covergent Realism." *Philosophy of Science* 19-49.

Laudan, Larry. 1984. "Explaining the Success of Science." In *Science and Reality*, by C.F. Delaney and G.M. Gutting J. Cushing, 83-105. Notre Dame: Notre Dame Press.

Leplin, Jarrett. 1997. *A Novel Defense of Scientific Realism.* New York City: Oxford University Press.

Lewis, Peter. 2001. "Why the Pessimistic Induction is a Fallacy." *Synthese* 371-380.

Maass, Wolfgang, Henry Markram, and Thomas Natschlager. 2002. "Real-Time Computing Without Stable States: A New Framework for Neural Computation Based on Perturbations." *Neural Computation* 2531-2560.

MacKinnon, Edward. 1982. *Scientific Explanation and Atomic Physics.* Chicago: University of Chicago Press.

Mackonis, Adolfas. 2013. "Inference to the Best Explanation, Coherence and Other Explanatory Virtues." *Synthese* 975-995.

McMullin, Ernan. 1984. "A Case for Scientific Realism." In *Scientific Realism*, by Leplin Jarrett, 8-40. Berkeley: Universty of California Press.

Newman, Mark. 2005. "Ramsey Sentence Realism as an Answer to the Pessimistic Meta-Induction." *Philosophy of Science* 1373–1384.

Nola, Robert. 2008. "The Optimistic Meta-Induction and Ontological Continuity: The Case of the Electron." *Rethinking Scientific Change and Theory Comparison* 159-202.

Plotnitsky, Arkady. 2009. *Epistemology and Probability: Bohr, Heisenberg, Schrödinger, and the Nature of Quantum-Theoretical Thinking.* New York City: Springer Publishing.

Poincaré, Henri. 1905. *Science and Hypothesis.* New York: Dover Publications.

Psillos, Stathis. 1994. "A Philosophical Study of the Transition from the Caloric Theory of Heat to Thermodynamics." *Studies in the History and Philosophy of Science* 159-190.

Psillos, Stathis. 1996. "Scientific Realism and the 'Pessimistic Induction'." *Philosophy of Science* 306-314.

Psillos, Stathis. 1999. *Scientific Realism: How Science Tracks Truth.* London: Routledge.

Psillos, Stathis. 2000. "The Present State of the Scientific Realism Debate." *British Society for the Philosophy of Science* 705-728.

Psillos, Stathis. 2003. "Thinking About The Ultimate Argument for Realism." In *Rationality and Reality: Conversations with Alan Musgrave*, by Colin Cheyne and John Worrall, 133-156. Springer.

Psillos, Stathis. 2011. "Scientific Realism with a Humean Face." In *The Continuum Companion to Philosophy of Science*, by Juha Saatsi and Steven French, 75-95. London: Bloomsbury Academic.

Psillos, Stathis. 2012a. "One Cannot be Just a Little Bit Realist: Putnam and van Fraassen." In *Philosophy of Science: The Key Thinkers*, by James Robert Brown, 188-212. London: Bloomsbury Academic.

Psillos, Stathis. 2012b. "What Is General Philosophy of Science?" *General Philosophy of Science* 93–103.

Psillos, Stathis. 2013. "Semirealism or Neo-Aristotelianism?" *Erkenntnis* 29-38.

Putnam, Hilary. 1978. *Meaning and the Moral Sciences.* Boston: Routledge & Kegan Paul Ltd.

Ramsey, Frank. 1929. "Theories." In *Foundations: Essays in Philosophy, Logic, Mathematics and Economics*, by D. H. Mellor. London: Routledge.

Rivadulla, Andrés. 2010. "Two Dogmas of Structural Realism. A Confirmation of a Philosophical Death Foretold." *Critica* 3-29.

Rosenberg, Clyde and Hardin, Alexander. 1982. "In Defense of Convergent Realism." *Philosophy of Science* 604-615.

Rouse, Joseph. 1981. "Kuhn, Heidegger, and Scientific Realism." *Mand and World* 269-290.

Roush, Sherrilyn. 2009. *Optimism about the Pessimistic Induction.* Technical Report, London: Centre for the Philosophy of Natural and Social Science, 1-35.

Saatsi, Juha. 2005. "On The Pessimistic Induction and Two Fallacies." *Philosophy of Science* 1088-1098.

Smart, J.J.C. 1963. *Philosophy and Scientific Realism.* New York: The Humanities Press.

Smart, J.J.C. 1985. "Laws of Nature and Cosmic Coincidences." *The Philosophical Quarterly* 272-280.

Smolin, Lee. 2007. *The Trouble With Physics.* London: Penguin Books.

Stanford, Kyle. 2003. "Pyrrhic Victories for Scientific Realism." *The Journal of Philosophy* 553-572.

Suppe, Frederick. 1989. *The Semantic Conception of Theories and Scientific Realism.* Chicago: University of Illinois Press.

Ure, Andrew. 1820. *Dictionary of Chemistry.* London.

van Fraassen, Bas. 1980. *The Scientific Image.* Oxford: Oxford University Press.

van Fraassen, Bas. 1985. "Empiricism in Philosophy of Science." In *Images of Science,* by Patricia Churchland and C. A. Hooker, 245-300. Chicago: Chicago University Press.

Walchover, Natalie. 2014. *Have We Been Interpreting Quantum Mechanics Wrong This Whole Time?* http://www.wired.com/2014/06/the-new-quantum-reality/.

Waldrop, Mitchell. 2012. *Computer Modelling: Brain In A Box.* February 22. Accessed 2014. http://www.nature.com/news/computer-modelling-brain-in-a-box-1.10066.

Worrall, John. 1989. "Structural Realism: The Best of Both Worlds?" *Dialectica* 99-124.

Wright, Crispin. 1992. *Truth and Objectivity.* Cambridge: Harvrd University Press.

Zahar, E. G. 1983. "Poincaré's Independent Discovery of The Relativity Principle." *Fundamenta Scientiae* 147-175.