A New Argument for Scientific Realism

by

Curtis Joseph Forbes
B.A., University of Alberta, 2006

A Thesis Submitted in Partial Fulfillment of the
Requirements for the Degree of

MASTER OF ARTS

in the Department of Philosophy

A New Argument for Scientific Realism

by

Curtis Joseph Forbes
B.A., University of Alberta, 2006

Supervisory Committee

Dr. Jeffrey Foss, Supervisor
(Department of Philosophy)

Dr. Audrey Yap, Department Member
(Department of Philosophy)

Dr. Carrie Klatt, Department Member
(Department of Philosophy)

Dr. Paul Wood, Outside Member
(Department of History)

Supervisory Committee

Dr. Jeffrey Foss, Supervisor
(Department of Philosophy)

Dr. Audrey Yap, Department Member
(Department of Philosophy)

Dr. Carrie Klatt, Department Member
(Department of Philosophy)

Dr. Paul Wood, Outside Member
(Department of History)

ABSTRACT

The debate between scientific realism and constructive empiricism is often cast in terms of a debate over what it is "rational" to accept about successful scientific theories. I cast this debate differently: I place these conflicting philosophies of science within our current political context, asking the question "which of these philosophies gives us the conceptual tools we need to allow science to adequately inform our public policy decisions?" I argue that most cases of long-term planning based on current science, such as curbing carbon emissions based on global warming theory, are decisions that can only be made if we approach scientific theories realistically. This vindicates the project of developing a realist epistemology, for only by inquiring into the truth with respect to unobservables can we make adequately informed public policy decisions.

Table of Contents

Acknowledgements

First and foremost, I must acknowledge the assistance of Audrey Yap. Without her confidence and constructive input, I would hardly have learned so much throughout my Master's studies, nor would I likely have produced a finished thesis. I would also like to thank Jeffrey Foss, my supervisor, for all of his productive input, inspiration, and faith in my abilities. Personal conversations with Anjan Chakravartty, Mary Butterfield, and Cindy Holder have also been instrumental to working out the details of the ideas displayed here. I must also thank the Social Sciences and Humanities Research Council of Canada and the University of Victoria for their financial support. Lastly, I would like to thank Trent Reznor and the rest of Nine Inch Nails for providing the majority of my thesis-writing soundtrack.

Dedications

For my father, and all his unwavering support

Introduction

In 1962, Michael Dummett famously argued that debates between realists and anti-realists result from their holding different conceptions of truth. While this is likely true with respect to many varieties of anti-realism (specifically the irrealism, instrumentalism, and nominalism that Dummett discusses), one form of anti-realism promises to challenge this blanket characterization of disputes between realists and anti-realists. This is Bas van Fraassen's constructive empiricism, its uniqueness evident in its explicit acceptance of a realist conception of truth. As van Fraassen puts it, "constructive empiricism is not anti-realist in Dummett's sense, since [like scientific realism] it also assumes scientific statements have truth-conditions entirely independent of human activity or knowledge" (van Fraassen, 1980, p.38).

Debates between constructive empiricists and scientific realists are therefore unique in that they cannot be reduced to linguistic or semantic disputes over the nature of truth, for they agree on this by hypothesis. Their dispute is purely epistemological. When debating the merits of their respective philosophies, the debate is often cast, at least implicitly, in terms of what it is rational for us to believe about successful scientific theories. Is the rational choice to believe what the constructive empiricist recommends, i.e. that successful scientific theories merely "save the phenomena"? Or is it rational to believe what the realist recommends, i.e. that successful scientific theories are (mostly) true?

Unfortunately, neither position seems especially compelled by rationality. This essay is meant to reorient the traditional debates between constructive empiricists and scientific realists. A more important and socially relevant debate concerns which position provides us with the conceptual tools we need to let science play its needed role in informing our decisions. This is the debate towards which I aim to shift the conflict between scientific realists and constructive empiricists. I argue that, when making long term plans, we will often need to ask realist questions about our scientific theories. As a result, we all have an interest in the answers to realist questions, and a need to develop a realist epistemology aimed at discovering the truth with respect to unobservables.

The first chapter will be devoted to outlining the similarities of scientific realism and constructive empiricism – specifically their shared characterization of scientific theories (namely a semantic or model-theoretic characterization), their shared view on the nature of truth, and their shared acceptance of a particular version of the observable/unobservable distinction. Chapter Two will be devoted to outlining their points of divergence given their shared foundations, as well as the traditional arguments for and against these positions. After having explicated the shared foundations (Chapter 1) and divergences (Chapter 2) of scientific realism and constructive empiricism, the final chapter will shift the debate between these philosophies towards a debate over which philosophy is more useful to us when using scientific theories to inform our long-term planning.

Chapter 1 – The Shared Foundations of Scientific Realism and Constructive Empiricism

§1 – Scientific Realism and Constructive Empiricism: Similarities

This chapter is dedicated to outlining the shared foundations of scientific realism and constructive empiricism. There are three shared features of these philosophies that I wish to outline before moving on to outline their contrasts and differences in Chapter Two. The first feature discussed here is their shared characterization of scientific theories according to the so-called semantic or model-theoretic approach to understanding scientific theories. The second feature is their shared account of scientific truth. The third shared feature of scientific realism and constructive empiricism is an understanding, within the semantic approach to characterizing scientific theories, of the observational content of scientific theories. Distinguishing the observational content of theories is more important for the constructive empiricist than it is for the scientific realist, but it does inform the realist's epistemology as well, so it is included in this chapter.

There are, of course, some realists who would for one reason or another choose to work outside the semantic approach to theories (see, for example, Psillos, 1999, esp. p.276-279 and Chakravartty, 2007). But, unless the realist too employs the semantic or model-theoretic approach to scientific theories and sees truth in the usual correspondence manner, it may be difficult to make sense of van Fraassen's reformulation of the observable/unobservable distinction, and therefore difficult to engage in substantive debate with the constructive empiricist regarding our proper epistemic attitude towards scientific theories. Accordingly, I will simply take for granted that a realist accepts the semantic account of theories and the "ancient" or "realist" conception of truth according to which "truth is correspondence with the facts."

In this chapter, I outline each of these shared features in turn, noting along the way some reasons that one might have for adopting this or that approach.

§2 – The Semantic or Model-Theoretic Account of Scientific Theories

The first feature shared by scientific realism and constructive empiricism is their view of what makes a certain scientific theory the theory that it is; that is, their view regarding what identifies, typifies, or characterizes a particular scientific theory. Some questions regarding the nature of scientific theories are left unanswered by this approach, but all advocates agree that scientific theories, at least in part, consist of a collection of *models*. This account of scientific theories, the so-called "semantic" or "model-theoretic" account, arose in the late fifties and early sixties, and is most readily associated with the early work of Patrick Suppes.

The semantic account of theories contrasts itself with a purely syntactic account of scientific theories, an account often associated with logical positivism. What proved to be the most problematic – and in retrospect is thus the most characteristic – aspect of this syntactic account of scientific theories is its characterization of theories as particular kinds of linguistic entities. Despite the lofty hopes that defenders of the syntactic view had for their philosophical program, there is undoubtedly something wrong with conceiving of theories in this way. Carnap himself abandoned the *purely* syntactic approach of own logical syntax program not long after publishing the foundation of this project – his *Logical Syntax of Language* – and accepted Tarski's semantic definitions of certain concepts like truth and logical consequence (Carus, 2008, p.36).

The development of an alternative, semantic characterization of scientific theories is mostly due to a 1961 paper by Patrick Suppes. His approach is heavily inspired by and reliant upon the work of Tarski in the field of formal semantics. In the mathematical sciences, a *structure* is defined as a set of elements, together with certain relations and functions defined upon these elements.[1] These structures are abstract, and non-linguistic; but they can be taken to satisfy certain *axioms*, which are simply statements made in

---

[1] There are a variety of possible ways to define a structure: Suppes, for instance, prefers a set-theoretic approach to defining structures, but there are other, equivalent approaches. Van Fraassen, for instance, prefers something called the "state-space" approach. There are also a wide range of views regarding the nature or ontology of abstract mathematical objects like structures, ranging from Platonism to fictionalism. Since they have little relevance to the issues at hand, I note these issues here only to explicitly leave them aside in order to discuss more pertinent aspects of the semantic approach to theories.

some formal language. A given structure is taken to *satisfy* an axiom if that axiom is true when the terms occuring in the axiom are interpreted as referring to the objects, relations, and functions of the structure.

A set of axioms is often called a *theory*. I will call this sort of theory a *mathematical theory* in order to contrast it with a *scientific theory*, which is something different. A given structure is considered a *model* of a mathematical theory if every axiom of the theory is satisfied by the structure.[2] This is the sense in which we speak of mathematical theories or axioms being "true in a model", where a model is "a non-linguistic entity in which a theory is satisfied." (Suppes, 1961, p.289).

Suppes claims that this concept of a model may be used without distortion as a fundamental concept in both the mathematical and empirical sciences (ibid.). This fundamental similarity of models in the mathematical and empirical sciences allows philosophers to import many techniques from mathematics into the philosophy of science. As the slogan for the semantic view goes, the appropriate tool for the philosophy of science is mathematics, not meta-mathematics; for scientific theories can be more usefully characterized with the tools and techniques of mathematics than with the tools and techniques of meta-mathematics. While there are, obviously, differences in the use of models in the empirical and mathematical sciences, these are just that – differences in *use* rather than differences in *kind*. Accepting this, and noting the primacy of models in scientific theorizing and scientific inquiry (see, for example, Hesse 1963), the semantic approach characterizes a scientific theory according to its specific class of models.

According to the semantic approach, to axiomatize a scientific theory is not to formulate a particular set of axioms: it is first and foremost to delineate the proper class of models

---

2 A familiar example of a theory in this sense would be the standard five postulates or axioms of Euclidian Geometry. There are certain abstract, mathematical structures which will, when we suitably interpret the names and predicates of the language in which these axioms are stated, satisfy all five axioms. A different theory would be Euclidian Geometry minus the parallel postulate, which would have a much different set of models.

or structures[3]. Mathematical theories just *are* sets of axioms, which are distinct from any of the structures that satisfy them. Scientific theories, by contrast, are (at least in part) *constituted* by a specific class of models or structures: whichever class of models makes up a given scientific theory determines which scientific theory it is. The mathematical theory we formulate in order to delineate a scientific theory's typical class of models may be thought of as inessential to a scientific theory, for we can often delineate the same class of structures using a variety of mathematical theories. Individual mathematical theories are, in this sense, dispensable to scientific theories, even though mathematical theories in general may be indispensable. As a result, the semantic view characterizes scientific theories non-linguistically, according to a stereotypical set of models.

In fact, on a radical version of the semantic approach, mathematical theories are seen as entirely dispensable: a scientific theory is seen as *nothing more than* a collection of models, and is therein an *entirely* non-linguistic entity. But it should be noted that abstract mathematical structures cannot, in and of themselves, be true or false, just as normal concrete objects like tables and chairs cannot be true or false. To call either true or false is a category mistake; for truth is a property of statements or sentences[4], and mathematical structures, tables, and chairs are obviously not sentences. Thus, if we take scientific theories to be nothing more than their typical collection of models, they become neither true nor false; and yet, realists wish to speak of scientific theories as true or false. So, if we are to be realists, we must be careful not to be too radical, and see a scientific theory as *nothing more than* a collection of models, i.e. as something *entirely* non-linguistic

Regardless of how radically anti-linguistic one is willing to be here, the semantic approach minimally insists that a scientific theory is not to be *identified with* or

---

3 "To ask if we can axiomatize [a] theory is then just to ask if we can state a set of axioms such that the models of these axioms are precisely the models in the defined class" (Suppes 1967, p.60, cf. French and da Costa, p.33).

4 I do not wish to deal with debates in the philosophy of language and epistemology surrounding the ultimate bearers of truth, be it beliefs, statements, sentences, or propositions. I take it as given that statements can be called true or false well enough, and use this term rather than any of the others more as a matter of expediency than of relevance. All the same, I also doubt the relevance of such issues to the main issues at hand, and thereby feel justified in my appeal to expedience.

characterized by a particular mathematical theory or set of axioms; rather, it is identified by its typical class of models. So, when we see two apparently distinct scientific theories purporting to apply to the same empirical domain, whose models are delineated using two distinct mathematical theories, we can determine whether or not these scientific theories are in fact distinct by comparing their respective classes of models. If they share the same class of models, they are the same scientific theory, despite being stated using different axioms, or in different languages[5]. In emphasizing that distinct scientific theories within the same scientific domain are differentiated by their classes of models, rather than (for example) by their syntactic rules or axioms, most pitfalls of linguistic approaches to characterizing scientific theories can be avoided[6].

In order to think of scientific theories as either true or false, however, one must commit to there being some *inextricable* linguistic aspect to scientific theories. These are what Giere (1988) calls "theoretical hypotheses." For any given scientific theory, there is some (generally implicit) interpretation of the names, objects, predicates and relations of its models that takes these abstract things to represent real-world objects and relations[7]. We can judge this preferred interpretation for how much relevant similarity there is between the target system or phenomenon and a scientific theory's models, and formulate our judgments as a statement of the form "features A, B, and C of the real world system S are like features D, E, F of the model M". This is the general form of a theoretical hypothesis, the most common example of which is the following:

> The position and velocities of the earth and the moon in the earth-moon system (i.e. the physical system X) are very close to those of a two-particle Newtonian system with an inverse-square central force (the

---

[5] This latter point can be made by noting the well accepted fact that scientific theories may be presented in a variety of natural languages; and yet General Relativity is General Relativity whether it is phrased in German, English, or Arabic.

[6] For instance, the semantic approach allows one to distinguish the empirical or observational content of scientific theories from their non-observable content at the abstract, non-linguistic level through the notion of a substructure. In this way, and as we shall see in more detail in section 5 of this chapter, the semantic approach provides new life to the philosophical tradition of empiricism by allowing one to identify the observational content of scientific theories in a non-linguistic manner.

[7] These real-world objects and relations are often called "concrete" structures in order to differentiate them from the abstract mathematical structures that serve as models of a mathematical theory.

> abstract entity M described in the *model*). (Psillos, 1999, p.274, cf. Giere, 1988, p.81 and Chakravartty, 2007, p.199)[8]

It is only by employing implicit (or explicit) theoretical hypotheses that the collection of models typifying a scientific theory can be linked to the world, and the theory itself thought of as truth-valued. So, Giere (1988, p.85) considers a scientific theory to consist of both a collection of models (delineated through their satisfaction of some mathematical theory) *and* a collection of theoretical hypotheses linking these models to the world.

The basic idea here is that once we have a collection of models, we can judge how well these models mimic or imitate certain real world systems when they are seen as representations of these systems. This is how scientific theories describe or represent the world: first a class of models is delineated, then this class of models is compared with the nomically permissible states of the objects and relations that make up some real world collection of systems through a collection of theoretical hypotheses.[9] According to the semantic approach, it is in this sense – the sense that the models of a scientific theory have a preferred comparison with certain real-world systems, i.e. a set of associated theoretical hypotheses – that a scientific theory can itself be spoken of as true or false. When a scientist says "general relativity is true," she means something like "the class of models that characterizes General Relativity very accurately captures the possible real-world, spatio-temporal relations between macroscopic objects permitted by the laws of nature when the relations in these models are interpreted as spatio-temporal relations."

To make all of this more transparent, we might note with Suppes (1967, pp.60-62) and French and da Costa (2003, pp.29-36) that there are two non-exclusive yet distinct ways to view scientific theories within the semantic approach: one "extrinsic", the other "intrinsic". Viewed extrinsically, theories are just collections of models – mathematical

---

8 Theoretical Hypotheses can also be universal in scope, e.g. "everything acts as a Newtonian system," "our universe is Newtonian," or "Newtonian mechanics can provide a model that accurately represents the motions of every real-world system of objects."

9 While the models of mathematical theories are used to satisfy its axioms, the models of scientific theories are used to compare this class of models with certain real-world systems. This is why Suppes claims that, while there is no difference in kind between the models used in the mathematical and empirical science, there is a difference in use.

structures, which are non-linguistic, abstract objects. Scientific theories cannot be the object of belief from the extrinsic view – they are just families of models, apart from any particular mathematical theory or set of theoretical hypotheses. These structures can, however, be investigated formally using various mathematical tools and techniques. This allows us to compare collections of models with one another, as well as bracket certain portions or subclasses of a particular scientific theory's class of models off from other portions. This can be very useful for reasons to be outlined in the next section.

Viewed intrinsically, by contrast, the models typifying theories can be (and often are) the objects of our epistemic attitudes. This is not to say that, when we view theories intrinsically, we *must* believe them. To view a theory intrinsically is simply to view its models in terms of their associated theoretical hypotheses. From an intrinsic view of theories we talk about the similarity between the relational structure of certain models and the relations of certain real-world properties. As Campbell (1994) notes, what is "referential" about scientific theories, on this view, are the *relations* defined in the models, given a standard or intended comparison of these models with the world (French and Da Costa, 2003, Ch.3, n.48)[10]. When we speak of a scientific theory as true or false, we simply mean that, from an intrinsic point of view, the models of that theory capture the properties of and relations between the real world objects referred to in its theoretical hypotheses, i.e. that its theoretical hypotheses are true.

§3 – Some Benefits of the Semantic Approach: Incommensurability, Selective Skepticism, and a Realist Conception of Truth

While acceptance of the semantic approach does not imply an endorsement of scientific realism or constructive empiricism, its non-linguistic characterization of scientific theories makes some jobs for the constructive empiricist and scientific realist easier. First and foremost, in viewing a scientific theory's typical class of models extrinsically, we can divide the class of models that characterizes a theory up into parts. We can then

---

[10] Emphasis of this point is typical of structural realists, particularly in James Ladyman's defense of his ontic structural realism, which denies that there is anything other than relations between real-world properties to refer to in the first place.

compare certain portions of different theories with one another, both in terms of subclasses of a theory's class of models and in terms of substructures of individual models within this class. This is important for realists because they need to avoid issues surrounding the incommensurability of successive scientific theories. With an extrinsic view of theories in hand, the realist can argue that certain substructures or subclasses of well accepted scientific theories are retained over time, throughout theoretical shifts into successive theories, at least in approximation. For example, we can show that the models of Newtonian mechanics are, in many instances, good approximations to relativistic models of the same target systems – those models of objects moving at very slow relative velocities within the same inertial frame, for example, are good approximations of Newtonian models of such systems. With such a notion of theoretical continuity in hand, the broadly cumulative (though fallibilist) picture of scientific inquiry needed for a defense of scientific realism can be achieved. How the realist finds theoretical continuity throughout the history of science is something that will be elaborated in Section 2.7 of the next chapter.

The tools provided by an extrinsic, model theoretic treatment of theories has benefits beyond the restoration of a cumulative picture of scientific inquiry in the face of essentially Kuhnian concerns about incommensurability. By speaking of substructures and subclasses of models, we can bracket certain portions of a theory's models off from other portions; for example, in terms of which of the real-world objects it refers to are *observable* by humans. This allows us, among other things, to be *selectively skeptical* about the truth or falsity of certain portions of our theory – to believe that, from an intrinsic view, certain portions of our theory are true while remaining agnostic about the truth or falsity of other portions.

As we shall see in Chapter 2, selective skepticism comes in many forms, and the difference between constructive empiricists and scientific realists is best understood as a difference in the portions of scientific theories for which they recommend belief and agnosticism. Selective skepticism is only made possible through an ability to divide up theoretical content. Where (according to some) the logical positivists failed in their attempts to isolate the observational content of scientific theories, van Fraassen claims that the semantic approach allows a new observable/unobservable distinction to be drawn that avoids the problems associated with linguistic attempts to identify the empirical or observational content of scientific theories. In this way, the semantic approach to theories allows van Fraassen to reinvent anti-realist empiricism, as we shall see in the last section of this chapter.

The semantic account of theories also facilitates, or at least is consistent with, a realist account of truth. As we shall see in the next section, scientific realism and constructive empiricism share a particular, non-deflationary, non-epistemic interpretation of the Tarskian t-sentence. It is this realist theory of truth that Dummett (1962) sees all anti-realists philosophers rebelling against. This is the so called "literal" or "correspondence" theory of truth, according to which truth is correspondence with the facts. This non-epistemic interpretation of t-sentences facilitates a distinction between a statement's truth-conditions and the conditions under which we could know that statement to be true. This distinction allows the scientific realist and the constructive empiricist to agree about what it takes for a statement to be true while disagreeing about which statements we should take ourselves to know. This is why constructive empiricism is not anti-realist in Dummett's sense: the constructive empiricist disputes the realist's epistemology, rather than the realist's semantics. It is to the details of this shared notion of truth that I now turn.

§4 – Truth as Correspondence: T-Sentences, Truth-Conditions and Non-Epistemic Truth

According to a realist conception of truth, truth is a "non-epistemic concept" (Psillos, 1999, p. xxi, cf. Chakravartty, 2007, p.10 and Alston, 1996). This simply means that a statement's truth or falsity obtains independently of our ability to determine its truth or falsity – truth depends on the actual state of things in the world, rather than on what we say about the world (contra nominalism and constructivism) or what we actually experience (contra idealism). What makes this a *realist* understanding of truth is that the truth or falsity of a statement depends on *reality*, and nothing else. As William Alston puts it:

> "If what is being stated is that grass is green then it is grass's *being* green that is both necessary and sufficient for the truth of the statement. Nothing else is relevant to its truth value" (Alston, 1996, p.7).

That nothing other than the state of the world is relevant to a statement's truth values does not mean that nothing else (such as, for instance, context or convention) can be relevant to its meaning or reference. Quite to the contrary – it seems undeniable that context and convention and many other things have quite a lot to do with the meaning of most statements. Nevertheless, the sole relevant factor in deciding a statement's truth value is whether or not the statement accurately represents the actual state of the world.

In conceiving of truth non-epistemically we can distinguish truth conditions from knowledge conditions. Once we take the meaning of a statement for granted, we can see that the truth-makers of that statement are solely the states of certain things in the world; these conditions for a given statement *being* true, however, are very different from the conditions for *knowing* that statement to be true. The former concerns whether or not a statement accurately represents the relevant worldly facts, whereas the latter concerns *our judgments* about whether a statement represents the relevant worldly facts accurately. This distinction supports the realist conception of scientific falliblism and scientific discovery: we may believe a false statement to be true, or vice versa. For the realist, one of the primary goals of scientific inquiry is trying to discover which statements are true and which are false.

In articulating what exactly a truth condition is, many realists will appeal to Tarski-style t-sentences, taking them to be "neat" expressions of the conditions under which any given statement is true or false. Take one of the classic examples of a t-sentence:

"Grass is green" is true if and only if grass is green.

We take the instance of "grass is green" inside the quotations to refer to the statement itself, and the instance outside the quotations to actually make the statement, i.e. to describe the state of things in the world. For the realist, the t-sentence is a clear way to display what is required for a factual statement to be true: a statement is true if and only if the world is as the statement says it is.

This use of t-sentences might seem like a trivial articulation of a statement's truth conditions, for it assumes that one already knows the meaning of the statement "grass is green." This is merely a consequence of our inability to describe the state of the world without using a language we are already familiar with. The triviality of truth conditions expressed via t-sentences is lessened once we realize that, if one does not know the meaning of the statement inside the quotations, the state of affairs described outside the quotations can be stated in a more familiar language – say in German rather than English. In this way, t-sentences can provide the realist with truth conditions for any factual statement in the object language in terms of a state of the world described in a more familiar meta-language. Using t-sentences in this way, realists make as much sense as they can of the doctrine that a statement is true when the world is as the statement says it is, without privileging any of the various ways that words may come to represent states of affairs.

Realists can thus separate the issue of how to determine the truth or falsity of a given statement from the issue of what it takes for a statement to be true. The former involves a complex interplay between history, experiment, hypothesis, instrumentation, measurement conventions, social institutions, goals, values, and attitudes, and it also depends on what we mean when we say the word "know". The latter, however, according to the realist, has only to do with the state of things in the world. It is precisely this non-deflationary, realist understanding of t-sentences, divorced from our epistemic policies and practices, that Dummett thinks anti-realists have rebelled against in favor of a more epistemologically laden notion of truth – "knowable" truth, as Dummett puts it.

Constructive empiricists, however, share this non-epistemic notion of truth with the realist. Where these two philosophies diverge is in the epistemological principles they license, that is, in their account of when we should take ourselves to know the truth or falsity of certain statements. How and why these philosophies diverge epistemologically will be outlined in Chapter Two. Before detailing these differences, there is one last shared similarity that must be addressed in this chapter: a new, model-theoretic method of demarcating the observational content of scientific theories.

§5 – Reviving Empiricism: The New Observable/Unobservable Distinction

Attempting to isolate the observational content of scientific theories is a theme common to all anti-realist empiricisms. The logical empiricists attempted to identify the observational content of scientific theories by means of a linguistic distinction between the observational language of a theory and its theoretical language. Van Fraassen rightly sees that such a distinction cannot be usefully made, and tries to formulate a new method of identifying the observational content of scientific theories, one that avoids the criticisms levied against the logical empiricists' method.

Maxwell (1962) argues against empiricists' attempts to demarcate a theory's observational content in two ways. First, he argues that any such demarcation cannot be made linguistically, that is, by identifying a theoretical vocabulary and an observational vocabulary. Second, he argues against the ontological significance of any such demarcation for a philosophy of science. Even a non-linguistic distinction between the observable and non-observable content of our scientific theories could have no ontological significance, he says, and is at best "an accident and a function of our physiological make-up" (Maxwell, p.38; cf. Van Fraassen, p.18).

Working within the non-linguistic characterization of theories known as the semantic approach, van Fraassen formulates a new, non-linguistic method of demarcating the observational content of theories that avoids both of Maxwell's criticisms. He agrees with Maxwell that one cannot distinguish the observational content of a theory by means of a linguistic distinction between an observational vocabulary and a theoretical vocabulary; but a *non-linguistic* distinction between observables and unobservables can be achieved using an extrinsic, semantic approach to understanding scientific theories.

Van Fraassen notes that within each model of a theory there will be some definable substructure which corresponds to a collection of "appearances" or *observables*. Such a structure is called an *empirical substructure* of the theory, which is the portion of the theory's models that stands for all and only those objects that we can perceive with our unaided sensory-perceptual capacities: those things that we can, as humans, see, hear, taste, touch, and smell. These are the perceptual objects of ordinary experience: tables, chairs, cows, milk, walls, glass, and mice, to name a few.

Observables, as a group, are most naturally contrasted with those objects that we cannot perceive: the *unobservable* entities typically posited by scientific theories as (at least partly) causally responsible for the state of observables: viruses, capillaries, electrons, positrons, molecules, DNA, photons, quarks, strings, and many others. These unobservables may only be detected through the use of scientific instrumentation, or not at all. The existence or non-existence of such putative unobservable entities must be inferred from our observations of observable entities. These unobservable objects are part of the *theoretical superstructure* of a theory, and are not included in the empirical substructure of a theory. Constructive empiricists, *qua* anti-realists, will remain selectively skeptical about the truth or falsity of all statements regarding the putative reference of theoretical superstructures (i.e. its truth with respect to unobservables), and will at most commit only to a theory's *empirical adequacy* (i.e. its truth with respect to observables).

In Newton's Theory (mechanics and gravitation), for instance, we see a distinction between apparent or relative motion and true or absolute motion (van Fraassen, 1980, pp.45-47). Apparent motions are the motions of certain observables like planets, cannonballs, human beings and billiard balls relative to other observables *as they are observed*, i.e. as they appear relative to a "fixed" observer. To describe and predict these motions, Newtonian mechanics posits classes of unobservable entities in absolute space – absolute vectors, positions, forces, etc. – that result in the apparent motions which we observe. As van Fraassen puts it:

> "In the mathematical model provided by Newton's theory, bodies are located in Absolute Space, in which they have real or absolute motions. But within these models we can define structures that are meant to be exact reflections of those appearances, and are, as Newton says, identifiable as difference between true motions. These structures, defined in terms of the relevant relations between absolute locations and absolute times, which are the appropriate parts of Newton's models, I shall call *motions* ... When Newton claims empirical adequacy for his theory, he is claiming that his theory has some model such that *all actual appearances are identifiable with* (*isomorphic to*) *motions* in that model." (ibid, p.45, emphasis in original).

What we observe are these "apparent" motions, which Newtonian mechanics sees as differences between "true" or "absolute" motions relative to an observer. The existence of the unobservable, absolute positions, vectors, and magnitudes that this theory posits as responsible for our observations could only ever be inferred from our observations of the apparent motions described in its models. We can identify similar empirical substructures within many of our other theories that speak of unobservables as well: in the case of atomic theory, for example, the empirical substructure would be the portion of this theory's models that stand for things like cloud chambers, metals, lenses, human observers, and other kinds of scientific instruments and instrumental readouts.

The observable/unobservable distinction is something that is modeled for each theory individually. In each case we will have to think about what it is that we are observing and what it is that we are inferring from these observations; and while there may be some indeterminate, vague, or problematic cases, there will be many clear-cut cases. Van Fraassen discusses the situation like this:

> "A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well up close, but the purported observation of micro-particles in a cloud chamber seems to me a clearly different case—if our theory about what happens there is right. The theory says that if a charged particle traverses a chamber filled

> with saturated vapour, some atoms in the neighbourhood of its path are ionized. If this vapour is decompressed, and hence becomes supersaturated, it condenses in droplets on the ions, thus marking the path of the particle. The resulting silver-grey line is similar (physically as well as in appearance) to the vapour trail left in the sky when a jet passes. Suppose I point to such a trail and say: 'Look, there is a jet!'; might you not say: 'I see the vapour trail, but where is the jet?' Then I would answer: 'Look just a bit ahead of the trail ... there! Do you see it?' Now, in the case of the cloud chamber this response is not possible. So while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the particle's being observed" (ibid, p.16-17).

This new *non-linguistic* distinction is not a distinction between the observational and theoretical vocabularies of a language. Nor is it an *ontological* distinction between what is more or less real. It is an *epistemological* distinction between certain kinds of putative entities in terms of how they are related to our sensory-perceptual apparatus (van Fraassen, 1980, p.14). This distinction between observables and unobservables is a function of our epistemic community's physiological makeup: of what we can, under suitable conditions and as a matter of historical accident, perceive with our unaided senses. The reason that the moons of Jupiter are observable, for instance, despite the fact that at present we can only see them with the aid of a telescope, is that we *could*, under suitable conditions, see them with the naked eye, if only we were close enough (say, in a spacecraft of some sort). As van Fraassen puts it, "[t]he human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations ... it is these limitations to which the 'able' in 'observable' refers – our limitations, qua human beings" (van Fraassen, 1980, p.17). This distinction is deeply anthropocentric, dividing objects according to our ability or inability, as humans, to perceive them. Such a distinction avoids both of Maxwell's objections, for despite its lack of *ontological* significance, van Fraassen rightly notes that such a distinction surely has *epistemological* significance, especially for a scientific community entirely made up of human beings with limited and fairly homogeneous sensory-perceptual capacities.

Unlike an instrumentalist's demarcation of the observational content of theories, van Fraassen's distinction does not demarcate the real from the unreal, the more real from the less real, the facts from the elaborate machinery we use track and order these facts, nor what we know to be true from what we merely assume to be true; rather, it demarcates the belief-worthy from the non-belief worthy. This distinction divides putative objects into those that we, qua human beings, can perceive with our unaided senses; 'observable' here means 'observable-to-us'. As a result, statements regarding observables and unobservables are treated symmetrically, at least in terms of their semantics, meaning that theoretical statements about unobservables are treated as true or false in exactly the same way as statements about observables.

Despite this *semantic* symmetry regarding a theory's claims about observables and unobservables, there is nevertheless an *epistemic* asymmetry regarding such claims. When we make a claim about an observable, such as a passenger jet, we could under suitable conditions judge the truth or falsity of this claim through a simple perceptual act. If what we perceive about this object is not what was claimed, we know that the claim is false. When we claim that a theory captures all (or some) relevant observable truths, we can go out and begin to check this through an extensive series of perceptual acts. If what the theory predicts about observables is not what we observe, we know that the theory is not empirically adequate, i.e. that it is not "true with respect to observables."

By contrast, when we make a claim about an unobservable entity, such as an electron, we must always infer the truth or falsity of this claim from the electron's apparent effects on observables. Similarly, when we claim that a theory captures all (or some) relevant facts about unobservables, we have no extensive series of perceptual acts that could be used to directly check the truth of this claim. We can only ever directly check the truth of our theories with respect to observables, and therefore we can only ever infer a theory's truth with respect to unobservables from its perceived truth with respect to observables. That is to say, our observations serve as the only available evidential basis upon which we might judge the truth or falsity of claims about unobservables.

The epistemic asymmetry of our claims about observables and unobservables results from the underdetermination of theoretical superstructures by their empirical substructures. For any given empirical substructure there will be multiple theoretical superstructures that share that empirical substructure. As a result, a belief that a theory's empirical substructure accurately represents the facts with respect to observables will never uniquely determine a belief that some particular theoretical superstructure accurately represents the facts with respect to unobservables. Theories are thus perpetually underdetermined by the available evidence – in accepting certain sets of statements about observables, we are not forced to accept any unique set of statements regarding unobservables. This underdetermination of theoretical superstructures by their empirical substructures is van Fraassen's main justification for being skeptical or agnostic about claims regarding unobservables – the defining feature of his anti-realist empiricism.

For van Fraassen, the observable/unobservable distinction is the most significant epistemological distinction, and he thinks that working scientists recognize this. Van Fraassen cites Newton, for instance, as a scientist who explicitly did not claim that his theory was true with respect to observables *and* unobservables, but merely claimed that it was empirically adequate. Accordingly, so far as belief in a theory's truth is concerned, van Fraassen sees acceptance of a scientific theory as involving only a belief that this theory is true with respect to observables, combined with an agnosticism regarding its truth or falsity with respect to unobservables. This contrasts with the realist's account of what it means for a scientist to accept a theory: for the realist, acceptance involves a belief that the theory is true with respect to *both* observables and *unobservables.* These divergent accounts of what it means for a scientist to accept a theory will be outlined in more detail in the next chapter.

Van Fraassen's new observable/unobservable distinction surely has epistemological significance. After all, why should we not be more confident that theories are true with respect to observable entities than with respect to unobservables? We can see and feel and touch observable things, and check our beliefs about them with our unaided senses. By contrast, no one has ever perceived an electron, at least not on any suitably thick notion of perception. Why risk being wrong and believing anything more about a successful scientific theory than that it is a good guide to truth regarding observables?

Do we have good reason to believe that successful scientific theories are good guides to truth about unobservables as well as observables? These questions, and others like them, are at the foundation of the dispute between scientific realism and van Fraassen's constructive empiricism. It is to the divergent features of these opposed philosophies, understood within the shared foundations outlined here, that I turn to in the next chapter.

Chapter Two – The Divergent Doctrines of Scientific Realism and Constructive Empiricism

§1 – Scientific Realism and Constructive Empiricism: Differences

In his 1980 book *The Scientific Image*, van Fraassen formulates a position he calls constructive empiricism. He does this in order to provide a tenable empiricist alternative to scientific realism. He formulates this position because, simply put, he thinks that empiricism is correct; but he also thinks that empiricism "could not live in the linguistic form the positivists gave it" (1980, p.3). Many of the deficiencies of a linguistic characterization of theories are, however, rectified through an adoption of a semantic or model-theoretic understanding of theories. In particular, the well known problems with identifying the observational content of theories through linguistic characterizations of scientific theories can be avoided. In this chapter I outline the central tenets of scientific realism and constructive empiricism, as well as the most traditional arguments for and against each of them.

Van Fraassen notes that there are at least two aspects to any account of scientific realism: first, it includes an account of what it means for a theory to be true, and second, it includes an account of what it means for a scientist to accept a theory. Van Fraassen is an anti-realist, but he nonetheless gives the same account of what it means for a theory to be true as the realist does. This realist conception of truth is the "ancient" or "literal" conception of truth, according to which truth is "the agreement with the facts of what is being asserted".

What makes van Fraassen an anti-realist is not his view of truth; rather, it is his view of acceptance (van Fraassen, 1980, p.38). Van Fraassen thinks that, when scientists accept a scientific theory, they do not believe that it is true with respect to both observables and unobservables. We can see their acceptance of a theory as belief that it is true with respect to observables alone, combined with a pragmatic adherence to a research programme based on the theory. While scientific theories and descriptions should be interpreted literally, the aim of science is not, for van Fraassen, to give us literally true theories: science aims only to give us theories that are empirically adequate, i.e. true with respect to observables.

Van Fraassen contrasts scientific realism with his constructive empiricism in terms of two factors. First, there is an account of what the aim of science is, and second, there is an account of what it means for a scientist to accept a theory. Accepting one version of the first factor leads one to accept a harmonious version of the second factor. He characterizes scientific realism thusly:

"*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true*" (1980, p.8).

Van Fraassen then states his empiricist alternative to scientific realism thusly:

"*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate*" (1980, p.12).

For van Fraassen, when the scientist accepts a scientific theory, she believes that it is true with respect to observables, but *not* unobservables. She does not have evidential reasons to believe that statements about unobservables are false – but given her inability to perceive them, and the underdetermination of theories by their empirical evidence, she has more than enough reason to remain agnostic about the truth or falsity of claims regarding unobservables. In claiming that there exists this epistemic asymmetry with respect to observables and unobservables, van Fraassen is upholding that long history of empiricism he is so confident is correct.

By "empiricism" van Fraassen seems to mean a general abhorrence of metaphysical speculation, scientifically informed or otherwise (see his 2002, Ch.1 and 2) . With respect to scientific theories, the general idea for empiricists is that *the only important part of scientific theories is their observational content*. Both "important" and "observational content" have been cashed out differently by different empiricists. The more phenomenally inclined empiricism might say "cognitively relevant" instead of "important," for instance; other possibilities include "factual", "actual", "positive", "verifiable", "denotative", "trustworthy", "real", "knowable", etc. Van Fraassen, for his part, would put this importance in terms of what scientists are warranted in accepting about a successful scientific theory: "to accept [a] theory involves no more belief ... than that what it says about observable phenomena is correct." (1980, p.57) For him, the observational content of a scientific theory is the important part because it is the only "belief-worthy" portion, where theoretical hypotheses that go beyond the observable phenomena are treated skeptically as just so much "non-belief-worthy" metaphysics. According to van Fraassen, the observational content of a scientific theory is just that portion of the theory that regards objects we can observe, as opposed to objects we must detect or infer.

All of this conflicts with realism because realism claims that in accepting a scientific theory the scientist believes that the theory is true with respect to both observables and unobservables. But surely realists are empiricists in some sense of empiricism, given their emphasis on the importance of experimentation and evidential support in theory evaluation and scientific advancement. Thus, we may distinguish two strands of empiricism, one claiming that experience is the *source* of all knowledge, and the other claiming that all knowledge of the world is (exclusively) *about* experience (Chakravartty, 2007, p.15). Most realists are likely empiricists in the first sense: it is the latter sense of empiricism that conflicts with realism, for realists believe that we can have knowledge of things which go beyond our experience. This is where realism and constructive empiricism diverge, for constructive empiricism is empiricist in both senses, denying that we can have knowledge of things that go beyond the possibilities of our immediate perceptual experience.

§2 – Scientific Realism: A three point thesis

Since constructive empiricism borrows heavily from scientific realism in a variety of ways, an explication of both positions should begin with scientific realism. Psillos (1999) and Chakravartty (2007) have both attempted to give general characterizations of the problems facing scientific realism, as well as to provide some responses to anti-realist objections on behalf of the realist. One of the primary competitors with which both of these authors contrast their account of realism is van Fraassen's constructive empiricism, making these explications of realism ideal and up-to-date candidates for informing my explication of realism.

As Chakravartty puts it, "The most pernicious way of understanding [scientific realism] is in terms of three lines of inquiry: ontological, semantic, and epistemological" (Chakravartty, 2007, p.9). Allow me to phrase the realist's typical answers to these lines of inquiry as a set of three claims or theses[1]:

> T1) – the **semantic** thesis: scientific theories are to be construed literally, and truth is to be conceived non-epistemically, meaning that scientific theories are considered true or false based on the state of things in the world. This commits realists to some sort of literal or correspondence theory of truth.
>
> T2) – the **ontological** thesis: (some significant portion of) the observable *and* unobservable objects, entities, and relations described in a given theory exist objectively or mind-independently as they are described, independently of our description, knowledge, or perception of them. This commits realists to the belief that a given theory is true with respect to both observables and unobservables.
>
> T3) – the **epistemological** thesis: scientists are warranted in accepting that T2 is true with respect to any genuinely predictively successful scientific theory, i.e. that the theory gives literally true descriptions of both the observable *and* the unobservable, mind-independent world. This commits realists to the belief that one is warranted in accepting that genuinely predictively successful theories are true with respect to both observables *and* unobservables.

Various qualifications beyond van Fraassen's short characterization of scientific realism have been integrated into these three theses. The central idea is that a scientist is warranted in believing that any genuinely predictively successful scientific theory is a good guide to literal truths about unobservables as well as observables. We can summarize all of this as the claim that "[t]he things our best scientific theories tell us about entities and processes are decent descriptions of the way the world really is" (Chakravartty, 2007, p.9).

---

[1] For alternative formulations of realism, see for example French and Da Costa, 2003, p.160.

T3 can be elaborated upon in a variety ways to provide some account of theory evaluation that goes beyond "genuinely predictively successful." Such accounts will often include descriptions of certain "virtues" that theories may or may not possess. Realists make many different caveats and develop various strategies of theory evaluation in order to determine which of our current scientific theories or explanations are "best," and therefore belief-worthy. This point will be elaborated on later in this chapter.

The constructive empiricist accepts T1 and the realist conception of truth implicit therein. But despite their agreement regarding T1, the realist and constructive empiricist still have plenty to disagree about. Their dispute is usually cast in terms of what it is *rational* to accept about successful scientific theories: whether a scientist should believe that a successful scientific theory is true with respect to observables and unobservables, or merely true with respect to observables. While I outline the debate in such terms here, the third and final chapter of this essay will be devoted to moving beyond casting the debate in terms of what it is *rational* to accept about a successful scientific theory, looking instead at the kind of questions we *need* answered about our scientific theories in order to help resolve important theoretical disputes that matter to our long-term planning. I will argue that we will sometimes need to ask realist questions about our scientific theories, and that van Fraassen's account of what it means for a scientist to accept a theory will not apply to some groups of non-scientists for whom truth about unobservables is incredibly important. I contend that when many people ask which theory they should accept, they are asking which theory they should accept in the *realist* sense because truth about unobservables is more relevant to their needs than simply truth about observables. While constructive empiricism might suffice as an account of what it means for the *scientist* to accept a theory, it fails as an account of what it means for other groups to accept a theory – for example, policy makers and long-term planners. This constitutes an argument in favour of developing a realist epistemology and against the general applicability of constructive empiricism.

Before presenting this new argument in favour of scientific realism in detail, I will stick to more traditional modes of characterizing the dispute between realists and constructive empiricists. I will first outline the traditional argument given in favour of scientific realism, followed by an outline of the traditional arguments against it. This will be followed by an explication of constructive empiricism and van Fraassen's arguments in favour of it. After all of this, I will sum up the situation by showing how scientific realism can avoid the main arguments against it and, following van Fraassen and Chakravartty, I will finish by characterizing the dispute between scientific realism and constructive empiricism as part of a larger dispute between opposed epistemological stances.

§3 – Arguing for Scientific Realism: The No-Miracle Argument

A single argument has traditionally been put forward as the predominant reason to accept scientific realism. It was made most famous by Hilary Putnam and Richard Boyd under the heading of "The Ultimate Argument", but as of late this argument has come to be almost uniformly called the "No-Miracles Argument" (NMA), and I will not deviate from this convention here. Although it has been phrased in many different ways and given many different names over the years, essentially the argument is this:

1) Most of our currently accepted scientific theories are very successful at making very accurate predictions.

2) The only (or at least best) explanation of this success is that most of what these theories say is true. The only alternative explanation is a kind of cosmic coincidence or miracle by which a false theory is able to make true predictions.

3) We do not want to accept cosmic coincidences or unexplained miracles.

Therefore,

4) We should accept that most of what these scientific theories say is true.

Scientific realism, in making the NMA, is thus said to be the only philosophy of science that does not make the success of science a miracle (Putnam, 1975, p. 73).

The NMA is a clear instance of inference to the best explanation (IBE), an argumentative form that calls on us to accept the best available explanans for any given explanandum[2]. In the case of the NMA, the explanandum is the predictive success of any given scientific theory, where the proposed explanans is that theory's truth with respect to observables and unobservables. The hypothesis that a successful scientific theory is true with respect to observables and unobservables is seen by realists as an explanation of its predictive successes, an explanation which anti-realist philosophies are supposed to lack. The only other explanation available is that a false theory is miraculously able to make true predictions. Since hardly anyone is willing to accept miracles as good explanations, when given only these two potential explanations and asked to accept the better of the two, one should accept, with the realist, that predictively successful scientific theories are most likely mostly true.

IBE is seen by most realists as the foundational method of scientific inference, and the NMA is often put forward as an attempt to explain the successes of scientific theories scientifically. Since scientists use IBE to favour certain scientific theories over others, they can and should use IBE to favour the realist's explanation of scientific success over others, since the realist's explanation is the best available; and so should everyone, because scientific methods of reasoning are rational, and we should all do what is rational.

The NMA could only ever be persuasive if we accept the realist's T1, a thesis accepted by some anti-realists such as constructive empiricists and (some) skeptics. Without accepting the non-epistemic notion of truth implicit in T1, no sense could be made of the claim in T2 that the objects described by our scientific theories exist "objectively or mind-independently, that is, independent of our description, knowledge, or perception of

---

2 An *explanandum* is a (set of) fact(s) to be explained, while the *explanans* is the (set of) fact(s) that would, if true, explain the explanandum

them". Only within a non-epistemic conception of truth could a statement be thought of as true without anyone knowing it to be true.

After assuming T1, the NMA justifies the epistemic policy of T3 using IBE. It says, essentially, that the only explanation we have for any given theory's predictive success is that T2 is true with respect to the theory, i.e. that the theory is mostly true, with respect to both observables and unobservables. The realist, qua realist, thinks that an IBE of this sort will always give us sufficient reason to believe that T2 is true with respect to any predictively successful scientific theory. As a result, the realist *licenses* the use of T3, meaning that they see inferring the truth of most of what a theory says based on that theory's predictive successes as a reliable method of inference. Realists think that this licensing is warranted because the only (or at least best) explanation we have for the success of a scientific theory's predictions about observables is that it correctly identifies the underlying unobservable causes of our observations, i.e. that it is true with respect to more than just observables. The only other possible explanation is the miraculous prediction of truths about observables by a theory that gets all the unobservable truths wrong. In lieu of accepting the existence of miracles, the realist is ready and willing to accept the "better" explanation – that any predictively successful scientific theory correctly identifies the unobservable causes of observables states of affairs, i.e. that the theory is true with respect to both observables and unobservables. Since the NMA interweaves the realist's ontological and epistemological theses, this argument is integral to maintaining a coherent scientific realism.[3]

Without an argument to the contrary, it seems reasonable to believe that the epistemological policy embodied in T3 represents a reliable method of inference. The problem for the realist is that many such counter arguments have been presented by anti-

---

[3] Despite the primacy of the NMA in defences of realism, many realists will admit that it is a fairly weak argument, drawing more on intuition than on sound reasoning, at least in the simple form that it is usually presented, as it is here. This argument is open to many objections, and has been the subject of much conceptual buttressing on the part of the realist. A more plausible tactic to take in the defence of realism is to defend it as a stance, a tactic that will be elaborated in the final section of this Chapter. Until then, I will continue along more traditional trajectories with respect to arguments for and against scientific realism.

realists of various stripes. Such arguments have generally come in one of three forms: a) a number of counter cases, where we recognize that T2 is manifestly *false* with respect to some predictively successful scientific theories, b) a better explanation of a theory's predictive successes that does not appeal to the truth of T2 with respect to that theory in order to explain its predictive successes, or c) an attack on the reliability or validity of IBE in general. As we will see in the next section, the first of these anti-realist counterarguments is by far the most pressing.

§4 – Arguing against Scientific Realism: The Pessimistic Induction and Alternative Explanations of Scientific Success

While the NMA is thought to be the typical argument made in favour of Scientific Realism, there is another argument that is almost as typical of anti-realist philosophies – the Pessimistic Induction (PI). The argument can be traced back to the writings of both Kuhn and Poincaré, who noted that the historical record seems to show radical theoretical discontinuity between successive scientific theories. Today this argument and its name come to us from the work of Larry Laudan (see especially his 1981 and 1984). At the center of this argument is an attack on the realist's NMA, specifically the IBE used by the realist to license T3. The PI challenges the supposed historical facts upon which the realist's IBE rests by offering up counter cases from the history of science – instances where the realist's epistemic policies would have led one to beliefs that, from our modern vantage point, are manifestly false.

So far, the explanatory value of T2 is the only positive reason that the NMA gives for thinking that T2 is true with respect to all predictively successful scientific theories. The only alternative explanation that T2 is weighed against is the miraculous ability of a false theory to produce accurate predictions. But even in the absence of any better explanation, it is clear that if an anti-realist could show that T2 were false with respect to a significant portion of predictively successful scientific theories, T3 would need to be rejected. If we did not reject T3 in such a situation, we would clearly be licensing an unreliable epistemic principle, for it would regularly allow us to infer the truth of false theories.

The PI argues that T3 *is* in fact known to be unreliable, given the radical discontinuity evident throughout the history of science. For in the past, very accurate predictions were made using theories that are, strictly speaking, *false* from our modern vantage point[4]. Licensing T3 would have allowed realists to infer the existence of many non-existent entities, including the luminiferous ether, caloric, and phlogiston. Since we now know that these entities do not exist, despite the fact that they were posited by genuinely predictively successful scientific theories in the past, inferring the truth of a predictively successful scientific theory is clearly unreliable. This also gives us good inductive reason to believe that T2 is false with respect to our *currently* accepted theories as well. Thus, we *cannot* license T3. In short, we actually know that T2 *is* false with respect a great many genuinely predictively successful scientific theories, despite its potential explanatory value, so we *must* reject T3 as an epistemological principle, and thereby reject scientific realism as a philosophy of science.

Articulation of the PI is usually followed by an anti-realist diagnosis of the problematic step in the NMA: the realist assertion that realism is the best or only explanation of a scientific theory's novel predictive success. "You were wrong," says the anti-realist, "there is another, perhaps even better explanation." In this way, the anti-realist attacks the IBE of the NMA, again not by attacking the legitimacy of IBE itself, but by providing

---

[4] Alan Musgrave and Stathis Psillos have emphasized the fact that the NMA will only work if we consider only scientific theories that are mature – that is, scientific theories that have made novel predictions. As a paradigm example from the history of optics, recall the prediction made by Poisson of a bright spot at the centre of an opaque circle's shadow. Since Fresnel's equations were formulated in order to describe much different optical phenomena, the bright-spot consequence is thought to be a novel consequence. The actual occurrence of this novel phenomenon constitutes independent evidence in favour of Fresnel's wave theory of light. The observation of this bright spot thus corroborates the wave theory of light, and elevates it to the status of a mature scientific theory. While this criterion of maturity for the realist's belief greatly reduces the number of cases Laudan is able to appeal to in making the case for radical discontinuity in scientific history (see Psillos, 1999), there are still a few problematic cases such as the undeniable maturity of (false) ether theories of light, Newtonian theories of motion, and caloric theories of heat. This is enough to cast doubt on the overall continuity of scientific theories, even if the criterion of maturity lessens that doubt significantly.

an alternative explanation of a scientific theory's genuine predictive successes. This alternative explanation of scientific success has perhaps been best and most famously articulated by van Fraassen. He writes:

> I claim that the success of science is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive – the ones which *in fact* latched on to actual regularities in nature. (1980, p.40)

Just as a successful species of organism survives for long periods of time in an environment crawling with potential competitors by adapting to its environment, scientific theories survive by adapting to their environment. This means making accurate predictions by capturing the real, natural, observed regularities that characterize its empirical domain. When a theory gets these observable facts consistently correct to a higher degree of accuracy than any other theory in its domain (i.e. when it is the most empirically adequate theory available) we accept that theory, and thereby allow it to survive. A scientific theory's ability to survive is based on the production of usable results; that is to say, we select theories for their production of usable results. "Usable results" here means making accurate predictions.

So, since scientific theories are selected and accepted for their usefulness in making accurate predictions, it is an unsurprising and anti-climactic result that the most well-accepted ones, in fact, make accurate predictions. Simply put, the success of science is no miracle because the only acceptable science is predictively successful science. We do not need the realist's *adequatio ad rem* hypotheses to explain the success of scientific theories – we simply note that we have selected certain theories for their success at being empirically adequate and move on.

The final criticism levied against the NMA is a direct attack on the validity of IBE, arguing that any justification of this rule of inference must be circular or question begging. I do not take this to be a significant criticism, for we can only justify most if not all of our accepted methods of inference, such as deductive reasoning, circularly. While justifying IBE using IBE would surely be circular, there is reason to think that such a justification would not be viciously circular, for it is rule circular, not premise circular (see Psillos, 1999, Chapter 4 for further discussion of this issue). From here on, I will leave this issue aside.

§5 – Constructive Empiricism: A Modified Realism

Given van Fraassen's rejection of the realist's proposed explanation of any given scientific theory's predictive success, he could be seen as modifying the realist's T3) to read something like this:

> T3') the *constructive empiricist's* **epistemological** thesis: scientists are warranted in accepting that T2' is true with respect to any genuinely predictively successful scientific theory, i.e. that the theory gives literally true descriptions of the observable but *not* the unobservable, mind-independent world. This commits constructive empiricists to the belief that one is warranted in accepting that predictively successful theories are empirically adequate.

and revising T2 as so:

> T2') the *constructive empiricist's* **ontological** thesis: (some significant portion of) the observable but *not* the unobservable objects, entities, and relations described in a given theory exist objectively or mind-independently as they are described, independently of our description, knowledge, or perception of them. This commits constructive empiricists *only* to the belief that a given theory is true with respect to observables, i.e., that it is empirically adequate.

Van Fraassen denies that we have good reason to believe what our most predictive theories say about the *unobservable* parts of our world[5]. He still thinks that we have good reason to believe what they say about the observable parts of our world; he is not a skeptic about "tables and trees" (van Fraassen, 1980, p.72). But in providing an alternative explanation of any scientific theory's predictive successes, van Fraassen is able to deny T3, which in turn allows him to remain agnostic or skeptical about the *truth or falsity* of T2, and therein about the truth or falsity of all statements regarding

---

[5] For further (though non-exhaustive) location of scientific realism in relation to its anti-realist competitors with respect to these three lines of inquiry, see Chakravartty, 2007, pp.8-17, especially Table 1.1

unobservables. As a result, he only asserts the truth of T2' with respect to predictively successful scientific theories, and the reliability of T3' as a method of inference.

Van Fraassen is simply agnostic about unobservables – he does not take an instrumentalist's stance towards unobservables, thinking of them as simply useful fictions, nor does he think that all statements about them are false. Theoretical hypotheses about unobservables are just as truth-valued as theoretical hypotheses about observables; nevertheless, van Fraassen thinks that we are unable to judge accurately the truth or falsity of theoretical hypotheses regarding unobservables, making it prudent to remain agnostic about all claims regarding unobservables (see Section 1.4 of this essay). Van Fraassen's anti-realist constructive empiricism is often and perhaps best described as a form of *selective skepticism* because he is skeptical about all claims regarding unobservables, but not about claims regarding observables.

§6 – Arguing for Constructive Empiricism: Reasons for Selective Skepticism

It should be noted that van Fraassen's reasons for rejecting the realist's T3 are not typical anti-realist reasons, insofar as the PI's attack on the NMA is considered typical of anti-realism. Resisting pessimistic views of the history of science *are not the constructive empiricist's reasons for selective skepticism* (Churchland, 1986). As we will see in the next section, resisting the pessimistic induction is the *realist's* reason for a different kind of selective scepticism; the realist rejects van Fraassen's reasons for being selectively sceptical outright. This is another reason, in addition to their shared notion of truth, that the debate between constructive empiricists and scientific realists is unique – unlike most anti-realists, van Fraassen is in the same place as the realist with respect to the PI: he needs to avoid it. He does this by noting that, by and large, there is a very non-disjoined accumulation and refinement of observational content throughout scientific revolutions (van Fraassen, 2006), but in general his philosophy is not concerned with navigating the fluctuations of scientific history. Scientific revolutions, it is simply claimed, affect mainly the non-empirical, unobservable levels of our theories.

Van Fraassen's motivations for his constructive empiricism involve, primarily, the underdetermination of theories by evidence. Recall that we can only ever observe certain portions of a theory called its empirical substructure, and that this substructure therefore serves as the empirical evidence in favour of any given theory. Recall also that many theories will share the same empirical substructure. Since (some portion of) the empirical substructure is what we use as evidence to test any given theory, we can see that no set of available evidence, no matter how broad, will ever uniquely determine a single theoretical superstructure with respect to both observables and unobservables[6]. *This* is van Fraassen's reason for selective skepticism. Because of the inevitable underdetermination of theories by evidence, van Fraassen thinks we should remain agnostic about the unobservable portions of theories (such as Newton's "true" or "absolute" motions, Poincaré's curved geometries, molecules, viruses, and capillaries) and be satisfied with saying that a particular theory gets the observable phenomena correct.

After all, this makes a lot of sense. It is the observational content of our theories that really matter to us, for this concerns the objects with which we directly interact in our daily lives. For example, it does not really matter to us what unobservable phenomena are responsible for the solidity and structural integrity of a speeding transit bus – what matters to us humans is that, if hit by such a solid observable, we will be damaged, as a result of its observable solidity and speed. Perhaps van Fraassen is right, and all that scientists need from a good scientific theory is for it to be a good guide to truth about observables. And in the long run, this is probably true – we could hardly want more from an ideal scientific theory than that it be empirically adequate everywhere and for all time. If we can make sense of scientists' acceptance of scientific theories without seeing them as making risky metaphysical commitments to the truth of these theories with respect to unobservables, there is no reason to see them as making such commitments.

---

6 Poincaré's famous discussion of alternative geometries is an illustrative example of how our observations can never uniquely determine a single theory about the unobservable shape of things – for he notes that we cannot decide, on the basis of observable evidence, whether everyday objects obey the laws of a flat Euclidean geometry or a curved hyperbolic geometry together with a certain omnipresent expansion-contraction field.

This is really how van Fraassen defends his constructive empiricism against scientific realism: he argues that realism is unnecessary since we can make sense of scientists' actions without seeing them as aiming for truth about unobservables as well as observables. As Ladyman puts it:

> "[V]an Fraassen argues that if we *need* go no further than belief in the empirical adequacy of theories to account for the nature and practice of science, then, if we do go further, we take an unnecessary epistemic risk for no extra empirical gain ... Van Fraassen rejects realism not because he thinks it irrational but because he rejects the 'inflationary metaphysics' that is an account of laws, causes, kinds and so on, which he thinks must accompany it ... He thinks constructive empiricism offers an alternative view that offers a better account of scientific practice without such extravagance." (Ladyman, 2002, p.224-225, cf. Van Fraassen, 1980, p.73)

Constructive empiricism sticks out its neck in going past the available evidence to infer empirical adequacy, because we can never exhaustively check the state of all observables for all time. Only such an exhaustive check could, barring any mistrust of our sensory-perceptual capacities, make us *certain* that a theory is or is not empirically adequate. Scientific realism sticks out its neck out even further, however, to infer truth with respect to unobservables as well as observables, and it gains absolutely nothing from such additional risk (or so it is claimed).

Thus, the empiricist route is less treacherous, with all the benefits of the more treacherous metaphysical route. Realists take more risk in licensing T3 than constructive empiricists do in licensing T3', with no apparent gain. For the assertion that a theory is true with respect to observables (i.e. the assertion of empirical adequacy) is a great deal weaker than the assertion of truth with respect to observables and unobservables. The constructive empiricist's account of theory acceptance is able to "deliver us from metaphysics" (van Fraassen, 1980, p.69), i.e. from the supposition that we need to (or even should attempt to) inquire into the truth about unobservables. This account of theory acceptance is therefore valuable because, according to the empiricist, metaphysics is risky, dangerous, and essentially fruitless.

But are there any situations in which the constructive empiricist's account of theory acceptance is insufficient? Are there any situations in which we must try to judge theories for their truth about unobservables? Is metaphysical inquiry really that fruitless? In the next and final chapter of this essay, I argue that there are situations in which we have reasons to favour realist, metaphysical inquiry into the truth about unobservables. But for now, allow me to show how realists usually deal with the two main objections to their view outlined in Section 2.4 of this chapter.

§7 – Defending Scientific Realism: Paying Attention to Detection

Van Fraassen's formulation of constructive empiricism and Laudan's PI helped realists question the idea that, as realists, they needed to hold successful scientific theories to be true full stop. Nowadays most realists will uphold their own form of selective skepticism, for given the underdetermination of theories by evidence, the turbulent history of science, and the perpetually idealized nature of scientific theories, it would surely be absurd to believe in the full truth of any theory, no matter how successful it is. Once we can cut up theories into their parts through an extrinsic, semantic view of theories, there are all sorts of epistemic lines we can draw for ourselves, not only between observables and unobservables. And while van Fraassen is on the right track in being selectively skeptical, he may nonetheless have drawn the line for belief-worthiness in the wrong place.

Just as van Fraassen displays an epistemic asymmetry with respect to observables and unobservables, Chakravartty (1999, 2001, and 2007) stresses another epistemic asymmetry, one that matters only to the realist, for it rests on a distinction between two kinds of unobservables. On the one hand, we have those unobservables which a theory tells us can be detected through the use of scientific instruments. These "detectables" include such putative entities as electrons, positrons, caloric, the luminiferous ether, mitochondria, cell walls, the Higgs boson, and proteins, as well as all of their characteristic properties, relations, and potential causal processes involving these entities. Some of these detectables have been detected (electrons, positrons, mitochondria, cell walls, and proteins), others have failed to be detected (caloric and the luminiferous ether), and still others are awaiting detection (the Higgs boson).

What detectables as a group share is detectability *in principle*. We may not be able to detect them at present, or we may fail to detect them because they do not exist, but in principle the putative objects in this class of unobservables are all detectable through their various causal effects on observables. On the other hand, we have those unobservables which a theory tells us cannot be detected through the use of scientific instruments. These "undetectables" include space-time points, numbers, universals, and other universes. All of these posited undetectable unobservables are things which we have *no* means of detecting *in principle*. As a result, these putative entities can, in no way, be the object of empirical investigation (Chakravartty, 2007, p.15).

Statements about undetectables, surely, can never be belief-worthy for the hard-lined empiricist – without the ability to observe or detect them, we have no way of checking to see whether or not these objects are really there, never mind if what we say about them is true or not. Thus, it would probably be prudent to always remain skeptical about statements regarding undetectables. But surely there is an epistemic asymmetry between something like an electron and Newton's "true motions". Detectables are epistemically different from undetectables, at least for the realist, because we can forge causal links with them, unlike undetectables.

Having knowledge of the existence (or non-existence) of certain detectable properties matters to the realist. If we know what unobservable detectable properties make up the unobservable, mind-independent world, we can use this knowledge to make novel predictions about the future interaction of these unobservable properties in unprecedented ways. More importantly, perhaps, one could use such knowledge of the unobservable to predict the *observable* consequences of such unprecedented interactions between unobservables.

To this end, we may again distinguish two sorts of detectables, one thought to be belief-worthy, the other not: these are the *detected* and the as-of-yet *undetected*, respectively. Chakravartty calls the latter class of detectables *auxiliary*, because despite our lack of causal interaction with them they often have great heuristic value for us in understanding a given system. An auxiliary entity, property, process or relation is one that has as of yet failed to be detected by the kinds of instruments that a theory predicts would be able to detect it, either because all attempts to detect it have failed or because no such attempt has ever been made. All auxiliary detectables have one of two fates: they either come to be detected, and their existence is thereby accepted by realists, or they regularly fail attempts at detection, are discarded in the course of a scientific revolution, and belief in their mind-independent existence having never becomes warranted for the realist.

Auxiliary detectables often play an important heuristic role in science. The early hypotheses regarding the luminiferous ether, for example, helped us to build models and understand the wave-like properties of light in terms of the more familiar wave properties of material media like water, strings, and elastics. But since no one ever managed to detect the properties of the ether, this entity remained an auxiliary posit, and eventually was discarded from future theories of light all together. Scientists had detected and described certain properties of light, such as wavelength and amplitude, and had described the laws governing these properties in abstract mathematical equations. But these laws did not involve the properties of the ether in any way, only the well-detected properties of light-waves.

These well-detected properties became belief-worthy (according to the realist of Chakravartty's sort) as soon as people started observing the novel consequences of wave theories of optics, such as the bright spot that appears in the shadow of a circular object (see n.4, this chapter). But since the properties of the ether played no role in the equations describing the other, detected properties of light such as wavelength and amplitude, there is no difficulty in divorcing the description of light's wavelength and amplitude from the description of the ether's putative properties. And since the equations describing these detected properties of light were retained *in full* in the next (ether-free) theory of light, the realist can, without risk of falling prey to PI-type arguments, claim that one is warranted in accepting the real, mind-independent existence of these detected properties of light while remaining agnostic about the existence of auxiliary detectables such as the ether. Generalizing this, the realist can claim that one has good reason to believe in the mind-independent existence of any detected detectables as they are described by a predictively successful scientific theory.

If, for example, the Michelson-Morley experiment and others similar to it had managed to detect an ether, to forge some causal connection with its properties as they were described by ether theories, scientist would have (according to the realist) had good reason to believe in the mind-independent existence of the ether. But since scientists continually failed to detect the properties of the ether, this auxiliary detectable was rightly reconciled to the flames in the scientific revolution that was brought on by Maxwell's theory of electromagnetism. Nevertheless, many of the equations describing the properties of light posited in Fresnel's theory were retained in full in Maxwell's theory, specifically those responsible for the observed, novel phenomena predicted by the theory – the descriptions of detected properties (see Psillos, 1999 and Chakravartty, 2007). Thus, the realist's prior commitment to the real existence of these properties would not have turned out to be misguided, and her agnosticism regarding the existence of the ether would have been vindicated. In this way, the realist is able to restore theoretical continuity in the history of science at the level of belief-worthy facts regarding unobservables.

This epistemological distinction between detected and auxiliary properties and entities is a distinction unavailable to the constructive empiricist, for she treats all claims about unobservables agnostically regardless of their having been detected or not. To make one's epistemic line in the sand at observables, one might argue, lops off too much of science as non-belief-worthy (see Chakravartty, 2007, Chapter 1). For there seems to be a steady and reasonable historical continuity not only among the observable content of theories, but also among detected detectables – scientists have believed in electrons and many of their most basic properties since they were first hypothesized and detected, and still do to this day (though they may believe some new things about them such, e.g. that they have "spin"). And we still believe in the massive properties of matter (such as its essential gravitational nature), despite the fact that we may now conceive of mass relativistically instead of absolutely. The luminiferous ether and caloric, however, were never detected, and accordingly would never have attained belief-worthy status for the realist. The historical revolutions of science left most unobservable but detected properties intact in all future theories, while removing some undetected or auxiliary properties and entities like caloric, phlogiston, and the luminiferous ether from future theories all together. With such qualifications for belief in hand, the realist can show that despite the historical record, forming beliefs about unobservables based on successful science is not nearly as risky as the anti-realist would have us believe.

The central idea in making such a distinction between detectables and auxiliaries is this: once we have detected a certain kind of unobservable, learned to manipulate it, and come into regular causal contact with it, we have good reason to believe in the existence of that unobservable as it is described by our theory[7]. If some properties are described by a theory, but are as yet undetected (i.e. are currently auxiliary), realists should remain as agnostic about the truth or falsity of statements regarding them as constructive empiricists are about all statements regarding unobservables. The history of science seems to support the idea that, once detected, certain objects and properties tend to be retained in future scientific theories, while some putative auxiliary objects and properties that continually fail to be detected come to be discarded in future scientific revolutions[8]. Realists, therefore, can believe that certain super-empirical or unobservable portions of our theories are true – those things that we have detected – while remaining consistent with the turbulent history of science.

---

[7] This is a lesson gleaned from the entity realism of Ian Hacking (1983) and Ronald Giere (1988). See Chakravartty, 2007, p.30-32.

[8] There is a very strong similarity between this account of scientific advancement and Hesse's (1963) famous view of scientific modelling and advancement in terms of neutral, positive, and negative analogies.

So, insofar as realists are mainly concerned with rebutting pessimistic views on the history of science while maintaining the maximal amount of commitment to the claims of successful scientific theories, they can do so by identifying a steady accumulation of, and commitment to, scientific truth that goes beyond the level of observables to the level of detected detectables. There is much unobservable continuity to be found so long as we are conscious of the difference between unobservables that we have detected and those that we have not (and those that we cannot). With such continuity found, and the ability to rule out belief in such things as caloric and the luminiferous ether *before* scientific shifts move us away from theories positing such entities, scientific realism can easily resist the PI.

Van Fraassen, of course, will not be converted to realism just from seeing that realism can get more continuity and accumulation in rebutting the pessimistic induction than constructive empiricism can. For as was noted, pessimistic views on the history of science are not van Fraassen's reasons for being selectively skeptical – he may be (potentially) vulnerable to the PI, but he is more concerned with the underdetermination of theories by their empirical evidence.

Nor would van Fraassen be converted to realism if a realist pointed out that the evolutionary explanation of scientific success he gives is not an adequate replacement for the realist's explanation. As Psillos puts it:

> "If we unpack van Fraassen's story, we find that it is *phenotypical*: it provide an implicit selection mechanism according to which entities with the same phenotype, i.e. empirical success, have been selected. But a phenotypic explanation does not exclude a *genotypic* account: an explanation in terms of some underlying feature which all successful theories share in common: a feature which has made them successful in the first place. The realist explanation in terms of truth provides this sort of genotypic account: every theory which. possesses a specific genotype, i.e. approximate truth, which accounts for this phenotype … Notice here that the realist explanation is *compatible* with van Fraassen's Darwinian account. Yet, the realist's is arguably preferable … [for] … the genotypic explanation has this warrant up its sleeve: if a theory is empirically

> successful because it is true, then it will keep on being empirically successful." (Psillos, 1999, pp.96-97)

All of this is unpersuasive for the constructive empiricist because, qua anti-realist empiricist, she is in the business of denying the need for such genetic, metaphysical explanations in general. Nevertheless, it shows that the alternative explanation of scientific success given by van Fraassen is not necessarily a sufficient replacement for the realist's explanation – it may explain why the only science we accept is successful science, but it does not explain what makes any *particular* theory successful.

Thus, it would seem that the defenders of constructive empiricism and scientific realism are just coming from different places, philosophically. Van Fraassen wishes to uphold the banishment of metaphysical speculation from scientific inquiry, given that no set of empirical observations can ever uniquely imply a particular unobservable ontology. The realist, on the other hand, thinks that metaphysical inquiry can often be useful, warranted, and effective – that it is, worth the risk, despite empirical underdetermination.

So despite the many arguments put forward in favour of one position over the other, it would seem that scientific realists and constructive empiricists simply have different views of what we are warranted in accepting about successful scientific theories. This is because, I argue, the empiricist and the realist value science for different things. These differing valuations of science are really what set the realist and the empiricist apart, and it is to this view of the conflict between realists and empiricists that I turn in the final sections of this chapter.

§8 – On Evaluating Scientific Theories: Pragmatic and Epistemic Virtues

When theories are evaluated for their relative strength, it is clear that scientists will often appeal to more than just a theory's empirical adequacy in explaining why they accept that theory. A scientist may appeal to a theory's internal coherence, its elegance and beauty, its breadth, depth, and many other so-called "super-empirical" virtues in deciding which of two empirically equivalent theories are more worthy of acceptance. As van Fraassen sees it, achieving empirically adequate theories is the primary goal of science – indeed, for the constructive empiricist, it is the sole truth-directed goal of scientific inquiry, for science aims at getting truth about observables, and cares not for truth about unobservables.

But the constructive empiricist can still make sense of scientists' appeals to a theory's super-empirical virtues when justifying their acceptance of a theory. There is more to acceptance for the constructive empiricist than belief that a theory is empirically adequate. Empirical adequacy is simply the sole *epistemic* or *truth-directed* virtue of scientific theories – the super-empirical virtues are, according to van Fraassen, *pragmatic* virtues, having to do not with truth, but with specifically human concerns. As he puts it:

> "When a theory is advocated, it is praised for many features other than empirical adequacy and strength: it is said to be mathematically elegant, simple, of great scope, complete in certain respects: *also* of wonderful use in unifying our account of hitherto disparate phenomena, and most of all, explanatory. … What can an empiricist make of these other virtues which go so clearly beyond the ones he considers pre-eminent?
>
> There are specifically human concerns, a function of our interests and pleasures, which make some theories more valuable or appealing to us than others. Values of this sort, however, provide reasons for using a theory, or contemplating it, whether or not we think it true, and cannot rationally guide our epistemic attitudes and decisions" (Van Fraassen, 1980, p.87).

Explanatory value, heuristic value, breadth, depth, simplicity, fruitfulness, an ability to explain its theoretical predecessor's empirical successes, its lack of ad hoc hypotheses, and pretty much any thing else we might value in a scientific theory beyond its empirical adequacy, are super-empirical virtues that contribute to our selection from among the many empirically equivalent theories available to us. However, these virtues are *not*, in van Fraassen's opinion, virtuous in that they make a theory possessing them more likely to be true with respect to either observables or unobservables. As he puts it,

> "In so far as they go beyond consistency, empirical adequacy, and empirical strength, [super-empirical virtues] do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory *independently of questions of truth*" (van Fraassen, 1980, p.88, emphasis added).

So, that a theory possesses some super-empirical virtues does not give one reason to believe this theory to be "more true," "more likely to produce accurate predictions," nor "more empirically adequate" than any other apparently empirically adequate theory that does not possess them. They are irrelevant to truth; and if super-empirical virtues are irrelevant to truth, the constructive empiricist will have no recourse to those virtues when trying to decide the case between the predictions of two apparently empirically adequate theories.

According to the constructive empiricist, accepting theories with super-empirical virtues can still be useful to scientists in their efforts to make scientific advances – to find ever more empirically adequate theories. At best, however, this gives one good reason to believe that working within a research program based on a super-empirically virtuous theory will be more fruitful than working within a research program based on a less super-empirically virtuous theory. This benefit of super-empirical virtues, however, is insufficient for deciding such epistemic issues as which theory's predictions are more likely to come true.

It is clear that once we take empirical adequacy to be the sole truth-directed goal of science, apparent empirical adequacy will be the sole epistemic virtue of scientific theories. Super-empirical virtues *must* be considered only pragmatic by the constructive empiricist, for if a theoretical virtue is not the sole epistemic virtue of empirical adequacy, it can only be valuable in being instrumental in achieving the sole epistemic goal of science. In this way, van Fraassen's valuation of scientific theories for their empirical adequacy affects his assessment of theoretical virtues as non-truth-directed.

The realist, by contrast, believes that super-empirical virtues, at least in part, are truth-directed virtues – that is, their possession by a theory, over and above its apparent empirical adequacy, gives one good reason to believe that this theory is truer with respect to observables and unobservables than a theory that does not possess them (cf. Chakravartty, 2007, pp.24-25). Scientists, according to the realist, choose theories with more super-empirical virtues because such theories are more likely to capture the truth with respect to observables and unobservables. The constructive empiricist would likely argue, however, that we do not need to see super-empirical virtues in this way, and may instead adopt an empiricist attitude towards science without losing the ability to make sense of the value scientists place on super-empirical virtues.

But, as I argue in the next chapter, the accounts of theory evaluation given by the scientific realist and the constructive empiricist are not equivalent in their prescriptions for belief with respect to *observables*: the kinds of inferences we can draw regarding the observable state of things diverge depending on whether or not we take the super-empirical virtues to be truth directed. Thus, whether or not super-empirical virtues are epistemic or truth-directed virtues is very important when we are planning our actions based on accepted scientific theories. In the next section of this chapter, I sketch a recent recasting of the debate between these two philosophies, one which sees them as opposed epistemic stances that are accepted or rejected on the basis of one's accepted set of values. This allows me to argue in the final chapter of this essay that scientific realism better serves many of our collective needs, especially our need for science to inform our long-term planning.

§9 – On Valuing Science and Epistemic Stances

So how is someone sitting on the fence between constructive empiricism and scientific realism to decide between these two philosophies of science? I assume that both are internally coherent, adequate to the phenomenon of science, accepting of our intuitive notion of truth, and quite parsimonious. No empirical evidence could come to favour one philosophy over the other, so how could someone come to favour one over the other?

Crucially, our choice between constructive empiricism and scientific realism is a function of our values: specifically what we value in scientific inquiry.

Whereas van Fraassen values science for its ability to produce empirically adequate theories, many realists value science for its ability to provide us with explanations by informing us about the nature of the unobservable world. Popper, for instance, saw the search for explanations of observed regularities in terms of their unobservable causes as the *sole* aim of science. For him, the search for empirically adequate theories turns out to be pragmatic, contributing instrumentally to our search for explanations by allowing us to weed out the manifestly false theories. In a way, all epistemic virtues would turn out to be instrumental on such a view: the only reason that we search for theories that are true with respect to both observables and unobservables is so that we can use these theories to explain things (truth being a necessary requirement of any adequate explanation). What we value in scientific inquiry, and what we thereby see as the aim of science, turns out to affect what we think makes a good scientific theory, what theoretical virtues we take to be instrumental or pragmatic, and what we should believe about predictively successful scientific theories. This point will be elaborated upon in the next chapter.

Because our acceptance of constructive empiricism or scientific realism is affected by our prior valuations of science in this way, Chakravartty and van Fraassen describe the conflict between these two philosophies as a part of a larger conflict between two opposed "epistemic stances". To understand what an epistemic stance is, we might think of epistemological analysis as divided into three separate levels. At the bottom level of analysis we have the level of putative facts, which are truth-valued descriptions of the mind-independent world. In the middle we have our stances or collections of epistemic policies, which guide us in our assessment of candidate facts. At the top level we have what Chakravartty calls the "meta-stance" level, which is the level at which we choose one stance over another.

A stance is "a cluster of commitments and strategies for generating factual beliefs ... One might think of them partially, after Paul Teller (2005), as combinations of epistemic 'policies' with respect to the methodologies one adopts in order to generate factual beliefs" (Chakravartty, 2007, p.17). Chakravartty summarizes the metaphysical and empirical stances in terms of the following epistemic policies:

The Metaphysical Stance

M1: Accept demands for explanation in terms of things underlying the observable.

M2: Attempt to answer such demands by speculating about the unobservable.

The Empirical Stance

E1: Reject demands for explanation in terms of things underlying the observable.

E2: *A fortiori*, reject attempts to answer such demands by speculating about the unobservable.

E3: Follow, as a model of inquiry, the methods of the sciences.

Accepting one of these stances does not necessarily commit one to scientific realism or constructive empiricism. Scientific realism and constructive empiricism are more like subspecies of the metaphysical and empirical stances: they are attempts to provide answers to the inquiries suggested by their respective stances. Realism, for instance, answers demands for explanation in terms of things underlying the observable by claiming that our most successful scientific theories are true; that is, it explains our observations by claiming that the unobservable world is as our best scientific theories describe it – it is made up of electrons, protons, electrical charges, atoms, and many other unobservable particles, magnitudes, and various causal properties. Constructive empiricism rejects the realist's answers to such demands, and instead claims that successful scientific theories are successful because they are empirically adequate; in short, that our most successful scientific theories are true with respect to observables.

We often want a collection of facts that can be used to inform our actions; but in order to generate such a collection of putative facts we need to accept a certain stance or set of epistemic policies. The empirical stance, from which the constructive empiricist begins, leads one to abhor metaphysical speculation about the unobservable, and the constructive empiricist thereby comes to produce factual beliefs about observables only. The metaphysical stance, on the other hand, from which the scientific realist begins, allows one to ask questions about unobservable things like causes and natures, and to search for explanations in terms of true statements regarding unobservables. Because they come from an opposed stance, constructive empiricists not only disagree with the epistemic security of the supposed "facts" regarding unobservables that realists believe, they disagree with realists' entire method of generating factual beliefs.

The epistemological licences of scientific realism and constructive empiricism take the many products of scientific inquiry, primarily theories or hypotheses, and use them to generate statements to be considered true. Scientific realists and constructive empiricists will each also provide tools for discerning which scientific theories are to be considered best and which parts of those theories are belief-worthy. The realist's epistemological tools of relative theory evaluation include the notion of theoretical maturity, the notion of a non-ad hoc theory, an account of theoretical virtues, an account of the nature of explanation and relative explanatory power, an account of relative evidential support, and a distinction between detected detectables and auxiliary detectables. All of these tools are fashioned in the hopes of discerning the truth about both observables and unobservables.

The constructive empiricist's tools will include many similar tools: an alternative account of theoretical virtues, an alternative account of the nature of explanation and relative evidential support, and of course a distinction between observables and unobservables. All of these tools are fashioned in the hopes of discerning the truth about observables. Their respective toolkits allow scientific realists and constructive empiricists to deem certain factual statements belief-worthy, i.e. to determine what statements should be considered true and/or which statements should be considered false. Without something like one of these positions, our actions are somewhat paralyzed, for we need a set of facts on which to base most if not all of our decisions.

It should be noted that "[epistemic] policies are not themselves true or false" (Chakravartty, 2007, p.17). We do not hold or accept a set of epistemic policies in the same way that we would hold or accept a set of facts – we *commit* to polices, we do not *believe* them. When we believe a particular fact or statement like "the cat is on the mat" we think that this statement is *true.* This is not the case with stances and epistemic policies. We may believe that a particular stance or set of epistemic policies will best serve some set of goals and values, but we certainly do not believe that a stance is true or false. In the final section of this chapter, I turn to the issue of how exactly one comes to accept or reject a particular epistemic stance.

§10 – On Choosing a Stance

From the meta-stance level we can ask the question: which stance should we adopt? Here we will find that our choice of stance is informed by two things – our values and a minimal sense of rationality. Beyond some minimal rationality requirements (basically logical consistency and probabilistic coherence), the debate at the meta-stance level comes down to a matter of what values the potential stance holder already holds[9]. On this view, "different and mutually incompatible stances may be rational – no one stance and resultant set of beliefs are compelled [by rationality alone]" (Chakravartty, 2007, p.21). This leads to a kind of *voluntarism* at the meta-stance level, for values are not "rationally compelled" either. So, rationality alone cannot determine our choice of stance: it is our choice of values that ultimately determines which stance we adopt. But what determines our choice of values?

---

9 The rationality requirement could in fact be interpreted as a kind of universal value, for given that the hallmark of irrationality on this view is "self-sabotage by one's own lights" (Van Fraassen, 2005, p.184, cited in Chakravartty, 2007, p.21), we could simply see the self-sabotage as something that everyone would value avoiding.

Our values are often (though not always) determined by the roles and contexts that we find ourselves in. As individuals that are at different times philosophers, scientists, parents, instructors, and/or citizens, we find ourselves wearing many different hats throughout our lives, and find ourselves holding to different values associated with each of these hats whenever we wear them. On this view, it is entirely possible that one might hold to a particular set of values at one time while holding to another at a different time. When we take on certain roles, we generally evidence ourselves as holding to certain goals or values: when I clean house, I evidence my value of cleanliness; when I give to charity, I evidence my value of low levels of human suffering; and when I attempt to base my long-term plans on currently accepted science, I evidence my value of science for its ability to inform long-term planning. In this way, certain values can be connected to certain roles, meaning that we can investigate what values and goals are held by "cleaners" or "planners" in general without actually looking at the values each particular individual cleaner or planner has themselves, aside from their role as cleaner or planner. We can also inquire into how the values of these roles might best be served in general, again without giving any attention to the peculiar values that each particular individual holds.

When one participates in political life – at once living under as well as influencing the laws of the governing land or nation – one evidences many things. In a liberal democracy, one of the primary values a participating citizen holds is a tolerance of other peoples' ways of life. Thus, in a public sphere inhabited by people of varying metaphysical dispositions, it might be appropriate to make do with the absolutely minimal amount of metaphysical speculation, so as not to a) infect our public deliberations with a commitment to undue metaphysical biases nor b) sidetrack more important debates with metaphysical squabbling. When one is in the privacy of their personal philosophical cloisters, the natural curiosity of the human mind may lead one to adopt a less anti-metaphysical stance. In such privacy, we may ask realist questions about successful scientific theories all we like in order to form beliefs about unobservables, or to seek out those explanations that some realists so value the scientific endeavour for helping to provide. But in the public sphere, when we all have to make decisions together, surely we should start from the most metaphysically neutral ground possible.

When making decisions as a collective body of political and epistemic agents, however, we unfortunately cannot do without at least a little metaphysics. In any situation where we employ our scientific knowledge to inform our actions, we will need some sort of epistemic stance to interpret science and generate a set of acceptable facts to base our actions on. Even if we accept an anti-metaphysical stance like constructive empiricism we will still be making certain metaphysical or ontological commitments about observables. Thus, since we cannot make decisions without committing to some set of facts (i.e. some assumptions about the way the world is), metaphysical minimalism should be the goal.

At first glance, constructive empiricism seems like a great candidate for such a metaphysically minimalist epistemic stance, at once adequate for our needs in the public sphere and suitably metaphysically minimal. I argue, however, that constructive empiricism cannot meet all of our needs when we attempt to make long-term plans; that is, metaphysical inquiry of the realist variety is required of us in some of our decision making. In the next and final chapter of this essay, I argue that constructive empiricism cannot serve many of the needs we have when making large-scale national or global decisions as a political collective.

Chapter 3 – A New Argument for Scientific Realism

§1 – Recasting the Debate between Scientific Realists and Constructive Empiricists

In this chapter I aim to produce a new argument for scientific realism. This is based on the view that most previous arguments for or against scientific realism cast in terms of what it is "rational" to believe about scientific theories have been doomed to failure. Once we accept that the conflict between scientific realism and constructive empiricism occurs within the larger context of a conflict between opposed epistemic stances, we can see why this is so: rationality underdetermines our choice of stance. The determining factor in our choice of stance is our set of values.

After discussing how we choose between competing stances, I go on to discuss some situations and roles we often find ourselves in, and the sorts of needs we have in these situations and roles. I then argue that realism best serves these needs, and hence the values that are evidenced in entering such situations or taking on such roles. In particular, I argue that the epistemic policies of scientific realism best serve our needs in making large scale, long-term plans, and in formulating public policy based on these plans. I make this point primarily by showing that the epistemic policies of one of realism's main rivals, constructive empiricism, cannot serve our needs in these situations. Thus, the project of developing an epistemology aimed at discovering the truth about unobservables – i.e. the adoption of the metaphysical stance, and therein the realist's philosophical project – is vindicated, for it serves many of our needs and therein our values better than alternative projects.

Many policy decisions require us to wager the truth and predictions of scientific theories against their potential for change, which is exactly what realists are doing in responding to the Pessimistic Induction (PI). Given that constructive empiricists justify their particular kind of selective scepticism without the need to rebut pessimistic views of the history of science, it is somewhat unsurprising that constructive empiricism is unable to serve our needs in other situations that force us to face up to PI-type considerations.

§2 – Criteria of Stance Evaluation: Rationality and Values

The debate between constructive empiricists and scientific realists can be seen as a smaller part of a much larger and much older debate – the debate between speculative metaphysicians and anti-metaphysical empiricists. This includes the debate within the Aristotelean tradition between the nominalists and the realists, between the early modern empiricists and the Scholastics, between the logical empiricists and the systematic philosophers of the previous two centuries, and between constructive empiricists and scientific realists. In addressing this long-standing dispute, Chakravartty (2007) follows van Fraassen in describing metaphysics and anti-metaphysical empiricism as opposed epistemic stances, as was outlined in the previous chapter. My argument begins by taking this view of the dispute between scientific realists and constructive empiricists seriously.

Our choice of an epistemic stance is, undeniably, a *choice*, at least in as much as my choice over a particular political platform or brand of cigarettes is a choice. I may have become comfortable with a particular brand, or a particular epistemic stance, or a particular political commitment. I may have been indoctrinated into it, and often may be unable to escape its influence on my thoughts. I may even be physically addicted to the product in question, or forced by a dictator to pay it continual lip service. But nonetheless, when I see that there are other options available to me, I may weigh and assess these options according to whatever it is that I hold most dear – the satisfaction of my nicotine cravings against the stress of quitting smoking and damage to my health, for instance, or the freedom of my people against my fear of the ruling tyrant's bludgeon. In the same way it is at least possible for one to weigh and compare stances against one another, even if one can rarely or only with great difficulty actually make the transition from one stance to another.

As was noted at the end of the last chapter, when we attempt to remove ourselves from the particular stance in which we are currently enmeshed, and attempt an analysis at the meta-stance level to help us decide which stance is best for us, we will see that our choice of stance is a function of two things: our values, and the constraints of a fairly thin notion of rationality (Chakravartty, 2007, p.21). Given that different individuals have different values, yet all individuals are supposedly constrained by "rationality," the former is an agent-relative constraint on stance choice, while the latter is a uniform constraint on any individual's choice of stance.

The thin notion of rationality operating at the meta-stance level is as a kind of minimal internal consistency, where a stance can be deemed irrational if it can be shown to be self-defeating "by its own lights." So long as a stance does not lead one inevitably to a contradictory position, it can be deemed rational. For example, empiricists abhor metaphysical speculation, and recommend that we do not engage in metaphysics. If it could be shown that metaphysical speculation inevitably resulted from an adoption of the empirical stance, this would amount to a proof that the empirical stance is self-defeating by its own lights, and therein irrational and untenable. Values, by contrast, are things that individuals hold for a variety of reasons, none of which are compelled by "rationality." Clearly, rationality does not impose much of a constraint on our choice of stance, leading to what is best called a *voluntarist* view of stance acceptance at the meta-stance level.

Given this view of stance acceptance, which denies that any single stance is rationally compelled, there are two types of arguments that one can make for or against a particular stance, and hence two ways in which one might try to convince someone thinking at the meta-stance level that they either should accept or reject a particular stance. The first argument-type utilizes the rationality constraint on stance acceptance, the second utilizes the function of one's values. I will call these two argument-types "arguments based on rationality" and "arguments based on values." An argument based on rationality would argue against the internal consistency of one stance or another, showing that even on the minimal sense of rationality operating at the meta-stance level, a particular stance is self-defeating and therefore untenable. Alternatively, an argument based on values would argue that given an agreed upon or shared set of values or goals, one stance is preferable to another for its ability to serve these values[1].

---

1 It may be that this characterization of the dispute between constructive empiricism and scientific

Many debates over scientific realism operate at the meta-stance level, and for the most part they employ arguments based on rationality. Such arguments go both ways: most attacks on the observable/unobservable distinction and constructive empiricism in general can be seen as attacks on the internal consistency or rationality of constructive empiricism (see, for example, Foss 1988 and 1992, Giere 1985, or Gutting 1985). The PI can be seen as an attack on scientific realism's supposed internal consistency, charging the realist with self-defeat by their own lights and the production of contradictory statements. The many defences of realism against the PI can be seen as attempts to show that realism is not self-defeating, and produces no such contradictions.

Despite the fact that arguments based on rationality make up the majority of the literature surrounding scientific realism, such arguments are likely doomed to failure because *no one stance is rationally compelled*, and neither the empirical stance nor the metaphysical stance seem definitively irrational. The kind of rationality that informs our decisions at the meta-stance level is so thin that most philosophies of science can unsurprisingly meet its demands. Given that our choice of stance is radically underdetermined by the constraints of rationality and the available evidence, the determining factor in our decision is likely to be our values. Thus, in order to make a convincing argument in favour of scientific realism or constructive empiricism, or the metaphysical and empirical stances from which they result, I think it would be prudent to make arguments based on values rather than arguments based on rationality.

In order to make an argument based on values we need to first identify certain values and then go on to show that such values are better served by one stance as opposed to another. Anyone holding these values could thereby be compelled to accept the stance that best serves these values. Because the influence of values is agent relative, as opposed to the

---

realism could be generalized to a great many philosophical disputes. If we see philosophical disputes between certain philosophical positions as we see this particular dispute over epistemic stances, it may be possible to similarly characterize these disputes as one of two kinds: debates about internal coherence that appeal to a thin notion of rationality, and debates over the fruitfulness of a position that appeal to assumed shared values. I do not argue that this is the case; I only note that this view of debates between stances may be a particular case of a more general philosophical phenomenon.

universality (or at least near uniformity) of the rationality requirement, any argument based on values in favour of scientific realism or constructive empiricism can take one of three forms.

The first form attempts to show that the metaphysical stance or the empirical stance better serves the shared values of the realist and constructive empiricist. This argument form identifies certain values that the constructive empiricist and the scientific realist both share, perhaps implicit values manifest in their philosophies, and then argues that these values are best served by one stance rather than the other. There are dangers in such an approach, however, for the warring parties may deny that they hold such values: or worse, it may become evident that they simply do not share enough values to favour one stance over the other, or even that they do not share any values at all. In such a situation, we would come to see this dispute as entirely irresolvable. I see this as the likely outcome of an argument based on values of this form, especially since opposing metaphysics seems to be a value of the constructive empiricist *qua* anti-realist empiricist.

The second form attempts to convince a particular individual that a particular stance is best for them. This argument form identifies the values held by a particular individual and argues that based on those values one stance is preferable to another. Such an argument is made in order to convince an individual to accept a particular stance by showing them how their own values are best served by one stance rather than the other. Surely, however, this can hardly be of general interest – what stance best serves Bob or Terry's values does not matter much to me, especially if Bob and Terry value far different things than I do. To account for the various reasons that a variety of individuals might prefer one stance to another is a large project, and while it is surely worthwhile, it has little relevance here. Arguments of this form are perhaps best left to personal discussions between individuals.

The third option, which I see as by far the most promising and interesting option, attempts to show that the values and needs we have in particular situations, roles, or contexts are best served by one stance rather than another. This argument form identifies some group of individuals, classified according to their common needs, that are a) prone to hold certain values and b) in need of a scientifically informed set of putative facts on which to base their decisions. Such an argument is made in order to convince members of this group that their needs, qua member of that group, are best served by one stance rather than the other. Arguments of this form are like arguments of the second form, but they apply to groups, rather than individuals. If the net is cast wide enough, such arguments may even be of general interest.

If it can be shown that one stance better serves the needs and values of a wide enough group, this would serve as a vindication of the epistemological project typified by that particular stance. For example, if it could be shown that scientific realism best served the needs of a group to which we all belong, this would vindicate the development of a realist epistemology. This is precisely the kind of argument I make, arguing that the political climate in which we exist requires us to ask realist questions about our scientific theories.

At the turn of the third millennium, we are more than ever reliant on science in all areas of our public sphere: conceptually, practically, and epistemically. There are many situations, contexts, roles or groups in our society that need to be informed by science in some way, from our individual decisions to our collective decisions. Thus, there are surely many groups of individuals that are in need of a scientifically informed set of putative facts on which to base their decisions, and therefore many groups that we might target with an argument of the third form. As a target for such an argument, allow me to offer a heterogeneous group that I will call "public policy makers." This group includes citizens, voters, CEOs, consumers, and parents, among others. What unites all of these individuals is that, in a democracy such as Canada at least, they are all ultimately responsible for making long-term plans for the society as a whole in the form of laws and regulations.

Public policy makers often need science to inform their long-term planning. This includes planning so long term that is can no longer be thought of as planning for one's own generation, but only as planning for future generations. Some cases are so global in nature that they can really only be seen as planning for our species as a whole, for now and into the future. Most environmental issues, I take it, at their limit, are cases such as these. By appealing to our needs in such situations, we can argue that one or another epistemic stance better serves such needs. If, in a democracy such as our own, the category of public policy makers is broad enough to include all capable citizens (in some sense), it could be argued that our nation, or even humanity as a whole, is better served by one or another stance. If showing that a particular school of thought or philosophical project best serves *humanity* does not count as a vindication of that school or project, I do not know what would.

To show that developing a realist epistemology aimed at discovering the truth about unobservables is of general interest only two things need to be shown: first, that nearly everyone belongs to one of the groups that make up the category of "public policy makers," and second, that members of these groups have an interest in discovering whether or not certain scientific theories are true with respect to unobservables. I will take the first of these points as given, for there are surely few people that do not fall into one of the groups under the umbrella term "public policy makers." To make the second point, it suffices to show that there is at least one scientific theory whose truth with respect to unobservables matters to public policy makers. The case I use to make this point is global warming theory, the theory that anthropogenic carbon dioxide emissions will cause catastrophic, world-wide temperature increases.

Before getting into the details of what public policy makers need from a set of epistemic policies, two themes from the last chapter should be elaborated upon: first, the values that drive most realists and empiricists to adhere to their respective philosophies, and second, the scientific realist and constructive empiricist's respective accounts of theory acceptance.

§3 – Realism, Empiricism, and the Aim of Science

What do realists, *qua* realists, value? Given the view of stance acceptance outlined in this essay (see Sections 2.9, 2.10, and 3.2), there must be a common set of values leading this crew to band together under a particular philosophical banner. This, I argue, is what they value in scientific inquiry. Rather than just express what they value science for explicitly, scientific realists will often couch their valuations of science in terms of the "true" aim of science. Interestingly enough, advocates of scientific realism will then often appeal to the aims of science in arguing for acceptance of their philosophical position; van Fraassen's own brief articulation of scientific realism characterizes it as a position regarding the aim of science.

"*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true*" (1980, p.8).

Constructive empiricism is then presented as an alternative account of the aim of science:

"*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate*" (1980, p.12).

It is clear that the second half of these characterizations flow from the first part: we accept that science aims at giving us true/empirically adequate theories, and this leads us to view acceptance as belief in truth/empirical adequacy. Thus, what one thinks the "true" aims of science are determines much of the rest of one's philosophy of science, just as ones values determine their choice of stance.

Now, what would drive someone to believe that the aim of science is one thing or another? Surely people have a variety of reasons, some leading to similar pronouncements and some divergent. But in general, realists value science for its demystifying power, which comes in the form of explanations. Van Fraassen cites Ernest Nagel as someone who sees explanation as the central aim of science, its very *raison d'être:*

> It is the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences. (Nagel 1961, p.4, cited in van Fraassen,

1980, p.92)

Popper, likewise, claims that "a scientific problem, as a rule, arises from the need for an explanation" (Popper, 1957, p.112). Our theoretical scientific knowledge, according to realists, is valuable for its ability to provide us with *explanations* of the observable regularities in our world. It is from this view of science, as the search for explanations, that the realist attitude arises. Popper, quite explicitly and self-consciously, says that "[t]he positive arguments [in favour of scientific realism] I have in mind rest on the relation between realism and the aim of science" which he sees as the search for "*satisfactory explanations* of whatever strikes us as being in need of explanation" (Popper, 1983, p.131-132).

The picture of explanation given by Popper is relatively simple. A reasonable explanation must logically entail that which is explained, it must not be ad hoc, it must have independent evidence in its favour, and it must be thought to be true (or at least not be known to be false)[2]. We will generally have some set of facts that we take ourselves to know, and we will attempt to explain these known (or at least accepted) facts by speculating or conjecturing about their unknown causes. Thus, explanation will often be the explanation of familiar and well-accepted facts by less familiar conjectures, including explaining known observable facts by (previously) unknown unobservable facts (Popper, 1983, p.132). Searching for explanations of this sort leads one to adopt the metaphysical stance, and thereby to adopt a realist attitude towards scientific theories. Realism is generally the stance of those who, like Popper, value science for it ability to provide us with such explanations, particularly explanations of observable phenomena and regularities in terms of their unobservable causal structure and governing laws.

Having explanations, however, in terms of knowledge about the unobservable causes of our observations, could be seen as a somewhat decadent intellectual indulgence. I see no

---

2 These criteria make sense of van Fraassen's initial characterization of realism as the view science aims at formulating true theories while remaining consistent with many realists' contention that science aims at providing explanations: for we cannot have adequate explanations unless we have true theoreies.

reason to doubt that we could do without explanation in most of our daily and political lives. Indeed, I think we more often than not *do* go without such satisfactory explanations. Empiricists, qua empiricists, eschew the kind of metaphysical speculation involved in providing explanations of the observable by the unobservable. What they value science for is (as I would put it) its ability to provide a complete description of our experience. As a result, they have no reason to seek the kinds of metaphysical explanations that the realist seeks. Empiricism is generally the stance of those who value a minimal amount of metaphysical speculation and commitment while still being able to accurately and fully describe our experience.

§4 – To Parasitize or Not to Parasitize

Now that we have an idea of what drives one to accept scientific realism or constructive empiricism, it will be prudent to discuss a dilemma faced by the constructive empiricist regarding the evaluation of theories in order to point out the difference between the constructive empiricist and the scientific realist's respective accounts of theory evaluation. The argument I am about to make depends on showing that the constructive empiricist's and the realist's criteria of theory evaluation give them divergent results about certain truths regarding *observables*. By their very nature they diverge with respect to truths regarding unobservables, but I will argue that constructive empiricism cannot even be a good enough guide for us with respect to truths about observables.

It might be thought that scientists have their own methods of theory appraisal, independent of any particular epistemic stance, which mainly concerns philosophers. The problems with such a view are easy to see. Scientists produce theories to solve certain problems they have, be they conceptual, empirical, or whatever; but in evaluating particular scientific theories, they must already be engaged in a philosophical endeavour. Theories cannot be evaluated without an account of what the aim of science is, for one must assess theories for their closeness to having achieved this goal. What the ultimate goal of science is, as we saw in the last section, is something disputed by *philosophers* of science. The scientist will adopt, if only tacitly, some sort of epistemic stance and the criteria of theory evaluation it endorses in order to judge the success or acceptability of a given scientific theory. Van Fraassen's claim is that the scientist could make do with the empirical stance and constructive empiricism's account of theory evaluation just fine – and frankly I am inclined to agree with him (or at least grant him this point here). But can the public policy maker "make do" with this account of theory evaluation and acceptance? I argue that they cannot, and therefore that *we* cannot make do without the development of a realist philosophy of science.

As we saw in Chapter 1, constructive empiricism and scientific realism are deeply bound up with one another conceptually, particularly in their characterization of scientific theories and their understanding of truth as correspondence with the facts. These shared features do not favour one philosophy over the other: they are shared, hence neutral. I argue that an account of theory evaluation cannot be neutral in this sense. When it comes to developing such an account, the constructive empiricist is faced with two options. First, the constructive empiricist might *parasitize* an account of theory evaluation from the realist. If prompted for an account of theory evaluation, the constructive empiricist might simply say that what makes for a good scientific theory is an issue for the realist to work out, and that once this is worked out she will simply qualify this account with "is true *only* with respect to observables." This would be the view of anyone who believes that theory evaluation can in fact be neutral in the above sense.

The other option, which van Fraassen seems to take, and which I argue is the only viable option, is to provide an independent account of theory evaluation. Van Fraassen's reasons for developing such an independent account are, I think, quite obvious. First, there are many forms of scientific realism, and many different accounts of theory evaluation. Wedding the constructive empiricist's account of theory evaluation to the realist's would spawn just as many constructive empiricisms as there are scientific realisms (which is a significant number). But a second and more pressing issue regards the fact that such a union would vindicate the very kind of metaphysical speculation the anti-realist empiricist abhors. Assessing the empirical adequacy of any given theory would require thinking about whether or not we should expect it to be true with respect to unobservables. This would amount to a kind of self-defeat for the constructive empiricist, a failure by their own lights, and therein would demonstrate the irrationality of constructive empiricism as a part of the empirical stance.

There is a third issue as well. In asserting the empirical adequacy of any given theory, the constructive empiricist goes beyond the available evidence, just as the realist goes beyond the available evidence to assert the truth of any given theory (see Section 2.6 of this essay). One might think that, since they are both going beyond the available evidence, the constructive empiricist might as well go a step further and assert that a theory is true with respect to unobservables rather than restraining themselves to asserting its empirical adequacy. But, as a van Fraassen notes, "it is not an epistemic principle that one might as well hang for a sheep as for a lamb" (van Fraassen, 1980, p.72). This is very true. But, should it not be an epistemic principle that a method developed for one purpose should not be expected to serve another? The realist's methods of theory evaluation aim to identify theories that are true with respect to unobservables, sometimes even at the expense of truth about observables. Wedding constructive empiricism to the realist's criteria of theory evaluation is thus incredibly optimistic, for it requires one to believe that principles developed to serve one goal would serendipitously serve another, *opposing* goal.

So, van Fraassen gives an independent account of theory evaluation, according to which the sole cognitive, epistemic, or truth-directed virtue of a scientific theory is its empirical adequacy. All other virtues, save merely logical or internal virtues, are pragmatic or instrumental in nature. Our valuing of these "super-empirical" pragmatic virtues, for the constructive empiricist, is solely a matter of their contributing to the production of ever more empirically adequate theories. We do not value these virtues in a theory because they make the theory more likely to be true with respect to observables or unobservables – rather, we value these virtues because they do things like help to suggest further areas of investigation, are generally more conceptually manageable, and in general contribute to the scientific endeavour of producing ever more empirically adequate theories. The super-empirical virtues aid science as an endeavour, not individual theories in their evaluation. This, according to van Fraassen, accounts for scientist's preference for empirically adequate theories that also possess certain super-empirical virtues. But, to be sure, on the constructive empiricist's account the fact that a theory possesses certain super-empirical virtues provides *no additional reasons for believing that theory to be true or empirically adequate*; it only provides additional reasons for accepting that theory if it is also considered empirically adequate (see van Fraassen, 1980, pp.87-89; Churchland, 1985; and Section 2.8 of this essay).

Thus, for the constructive empiricist, that an empirically adequate theory possesses certain super-empirical virtues provides good reason to commit to a research program based on that theory. That a theory whose empirical adequacy is disputed possesses certain super-empirical virtues *cannot*, therefore, provide good reason to accept that theory as empirically adequate, for the super-empirical virtues are not truth-directed. The realist, on the other hand, believes that the possession of super-empirical virtues are truth-directed, specifically directed at ascertaining the truth about unobservables. For the realist, that a theory whose empirical adequacy is disputed possesses certain super-empirical virtues does, therefore, provide good reasons for believing a theory to be true, truer, or more likely to be true than a theory without them.

To see the difference that constructive empiricism and scientific realism's respective accounts of theory evaluation can have for our beliefs about observables, suppose we are faced with a pair of conflicting scientific theories. Theory A posits an unobservable process that, if it existed or occurred, would result in some novel state of affairs for observables at some point in the future. Theory B denies the existence of this process and thereby the predicted future consequence(s) for observables. Furthermore suppose that, so far as we know, both of these theories are empirically adequate – that is, they both give similar short term predictions, which match our records and observations, while nevertheless diverging in their long term predictions. Now, the obvious question for anyone making plans upon which the occurrence or non-occurrence of this novel state of affairs hinges is: which theory gets the observable facts correct? Does the unobservable process described in theory A occur? A great deal can hinge on the answer to this question, for if we knew the answer we would know what the state of observables will look like in the future, and could base our plans on such knowledge. If realists think they know that the unobservable properties and relations described by theory A do exist, they will have good reason to believe its novel prediction about the state of observables in the future. The constructive empiricist, by contrast, cannot appeal to the truth about unobservables in weighing these theories against each other, and must assess theories A and B for their truth with respect to observables alone.

The constructive empiricist faces a dilemma here regarding what to believe about a theory's predictions in new domains, which I will illustrate through an example from the history of science. Suppose the modern constructive empiricist and scientific realist were in the place of the ether theorists. Both accept Fresnel's ether theory of light in their own way. Now, before it was ever run, what kind of result would each of these individuals have anticipated from the Michelson-Morley experiment? Now suppose that, in accepting the empirical adequacy of ether theories, the constructive empiricist would have believed the predictions made by ether theorists of some non-null result for this experiment. The realist, on the other hand, utilizing something like Chakravartty's distinction between detected and undetected properties and relations, would have remained agnostic about these results because no causal connection had ever been forged with the properties of the ether that would have been responsible for a non-null result. Constructive empiricists would here recommend belief where realists would recommend agnosticism: and the constructive empiricist would have been wrong! Perhaps constructive empiricism is not less risky than realism after all!

So, if the constructive empiricist is asked what to believe about the predictions of a successful theory in a new domain, and answers that one should believe what they say about observables, the realist and the constructive empiricist's prescription for belief will diverge where these predictions involve properties and entities that are currently auxiliary properties and entities: that is, when they involve unobservables that have *never* been detected. There is an alternative open for the constructive empiricist: rather than recommend *belief* in an apparently empirically adequate theory's empirical adequacy in untested domains, the constructive empiricist could recommend *agnosticism* about the empirical adequacy of theories in untested domains in general. But, if she chooses this alternative, a new divergence arises between the realist's and the constructive empiricist's prescriptions for belief. This new divergence occurs where these predictions involve unobservable properties and entities that *have* been detected. For the realist believes she has good reason to believe a well tested theory's predictions in new domains, so long as these new domains only involve the interaction of unobservable properties that have been previously detected. The constructive empiricist, by contrast, has no recourse to truth about unobservables in deciding what novel predictions to believe.

Thus, the constructive empiricist faces a dilemma between recommending agnosticism regarding new domains in general and not recommending agnosticism regarding new domains at all. Whichever horn of this dilemma the constructive empiricist embraces, it leads to a divergence in the prescriptions for belief about observables given by the scientific realist and the constructive empiricist. I take it that this dilemma is inescapable for the constructive empiricist *without appeal to unobservables*, i.e. without self-defeat by empiricist lights.

Thus, the constructive empiricist must accept that their recommendations for belief regarding *observables* differ from the realist's. Without being able to appeal to the reality of certain unobservable but detected properties and processes (i.e. without giving up empiricism), or the truth-directed nature of certain super-empirical virtues, the constructive empiricist will disagree with the realist about what observational portions of a successful theory are belief-worthy: either recommending belief in a theory's predictions in new domains where a realist would recommend agnosticism, or recommending agnosticism regarding a theory's predictions in new domains where a realist would recommend belief.

For the scientist, of course, such inevitable divergence does not matter very much. The epistemic policies and account of acceptance given by constructive empiricism suffices for the scientist, because what is interesting to the scientist is waiting to see what actually happens – our observations will ultimately decide for us which theory is correct. Scientists want to go out and test the empirical adequacy of a scientific theory that predicts a certain novel phenomenon by attempting to observe this phenomenon: for them, time and testing will tell. Scientists may as well choose theories according to their apparent empirical adequacy, and make any further assessments of theories according to non-epistemic criteria: ultimately, empirical adequacy may be all that scientists need from a theory.

While constructive empiricism may suffice for scientists, it does not suffice for public policy makers. For instance, public policy makers may need to plan for or avoid a particular novel phenomenon, and do not have the luxury of time and direct empirical testing to come to a decision about whether or not this phenomenon is likely to occur. In short, public policy makers need super-empirical methods of deciding whether certain predictions are likely to come true, for sometimes they cannot go out and directly test them. The realist provides such super-empirical methods in the form of an epistemology aimed at discovering the unobservable causes of the observable features of our world – for if we know the truth about unobservables we can use this knowledge to inform our predictions about observable states of affairs, even unprecedented or novel observable states of affairs. Without such realist epistemological tools, the public policy maker may be unable to get what they need from science: an appropriate set of facts to help them make important policy decisions. For if a theory predicts certain unprecedented and catastrophic states of observable affairs, states one wishes to avoid, one cannot go out and test this theory's empirical adequacy in this domain: for this would require bringing about the very state of affairs one wishes to avoid. So, while constructive empiricism might provide an adequate account of what it means for a scientist to accept a scientific theory, it may not be an adequate account of what it means for a public policy maker to accept a theory, for the public policy maker sometimes needs to accept theories in a realist manner.

§5 – Planning from Stances

In choosing a stance that can allow scientific theories to adequately inform our decisions, we will find that the needs and values informing our choice of stance will be indexed to the specific task at hand. We may need science to help us produce *evidence*, to help us favour one putative account of the facts over another based on a limited set of known facts. This is often the case in court trials, where a limited amount of forensic evidence needs to be extrapolated into a larger account of the facts of the crime. Since evidence must be observable, in these cases we will only care about the facts surrounding and connections between observables like burglars, bullets, and blood samples. The unobservable truth of the theories informing such judgments is irrelevant to our needs here. What is relevant is that these theories capture relevant, regular, and well tested correlations between observables like the results of certain blood tests and the origin of the samples, or between certain patterns of blood spattering and bullet trajectories. We simply note that certain theories are (or appear to be) empirically adequate in the relevant domains, and then go on to use them to inform our reconstruction of the event from our limited evidence. What matters for us in these cases, in short, is that scientific theories give us the truth about observables, because when it comes to evidence all we care about is the well established truth about the observable. Thus, the constructive empiricist's account of acceptance should suffice here: for a judge or jury in a criminal trial, for instance. Such an account of acceptance represents the minimal amount of metaphysical speculation and commitment needed in order to allow science to serve its needed role in these contexts.

Distinct from these cases are cases where we need science to help us produce *plans*. In such cases, we are often forced to reconcile our scientifically informed decisions with the fact that science is inherently fallible. Basing policy decisions on a fallible enterprise like science is an inevitably risky business. Nevertheless, it is likely preferable to the alternative: basing policy decisions on blind ignorance. Public policy makers need science to inform their decisions, but they are also in need of a set of epistemic policies that can help them navigate the products of modern science in the face of continual scientific flux. In the hopes of making debates over realist and empiricist epistemic policies matter in a resolvable way, I wish to discuss these two philosophies in terms of their potential for helping public policy makers navigate the flux and uncertainty of scientific knowledge. I claim that policy makers will often need to ask realist questions about our scientific theories – specifically, questions about the truth of their hypotheses regarding unobservables – and that constructive empiricism is insufficient for at least some of our collective purposes. Thus, the development of a realist epistemology is necessary in order for us to resolve (or even make sense of) many political issues that depend on evaluating and basing our decisions upon the theoretical products of modern science.

Recall the Pessimistic Induction (PI), which points out that the history of science is a veritable graveyard of failed and falsified scientific theories. With so many failed theories, what reason do we have to base our current policy decisions on any of our current theories, given that they are likely to be replaced in the future? To make this query less abstract with an example to be elaborated upon in the next section, we might ask: why should we curb our carbon emissions and therein our fossil fuel consumption if our current theories of global warming are liable to change in the future?

In attempting to face up to the problems posed by the PI directly, scientific realism is the philosophy that most needs to anticipate scientific shifts. If realists are to avoid the problems posed by (seemingly) radical shifts in the ontological commitments of well-accepted scientific theories, they need a method of anticipating scientific shifts, and committing only to the truth of those parts of our theories most likely to be retained throughout scientific revolutions. The realist has in fact developed a variety of conceptual tools that she can deploy to these ends: the distinction between detected properties and auxiliary properties, the notion of theoretical maturity, and many other traditional realist caveats on what makes (certain portions of) a theory belief-worthy (see sections 2.7 and 2.8 of this essay). With such methods in her arsenal, there is good reason to think that the epistemological tool kit of the scientific realist would be best adapted to successfully or reliably placing bets on the truth of scientific theories in the face of science's fallible and fluctuating nature. The realist's selective scepticism, after all, is developed precisely to rebut those pessimistic views of the history of science that take this flux to undermine any claim of theoretical continuity over time.

The constructive empiricist, by contrast, formulates her selective scepticism in response to concerns about the underdetermination of theories by evidence, and has no real interest in rebutting pessimistic views of the history of science. Given their asymmetric concerns, it would be quite unsurprising if the scientific realist were able to provide better tools for anyone attempting to make decisions informed by science while admitting that theoretical science is inherently fallible and subject to revolutionary change. To put it a little differently, realism might provide a better account of what it means for the *public policy maker* to accept a theory, even if constructive empiricism provides a better account of what it means for the *scientist* to accept a theory.

When we need to see into the future, using one of two as yet empirically underdetermined theories, we need to see which of these two divergent empirical substructures are favoured by the more limited observable evidence available to us. The scientist and the constructive empiricist will search for more evidence – she will attempt to discover which of these two theories is in fact empirically adequate by attempting to observe the divergent phenomenon of interest. She will let her observations decide the case, because she cannot appeal to super-empirical virtues as epistemic virtues, or virtues which are relevant to a theory's truth. Such an empirical test will allow her to select the theory which does in fact get the observable phenomena correct. Until such a crucial test is performed, the scientist will likely accept the theory with the most super-empirical virtues; but they will do so for entirely non-epistemic (i.e. purely pragmatic) reasons. All of this is highly consistent with the practice of working scientists, for most will prefer theoretical disputes to be ultimately decided by an empirical test rather than an appeal to one theory's super-empirical virtues. But the policy maker has different needs, and may need to settle for something less than a direct empirical test in adjudicating between the divergent predictions of two empirically adequate theories.

Because the constructive empiricist does not see super-empirical virtues as truth-directed virtues, she cannot appeal to them in trying to decide which theory is more likely to be a good guide to truth about observables. The realist, on the other, has a different tactic available. Rather than trying to see which theory is a better guide to truth about observables directly, through empirical tests, she might enquire into which theory is a better guide to truth about unobservables, or appeal to a theory's super-empirical virtues (which the realist sees as truth-directed). The realist could then appeal to these truths and virtues in deciding which theory's observable predictions are more likely to be true, without the need to set up an empirical test. As was noted, this can be especially useful if one cannot set up an empirical test without possibly bringing about dire consequences.

Realism aims to provide a conceptual toolkit that would help resolve certain theoretical disputes that matter to the public policy maker, without the need to check the empirical adequacy of the conflicting theories directly. These tools are unavailable to the constructive empiricist, who could be forced to risk the very undesirable observable states of affairs predicted by a theory that one wishes to avoid in order to determine whether or not these observables states would result from certain actions. This would be the case whenever one wished to avoid an entirely novel or unprecedented state of affairs predicted by an apparently empirically adequate theory. In such situations, we will need realist epistemic policies to help us make long-term plans, for only realism gives us a method of assessing the predictions of a theory without direct empirical testing of those predictions.

I take this to be a vindication of the metaphysical stance over the empirical stance that goes beyond mere individualistic voluntarism. But this vindication is not one that compels *scientists* or *individuals* in general to accept the metaphysical stance. Rather, it notes that many of our most important political decisions hinge on the answers to questions that only the realist dare ask, questions that the constructive empiricist cannot even begin to answer in a satisfactory manner without risking the very consequences we wish to avoid. This vindication is one that compels only the *public policy maker* to (often but not always) accept the metaphysical stance, and to (often but not always) inquire into the unobservable truth of certain scientific theories.

To be sure, a realist attitude is not *always* necessary, in all instances of planning based on scientific theories. Thankfully, showing that there is one such case will suffice to vindicate the development of a realist epistemology, and the acceptance of a metaphysical stance from which such a realist porject results. The case I use to make my point here, however, is an extremely important one, so important that it has drawn the attention of people in positions of power from all over the planet: global warming theory. I do not doubt that there are many areas of the political sphere where a constructive empiricist's metaphysically minimal epistemic policies might be more appropriate, even when we are planning for the future. But given the immediate importance of the issues surrounding whether or not global warming theory is true with respect to unobservables, this case serves as a very strong vindication of developing a realist epistemology.

§6 – Global Warming: a case in point

Foss (forthcoming) discusses two competing hypotheses regarding the effects of so-called "greenhouse gases." One of these theories, the null hypothesis or natural variation (NV) hypothesis, asserts that the earth's climate, through a variety of chaotic but deterministic processes, displays unpredictable variation within a certain range of temperatures. The other theory, Global Warming Theory (GWT), posits an additional unobservable process with certain long-term observable consequences, namely "the greenhouse effect."

The greenhouse effect is this: excess carbon dioxide (and other greenhouse gases) pumped into the atmosphere by humans burning fossil fuels heats the temperature of the earth over time. The way that carbon dioxide supposedly does this is by trapping excess solar radiation, preventing it from escaping into outer space In the models of GWT, the greenhouse effect is a parameterization in terms of radiant heat exchanges between the earth's sun, surface, atmosphere, excess pollutants, clouds, oceans, etc. (see Foss, forthcoming, Chapter 7). This process is unobservable because it involves unobservable entities: what we can observe are certain phenomenal temperatures, sea levels, and readouts from various scientific instruments, but GWT posits an unobservable process operating behind the observable scene where excess emissions of carbon dioxide (a molecule and therein an unobservable entity) traps solar radiation (another unobservable entity). In GWT, the trapping of excess solar radiation from anthropogenic atmospheric carbon dioxide is posited as a *cause* of observable terrestrial temperature increases over time, and causes are unobservable because we can only ever infer them, not observe them.

The consequences of GWT being true with respect to unobservables could be disastrous given the amount of greenhouse gases pumped into the atmosphere by human combustion of fossil fuels. Add to this the fact that we have had some very warm years as of late, and it is no wonder that people of all stripes from around the world have begun to take this theory very seriously. Citing "the precautionary principle," many people will claim that whether or not we know global warming theory to be true or false, we should curb our greenhouse gas emissions just in case it is true. But the amount of curbing necessary could severely disrupt our global economy, with the potential to impoverish billions of people, especially in the developing world. There are a great many other hypotheses regarding similarly apocalyptic scenarios – should we take them all as seriously, merely as a matter of precaution? Surely we can do better than blind precaution, especially when such precaution could cause such a massive drop in human prosperity.

NV and GWT agree in their observable predictions in the short term, but their long term predictions diverge – one predicts a rise in temperatures over time that correlates with levels of $CO2$ emissions (GWT), the other predicts chaotic (yet deterministic) variations of temperature within a particular boundary (NV). In assessing the divergent predictions of these two live hypotheses, the realist will try to detect unobservable properties and their putative relations, and will look at the super-empirical virtues of each theory to see if we have reason to believe its predictions over the other theory's predictions. For instance, if certain properties and relations are detected as they are described by GWT, or GWT's super-empirical virtues suggest its truth, the realist would have good reason to recommend immediate curbing of greenhouse gas emissions, despite its potentially detrimental effect on the global economy. If, on the other hand, attempts to detect the properties and relations it posits fail, or its super-empirical virtues are found lacking, the realist will have good reason not to believe the long-term predictions of GWT. The constructive empiricist, by contrast, would have to test the two theories where they diverge: precisely at the point of interest, some 50 years in the future when (if GWT is true) temperatures would have risen to unprecedented heights. This would essentially mean continuing to pump carbon into the atmosphere and waiting to see whether global warming happens or not. The novel prediction of global temperature increases following gigatonnes of carbon emissions by humans is, in terms of observables, precisely what sets GWT apart from NV; and yet, at the same time, it is also the very circumstance we wish to avoid. Obviously, putting these two competing theories to the definitive empirical test is not an option for anyone who wishes to actually avoid catastrophic global warming.

While van Fraassen may be correct that, in accepting a theory, the scientist need only believe it to be empirically adequate, the scientist and the policy maker are engaged in different endeavours, and have different goals. As Foss notes, "Scientists must judge whether a theory is strong enough to be accepted as the basis of further scientific research, whereas [policy makers] must judge whether a theory is strong enough to be accepted as the basis of public policy" (Foss, forthcoming, p.20-21). Accordingly, we should expect our accounts of what it means for scientists and policy makers to accept a theory to differ. I contend that they do differ, and that the policy maker has standards of acceptance closer to the realist's than the constructive empiricist's.

In basing our policy decisions on scientific theories, we will often *need* super-empirical reasons for favouring one theory over another as a description of *unobservable* reality. We need reasons to believe in the existence or non-existence of the unobservable process of anthropogenic global warming, reasons other than direct and definitive empirical tests. Without a method of deciding whether GWT is likely to provide accurate long-term predictions that does not involve direct empirical tests, we cannot base our public policy decisions on a theory like GWT.

When two theories are, so far as we can tell, empirically adequate, the constructive empiricist has nowhere to turn for further adjudication of their belief worthiness; she is thus trapped if she tries to form long-term plans based on unsettled debates between competing theories like NV and GWT. According to her criteria of theory evaluation, she cannot appeal to super-empirical virtues as making a theory more (or less) *belief*-worthy. She can appeal to such virtues as making a theory more *acceptance*-worthy, as having more potential for stimulating scientific advancement, but this does not give her any reason to think that one theory's long-term predictions will be more accurate than another's. At best it gives her reason to think that committing to a research program involving this theory will be more fruitful, in the long run, as discovering its inadequacies drives us to produce more empirically adequate theories. With respect to truth, one should commit to no more than that the theory appears to be empirically adequate in certain well-tested ways. But, as a result, constructive empiricism is insufficient for our political planning purposes.

By contrast, the realist's view of the super-empirical virtues as truth conducive, and the epistemological and conceptual tools they develop for attempting to determine the truth about unobservables, clearly do suit many of our long-term planning needs as public policymakers. Metaphysical speculation of the realist variety is the public policy maker's only recourse in many circumstances. Given the constructive empiricist's account of theory evaluation, there can be no epistemological reason to accept GWT or NV over the other, save checking the empirical adequacy of GWT directly – that is, by bringing about or failing to bring about the very consequences one wishes to avoid by basing our policy decisions on current science. The scientific community may be able to make do with an aim of achieving empirical adequacy, but they have all the time in the world. For public policy makers this is obviously not the case. For them, time is of the essence, and they need super-empirical methods of favouring the predictions of one theory over the other *right now*, before their long-term empirical adequacy in new domains can be tested directly. Developing methods for doing this is the exclusive purchase of realist epistemologies.

§7 – On Justifying Empiricism

Given his love of the spirit of logical empiricism, maintaining that spirit in the form of empiricist epistemic policies and mathematical rigor, van Fraassen might be criticized for not giving as political or pragmatic a justification for his empiricism as the logical empiricists gave for theirs. It is well known that the goals and motives behind the development of logical empiricism were mostly politically progressive. The Vienna Circle wanted to improve communication, and tried to resolve many if not all philosophical squabbles by dissolving metaphysical disputes into disputes over the usefulness of certain ways of speaking. The anti-metaphysical policies of logical empiricism seemed the most fitting philosophy for such goals; but van Fraassen's reasons for maintaining empiricism takes anti-metaphysical speculation as a value in itself.

If we take a lack of metaphysics in the public sphere to be a worthy goal, we might appeal to van Fraassen's constructive empiricism as a reasonable public policy, devoid of useless metaphysical speculation, that we might accept within the public sphere when debating what policy decisions we should make. However, as I have shown, constructive empiricism is insufficient for some of our collective planning purposes. Sometimes the public policy maker may need a nice, lean, well thought out realist epistemology to inform their policy decisions.

And, one might add, is it really so much to ask that we all accept, in political discourse, the existence of electrons and electromagnetic fields, not merely the empirical adequacy of those theories which posit their existence? After all, the existence or non-existence of unobservable entities, properties, and relations matters to the policy decisions we make based on our current science, and even the kinds of arguments we are inclined to accept. The deep truth of a theory like evolution by natural selection often matters to us much more than its empirical adequacy. In the case of global warming, all we can observe are certain instrumental readouts and phenomenal temperatures – but what we really want to know about is the unobservable causal connection between our use of fossil fuels and observable temperature patterns, possibly through the unobservable "greenhouse effect". Only realism provides us with the kinds of tools we need to investigate the unobservable causal links between fossil fuel consumption and global temperature increases *before* we can check their empirical adequacy in the domain of the future; indeed, only realists even *attempt* to provide such tools.

Like the empiricism of the logical empiricists, the epistemic policies of scientific realism could have their political justifications, for many policy decisions turn on our beliefs about the existence or non-existence of unobservable entities and processes. While the metaphysical minimalism of certain forms of empiricism might help us dissolve some pointless political disputes, and improve communication as the logical empiricists had hoped, realist epistemological considerations surely have their own place in the public sphere. Without methods of discerning which unobservable entities and processes we have good reason to believe in, many of our political disputes will be intractable. The kinds of inquiry that scientific realists are comfortable entering into – regarding causes, natures, and modal properties – are extremely useful for policy makers, because they facilitate long-term forecasting in a manner unavailable to the modality-fearing empiricist.

Conclusion

Traditionally, debates between realists and empiricists are cast in terms of either an epistemology for the everyman, or an epistemology for the working scientist. The debate is seen as either a dispute over a) what it is "rational" to believe about science (i.e. which position it is more "rational" to accept), or b) what the best interpretation of a working scientist's commitment to a particular theory is (i.e. what it means for a scientist to accept a theory). A more pressing dispute is too often neglected: which position and epistemology is the most useful for a group, for our nation, or even for the entire human race. I have argued that realism, not constructive empiricism, can be (and I suspect often is) the more appropriate stance for those individuals in our society that play the role of public policy maker: a role that virtually all of us play to some degree or another.

In thinking of the debate in these terms, the more transcendental question of whether causation is really in the world mind-independently can be set to one side, eclipsed by the fact that realist efforts to determine the unobservable causes of observable states of affairs is a way of interpreting science that serves us very well, perhaps even better than empiricist methods. The metaphysical stance should be pursued for those reasons alone. This is a vindication not of the tenets of a particular brand of scientific realism, but of realism and the metaphysical stance from which it results as philosophical *projects* more generally conceived. It establishes our shared need to develop a realist epistemology, and to inquire into the truth regarding unobservables.

It is difficult to ask scientists to divorce themselves from their other roles when they act as scientists. For the scientist qua scientist, novel predictions may only be of pragmatic interest to her projects, suggesting new areas for testing the empirical adequacy of her theories. When presented with apparently empirically adequate theories that diverge in their predictions, the scientist would like to test these theories against one another by way of empirical test. That is to say, the scientist qua scientist cares about novel predictions only as they relate to a theory's empirical adequacy.

But the scientist qua public policy maker cares differently about her theory's novel predictions. The truth or falsity of a theory's novel predictions can be very important for her long-term decisions. Realism, unlike constructive empiricism, can often give good (and reliable) reasons to believe a theory's novel predictions, before they have been directly tested: if they involve only *detected* unobservables, they are likely to come true. That is, a bet on the occurrence of the predicted event is a good bet. When it comes to novel predictions that involve *undetected* unobservables, agnosticism is prudent. A bet in these cases is blind, even for the realist. But without recourse to truth about unobservables in this way, the constructive empiricist cannot properly anticipate which novel predictions are likely to come true, and therein cannot inform or justify her predictions about the future as well as the realist can. This may be ok for the practicing scientist qua scientist, but not the scientist qua public policy maker.

Given this, when scientists are put in front of a television camera, and asked empirical questions about politically important things like what will happen in the future, it is no wonder they act like realists, talking about what we "know" about unobservables, how electrons are real, etc. They are not merely speaking from their accepted theory *ex cathedra* as van Fraassen would see them – they are, as experts, placing their *realist* bets down on the table for everyone watching to see, so that we may all as public policy makers make decisions about the future based on their informed speculation about the nature of the unobservable world.

Popper's realism, and his understanding of the nature of scientific inquiry more generally, is often seen as inextricably political. According my view here, this is not a bad thing. Scientific inquiry is likely affected by philosophy of science, and by political issues. This is not a bad thing either. For we must remember, Popper was a guesser: the only positive reason that he would give to believe a theory's novel predictions is that this theory is our best guess at the truth. And guessing, too, is not a bad thing, at least not when a) we have reason to believe that we have a reliable system for placing solid bets and b) we are *forced* to place a bet.

Realists, more generally, are guessers: they take greater risks, because they feel there is more to gain. And there is: the ability to predict which of a partially-tested theory's untested novel predictions will come true. We can never get outside either our metaphysics, nor outside our scientific theories, to check whether the facts are as we think they are – whether electrons really exist, and what the causal connections that exist in nature really are (if there really are any). To use the common phrase, we cannot take a "God's-eye-view". But this does not, of course, imply that we should not see the proper identification of causes and unobservable truth as one of the truth-directed goals of a scientific endeavour motivated by our common values. And if we do, claims the realist, we can make more informed bets about the future; and who would not value that?

Can we really expect scientists to be the empirical adequacy seekers that constructive empiricism describes, who are ready to wait for empirical tests of novel predictions rather than inquire into the nature of unobservables? Given their dual role as public policy maker, I doubt it. It may be difficult, risky, fruitless, or even futile; but developing epistemological tools to help us inquire into the truth about unobservables, and even allowing such a realist epistemology to inform our scientific inquiry, is often very useful. Such metaphysical inquiry can even be necessary, given that it can sometimes be the only manner in which science can even begin to aid our planning for the future.

Bibliography

Alston, W. P.: *A Realist Conception of Truth*; New York, Cornell, 1996

Boyd, R: "Lex Orandi est Lex Credendi"; in *Images of Science: Essays on Realism and Empiricism with a Reply from Bas C. van Fraassen*; Churchland P. and C. A. Hooker (eds.), Chicago, University of Chicago Press, pp.3-34, 1985

Campbell, K.: "Selective Realism in the Philosophy of Physics"; *The Monist*, Vol. 77, pp.27-46, 1994

Carus, A. W.: "Carnap's Intellectual Development"; in *The Cambridge Companion to Carnap*; Friedman M. and R. Creath (eds.), Cambridge, Cambridge University Press, pp.19-42 2007

Chakravartty, A.: "Semirealism"; in *Studies in the History and Philosophy of Science*; Vol. 29, No. 3, pp.291-208, 1998

Chakravartty, A.: "The Semantic or Model-Theoretic View of Theories and Scientific Realism"; in *Synthese*; 127, pp.325-345, 2001

Chakravartty, A.: "Structuralism as a form of scientific realism"; in *International Studies in the Philosophy of Science*; Vol. 18, No. 2 & 3, pp.151-171, July 2004

Chakravartty, A.: *A Metaphysics for Scientific Realism*; Cambridge, Cambridge University Press, 2007

Churchland, P.: "The Ontological Status of Observables: In Praise of the Super-Empirical Virtues"; in *Images of Science: Essays on Realism and Empiricism with a Reply from Bas C. van Fraassen*; Churchland P. and C. A. Hooker (eds.), Chicago, University of Chicago Press, pp.35-47, 1985

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

Dummett, M.: "Realism", in *Synthese*, Vol. 52, pp.55-112, 1982

French, S. and N. C. A. da Costa: *Science and Partial Truth*; New York, Oxford University Press, 2003

Foss, J: "On Accepting van Fraassen's View of Science"; in *Philosophy of Science*, Vol. 51, pp.79-92, 1984

Foss, J: "On Saving the Phenomena and the Mice: A reply to Bourgeois Concerning van Fraassen's Image of Science"; in *Philosophy of Science,* 58, pp.278-287, 1991

Foss, J.: *Beyond Environmentalism: A Philosophy of Nature*; New York, Wiley and Sons, forthcoming

Giere, R.: "Constructive Realism"; in *Images of Science*; *Essays on Realism and Empiricism with a Reply from Bas C. van Fraassen*; Churchland P. and C. A. Hooker (eds.), Chicago, University of Chicago Press, pp.75-98, 1985

Giere, R.: *Explaining Science: A Cognitive Approach*; Chicago, University of Chicago Press, 1988

Gutting, G.: "Scientific Realism versus Constructive Empiricism: A Dialogue"; in *Images of Science: Essays on Realism and Empiricism with a Reply from Bas C. van Fraassen*; Churchland P. and C. A. Hooker (eds.), Chicago, University of Chicago Press, pp.118-131, 1985

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

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

Kirkham, R. L.: *Theories of Truth: A Critical Introduction*; Massachusetts, MIT Press, 1995

Ladyman, J.: *Understanding Philosophy of Science*; New York, Routledge, 2002

Laudan, L.: "A Confutation of Convergent Realism"; in *Philosophy of Science*, 48, pp.19-49, 1981

Laudan, L.: "Explaining the Success of Science"; in *Science and Reality*; Cushing, J. et al. (eds.), Notre Dame, Notre Dame University Press, 1984

Maxwell, G.: "The Ontological Status of Theoretical Entities"; in *Minnesota Studies in Philosophy of Science*, III, 1962

Muller, F. A.: "Can a Constructive Empiricist Adopt the Concept of Observability?"; in *Philosophy of Science*; Vol. 71, Issue 1, pp.80-97, Jan 2004

Nagel, E.: *The Structure of Science*; Harcourt, Brace, and World: New York, NY, 1961

Popper, K.: *The Logic of Scientific Discovery*; New York, Routledge, 1935

Popper, K.: *The Poverty of Historicism*; New York, Routledge, 1957

Popper, K.: *Realism and the Aim of Science*; New York, Routledge, 1983

Psillos, S.: *Scientific Realism: How Science Tracks Truth*; New York, Routledge, 1999

Putnam, H.: *Mathematics, Matter, and Method*; Cambridge, Cambridge University Press, 1975

Suppes, P.: "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences" in *Synthèse*, Vol. 12, pp. 287-301, 1960

Van Fraassen, B. C.: *The Scientific Image*; Oxford, Oxford University Press, 1980

Van Fraassen, B. C.: *The Empirical Stance*; New Haven, Yale University Press, 2002

Van Fraassen, B. C.: "Structure: Its Shadow and Substance"; in *British Journal for the Philosophy of Science*; vol. 57, No.2, pp.275-307, 2006

Worrall, J.: "Scientific Realism and Scientific Change"; in *Philosophical Quarterly*, vol. 32, pp.201-231, July 1982

Worrall, J.: "Structural Realism: The Best of Both Worlds?"; in *Dialectica*; Vol. 43, No 1-2, 1989

Worrall, J.: "Revolution in Permanence: Popper on Theory-Change in Science"; in *Karl Popper: Philosophy and Problems*; A. O'Hear (Ed), New York, Cambridge University Press, pp.75-102, 1995