

Alex Wilson

A PYRRHONIST EXAMINATION OF SCIENTIFIC KNOWLEDGE

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Abstract:

In the recent literature in the philosophy of science there is much discussion of scientific knowledge, but rarely an explicit account of such knowledge. Employing the *Pyrrhonist* skeptical modes, I examine the implicit ‘justified true belief’ analysis of scientific knowledge presented by Stathis Psillos, the primitivist account offered by Alexander Bird, and Bas van Fraassen’s voluntarist epistemology. I conclude that all of these positions appear to fail. Psillos’ account relies on a theory of reference that cannot block skeptical challenges to scientific realism, nor can it identify natural kinds in a non-ad hoc manner. Bird’s account also cannot refute skeptical challenges to it, nor can it adequately show how the full truth necessary for knowledge is acquired. Van Fraassen’s voluntarist epistemology attempts to avoid skepticism at the cost of inconsistency. From this representative sample of accounts I argue that there is seemingly no account of scientific knowledge that can *as yet* withstand *Pyrrhonist* skeptical scrutiny.

In the first chapter of my dissertation, I give an overview of *Pyrrhonist* skepticism and the neo-*Pyrrhonism* of Robert Fogelin and Otavio Bueno, respectively. In the second chapter, I exposit Psillos’ semantic realist position, and argue that he gives an implicit justified true belief analysis of scientific knowledge. Moreover, I examine Bird’s primitivist account of knowledge. In chapter three, I discuss van Fraassen’s philosophy of science as stated in constructive empiricism and empiricist structuralism, and his voluntarist epistemology. In chapter four, I argue that all of these different views fail to provide a compelling theory of scientific knowledge. In the fifth chapter, I consider how the traditional *Pyrrhonist* take on the relation of theory to practice, and the positive

epistemic additions of Fogelin and Bueno's neo-Pyrrhonisms. I conclude that the traditional Pyrrhonists were acting inconsistently when they sought out new theories to influence their practice, and that the positive epistemic additions to the skeptical modes of Pyrrhonism fall prey to the modes themselves.

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Chapter 1.

Introduction

1.

Knowledge comes in many forms. For example, there is *know-how*, such as knowing how to ride a bicycle, or bake a cake. In the case of perceptual knowledge, one knows something immediately merely by perceiving something, such as knowing the sky is blue by looking at it, or knowing sugar is sweet by tasting it. Moreover, one can *know-that* something is the case, such as knowing that Toronto is the largest city in Canada, or knowing that Earl Grey is a type of black tea. This last form of knowledge is called *propositional knowledge* because it is expressed in the form of *propositions*, which are statements that are either true or false. The form of knowledge that has most interested philosophers is propositional knowledge. The standard analysis of knowledge is that it consists of three components: *justification*, *truth*, and *belief*. Thus, according to the standard analysis, knowledge is justified true belief.¹

Presumably, we want beliefs that are true of the world so that we can better reach whatever goals we have, or we may even want true beliefs for their own sake. For example, we want to know what the weather is like outside before we venture outside so that we can dress appropriately. Moreover, many people want to know what the chemical composition of the Moon is, though this may have no effect on their daily lives. We

¹ As Edmund Gettier (1963) notes this analysis of knowledge can be traced back to Plato's *Meno* 98 and *Theaetetus* 201. Gettier also mentions Roderick Chisholm and A.J. Ayer as proponents of this analysis. However, since Gettier's attack on the standard analysis of knowledge launched a search for a new analysis that incorporated the original components of knowledge with a fourth component to block Gettier style counterexamples. For an examination of these new analyses of knowledge see Shope (1983).

could make a guess as to what the weather is like, or what the Moon is made out of, but we would have no way of knowing if we were wrong or right without acquiring some reason for believing that we were wrong or right. Furthermore, if we do not have any reason to support our belief, then we could be led astray by someone giving us false information. Thus, we need reasons for our beliefs, or justification, in order to have knowledge.

In the Western world at least, science is regarded as one of humanity's greatest cognitive achievements, but in the philosophy of science there is rarely any explicit discussion of *scientific knowledge*. There is explicit discussion of truth, justification, and belief, but these topics are rarely combined to form an analysis of scientific knowledge. Rather, there is much explicit inquiry into what constitutes scientific progress. The notion of scientific progress is often formulated in epistemic terms as the acquisition of true beliefs about the world as expressed in scientific theories and models. Taken literally, scientific theories and models purport to describe entities and processes, such as genes and chemical bonding, that are unseen by the naked eye. How much progress science supplies us in terms of true beliefs is a matter of debate. On one side of the debate are the *scientific realists* who claim that scientific theories and models give true descriptions of an unobservable world that causes observable phenomena. For example, according to scientific realists, the term "atom" refers to an entity which exists in the same way that observable material objects exist, such as tables and trees, though an atom cannot be seen. On the other side of the debate are the *anti-realists* who do not hold that scientific theories give a true description of an unseen world. Some anti-realists deny that the unobservable entities and processes posited by scientific theories exist.

According to this type of an anti-realist, the term “electron” refers only to various observable phenomena, such as the white streak in a cloud chamber. Other anti-realists claim that scientific theories and models may give true descriptions of unseen entities and processes, but that the truth value of these descriptions is unknown.

As noted earlier, there is much attention given to defining knowledge in general epistemology, but there is little attention given to defining scientific knowledge in the philosophy of science though the term “scientific knowledge” is often used. Rather, in the philosophy of science epistemic topics such as truth, justification, and belief are discussed in relation to scientific progress. For instance, scientific realists argue that their belief in the existence of unseen entities posited by a given theory is justified by the successful experimental outcomes of the theory in question. Whether or not successful experiments can be used as justification for the belief in unseen entities is a matter of controversy in the philosophy of science. This debate about whether or not one is justified in believing that a given scientific claim is true parallels debates about justification in relation to true belief in discussions about knowledge in general epistemology. In this dissertation, I shall attempt to show that the scientific realism debate seems to be a debate about scientific knowledge. I will endeavor to examine a representative sample of three philosophers of science and their different accounts of scientific knowledge.

To date the most comprehensive account of scientific realism comes from Stathis Psillos. He defends scientific realism from *semantic*, *metaphysical*, and *epistemological* attacks on the notion of scientific progress as the accumulation of nearly true beliefs about an unseen world. As I shall attempt to show in Chapter Two, Psillos contends that

we do not often acquire fully true beliefs about the world, but we often acquire beliefs that come successively closer to the *full truth*. The notions of *near truth* and full truth will be discussed in detail in Chapter Two. Psillos explicitly defends this notion of scientific progress as the accumulation of true beliefs but, he emphasizes the importance of justification. Justification serves both to lead us to true beliefs and show that the beliefs accumulated are true, or nearly so. Thus, while Psillos explicitly discusses scientific progress, he does so in terms of general epistemology's standard justified true belief analysis of knowledge. Moreover, he often discusses scientific knowledge going so far as to entitle one of his books *Knowing the Structure of Nature* (2009). However, he does not combine these three epistemic features into a definition of scientific knowledge. Nowhere in his writings does he give an account of scientific knowledge explaining what epistemic features comprise scientific knowledge. However, in Chapter Two, I shall try to show how Psillos can be interpreted as giving either an implicit *true belief* or a *justified true belief analysis* of scientific knowledge.

Alexander Bird (2007), also a scientific realist, notes the lacuna in the philosophy of science concerning scientific knowledge and seeks to fill this gap with his own account of scientific knowledge. He bases his account on Timothy Williamson's (2000) notion of knowledge as a primitive and unanalyzable concept. Williamson's epistemology is motivated by Gettier counter-examples to the justified true belief analysis of knowledge. These counter-examples are known as *Gettier cases* after Edmund Gettier's (1963) counter-examples to the standard analysis of knowledge. Gettier cases show that justification, truth, and belief are not jointly sufficient for an analysis of knowledge. In a Gettier case, the justification used to arrive at the true belief does not actually do so. One

instance of a Gettier case is the following scenario Gettier gave to show that justification, truth and belief were not jointly sufficient for knowledge: Smith, has a justified belief in the false proposition that Jones owns a Ford. On the basis of this proposition, Smith infers, and therefore is justified in believing, that either Jones owns a Ford or Brown is in Barcelona. Brown just so happens to be in Barcelona. Thus, the proposition is true. Though Smith is justified in believing this true proposition, Smith does not know this proposition.² Thus, a person can have a justified, true belief, but still not have knowledge.

The typical response to Gettier's challenge is to find a fourth condition to knowledge in addition to the standard three that will block Gettier cases. To date there is no consensus in epistemology as to which of these responses successfully blocks Gettier cases. Williamson accepts that there is no new definition of knowledge that can block Gettier cases, thus he claims that knowledge should be treated as unanalyzable. Rather than discuss knowledge in terms of justification, truth, and belief (and some Gettier blocking fourth element), Williamson discusses justification, truth, and belief in terms of knowledge. Since knowledge is not composed of these three (or more) elements, there are no Gettier cases to undermine a subject's acquiring knowledge. By following Williamson, Bird gives an account of scientific knowledge that is radically different from the standard analysis of ordinary knowledge. For this reason, and for his explicit discussion of scientific knowledge, I have included Bird's account in the sample of accounts of scientific knowledge to be examined. His account of scientific knowledge shall be described in Chapter Two.

² My summary is borrowed from Paul K. Moser and Arnold vander Nat's (eds) introduction to Gettier cases in their *Human Knowledge: Classic and Contemporary Approaches* (2003), p.305.

Bas van Fraassen is the foremost anti-realist philosopher of science. He holds that the unseen entities and processes posited by scientific theories and models do not exist, or at least not in the way described by scientific theories and models. Rather, he sees himself as working within the broader epistemological tradition of *empiricism*, hence he maintains that knowledge of the world is gathered solely through our senses. In his (1989), he gives an explicit account of knowledge, but does not give an explicit account of scientific knowledge. If his general account of knowledge is meant to apply to scientific knowledge, he does not clearly say so. Van Fraassen's account of knowledge differs radically from traditional accounts of knowledge in that he rejects justification. Rather, his account of knowledge takes prior opinion as the starting point for inquiry. One's prior opinion does not need to be justified to be held. A principal concern for van Fraassen's epistemology is the rationality of opinion change given prior opinion as the starting point of inquiry. In the philosophy of science, there is much controversy surrounding the rationality of theory change during scientific revolutions. In his (2002), van Fraassen applies his general epistemology and theory of rationality to the problem of theory change in the philosophy of science. Chapter Three shall be devoted entirely to giving a sketch of van Fraassen's epistemology, theory of rationality, and account of scientific knowledge.

In Chapter Four, I attempt to show that the accounts of scientific knowledge examined in this dissertation all *seem* to fail. However, I am *not* arguing that an account of scientific knowledge, or a plurality of accounts, is impossible *in principle*. Rather, following Catherine Elgin (2006), I note that the truth condition for the acquisition of knowledge appears to be a problem that plagues all of the accounts of scientific

knowledge examined. It is important to note, that while she criticizes philosophers of science for adopting a position called *veritism*³, I shall challenge Psillos, Bird, and van Fraassen on their commitment to *facticity*. The latter notion only requires that an item of scientific knowledge have at least approximate truth and not full truth. In her critique of scientific knowledge, Elgin does not mention any particular philosophers of science as proponents supporting the position that knowledge is the proper goal of science. I have chosen to examine the accounts of scientific knowledge advanced by Psillos, Bird, and van Fraassen because they are representative of various attempts in epistemology and the philosophy of science to stipulate certain epistemic norms. Psillos follows analytic epistemology's justified true belief analysis of knowledge. Bird, following Williamson, endeavors to break from this analysis. Van Fraassen goes further than Williamson and Bird by eschewing talk of justification and privileging perceptual knowledge over inferential knowledge. However, all of these philosophers' accounts of scientific knowledge feature truth as an essential feature of knowledge. Because all three philosophers regard truth as being a property of *facts*, or how the world actually is, I shall call their view that knowledge requires truth facticity. I shall call the commitment to the view that facticity is necessary for knowledge the *facticity criterion*. *Therefore, in this dissertation, I hope to show from this small but representative sample that accounts of scientific knowledge committed to facticity are unable to meet this commitment.*

Moreover, if I have succeeded in the former task, then I hope this will lead to equipollence⁴ in the reader, not only regarding the accounts examined, but over whether or not a successful account of scientific exists. Since I have not examined all accounts of

³ The definition of veritism is given on page 26.

⁴ The notion of equipollence will be discussed in the section below on page 10.

scientific knowledge, particularly all those committed to facticity, I cannot conclude that there is no acceptable account of scientific knowledge. However, any account of scientific knowledge committed to facticity will seemingly have to overcome the challenges presented against the philosophical positions examined in this dissertation. Thus far, there does not appear to be such an account of scientific knowledge. Beyond this dissertation, I shall examine whether or not alternative approaches to the epistemology of science, such as Elgin's notion of understanding, succeed.⁵

2.

In my attempt to show how the accounts of scientific knowledge examined in this dissertation are undermined by the truth requirement I shall be using a variety of methods that are intended to induce doubt. These methods were collected and systematized by Sextus Empiricus in his *Outlines of Scepticism* [1994]. Sextus was a *Pyrrhonist* skeptic who lived in the second and third centuries CE somewhere in the Roman Empire. In this section, I shall attempt to give a brief sketch of how *Pyrrhonism* fit into the philosophical climate of its time to show why it saw peace of mind as the ultimate goal of inquiry (*PH* 1: 26-30). Then, I shall give a short overview of the methods employed by Pyrrhonists for inducing doubt. While I shall be employing the various methods collected by Sextus, I will endeavor to explain why I do not use these methods for attaining peace of mind, but only for their apparent ability to test truth claims. I will conclude this section by examining two contemporary versions of Pyrrhonism and how my version of Pyrrhonism differs from them.

⁵ Her views on knowledge and scientific knowledge will be discussed in detail in section three below.

Philosophy in Ancient Greece and Rome sought to give a true description of the world so as to guide us toward the good life. What counts as a good life is a subject of controversy. For the Epicureans the good life is a life of pleasure (Gosling 1996, 239), for the Cynics it is a life in accord with nature (Clark 1996, 173), for the Stoics it is to be indifferent to the pleasures and pains of life (Sharples 1996, 852). These various schools of thought each have their own views on what we can know and how we come to know anything. Every philosophical school of thought argues that knowledge of what exists in the world and of human nature is necessary to know how to live the good life. For example, the Epicureans think that it is our nature to desire pleasure and avoid pain, and that we have free will. Thus, the good life is one where we choose to seek pleasure and avoid pain. The Stoics argue that the universe is rational and governed by fate. They claim that the good life is a life of reason and calm acceptance of our immutable fate. Therefore, for all of these schools, knowledge seems to be a key factor in living the good life.

Against the view that knowledge was necessary for the good life arose Pyrrhonism, named after Pyrrho of Elis who refused to assent to any belief (*PH* 1: 7). Unlike their opponents, the Pyrrhonists did not claim to have arrived at any truths about human nature, the nature of the world, or the good life. Moreover, it is important to note that the Pyrrhonist does not claim that reality is inapprehensible, only that there seems to be no doctrine that has clearly shown that it has apprehended reality. Sextus Empiricus, in his [1994], takes pains to distance Pyrrhonism from the Academic skepticism of Plato's Academy (*PH* 1: 226). The *Academic skeptics* claimed that they knew that did not know anything, and that knowledge was impossible to acquire. The Pyrrhonists

doubt that they have knowledge, but do not believe that they do not have knowledge. Perhaps knowledge can be obtained, and perhaps the Pyrrhonists have attained it without knowing that they have. Therefore, the Pyrrhonist does not claim that knowledge cannot be obtained. Perhaps it can, only that they do not know if knowledge has ever been acquired. Both those who claim to have found the truth and those who claim that the truth cannot be attained the Pyrrhonists called *dogmatists* (*PH* 1: 3). The Pyrrhonists claim to be continually searching for the truth by investigating the various views of the dogmatists.

In the course of searching for the truth a life of tranquility seems to be attained. The Pyrrhonists did not claim that tranquility was the proper goal of life, only that if one desired this goal for their life, then the Pyrrhonist style of investigation could lead to this goal (*PH* 1: 4-6). The modes that the Pyrrhonist uses are methods for inducing the suspension of judgment. The reason the Pyrrhonist seeks to induce the suspension of judgment is to reach equanimity, the ultimate goal of Pyrrhonism. This goal shall be discussed below, but, since the Pyrrhonist endeavors to suspend judgment, they are not concerned with the soundness of the modes they use. Hence, the Pyrrhonist does not claim that the mode that has apparently undermined an argument *really* has undermined the argument in question.

Pyrrhonist skepticism is the ability to use these, and perhaps other modes yet to be discovered, to evaluate claims to the truth (*PH* 1: 8). Thus, Pyrrhonism is not a doctrine, but a skill developed to examine truth claims. Since all philosophical doctrines purport to give a true description of reality, the Pyrrhonist, having not yet found a doctrine that can withstand the modes, suspends judgment on all doctrines (*PH* 1: 16-17).

Instead of putting forward their own description of reality to be tested, the skeptic keeps searching, testing whatever doctrines they come across.

The most general mode employed by the Pyrrhonist is *equipollence*, or opposing apparently equally strong conflicting accounts against each other (*PH* 1: 10). For example, if an Epicurean states that health is a necessary condition for happiness, then a Pyrrhonist will note that the Stoics argue that health is not a necessary condition for happiness. The Pyrrhonist will seem to adopt the arguments of the Stoic, but they do not. Likewise, against a Stoic arguing that health is not a necessary condition for happiness the Pyrrhonist may use the Epicurean argument against them. The Pyrrhonist will use whatever argument is necessary to suspend judgment on the argument in question without believing in the soundness of the argument being used.

Pyrrhonists employ many modes, but some of these can be generalized into one larger mode. For instance, the Ten Modes (*PH* 1: 35-163) are instances of how perception of the same object seems to be relative among animals and humans, among the five senses, among various ethnic groups, and so on. These ten modes can be generalized into a *mode of relativity* (*PH* 1: 166). For instance, honey tastes sweet to a healthy person and bitter to a jaundiced person. Since the taste of honey changes under different circumstances, it is unclear what the real taste of honey is.

Other than equipollence and relativity, the other modes I shall be using are the modes of hypothesis, circularity, and infinite regress. The *mode of hypothesis* occurs when a dogmatist states a particular claim about the world without any justification (*PH* 1: 168). Doing so is problematic since another dogmatist can make a contradictory claim without support. Believing in the existence of God merely on faith is an example of this

mode. The *mode of circularity* occurs when the dogmatist uses the doctrine in question to argue for the truth of the same doctrine (*PH* 1: 169). For example, concluding that induction is a reliable method for uncovering truth by pointing to the past success of induction for uncovering truths is circular. The *mode of infinite regress* occurs when the reasons brought up to justify belief in a certain claim themselves need to be justified by other reasons and so on to infinity (*PH* 1: 166). For instance, to believe in the existence of a world independent of our sensations of it requires reasons to believe that our senses perceive a world external to our minds. However, the justification for believing that our senses perceive a world independent of our minds needs justification as well. For instance, if a dogmatist were to rely on induction to justify the mind-independence of the world, then belief in the truth of induction needs to be justified too, and whatever is used to justify induction requires a justification of its own, and so on. As we shall see in Chapter Two, Psillos justifies belief in induction through the mode of hypothesis when he claims that we need a particular intuition as our starting point for accepting the logic that supports the belief in induction. Thus, the regress can be stopped, but seemingly only through the modes of hypothesis and circularity, which appear to be fallacious.

The Pyrrhonist's purpose in suspending judgment through the use of these modes is the attainment of *ataraxia*, or "tranquility" which Sextus describes as "freedom from disturbance or calmness of the soul" (*PH* 1: 10). The Pyrrhonist assumes that those who hold opinions on the true state of the world, particularly on what really is good and bad, are disturbed. Their disturbances arise because they sometimes find themselves in situations they see as bad and they feel badly as a result. Or they disturb themselves by pursuing what they see as good. When they attain what they see as good they are

immoderately happy, but are also in perpetual fear of losing the supposed good they have acquired. However, the Pyrrhonist who has attained ataraxia suspends judgment on what is what is truly good or bad, and hence does not crave or avoid anything intensely.

Rather, the Pyrrhonist who has attained ataraxia is moderately tempered since they are capable of being disturbed only by what is forced upon them. For instance, a Pyrrhonist will still shiver when cold, but will not be as disturbed by the cold as the dogmatist who is bothered both by the appearance of cold and the thought that feeling cold *is* bad. By not taking on the opinion that being cold really is bad, the Pyrrhonist does not take on an extra disturbance. The Pyrrhonist does not actively try to remain calm through all circumstances until they have formed the habit of being calm. Rather, if the modes produce genuine suspension of judgment often enough, then ataraxia suddenly just appears.

While I employ the Pyrrhonist's modes to examine three different accounts of scientific knowledge, my goal is not to bring about ataraxia in the reader or in me. If we learn how to suspend judgment on all matters concerning reality and attain ataraxia as a result, so much the better. Rather, I am interested in employing the modes as a means for testing the soundness of arguments. Many, if not all, of these modes come from the dogmatists themselves. The dogmatists seek to undermine their opponents' doctrines using the same methods applied by Pyrrhonists against all doctrines. However, the dogmatists do not fully employ these same modes to test the truth of their own doctrines. As shall be shown in Chapter Four, all three philosophers of science examined in this dissertation rely on dogmatic assumptions and circular reasoning in the assertion and defense of their accounts of scientific knowledge. If these modes are an acceptable

means to demolish a doctrine incompatible with one's own, then these modes may be used against all doctrines. If none of the positions under examination can withstand the scrutiny of these modes, then we should suspend judgment on them, not to attain ataraxia but because they are epistemically unacceptable. Hence, my interest in Pyrrhonism is purely epistemological. I call this purely epistemic version of Pyrrhonism *Epistemic Pyrrhonism*, as it employs the Pyrrhonist modes only for the purposes of evaluating truth claims. *In this dissertation, I intend to use Epistemic Pyrrhonism to induce a state of equipollence in the reader regarding the accounts of scientific knowledge under examination.*

Epistemic Pyrrhonism differs not only from traditional Pyrrhonism, but from *neo-Pyrrhonism* as well. Neo-Pyrrhonists also abandon the attainment of ataraxia as the goal of their inquiries while employing the modes against all doctrines, but they attempt to show that some form of cognitive success is possible that does not fall victim to the modes. Robert Fogelin (1994) and Otávio Bueno (forthcoming) also are two neo-Pyrrhonists who are only interested in the epistemological aspect of Pyrrhonism and not the attainment of ataraxia. Fogelin focuses on applying the modes to theories of justification in general epistemology, but gives a neo-Pyrrhonist account of knowledge. Bueno applies the modes to the epistemology of science, yet he argues that we can acquire understanding of the world through science. While I join Fogelin and Bueno in dismissing the practical dimension of Pyrrhonism, I do not agree with the search for cognitive success in their neo-Pyrrhonist projects. In the paragraphs below, I shall give a brief description and criticism of Fogelin first, and then I shall turn my attention to Bueno's neo-Pyrrhonist philosophy of science.

Fogelin follows Michael Frede's (1998) interpretation of Pyrrhonism as a tool against claims of theoretical knowledge and not common knowledge claims. What separates the two types of claims are the levels of scrutiny applied to each type of claim. *Levels of scrutiny* are the levels of justification required for a person to be *said* to know something within that framework (95). Fogelin's interpretation seems to be *contextualist*, but he denies this claiming that different levels of justification support different levels of knowledge. For instance, the level of justification needed for an ordinary knowledge claim, such as, "I know it is sunny today", is low. All that is required to justify this claim is that one has looked outside to see if the weather is sunny or not. For the purposes of everyday life this particular justificatory procedure will suffice to support this knowledge claim. This context is the *ordinary context*. However, if one requires apodictic certainty, then one has arrived at the *philosophical context*. In this context, one has to show that one is not dreaming that it is a sunny day, or that one is not a disembodied brain in a vat deceived by evil scientists. Rather, Fogelin argues that he has not made knowledge relative to a justificatory framework as the contextualists do. For Fogelin, one can be said to know within a particular framework, but there seems to be no privileged framework, or plurality of frameworks, that can show there is a fact of the matter as to whether an epistemic claim is ultimately true or false (98). His version of Pyrrhonism allows for a Pyrrhonist to make knowledge claims as long as the level of scrutiny they are working within would allow them to say that they know. If the cost of error goes up, then so do the levels of scrutiny and the Pyrrhonist withdraws their claim to know if the requirements for justification cannot be obtained. For instance, Fogelin argues that, in the ordinary justificatory framework, he knows many important things, such as the

location of the fire escape. However, in a framework that requires certainty he withdraws his claim to know since he may be a brain in a vat.

Fogelin contends that traditional Pyrrhonism operated under levels of scrutiny, where in everyday life the Pyrrhonist claimed to know many things, but withdrew knowledge claims when asked to reflect on them at a high level of scrutiny. In a higher level of scrutiny one may be required to show that they know they are not dreaming, not insane, not in a computer simulation, and so on. To support this claim, Fogelin cites Michael Frede's arguments to the same effect. Frede, in "The Skeptic's Beliefs" (1998), claims the Pyrrhonist is only interested in undermining claims to knowledge about things that are not evident, that are arrived at through the use of reason. However, everyday knowledge claims were acceptable. To support this conclusion Fogelin cites Sextus' comment on skeptical phrases employed when using the modes to affect a suspension of judgment: "We must...remember that we do not employ them universally about all things, but about those which are non-evident and are objects of dogmatic inquiry" (*PH* 1:208 in Fogelin 1994).

Against the Frede/Fogelin interpretation of Pyrrhonism it appears to me that the Pyrrhonist does not recognize differing levels of scrutiny from which to make knowledge claims. Instead, all claims to knowledge are equally suspect. The only claims that are not suspect are claims about appearances, and it is in this sense that the modes are not universally applied. As mentioned earlier in this section, the Pyrrhonist suspends judgment on such mundane matters as the sweetness of honey. The Pyrrhonist makes manifest their constant doubt through the skeptical phrases when their interlocutor explicitly makes a claim about reality. Until then the Pyrrhonist follows the customs and

conventions of the society they find themselves in, and Greek and Roman societies speak in terms of how things are, not in terms of how they appear. For example, Sextus notes (*PH* 1: 135) that the skeptic uses the word “is” to mean “appears”, as when they say that ‘everything is relative’ to mean ‘everything appears relative’. Hence, when the Pyrrhonist seems to be making a claim that the world is a particular way by using the word “is” they are using that word instead of “appears” because that is how the society around them speaks. Thus, the Pyrrhonist speaks loosely when they say, for example, that “honey is sweet”. For the Pyrrhonist this statement is only shorthand for the observation that “this apparent honey appears sweet to me”. Therefore, when the Pyrrhonist says that something is the case they are not making a claim to knowledge at a lower level of scrutiny, instead they are speaking as others do around them in the context of daily life. But, the Pyrrhonists are always suspending judgment on all knowledge claims, no matter the level of scrutiny.⁶

Frede and Fogelin argue that the Pyrrhonist could not live their skepticism if they did suspend judgment on all matters, mundane or philosophical. While I disagree with their interpretation, I shall not discuss my view on the matter here, but in Chapter Five where I shall examine whether or not there can be any kind of positive cognitive aspect that can be realized within Pyrrhonism, new or old. Regardless if the Pyrrhonist could live their skepticism or not, the epistemological challenges put forth by Pyrrhonism are still a serious threat to all philosophical doctrines and thus should be taken seriously in epistemology.

⁶ It is important to note that I will be following the same convention as the traditional Pyrrhonists so that when I have written that something *is* the case (e.g. “Psillos argues that...”) this is just shorthand for saying that something *appears* to be the case (e.g. “Psillos appears to argue that...”).

Turning now to Bueno (forthcoming), he does not explicitly endorse or criticize Fogelin's neo-Pyrrhonism, nor does he reflect on how pervasive and livable Pyrrhonism may or may not be. What does interest Bueno is showing how Pyrrhonism is compatible with many aspects of van Fraassen's *constructive empiricism* and with scientific practice. Moreover, Bueno contends that there is a positive epistemological contribution that can be made within the constraints of Pyrrhonism which he calls *understanding*. Bueno's understanding follows van Fraassen's, and not (as shall be shown in section four of this chapter) Catherine Elgin's characterization of understanding. I shall discuss van Fraassen's notion of understanding later in this section when I examine Bueno's positive addition to Pyrrhonism. Below, I will describe Bueno's argument that Pyrrhonism shares many features with constructive empiricism and with scientific practice.

I shall give a brief sketch of van Fraassen's epistemology to better expisit Bueno's project of making the former compatible with neo-Pyrrhonism. Through his criticism of scientific realism van Fraassen puts forth his own view, constructive empiricism. Constructive empiricism holds that the proper aim of science is to construct empirically adequate theories. A theory is *empirically adequate* if everything the theory says about what is observable is true. The constructive aspect of constructive empiricism is in the practice of building models in science that, van Fraassen contends, need only have true descriptions of the observable phenomena, while the truth value of the description of unobservable phenomena is false since only observable entities exist. What is observable is what can be perceived with the unaided senses. For instance, Newton's model of the motions of the planets was empirically adequate because (with the exception of Mercury and the Moon) it was predictively accurate and thus true.

However, the hidden force of gravity that was posited to account for this predictive accuracy is not real.

The *empiricism* in constructive empiricism refers to van Fraassen's antagonistic stance towards explanation and *analytic metaphysics*, and his admiration for the scientific attitude of keeping an open mind. According to van Fraassen, analytic metaphysics is a sub-division of western philosophy that seeks to uncover the one true nature of reality.⁷ Different metaphysical theories posit different accounts of how reality is constituted. For instance, *materialists* claim that all reality is composed of matter in space and time, while *subjective idealists* hold that reality is made up of nothing more than sensations and the minds that perceive them. Van Fraassen sees himself as carrying on the *nominalist* tradition in philosophy against the *Aristotelian* practice of positing unobservable causes, dispositions, qualities, universals and so forth to account for why the observable phenomena behaved as they do. For example, the Aristotelian will account for causation by positing unseen necessary connections between objects while the nominalist claims the observable phenomena do not show a clear causal connection with any unobservable entities or processes. Van Fraassen mentions Molière's ridiculing of *virtus dormitiva* as an example of the superfluity of metaphysical explanations (1980: 2).

Returning to Bueno, he begins his argument by pointing out that Pyrrhonism is not a skeptical doctrine, like Cartesian skepticism, stating that knowledge is impossible.

Such a position as Cartesian skepticism is a self-refuting claim about the reality of

⁷ As I shall attempt to describe below in Chapter Three, van Fraassen holds that analytic metaphysics seeks to give an explanation for what we observe by endeavoring to give a true description of reality. Van Fraassen rejects the need for such explanations. He contends that we do not need an explanation for why the world is independent of our minds, nor do we need an account of what the world is ultimately composed of. Rather, we accept that the world is external to us because we have no reason to think otherwise, hence no explanation is needed. Moreover, we muse over the ultimate composition of the universe because it gives us understanding. This particular notion of understanding is discussed below on page 20.

knowledge. Rather, the Pyrrhonist is not in the business of developing a doctrine about reality, but in using the modes to test claims about reality. After giving a brief sketch of the Pyrrhonist modes, Bueno shows how the mode of relativity in particular is very similar to *underdetermination* arguments employed against scientific realism. The former purport to show that the same object can create different appearances under different circumstances. Therefore, the real nature of the object is unclear. Bueno uses the different tastes honey can produce in healthy people and those suffering from jaundice as an example of underdetermination among appearances. Underdetermination arguments claim to show that the same phenomena can lead to different accounts of the unobservable causes for the phenomena in question.

An instance of underdetermination in science comes from quantum physics. In the ontology of non-relativistic quantum mechanics the world is ultimately composed of particles, but in the ontology of quantum field theory the world is fundamentally composed of fields. The predictive accuracy of both theories is equal, so a choice between which of the two theories to accept cannot be made on empirical grounds. Both the mode of relativity and the underdetermination argument are used to create suspension of judgment. The mode of relativity produces suspension of judgment about the true nature of observable objects, while underdetermination arguments create suspension of judgment about unobservable objects.

Bueno goes a step further than traditional Pyrrhonism to claim that underdetermination arguments should not just lead to the suspension of judgment, but to understanding. For Bueno, following van Fraassen, understanding is the ability to see a particular interpretation of phenomena as a way in which the world *could be* without

accepting that interpretation as being true. Bueno uses van Fraassen's attitude toward the conflicting interpretations of quantum mechanics given in the paragraph above to illustrate how this notion of understanding operates. As noted above, a choice between which of the two theories to accept cannot be made on empirical grounds. Other considerations, such as *simplicity* and *explanatory power*, could also be taken into account in theory choice, but they cannot tell us which theory is true. These other considerations will be discussed later in this section. The important point here for Bueno is that both theories provide understanding, that is, they show us how the world could be.

Besides the use of underdetermination arguments and the adoption of understanding, Bueno's neo-Pyrrhonism shares many features with constructive empiricism. However, as noted above in the section on van Fraassen, he claims not to employ the underdetermination or *pessimistic meta-induction* arguments against scientific realism. The latter arguments states that because many past empirically successful scientific theories are now seen as false it is likely that current scientific theories will be falsified in the future. Bueno remarks that both constructive empiricism and neo-Pyrrhonism recognize that a belief in unobservable entities is not necessary to investigate the empirical world. But, there is a difference here between the constructive empiricist and the neo-Pyrrhonist. The former believe that observable objects exist, while the latter suspend judgment about the existence of observable objects as well. Moreover, truth is not the goal of inquiry for the constructive empiricist or the neo-Pyrrhonist. The constructive empiricist seeks empirical adequacy while the neo-Pyrrhonist seeks understanding. While both goals do not require truth to be attained, understanding can

explicitly be acquired through works of fiction too. Bueno notes how works of fiction often provide understanding about human behavior.⁸

Since both Neo-Pyrrhonism and constructive empiricism do not pursue truth both are critical of analytic metaphysics. A central concern of metaphysics is *ontology*, or the categorization of what does and not exist. Empiricism accepts as existent only those entities that can be directly experienced and metaphysics posits entities beyond perceptual experience. Hence, empiricism claims to eschew analytic metaphysics, though its commitment to the mind-independent reality of what is perceptible is metaphysical. Neo-Pyrrhonism employs the modes leading to the suspension of judgment about particular metaphysical theories, and the entities they posit, rather than dismissing the field of metaphysics altogether. Moreover, the neo-Pyrrhonist is not committed to the *direct realism* as understood by constructive empiricism. Direct realism states that our perceptions of the world are immediate and not mediated through our minds. For instance, if we see a smoke colored cat, then the coloring of the cat is a part of her that we directly perceive. This position is opposed to *representationalist* theories of perception which state that our perceptions, such as color, are produced by our minds and not in the world. Thus, the smoke coloring of the cat is a mental representation of something perceived in the world and not of a feature of the world that exists independently of our minds.

Since direct realism is another doctrine purporting to be true of reality the neo-Pyrrhonist suspends judgment on it. Although the neo-Pyrrhonist follows the

⁸ Catherine Elgin makes the same claim in support of her version of understanding. She uses Shakespeare's *Othello* as an example of how we can gain understanding about how the cluster of apparently admirable traits exemplified by Othello are capable of being manipulated to self-destruction by a character like Iago (2006: 212).

appearances, unlike the empiricist, they do not assert anything about the real nature of those appearances. The disparities between van Fraassen's empiricist stances (both his constructive empiricism and empiricist structuralism) shall be discussed in detail in Chapter Four. Bueno concludes that if empiricists seek to have fewer commitments than scientific realists, then neo-Pyrrhonism involves the fewest commitments. Thus, the empiricist would be more consistent by becoming a neo-Pyrrhonist. Bueno does not mention this, but neo-Pyrrhonism may also be an acceptable position under van Fraassen's conception of skepticism. According to van Fraassen, skepticism makes a dual claim that even if there is objective truth, it is impossible to attain, and/or rational opinion is impossible. Since neo-Pyrrhonism withholds judgment on both of these claims, and is not self-refuting, then neo-Pyrrhonism is rationally acceptable within van Fraassen's conception of rationality.

With regards to scientific practice, Bueno shows how neo-Pyrrhonism is applied to the methods used by the scientific community to test theories for acceptability. As with philosophical doctrines, scientific theories postulate entities that are not directly observable. Disagreement arises as to which (if any) of the theories is true. Returning to the example of non-relativistic mechanics and quantum field theory, as noted above the two theories have incompatible ontologies, but are equally empirically successful. One may choose to accept one theory over the other as true according to methodological criteria, such as explanatory power or simplicity. However, there is disagreement as to how these criteria are to be understood. For example, the simplest theory may be understood as the one with the fewest equations, or the fewest postulated entities.

Depending on which notion of simplicity is adopted, one may appear simpler or more complex than the other.

Similar problems arise for the notion of explanatory power. There is no agreement as to what explanatory power consists in. On some accounts explanatory power is successful identification of the causal processes engaged in the events under investigation. Other accounts of explanatory power focus on the connections between explanation and unification. Still other accounts see explanation in pragmatic terms, as how certain items of information are employed (on these accounts the information need not be true to be useful). Depending on which of these significantly different accounts is adopted, one theory may be viewed as having more explanatory power than its rival. It is unclear how these methodological disagreements can be settled. Thus, the neo-Pyrrhonist suspends judgment on the theories in question and on the methodological disagreements.

While Bueno starts with a disagreement in the foundation of physics, he shows how the neo-Pyrrhonist modes can lead to the suspension of judgment regarding other areas of science. Specifically, he considers how disagreement about the shape of the atom in experimental physics cannot be resolved, nor can the dispute between nominalists and *Platonists*⁹ on the nature of mathematical objects. On both these matters the neo-Pyrrhonist suspends judgment.

Now Bueno addresses the question of whether there is a neo-Pyrrhonist account of scientific knowledge. He notes that the neo-Pyrrhonist account of knowledge would be one that does not make a claim about the actual nature of knowledge. Neo-Pyrrhonist

⁹ Platonists in the philosophy of mathematics hold that numbers and other mathematical objects are entities that exist mind-independently.

knowledge is not a thing that is attained, but a process of investigation. The neo-Pyrrhonist is not only concerned with the practical applications of this knowledge, such as making accurate predictions and developing technology, but also in expanding their understanding of the world. As mentioned above, understanding consists in the many incompatible answers given in response to a particular question (such as the nature of the ultimate constituents of world, of atoms, of mathematical objects). By suspending judgment on the truth of these answers we open ourselves to the insights offered by each account into the phenomena in question. Furthermore, the neo-Pyrrhonist's inability to decide on a particular issue shows how the suspension of judgment arises, and why, in such a situation, this state is to be expected.¹⁰ Bueno's application of van Fraassen's notion of understanding to neo-Pyrrhonism cannot withstand the scrutiny of the Pyrrhonist modes. I shall leave this criticism of Bueno's neo-Pyrrhonism to Chapter Five where I contend that there is as yet no version of neo-Pyrrhonism that has successfully shown how a positive cognitive goal can escape the corrosive force of the skeptical modes.

3.

Having discussed in section one the three accounts of scientific knowledge that will be examined in this dissertation and Pyrrhonism, I shall now give a sketch of Catherine Elgin's criticisms to any account of scientific knowledge. She provides

¹⁰ However, Bueno's application of van Fraassen's notion of understanding to neo-Pyrrhonism cannot survive the scrutiny of the Pyrrhonist modes. I shall leave this criticism of Bueno's neo-Pyrrhonism to Chapter Five where I attempt to argue that there thus far seems to be no version of neo-Pyrrhonism that successfully shown how a positive cognitive goal can withstand the corrosive force of the skeptical modes.

sophisticated objections to this project in her (1996, 2004, 2006). In her (2006), Elgin notes that knowledge is factive. If a putative knowledge claim turns out not to be true, then it is not a claim of knowledge at all. Since knowledge needs truth, there is much concern in epistemology with obtaining the truth. Alvin Goldman calls this overriding concern for truth in epistemology “veritism”. The features of veritism are that, “we should accept only what we consider true, take pains to insure that the claims we accept are in fact true, and promptly repudiate any previously accepted claims upon learning that they are false” (2004, 113). Thus, a veritist is committed to the view that: (1) truth is necessary for knowledge, and (2) knowledge is the goal of epistemology. However, Elgin argues that such high standards of verity do not reflect how we actually cognize. Moreover, such a narrow focus cannot do justice to our cognitive successes since these achievements are dependent on falsehoods.¹¹

Elgin examines how well science would fare from a veritist viewpoint. She chooses science as her test case because it is “undeniably a major cognitive achievement” and its methodology is “self-reflective” which allows its epistemically valuable factors to be more readily ascertainable (2006, 199). Furthermore, epistemology should be able to give an account for the success of *good science*, or “science that affords epistemic access to its subject matter” (199). She surmises that science, because of its cognitive successes and self-reflexive methodology, would be an illuminating starting point for a revamping of general epistemology.

¹¹ Cf. page 32. While I use facticity, and not veritism, to judge the success of accounts of scientific knowledge, I mention Elgin’s criticisms here since they apply to accounts which allow for approximate truth to be a component of knowledge. Moreover, Psillos, Bird, and van Fraassen seem to strive for full truth in their respective accounts of scientific knowledge, but they do not explicitly claim that full truth is a *necessary* condition for scientific knowledge. Hence, Elgin’s claim that philosophers of science are committed to veritism may be too strong.

Veritism, Elgin contends, cannot provide an account of why good science is good. According to veritism, even good science is epistemically bad. Our best science is beset by anomalies and other salient problems, and where science is successful it is false. Good science relies on models which idealize, abstract and approximate from the phenomena to achieve its successes. A favorite example employed by Elgin is the ideal gas law. This law assumes that molecules are perfectly elastic spheres that do not exhibit mutual attraction and occupy negligible space. Yet, no such molecules are purported to exist, they are imagined only for the purposes of making certain thermodynamic features of observed gases more apparent. According to veritism, any account that represents molecules in this fashion is false and thus epistemically unacceptable. However, if the behavior of, e.g helium, digresses from the ideal gas law to a negligible degree (where the temperature is high and pressure is low), then scientists will use the law to calculate the thermodynamic properties of the gas. Thus, though the ideal gas law is false, it still provides illuminating insights into the behavior of observed gases.

Elgin provides other practices that diverge from the observations that are an integral part of scientific practice, such as: curve smoothing, *ceteris paribus* claims, stylized facts, and *a fortiori* arguments from limiting cases (2004, 116-119). Furthermore, observations are messy, by simply observing one does not immediately know what is of importance and what is not. Hence, scientists must select what features to concentrate on when choosing the scale of their research and when categorizing their findings. For example, what we call “water” often comes from a variety of sources (the sky, the tap, the pond) and the substances are mixed in various ways. The liquid from the tap will have higher concentrations of chlorine and fluoride than the liquid from the sky

which has higher concentrations of sulfate and nitrate, while the liquid from the pond has a higher mineral content than both of the former examples. Yet, all of these liquids are called “water” because H₂O is stipulated as the common denominator for all of these liquids, and the other substances are considered “impurities”. Oftentimes the impurities are slight, and so the water can be used towards some scientific purpose. Sometimes the impurities are more salient and must be filtered out. By having H₂O as a common denominator we can measure the purity of each sample against it.

However, if scientists followed veritism, then science would become too cognitively costly to practice. Each sample of liquid would require a breakdown of its component chemical, biological, and mineral parts, and such a profile would conceal what every sample had in common. Instead, by treating the three samples as a single substance with various impurities emphasizes the characteristic they all share. Only after stipulating a common denominator can we begin to examine what makes them different. For example, we can now investigate why the impurities of the rain water differ from the impurities of the tap water (2006, 205).

Veritism would not be able to countenance the scientific practice of experimentation. Experiments are blatantly artificial. Arguably, a laboratory can be interpreted as a fictional setting where the conclusions of the experiments performed there are, “projections from fiction to fact” (2004, 125). For instance, to determine whether a particular substance is carcinogenic scientists put genetically identical mice into similar environs and expose half of them to the large doses of the substance while leaving the other half unexposed. The shared genetic makeup and the similar environment are intended to screen off the possibility that a genetic or environmental

factor may be the cause of any cancer appearing in the mice. This screening off procedure exemplifies the effects (if any) of the substance in question. An assumption is made that, since mice and humans are both mammals, if humans were similarly exposed they too would (or would not) develop cancer. Another assumption is that exposure to large amounts of the substance in small mammals over a short period of time will mirror the effects of exposure to small amounts of the substance over long periods of time. To succeed at imparting understanding the experiment has to be interpreted in the correct way. The experiment is not meant to be representative of mice in their natural environment; rather it is designed to highlight one particular aspect of the mice and of mammals in general (in this case, the effect of a certain substance on mammals).

The experiment diverges from nature considerably for the express purpose of emphasizing the effect of the substance in question on the mice. For instance, the mice are bred to have a particular genetic structure, they are protected from predators, their exposure to the substance is far more than they would encounter in the wild. Yet, the experiment is informative precisely because it is so artificial. The artificiality of the experiment is meant to emphasize certain features of the mice and downplay others for the express purpose of revealing an aspect of nature that would be overshadowed in their natural environment.

Thought experiments, as Elgin characterizes them, are “imaginative representations designed to reveal what would happen if certain conditions were met” (1996, 210). The conditions imagined are such that the thought experiments often cannot be physically performed, and possibly never will be. Nevertheless, they supply an understanding of the phenomena they purport to be about. For instance, Einstein’s

thought experiment concerning equivalence of gravitational and inertial mass involves what one would see if one could ride a wave of light. Einstein did not claim that his thought experiment was true, clearly if one were shrunk to such a size that one could ride a wave of light, one would not see anything since their retina would be smaller than a photon, and so on. Hence, a suspension of disbelief is necessary for the thought experiment to work. The problems one would have riding a wave of light are irrelevant to the purpose of pointing out the implications of the finitude of the speed of light (212).

While thought experiments are fictional, not just any fiction will enhance understanding. A thought experiment will only work if the assumptions that can be properly ignored are correct, or it will mislead. But, as we have already seen above, this is the case for all experiments. Models too are fictions characterized “as a symbolic construct that exemplifies features it shares with the phenomena it models but diverges from those phenomena in other, unexemplified, respects” (213). She uses a toy model of a protein as an example of how the model differs from an actual protein. The model does not accurately represent the shape, size, or color of the protein, but these representational failures do not make the model defective. The important features the model does exemplify are the structural similarities it shares with the protein. The divergences from the actual protein are important as well since the larger sized, color-coded, and durable model allows the features it exemplifies to become apparent so that they can be identified with less difficulty when the proteins are directly observed.

The reason Elgin gives for why fictions are capable of making cognitive contributions is that the divergences from the phenomena are negligible in recognizable ways. This same negligible divergence accounts for why good theories continue being

good despite anomalies. The theories are good up to a certain threshold, and that threshold is where the divergences become non-negligible. Two examples Elgin gives of negligible divergences are the ideal gas law and Newton's laws of motion. In the first instance, a collection of gas molecules nearly complies with the ideal gas law. In the second instance, a nearby slowly moving object's motion nearly complies with Newton's laws. In both of these examples, the laws provide a context in which to formulate and answer the where, how, why, and with what repercussions divergences occur.

Thus, Elgin seems to have shown that falsity permeates every aspect of science, from its theoretical aspects (models and laws) to its practice (experimentation, both physical and thought). If the whole truth is needed for knowledge, then there can be no scientific knowledge. Verisimilitude too is not to be widely found in science. But, there are more difficulties for an account of scientific knowledge. Even if science was wholly true, knowledge comes in "discrete bits" (2006: 200) and science is holistic. As Quine appears to have shown, sentences are not tested in isolation. They cannot be since the sentences are mutually supporting and hence do not have separately testable consequences.¹² To illustrate her point, Elgin notes that without an evolutionary theory nothing could count as evidence for or against the claim that a behavior instantiates reciprocal altruism. These claims can only be true within the context of a theory, not independently of it since the theory provides the individuation of the items they putatively refer to, such as gene, or phylum. Therefore, since science is false and holistic, knowledge cannot be the goal of science.

¹² As shall be shown in Chapter Two, Psillos argues against holism as part of his argument for the stability of reference through theory change. The stability of reference through theory change supports his view that science acquires truth about the world. However, in Chapter Four, Hasok Chang (2003) and P Kyle Stanford (2003) show how holism is not necessary to argue against the stability of reference through theory change.

One may object that philosophers of science are not committed to veritism since they speak of approximate truth rather than full truth, and they do not explicitly state that full truth is a necessary condition for the scientific knowledge. While this may be true, Elgin's criticism can be applied to accounts of scientific knowledge that countenance approximate truth. As shall be shown in Chapter Four, the concept of truth falls prey to the mode of relativity since there is no agreement as to what constitutes approximate truth. Psillos argues that formal accounts of this notion fail, hence he develops an intuitive account of approximate truth. However, I shall attempt to contend that it is unclear why we should accept his intuitions as to what is and is not approximately true. Thus, Elgin's criticisms may encourage equipollence in those readers for whom a formal account of approximate knowledge is needed for the notion to be acceptable, or for those whose intuitions compel them toward a less tolerant concept of approximate truth, or for those who are committed to veritism.

Furthermore, one may counter that knowledge does not have to be abandoned as the goal of epistemology and science; rather we can just change the notion of knowledge in such a way as to exclude its shortcomings. Elgin disagrees since, even purged of its objectionable elements, the notion will likely keep some connections to its old faults. Hence, a new goal for epistemology, free of past negative associations, is necessary. Elgin argues that understanding should be this new goal. Understanding shall be discussed in the following section. In the section on neo-Pyrrhonism earlier in this chapter, I examine Otávio Bueno's argument that van Fraassen should adopt a neo-Pyrrhonian stance. Bueno accepts van Fraassen's notion of understanding. As shall be

shown in the section below, Elgin's take on understanding differs greatly from van Fraassen's account.

4.

In her (2004), Elgin notes, that if we take veritism as our standard, then most of our best science is epistemically unacceptable because it is false. Furthermore, the falsity of science is responsible for much of its cognitive achievements. As she states, "The problem comes with the laws models, idealizations, and approximations which are acknowledged not to be true, but which are nonetheless critical to, indeed constitutive of, the understanding that science delivers" (2004, 113-114). This state of affairs leads to a dilemma: if we accept veritism, then science should be taken as "either cognitively defective or as non-cognitive" or we can reject veritism and abandon, modify, or relax the truth requirement and "remain cognitivists about, and fans of science" (2004, 114). The character successful cognition provides cannot be knowledge, since this relies on truth. Rather, cognitive success for Elgin resides in understanding. According to Elgin, to understand a theory "is to properly interpret its symbols" (2006, 215). This skill requires one to be able to recognize the difference between factual and fictional sentences, hold tacit presuppositions, and accurately interpret the scope and selectivity of exemplars, and so on. The reasoning involved can be non-propositional. The understanding of a theory's domain "is to be in a position to recognize, reason about, anticipate, explain, and act on what occurs in the domain on the basis of the resources the theory supplies" (215). Understanding is not all or nothing, but has a range. A basic understanding allows us to

recognize unrefined features, to provide sketchy explanations, to reason in broad terms, to develop rudimentary anticipations. As understanding improves, “our recognition, reasoning, representations, and explanations become better focused and more refined” (215).

As an illustration of how understanding works Elgin (2009) uses the example of an 8-year-old’s conception of evolutionary theory. One of the 8-year-old’s core beliefs about evolution may be that humans evolved from apes. A more refined understanding of evolution is that humans and apes evolved from a common ancestor who was not an ape. The child’s claim shows some understanding of evolution. Although her claim is strictly false it is cognitively more valuable than if she claimed that we evolved from spiders, or that we did not evolve at all. According to Elgin, such crude formulations are widespread in science education. Children are given a gross characterization that points in the direction of the more sophisticated understanding of the phenomena, successively refining the crude characterizations until the sophisticated understanding is reached. As Elgin states, “Think of the trajectory from naïve folk physics through Newtonian Mechanics to relativity and quantum mechanics” (2009: 325).

The deeper understanding one has does not necessarily mean that the understanding has more true propositions in its coherent, unified, and integrated web of propositions. As noted in the last section much of good science relies on various forms of falsification and fiction. However, understanding is not indifferent to truth and, while she argues (2009) that understanding is not factive, some part of one’s understanding must still answer to facts. In her (2004), Elgin contends that, even though scientific theories employ falsehoods, these falsehoods help to make sense of the facts, but the

theory itself must be testable against certain epistemically accessible facts to be considered a candidate for understanding. If the theory does not have a testable relation with some constellation of facts, then the theory is discredited. What counts as epistemically accessible is decided by considerations of simplicity, scope, evidence, and so on. Where the sentences in a theory diverge from the observed phenomena, “the way the world is” decides whether or not the theory in which these sentences appear is acceptable or not. For instance, if evidence arose that friction is a major factor in collisions between gas molecules, then unless modifications elsewhere can be made, theories that represent collisions as perfectly elastic spheres will be unacceptable. The requirement that a theory be testable and defeasible shows that the theory preserves some fidelity to truth.

Elgin accepts the conclusions of the pessimistic meta-induction and the underdetermination thesis. The acceptance of these skeptical challenges leads her to adopt *ontological pluralism* where, in different contexts, different ontologies may be adopted depending on one’s goals. For instance, the ontology of Newton’s physics works well when applied to medium-sized objects, such as cars, but not for the very small such as, sub-atomic particles. Yet, even when one theory seems to supersede an incompatible alternative, Elgin recognizes that such a victory may not be permanent. For example, Newton’s notion of absolute space was thought to refute Leibniz’ concept of relative space, but now Einstein seems to vindicate Leibniz’ relative space and to have overthrown Newton’s absolute space. However, Elgin claims that there are cases where we can know what does not exist, as when she mentions phlogiston as an exemplar of misunderstanding since it does not exist. She notes that the non-existence of phlogiston

“decisively discredits the laws of phlogiston theory” (1996, 183). Moreover, Elgin states that “Since phlogistic laws purport to be factual, their falsity is their undoing” (1996, 183). However, as shall attempt to argue below, phlogiston theory was an advance in understanding according to Elgin’s own criteria.

Thus, in at least four works (1996, 2004, 2006, and 2009) Catherine Elgin argues against the primacy of knowledge in epistemology and champions the centrality of understanding instead. As shown above, the central reason for her abandonment of knowledge as the ultimate concern of epistemology is that knowledge requires truth, but truth can be hard to obtain, or once obtained, not useful or even a hindrance to cognition. However, I shall endeavor to show that, even with a relaxed truth requirement, Elgin’s notion of understanding falls prey to objections similar to those she raises against knowledge.

A major weakness in Elgin’s characterization of understanding is that she leaves the notion of misunderstanding extremely vague. She mentions that the theory of phlogiston is an instance of misunderstanding since phlogiston does not exist (1996: 183). However, by her own standards, phlogiston is an instance of understanding, just not full understanding. According to James Conant (1948), phlogiston theory could account for most of the chemical phenomena of the mid-eighteenth century. Specifically, it explained the preparation of metals from ores. However, the theory ultimately could not account for the increase in weight in metals that had been heated since the theory predicted that the metals should lose weight as phlogiston was expelled from the metal during heating. This anomaly and Antoine Lavoisier’s discovery of oxygen led to the demise of phlogiston theory. Yet, phlogiston was an advance over alchemy. As Conant

notes in his (1951), alchemists and metal makers amassed a collection of apparently unrelated facts about the four elements (earth, wind, fire, water).

Among the facts that collected were that the heating of certain materials (*earths*) with charcoal would produce metals, and metals seemed to be the same superficial properties. Other solids were called earths, or what we know as oxides. Still other solids, such as charcoal and sulfur, were called *combustible principles*. The process of heating metals with charcoal could be reversed since often the same heated metal (for instance, tin) produced an earthlike substance. Such artificial earthlike substances (oxides in today's language) could regain the metal if the earthlike substance was treated with charcoal. A pure earth of this sort was called a *calx*. The process of forming it by heating a metal was called *calcination*. These disparate facts were united by the introduction of a principle called "phlogiston".

Phlogiston was intimately related to Aristotle's element "fire" though how was not clear. To clarify the relationship between calxes and metals, a common principle, phlogiston, was posited. This principle seemed evident in the process of making various metals from their calxes and reversing the process. When phlogiston was added to a calx you had a metal, when removed from a metal a calx was formed. Thus, phlogiston was a metalizing principle. From these phenomena phlogiston theorists inferred that, with the exception of gold and a small number of other metals, the calxes were naturally formed and not the metals. Hence, the calxes seem to be simpler materials. It seemed evident that something had to be added to calxes to make them metals. Because the metals were so similar in appearance, the "something" (phlogiston) that was added was clear in all cases.

The seemingly disparate facts collected by the alchemists and metal makers were elegantly united by this common metalizing principle, and it was immediately accepted. Phlogiston provided a pattern within which a mound of apparently unrelated phenomena could be fitted together. Establishing whether substances were plentiful or scarce in phlogiston seemed easy to establish. Substances abundant in phlogiston were easily combustible, and fire was likely an instantiation of phlogiston or at least worked with it. Charcoal was a phlogiston abundant material, and upon heating with a metallic calx surrendered its phlogiston to the calx, thus creating a metal. By itself charcoal burned, the phlogiston manifesting itself as fire or combining with air. Sulfur was readily found in nature, it burned when heated and generated an acid (sulfuric acid by contemporary lights). In the age of phlogiston, it was evident that sulfur was only vitriolic acid highly “phlogisticated”; the burning liberated the phlogiston and produced the acid. Because of these seeming explanatory successes, phlogiston theory was nearly universally accepted at the time of the American Revolution. Furthermore, it was the foundation of the chemistry then taught to college students as part of the natural philosophy curriculum.

However, there was a flaw, known for 150 years before the overthrow of phlogiston theory, discovered by Jean Rey in 1630: there was an increase in the weight of the calx from the tin from which the calx was formed. This flaw was phlogiston theory’s eventual undoing as the phlogiston theorists could not account for how a substance would gain weight when it putatively lost phlogiston. Some phlogiston theorists, notably Joseph Priestly, claimed that phlogiston had negative weight. However, once Antoine-Laurent Lavoisier showed that combustion can only occur with a gas that has weight, namely oxygen, phlogiston theory was overthrown.

Elgin's choice of phlogiston as an example of misunderstanding is curious since there are parallels with it and Newton's gravity; the latter she sees as a case of understanding. If Thomas Kuhn (1970) is correct, then Newton's gravity was the innate attraction-at-a-distance between particles in space and time, while Einstein's gravity is a curvature in space-time. Clearly, these two characterizations of gravity are incompatible, just as the characteristics of phlogiston are incompatible with oxygen. Elgin seems to think that Newton had a good understanding of gravity since his calculations were accurate and are still used today in a limited domain.

However, the chemical and metallurgical techniques developed during the dominance of phlogiston are still in use today as well. For example, the transformation of ores to metal is still carried out by smelting charcoal with the ores. The traditional story told of why phlogiston was replaced with oxygen is that burnt objects were supposed to lose phlogiston, and thus a certain amount of weight, when burned. Yet, Newton's theory of gravity failed to predict the perihelion of Mercury. Despite this predictive failure, Elgin does not claim Newton misunderstood gravity, rather she claims that he understood gravity "very well" (1996:124). Although there were clear advances in chemistry due to phlogiston theory, Elgin does not count these advances as at least being a case of crude understanding in chemistry and metallurgy. Thus, it is not evident why phlogiston is counted by Elgin as a clear case of misunderstanding while Newton's gravity is considered a case of understanding, and at a sophisticated level. The mere claim that phlogiston does not exist is not enough to be a case of misunderstanding as the gravity Newton spoke of does not appear to exist either. Moreover, as shown above phlogiston was a clear advance in understanding according to Elgin's own criteria.

Therefore, a major flaw in Elgin's characterization of understanding is that she does not have a well developed account of what a misunderstanding is. Without such an account the term "understanding" threatens to become meaningless as almost anything could count as understanding. As shown in this section, the one case of misunderstanding she cites seems to be a case of understanding by her own criteria. Furthermore, as shall be shown in future research, since Elgin's notion of understanding ultimately rests on at least some true statements and representations, her epistemology falls prey to some of the same objections she leveled against accounts of scientific knowledge.

For example, she notes that any word or statement in a language is a representation of the world, and that any representation is false since it simplifies and abstracts from the world. Thus, any item of knowledge that a case of understanding rests on cannot also rest on truth according Elgin's own criticisms against veritism. Thus far, the prospects for understanding replacing knowledge as the cognitive goal of epistemology and science do not look promising.

5.

I conclude my dissertation by showing how there is no account of scientific knowledge *to date* that can withstand the scrutiny of the Pyrrhonist modes. Bueno's notion of understanding as a cognitive goal for a neo-Pyrrhonist philosophy of science is examined. He argues that a neo-Pyrrhonist can create new theories to gain more understanding of how the world could be and for the practical pay-off the new theory

may have. I attempt to show that his notion of understanding is undermined by the modes. In my examination of Bueno's notion of understanding I consider the claim made by Frede (1983) that Pyrrhonists who became doctors followed the practices of the Methodical school of medicine. The *Methodists* were agnostic regarding the existence of unobservable entities, including those posited in theories of medicine. However, they would study theories to see if taking the theory *as if* it were true would improve their medical practice. Frede argued that the Pyrrhonists adopted this Methodical practice of using theories to improve practice while not taking the theory to be representative of reality.

I attempt to show that, if the Pyrrhonists did examine theories as a way to improve their practice, then they were seemingly inconsistent. In order to do so consistently, the Pyrrhonist has to show that there is a link between theory and practice. I endeavor to argue that there does not seem to be a way for the Pyrrhonist to make the connection between theory and practice. This argument works *a fortiori* against Bueno's claim that a neo-Pyrrhonist scientist can create new theories, not only to achieve understanding, but for practical results in terms of new technology. The modes appear to undermine the connection of theory to practice, thus it seems that the Pyrrhonists were inconsistent and Bueno's neo-Pyrrhonism seemingly cannot allow for theories to be created for their practical pay-off. Thus, I shall ultimately conclude that there seems to be no way to advance epistemic goals or practical goals in science.

Chapter 2.

Stathis Psillos and Alexander Bird: Two Scientific Realist Accounts of Scientific Knowledge

Until Edmund Gettier's (1963), analytic epistemologists often defined knowledge as justified true belief. Gettier argues that this definition was insufficient since justification and truth could come apart in certain scenarios. However, epistemologists since Gettier have continued to analyze knowledge in terms of justification, truth, and belief, but with an additional fourth condition that is intended to block Gettier counter-examples. Stathis Psillos seems implicitly committed to a definition of scientific knowledge as justified true belief together with a fourth condition that has been developed by John Pollock.¹³ In this chapter, I shall attempt to show that Psillos does seem to hold to the definition of scientific knowledge mentioned above. I have chosen to examine Psillos' account of scientific knowledge because it is an exemplar of a justified true belief account of scientific knowledge. Since I am focusing on the facticity of scientific knowledge, I shall not discuss how Gettier cases may be used against this account of scientific knowledge. Rather, I shall be focusing on Psillos' arguments in favor of science being able to acquire truth about the world.¹⁴

I will also discuss Alexander Bird's account of scientific knowledge. It is an exemplar of an account of knowledge that takes a radical departure from the justified true belief account of knowledge. Motivated by a continuing failure to overcome Gettier

¹³ Cf. pages 100-104.

¹⁴ In Chapter Four, I will contend that, while I may be mistaken that Psillos has an account of scientific knowledge, my arguments against commitment to facticity can be used *mutatis mutandis* against his account of scientific progress as the accumulation of true beliefs.

counter-examples, Timothy Williamson (2000) proposes to treat knowledge as a primitive, unanalyzable concept. Following Williamson's take on knowledge, Bird seeks to develop a detailed account of scientific knowledge. I shall argue that Bird's account is committed to facticity despite his contention that truth is not a necessary condition of knowledge.¹⁵

In this chapter, I will begin by giving a sketch of the "no miracles argument" since it is widely accepted among scientific realist to be the meta-argument from all forms of scientific realism are derived. Then, I shall describe Psillos' *semantic realism* starting with the three intuitions Psillos holds to be foundational of scientific realism. While the focus of my dissertation is the facticity of scientific knowledge, I will discuss these three intuitions of scientific realism since they are the bases for Psillos' facticity arguments in favor of scientific realism.¹⁶ Subsequently, I shall discuss his responses to three challenges for his form of scientific realism: (1) the charge that the "no miracles argument" is viciously circular, (2) the challenge posed by the *underdetermination thesis*, and (3) the challenge posed by the *pessimistic meta-induction*. Moreover, I will examine the objection that the "no miracles argument" is viciously circular because this argument is the primary meta-argument for scientific realism. If the "no miracles argument" fails, then scientific realism fails. Establishing the non-circularity of the "no miracles argument" is necessary to the test the soundness of the argument. A circular argument cannot be tested. In particular, if the "no miracles argument" is found to be viciously circular, then Psillos' and Bird's respective accounts of scientific knowledge have no

¹⁵ In Chapter Four, I will then show how his account seems to fail to meet the facticity criterion for the acquisition of truth as a necessary condition for knowledge.

¹⁶ In Chapter Four, I shall argue that since these intuitions are foundational they are not argued for and thus fall prey to the Pyrrhonist mode of hypothesis (Cf. Chapter One, page 11). Hence, Psillos' scientific realist position fails from the beginning.

basis. However, the rest of the arguments described in this chapter will cover Psillos' and Bird's responses to arguments against the ability of science to uncover truth (e.g. the pessimistic meta-induction) and their respective arguments for how science does acquire truth.

A key feature of both Psillos' defense of scientific realism against the underdetermination thesis and the pessimistic meta-induction is the *causal-descriptive theory of reference*, thus I shall discuss this component of his theory in detail. In the course of his defending scientific realism Psillos presents his theory of justification. While criticizing constructive empiricism, Psillos states his position on the superiority of belief in the truth of a theory over acceptance¹⁷, hence I will more closely examine this element of his view in Chapter Four in the section on van Fraassen. However, since Psillos explicitly claims that the measure of scientific progress is the accumulation of true beliefs, the notion of truth is crucial to his view and, thus I shall devote three sections to this component of his theory. Having described Psillos' positions on justification and truth, I will show the development of his implicit account of scientific knowledge from (1999) to (2009). It is important to note that *if* Psillos equates scientific knowledge with scientific progress, then it appears that the criticisms aimed at his views on truth will still undermine his account of scientific knowledge.

Alexander Bird explicitly contends that we can gain scientific knowledge, and that the accumulation of such knowledge is the mark of scientific progress, "I shall argue for the *epistemic approach*, the simple-minded cumulative knowledge account of progress" (2007: 65, emphasis mine). His arguments criticize the semantic realism of

¹⁷ The notion of acceptance will be discussed in Chapter Three.

Psillos and Illka Niiniluoto, and non-realists, such as van Fraassen and Thomas Kuhn¹⁸. Bird challenges the former because they hold that science can only give us approximately true beliefs. He attacks the latter for denying science can achieve knowledge of the unobservable world and that the proper goal of science is to solve puzzles, respectively. As shall be shown in the section on Bird below, he contends that the semantic realists' goal is too easy to achieve, that the scientific realists and non-realists do not reflect actual scientific practice, and that knowledge should be the goal of science. In the final section of this chapter, I shall describe Alexander Bird's epistemic approach to scientific progress which is based on the epistemology of Timothy Williamson (2000). I will attempt to argue that Bird's account of scientific knowledge is committed to facticity.¹⁹

2.1 The "No Miracles Argument"

During the first half of the twentieth century empiricism dominated the epistemology of science. In particular, *logical empiricism* was the received view in the philosophy of science until the 1960's. One of the main tenets of logical empiricism is that there is a clear linguistic distinction between *theoretical terms* and *observational terms* in a scientific theory. The former terms putatively refer to entities and processes

¹⁸ While Kuhn did not himself as taking a stand between scientific realism and anti-realism, Bird sees him as a kind of *instrumentalist*. Bird argues that Kuhn claimed scientific theories are neither true nor false, rather their value lies in their ability to solve puzzles. Instrumentalism will be discussed in this chapter in the section on Bird.

¹⁹ In Chapter Four, I will show that Bird's reliance on facticity seems to undermine his account of scientific knowledge.

(such as, “electron” and “gene”) that cannot be perceived by the unaided senses, while the latter terms refer to entities and processes (such as “red” and “solid”) that can be perceived by the unaided senses. Logical empiricists, such as Rudolf Carnap, claim that one could eliminate the language describing unobservable entities from a scientific theory and interpret the theoretical language in terms of the observation language. The laws of nature that describe the connections between unobservable entities and processes could be reinterpreted as connections between observable entities and processes. For example, the hidden force of gravity could be reinterpreted as a generalization of the repeated observation of falling objects. Thus, logical empiricists assert any theoretical language is dispensable. However, this claim does not mean that logical empiricists denied the existence of unobservables. The dispensability of theoretical language only showed that it could be reinterpreted into an observation language, not that it was untrue. If one believes that scientific theories uncover a hidden realm of invisible entities and processes, then one is free to do so. Therefore, logical empiricists can be found to include scientific realists and anti-realists among them.

Hilary Putnam (1975) challenges the logical empiricists’ theoretical/observable language dichotomy. He argues that the dichotomy between an observation language and theoretical language is untenable. Observation terms can be used to refer to unobservable entities without a change in meaning, hence putative “observation terms” may refer to unobservable entities (Psillos 1999: 24). Putnam uses the following instance to illustrate his point, “‘Red’, for example, was so used by Newton when he postulated that red light consists of *red corpuscles*” (Putnam 1962: 218).²⁰ What we see are red things, but not

²⁰ This is Putnam’s interpretation of Newton’s explanation for how we see red things. It is an open question as to what Newton said about corpuscles.

the red corpuscles that make what we are seeing the color red. Furthermore, many theoretical terms seem to refer to observable phenomena, as when radioactive material poisons someone. The conclusion here is not that observation statements and theoretical statements cannot be distinguished, only that there is no clear cut dichotomy between such statements. Since such an absolute distinction is impossible to make then there is no way to create languages solely comprised of theoretical or observation statements. The upshot for logical empiricists is that without the theoretical/observation language dichotomy, they cannot show that theoretical language is superfluous because it is translatable into an observation language.

The failure of logical empiricism to create an absolute distinction between observational and theoretical terms seemed to show that reference to theoretical terms were necessary to explain the empirical success of science. If theoretical terms were not superfluous, then it seems they are necessary to account for the empirical success of science. As Putnam puts it, the inability to account for the success of science would make the success of science a “miracle”. Thus, the argument he formulated in support of scientific realism is called the “no miracles argument”. This argument gives a sketch of how the empirical success of science can be explained on semantic and metaphysical grounds:

The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle. That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same terms can refer to the same even when they occur in different theories – these statements are viewed not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relations to its objects.

(Putnam 1975 as quoted in Psillos 1999, 71)

The “no miracles argument” is the inspiration for Psillos’ and Bird’s scientific realism. It is important to note that the “no miracles argument” does not claim that *all* successful scientific theories are true, but only those that are part of a “mature” science. The concept of a mature science shall be discussed in the section below on Psillos.

Psillos (1999) unpacks the “no miracles argument” to show explicitly what kind of semantic, metaphysical, and epistemological assumptions are necessary to reveal what parts of successful scientific theories account for their success. While Psillos claims that these assumptions are the intuitive bases for scientific realism in general, his semantic view is particular to a specific kind of scientific realism called “semantic realism”. This scientific realist view will be discussed in detail below. Bird accepts the conclusion of the argument, but does not give a detailed account of what kind of semantics would be necessary to show that the success of scientific theories is not miraculous. However, while the “no miracles argument” only argues for the truth of scientific theories, both Psillos and Bird regard this argument as supporting the notion of scientific knowledge. The “no miracles argument”, and scientific realism in general, have come under skeptical attack from scientific anti-realists. Both Stathis Psillos (1999, 2009) and Alexander Bird (2007, 2010) seek to defend scientific realism from the challenges posed by scientific anti-realists, such as the pessimistic meta-induction and the underdetermination thesis. In the course of answering these objections to scientific realism, both Psillos and Bird offer a positive account of scientific knowledge, respectively.

2.2 Psillos' Semantic Realism

2.2.1 A Summary of Psillos' Scientific Realism

As described in Chapter Two, Psillos begins his exposition of his scientific realism by stating three theses as the basis for scientific realism: the *semantic thesis*, the *epistemological thesis*, and the *metaphysical thesis*. Following these three theses, he constructs a version of scientific realism that claims to be consistent between its semantic, epistemic, and metaphysical views. These theses and the positions that follow from them are in turn based on the “no miracles argument”. The semantic aspect of scientific realism is covered by the *causal-descriptive theory of reference* which purports to show how it is that terms in scientific theories refer to unperceived entities. The causal-descriptive theory putatively undermines the challenge posed by the *pessimistic meta-induction*. The causal-descriptive theory of reference also reveals how it is that natural kinds can be differentiated from mere posits, and thus follows from the metaphysical thesis as well. Scientific theories can justify their claims about the world because they employ *ampliative-abductive* techniques, such as the *inference to the best explanation*. The inference to the best explanation and (in particular) the claim that theoretical virtues are epistemic, putatively block the challenge posed by the underdetermination thesis. I have attempted to argue that these three theses and the arguments that follow from them seem to implicitly support a justified true belief analysis

of scientific knowledge. Moreover, I have tried to show that Psillos' account of scientific knowledge appears to be committed to facticity. In the sections below, I shall endeavor to show that his version of scientific realism seemingly does not meet the facticity criterion, and hence that his account of scientific knowledge appears to fail.

2.2.2 Psillos' Three Stances of Scientific Realism

Psillos argues that scientific realism is a philosophical package that is the incorporation of three theses: a metaphysical thesis, a semantic thesis, and an epistemic thesis. The metaphysics employed by Psillos is analytic metaphysics, which seeks to show the structure of reality.²¹ The metaphysical thesis is that the world has he calls a "natural kind structure" (1999, xix). A *natural kind structure* is a structure that exists independently of humans, is not created by humans, and is mind-independent. Opposed to this view are nominalist accounts, such as Berkeley's *subjective idealism* which claims that material objects do not exist, rather so called "material objects" (such as trees and buildings) are only *ideas*, or thoughts. These ideas are dependent on a mind to perceive them or they will cease to exist. A different form of idealism maintains that there is a world that is independent of the mind, but that is not directly knowable. Instead, the mind must organize the sensory data it receives from the world to make sense of the world. Thus, humans do not discover the structure of the world, instead they structure the world in various ways according to their interests. For instance, For instance, Norwood

²¹ Cf. Chapter One, page 18.

Hanson (1958) imagines Johannes Kepler and Tycho Brahe watching the sun at dawn. The former sees the Earth moving around the stationary sun, while the latter sees the sun moving around the stationary earth. Both astronomers have the same physiological process of sight occurring within their bodies, but they see the same phenomena in different ways because they are committed to helio-centrism and geo-centrism, respectively. Hence, theoretical presuppositions condition how one interprets the phenomena one perceives. The affect of theoretical presuppositions on observation is called the *theory-ladenness of observation*. Psillos accepts that theories do determine how one sees the world, but not to the extent of philosophers such as Hanson. As shall be shown in the section on underdetermination, Psillos contends that one theory can be shown to be the true, or truer, interpretation of the world. He would say that helio-centrism is truer than geo-centrism. Therefore, rather than imposing a structure upon the world, scientific theories uncover and delineate an already structured and mind-independent world.

The semantic thesis states that scientific theories take the literal descriptions of their intended domain, both observable and unobservable, as truth-conditioned. Thus, the sentences in them are either true or false. The view in the philosophy of science that is committed to the semantic thesis is semantic realism. Psillos claims that the semantic thesis is the “essence” of semantic realism (1999, xx). Thus, the observational statement “snow is white” is true if and only if snow is white, and the theoretical statement “neutrinos are massless” is true if and only if neutrinos do not have mass. The semantic realist, by taking such theoretical statements at face value establishes the truth conditions for an assertion. An anti-realist way of taking theoretical statements is to see them as

shorthand for observable phenomena. For example, as shall be discussed in detail in Chapter Three, van Fraassen holds that the term “electron” refers, not to a particle or a cloud surrounding the nucleus of an atom, but only to the measurement of electrical charge in certain experimental settings. When “electron” refers to the measurement of electrical charge in certain experimental settings then the term is true. Hence, according to this anti-realist view, terms that purport to refer to unobservable entities are false at face value.

The epistemic thesis takes successful scientific theories as those that, as Karl Popper has claimed, have survived repeated testing and are approximately true of the world.²² The notion of approximate truth will be discussed in detail in this chapter. Because the theories are well-confirmed the entities posited by them, or at least entities very similar to those posited, must exist in the world to explain the empirical success of the theories. The point of the epistemic thesis is that science can and does capture theoretical truth as much as it can and does capture observational truth. Psillos characterizes this view as *epistemic optimism* in opposition to the skepticism of scientific anti-realism. *Theoretical truth* is the truth of what scientific theories assert about unobservable entities and processes, and *observational truth* is the truth of what theories assert about observable processes and entities. Scientists arrive at the theoretical beliefs because the methods of inference and experimentation they employ are *reliable*: they are apt to generate approximately true beliefs and theories. The methods of experimentation scientists use to test their theories are *ampliative*, that is, they generalize to a conclusion from repeated observations. For instance, after repeatedly observing countless numbers of black ravens one is justified in concluding that all ravens are black though not all

²² Cf. Ch. 10 of *Conjectures and Refutations*.

ravens (past, present, and future) have been observed. If a non-black raven is observed (say, an albino raven is discovered), then this additional premise undermines the conclusion that “all ravens are black”.

Ampliative arguments are one of two major argument forms, the other being *deductive*. Deductive arguments generate conclusions that are certain because the premises deal with concepts and not observations, thus deductive arguments can be shown to be true using letters in place of terms or statements. The letters could stand for anything or nothing since the structure of the argument will always demonstrate that *if* the premises were true, then the conclusion would always be true too. Take, for instance, *modus ponens*:

If *A*, then *B*
A
Therefore, *B*.

A could stand for something that has been observed, or for something fictional. For example, *A* could stand for H.G. Wells’ morlocks. No morlocks have been observed, but *if* morlocks existed, then they would necessarily have certain features because H.G. Wells stipulated these features: subterranean dwelling, ape-like characteristics, and no melanin in their skin. The definition of the concept “morlock” makes these features necessary. According to scientific realism, scientific practice is concerned with uncovering truth about the world. Truth about the world is gained through observation, not through reasoning about concepts. The generalization that “all ravens are black” was formed after observing a number of ravens, while the generalization “all swans are white” were falsified by the discovery of black swans in Australia. In neither of these cases was a conclusion reached or shown to be false merely through the ruminating on the definitions

of “raven” and “swan”, but through observing these animals and noting their characteristics. Hence, scientific methods are ampliative rather than deductive.

Psillos interprets van Fraassen’s empiricism as being agnostic towards unobservable entities. Psillos stresses that empiricists need not deny that science may arrive at theoretical truth because this may happen accidentally. However, empiricists should deny that we can know that science has captured theoretical truth. Therefore, justification is necessary to show that it is sometimes reasonable to believe that science has captured theoretical truth (or near truth). The truth of the theory is justified by assuming that the best explanation for why the theory in question would be successful is that the unseen world it describes exists as the theory describes it (or nearly so). This form of justification is by inference to the best explanation. Inference to the best explanation is an example of what Charles Sanders Peirce (1931-35) called *abduction*, where a hypothesis is posited to explain an event. As Gilbert Harman (1965) argues, there are many explanations that can be given to a particular set of phenomena. For example, if we see someone pull their hand away from a hot stove, we can assume that the heat of the stove caused them to feel pain and, hence, to remove their hand from the stove. Clearly, to explain these phenomena, we infer the existence of an unobservable pain state in the person being observed. A competing explanation is that the person is an automaton with no consciousness and merely acting in such a way that suggests being in pain. Or, the person is a character in a dream and merely a mental projection of some subconscious insecurity or fear.

According to Harman, we make an inference to the best explanation of the phenomena we observe, whether in daily life or in science. In the case of the person

pulling their hand back from the hot stove, the simplest explanation and most likely explanation is that the person is in pain. We arrive at this conclusion because there is no evidence that the person is an automaton (e.g. they do not rust when dipped in water)²³, or that we are in a dream state (i.e. our experience seems to be consistent). Furthermore, an explanation should uncover the causal processes responsible for the occurrence of a given phenomenon or set of phenomena. Psillos contends that the ampliative-abductive methods employed by scientists justify the belief that successful scientific theories are true (or approximately so). I shall attempt to show later in this chapter that Psillos' commitment to justification, truth, and belief reveals that he holds to an implicit justified true belief analysis of scientific knowledge.²⁴

2.3 The Circularity of the “No Miracles Argument”

In this dissertation, I am examining the facticity commitment of certain accounts of scientific knowledge. However, the main argument for scientific realism would fail immediately if it were shown to be the result of fallacious reasoning. Hence, before turning to challenges against the facticity of the accounts in question, I shall discuss the alleged vicious circularity of the “no miracles argument”.

²³ I am indebted to Antoine Wilson's short story “Everyone Else.” *The Paris Review*, Issue #171, Fall 2004 for this example.

²⁴ In Chapter Three, I shall describe van arguments against the inference to the best explanation. Moreover, in Chapter Four, I will discuss Hasok Chang (2003) and P. Kyle Stanford's (2003) objections to the inference to the best explanation .

Psillos concedes that the no miracles argument is circular, but argues that it is not viciously circular. For an argument to be *viciously circular* he states that the argument has to be *premise-circular*. To explain premise circularity, Psillos cites Richard Braithwaite's (1953): "one claims to offer an argument for the truth of α , but explicitly *presupposes* α in one's premisses. Such an argument has no probative force for anyone who does not already accept that α is true," (1999, 82). However, an argument that is *rule-circular* may not be viciously circular. As Psillos states, "rule-circular arguments are such that the argument itself is an instance of, or involves essentially an application of, the rule of inference vindicated by the conclusion," (1999, 82). The relevant differences between premise-circularity and rule-circularity are that the conclusion of a rule-circular argument is not one of the premises, and the argument is not such that one of the reasons offered for the truth of the conclusion is the conclusion itself. Therefore, the rule-circular argument is not blatantly viciously circular.

Psillos contends that the no miracles argument is a rule-circular argument. He gives the following structure to the argument:

NMA

(A)

(A1) Scientific methodology is theory-laden.

(A2) These theory-laden methods lead to correct predictions and experimental success (instrumental reliability).

How are we to explain this?

(C1) The best explanation (of the instrumental reliability of scientific methodology) is this: the statements of the theory which assert the specific causal connections or mechanisms in virtue of which methods yield successful predictions are approximately true.

(B)

(B1/C1) Theories are approximately true.

(B2) These background scientific theories have themselves been typically arrived at by abductive reasoning.

(C2) Therefore, (it is reasonable to believe that) abductive reasoning is reliable: it tends to generate approximately true theories.

(2011c, 23-24)

The premises of the no miracles argument state that scientific methodology relies on theory to guide experimentation and that scientific methodology produces generally accepted instrumental and predictive success. Next, through a general inference, or *meta-inference*, to the best explanation, the argument concludes that the background theories serving as the premises for this general inference are nearly true. Since these nearly true theories have normally been formed by inferences to the best explanation, this detail, along with the conclusion of the meta-inference to the best explanation, entail that the inference to the best explanation is reliable. From this formulation of the no miracles argument, Psillos concludes the following:

So, the truth of the conclusion of [“no miracles argument”] is (part of) a sufficient condition for accepting that [inference to the best explanation] is reliable. [“no miracles argument”] is clearly *not* premiss-circular. The conclusion of the meta-[inference to the best explanation] (that theories are approximately true) is not among the premisses of the argument. In fact, no assumption about the approximate truth of the theories is made within the premisses, either explicitly or implicitly. Besides, there is no a priori guarantee, as clearly there would have been if this argument were premiss-circular, that the conclusion of [“no miracles argument”] will necessarily be that theories are (approximately) true. The conclusion is true, if at all, on the basis that it is the best explanation of the premisses, but it might not have been the best explanation.

(1999, 83)

Moreover, he contends that this last point is implicitly admitted by the opponents of the no miracles argument because they are careful to create their own arguments for the success of science. Since they argue that the conclusion of the “no miracles argument”

does not have to be the intended scientific realist conclusion, they implicitly concede that the “no miracles argument” is not premise-circular.

One may object that one has to assume the reliability of the rule being employed in a rule-circular argument. However, if this assumption rests on the prior acceptance of the conclusion of the rule-circular argument, then those putting forth a rule-circular argument are ostensibly trapped in a vicious circle. The proponents of a rule-circular argument would have to prove the conclusion before they accepted the rule employed to derive it. Yet, they could not prove the conclusion since they first accepted the reliability of the rule. This objection follows from an internalist theory of justification. *Internalism* holds that a subject must be conscious of how a particular true belief is justified in order to have knowledge. Internalist accounts demand more than mere reliability and truth; they require that one have a justification independent of mere reliability for believing that the conclusion generated by the rule is true. On an internalist account rule-circular arguments seem to be vicious since the justification for the rule would have to be independent of the rule.

In response to this demand for an internalist justification, Psillos denies that any assumptions about the reliability of the rule must be manifest, implicitly or explicitly, when a rule is used. Furthermore, the reliability of the rule does not need to be established before one can use it in a justifiable manner. However, he admits, “[t]his is controversial” (1999, 83). He appeals to an externalist theory of justification to support his view. *Externalism* allows for a subject to know something without knowing the evidence in favor of that item of knowledge. Psillos’ point is that when an instance of a rule is invoked as the connection between a set of true premises and a conclusion, what

counts for the correctness of the conclusion is whether or not the rule is reliable. What matters for reliability in this case is not:

whether or not the contingent assumptions which are required to be in place in order for the rule to be reliable *are* in fact in place. If the rule of inference *is* reliable (this being an objective property of the rule), then, given true premisses, the conclusion will also likely be true (or, better, likely to be true—if the rule is ampliative). (1999, 83)

For example, we can imagine an inference machine that, when fed true premises and asked to form conclusions from them, draws true conclusions most of the time. From the machine's behavior we can assume that it is working off some rules of inference such that it activates a rule and reaches a true conclusion. The machine does not make assume anything about the rules it employs, it just uses them. As long as the machine reliably produces true conclusions, then we do not need to concern ourselves with identifying the rules being used and demonstrating why they are reliable in order to justify our belief that the machine is reliable. Rather, it is only when the machine starts drawing consistently false conclusions that we need to concern ourselves with identifying the rules and showing why they fail.

By appealing to externalist justification no assumptions that need to be made about the reliability of the rule of inference, whether implicit or explicit, matter for the correctness of the conclusion. The conclusion will be correct or not whether or not anyone can provide a justification for why the conclusion is correct. Therefore, the correctness of the conclusion does not depend on a defense of the rule.

However, a defense of the rule is necessary for the defense of the reliability of the rule of inference if one demands justification for why the conclusion using the rule of inference is correct. Psillos again adopts an externalist theory of justification that does

not require reasons for holding a rule to be reliable. According to externalism, all that is necessary for the justification of a particular rule is that the rule is reliable and the belief generated by the rule is true. Thus, Psillos contends that scientific realists who rely on the no miracles argument should be externalists because the no miracles argument is rule-circular, and externalism allows that rules can justify themselves if they reliably generate true beliefs.²⁵

2.4 Undetermination

To defend against the underdetermination thesis, Psillos attacks two premises that are often used to support the conclusion of the argument. The first premise, the *empirical equivalence thesis*, assumes that an alternative theory can always be found that will fit the observable evidence equally well as the theory in question. The second premise, the *entailment thesis*, states that the only epistemic constraint on a theory is the entailment of evidence for the theory. To combat the first premise, Psillos attacks the first premise by challenging the *Duhem-Quine thesis* which states that a theory cannot be tested in isolation since every theory is supported by background assumptions called *auxiliary hypotheses*. An example of an auxiliary hypothesis is Newton's celestial mechanics that

²⁵ In Chapter Four, I shall try to argue that externalism is an instance of the mode of hypothesis. Using Lewis Carroll's "What the tortoise said to Achilles", I will attempt to show that Psillos' defense of abduction falls prey to the modes of hypothesis and infinite regress. Furthermore, I shall note Psillos appears not to have accounted for why our seemingly successful use of logic does not need an explanation, but the success of science does. Therefore, Psillos has not adequately explained why some of the success of some of our practices need an explanation for their success, but not others.

was used to derive the orbit of planets. When the orbit of Uranus could not be predicted accurately, scientists did not discard Newton's theory, but sought to keep the theory by questioning another auxiliary hypothesis: that the solar system had only seven planets. In this case the strategy paid off and led to the discovery of Neptune. Yet, this same strategy failed when the planet "Vulcan" was posited to account for the failure of Newtonian celestial mechanics to accurately predict the orbit of Mercury. The failure to accurately predict Mercury's orbit eventually led to Newton's theory being superseded by Einstein's theory of general relativity. These examples illustrate that it is not always predictable whether a hypothesis in a theory, or an auxiliary hypothesis that supports the theory, fails to be confirmed. Thus, theories and the auxiliary hypotheses that support the theories are tested as a whole (Stanford 2013).

However, Psillos claims that the threat posed to scientific realism by the Duhem-Quine thesis is minimal. He contends that the auxiliary hypotheses that support a theory are often trivial. For example, as shall be discussed below, he contends that the positing of heat as a material substance called "caloric" was a trivial addition to 18th and 19th century theories of heat. Psillos notes that the history of science does not support the empirical equivalence thesis since one theory eventually breaks the tie between two equally empirically well-supported theories. The wave and corpuscular theories of light became empirically distinguishable by Foucault's 1853 experiment, regarding the velocity of light through air and through water. By showing that light moves more slowly through water than through air, Foucault refuted the corpuscular theory of light. The Ptolemaic and Copernican theories were distinguished when the latter was inserted into the framework of Newtonian dynamics. Psillos claims that it is not clear that non-

trivial auxiliary assumptions can always be found. If this is so, then the Duhem-Quine thesis becomes, “at best, a promissory note,” (1999, 165). Furthermore, if resolutions such as those above become available, then the underdetermination of theory by evidence is not a significant problem for the scientific realist.

However, when dealing with highly theoretical matters, such as the structure of space as represented by incompatible geometries, Psillos accepts that theories are underdetermined by evidence. He claims these highly theoretical matters are beyond our powers to adjudicate since these controversies take place at our epistemic limits. For instance, there are many instances concerning the physical structure of space. Psillos asks us to suppose that two-dimensional beings live on the surface of a hemi-sphere and they cannot leave it. They attempt to reveal the physical geometry of their world. They employ rigid rods to measure distances on the surface. They triangulate their world, and discover that the sum of the angles of a large triangle is more is larger than that of two right angles. Quickly, they conclude that they live on the surface of a semisphere. However, a mathematician of this world asserts that they are collectively in error. He hypothesizes that their world is a plane, not the surface of a semisphere. Furthermore, he posits a universal force (i.e. a force which acts upon everything in this world in the same way). Specifically, this force causes all moving rods to contract as they move away from the center and towards the periphery. Therefore, says the mathematician, as the measuring rods contract while in motion they appear to fit smaller intervals away from the center. The mathematician concludes that the inhabitants of this world are mistaken since they have not taken into account the contraction of the moving rods. All the

observable phenomena fit both theories, ostensibly leaving the inhabitants with no rational choice between these two theories.

This fictional account can be extended to more realistic cases. Psillos cites Reichenbach's (1958) example:

we may choose a model of Euclidean geometry as the physical geometry of the universe. Then we can create all and only the empirical consequences of the general theory of relativity provided that we postulate universal forces which make moving bodies (e.g. moving rods) contract accordingly. So, roughly, the theories T_1 = (rigid rods and non-Euclidean geometry) and T_2 = (contracting rods and Euclidean geometry) are observationally indistinguishable (cf. also Carnap 1966: 157). Hence, even though the strong thesis – for any theory there are interesting empirically indistinguishable alternatives – is implausible, a weaker thesis is that there are some interesting cases of empirical equivalence is correct.

(1999, 167)

Thus, the scientific realist can concede the existence of local evidential equivalence that is at our epistemic limits (1999, 168). The scientific realist would only need to worry if empirically equivalent theories were a global phenomenon. But, as shown above with the examples in the paragraph above, the historical evidence shows that empirically equivalent theories have been distinguished by subsequent empirical evidence.

Regarding the entailment thesis, Psillos contends that, while two theories may fit the same empirical evidence, they are not supported to the same degree by the evidence. In scientific practice not every positive instance of a hypothesis is taken to be evidence in favor of the truth of the hypothesis. For instance, control groups are used in experiments to rule out spurious causes. Psillos uses the example of a patient praying for a quick recovery from a cold being cured of the cold in three days to illustrate a positive instance of the false hypothesis that praying cures colds. However, the evidence does not support the hypothesis since the evidence would fit a hypothesis, and the evidence can be

accounted for by alternative hypotheses (such as, that the patient's immune system fought off the cold after three days) (1999,170). Moreover, hypotheses can be supported by evidence that does not logically follow from them. Evolution does not follow from geology's findings that the age of the Earth is 4.5 billion years old, but this evidence support the timeline needed by the theory of evolution for life to evolve. Thus, since not all evidence supports a hypothesis, the entailment thesis fails.

Furthermore, while two theories may enjoy the same evidential support, they do not thereby enjoy the same epistemic support. As shown above, positive instances of a hypothesis can be used as evidence in favor of a false hypothesis. The hypothesis that the common cold was cured through prayer and the hypothesis that the cold was cured by a patient's immune system share the same evidential support. Clearly, a measure of a theory's epistemic support is required to test its truth value. Epistemic support can be acquired through cognitive virtues such as simplicity, completeness, predictive power, novel predictions, and so on. These cognitive virtues of theories are *theoretical virtues*. Theoretical virtues are taken into account when ascertaining the explanatory power of a theory, and, Psillos maintains, explanatory power is the theoretical virtue that carries the most epistemic weight. For example, Darwin's theory of evolution better explained the fossil record, the extinction of species, and led to novel predictions in the fossil record (missing links). Following Ernan McMullin (1987), Psillos argues that these virtues are indicative of truth. McMullin claims that theoretical values lead to the best explanation of the phenomena under investigation and that the best explanation is true. However, the scientific realist has to show which theoretical virtues are related to the likelihood of a theory's truth.

Psillos gives the following arguments as a sketch of how theoretical virtues relate to the truth of a theory. He maintains the virtues which comprise the explanatory power of a theory are, indirectly, evidential. They direct the scientists' judgments about the theoretical plausibility of rival theories. Among the various theories which could, or do, become available at any given time, scientists pick those which have the most explanatory power, compared to the other background theories they accept. These background theories categorize the new theories by their theoretical plausibility. Moreover, these background theories were accepted because they have evidential support and show similar theoretical virtues. Therefore, their evidential support and theoretical plausibility are preserved, and displayed in, the new theories which they warrant. As Psillos states, "The virtues which constitute explanatory power become evidential precisely because they are present in theories which enjoy theoretical plausibility and evidential support" (1999, 172).

For instance, Darwin followed the geological theory of *uniformitarianism* which states that the Earth changes through gradual processes, such as erosion, that are currently observed. Natural Theology relied on the geological theory of *catastrophism* which stated the Earth changed through catastrophic processes as described in the Bible, such as the Deluge. The Bible's version of historical events could not be confirmed independently through observation. The lack of independent verification makes *catastrophism* ad hoc. Following *uniformitarianism*, Darwinism could explain how natural processes, such as flooding of the intensity experienced today, could cause tenrecs from mainland Africa to appear on Madagascar. Moreover, Darwinism can account for why the tenrecs on Madagascar have a primordial form compared to their cousins on the

mainland. When the Mozambique Channel between the mainland Africa and Madagascar expanded Madagascar became isolated from the more sophisticated mammals that evolved on the mainland. Thus, the tenrecs of Madagascar did not have to compete with these more sophisticated predators, and they kept their primordial form. Therefore, the lack of ad hoc hypotheses and explanatory power enjoyed by uniformitarianism in turn increases the explanatory power of Darwin's theory.

If one accepts that the virtues that comprise explanatory power are evidential because these virtues appear in other theories that are theoretically plausible and supported evidentially, then the scientific realist only has to show precisely how these virtues affect the degree of confirmation to the theories. The past record of mature theories can be employed to assign prior probabilities to current theories. The past record of scientific theories can be considered the "background knowledge" by which the plausibility of developing theories can be evaluated (Psillos 1999, 172). In particular,

the suggestion is that, given two theories T and T' , which have the same observational consequences but are differentiated in respect of some theoretical virtues, one should regard T more plausible than T' if, given the past record, theories which exhibit the virtues of T are more likely to be true than are theories like T' . (1999, 172)

If theories which never relied on ad hoc adjustments have often been better supported by the evidence than theories with ad hoc additions, then this consideration should be employed when judging the prior probability of other theories in order to give theories that do not have ad hoc features a higher rank. Clearly, discerning which theoretical virtues have been related to well-confirmed theories can be the result only of significant empirical and historical research. As shown above, Natural Theology relied on ad hoc adjustments which led to its being overthrown by Darwinism. Moreover, phlogiston

theory postulated that phlogiston is released upon combustion and saturates the air. However, when some materials gained weight after being burned phlogiston theorists claimed phlogiston had “negative weight”. Phlogiston’s negative weight could not be independently verified and phlogiston theory was replaced by oxygen theory.

The next difficulty for the scientific realist comes from Larry Laudan’s (1996) *non-uniqueness thesis* which states that a theory may arise which is as well supported as another theory in terms of evidence and theoretical virtues (1996, 175). If theoretical considerations are supposed to break the tie between two empirically equivalent theories that are incompatible but seem to be equally true, then in this case the tie cannot be broken. Psillos thinks that this thesis is only a mere logical possibility. Historical examples are needed for this challenge to be a serious threat to rational theory choice. Laudan holds that he has supplied historical examples of the non-uniqueness thesis, including discarded scientific theories such as the crystalline spheres and the caloric theory of heat.

Psillos responds the non-uniqueness thesis by arguing that examples such as the crystalline spheres were not predictively successful, but simply fit observable facts. However, if the best current theory is replaced by another theory with better evidential support, such as when phlogiston was replaced by oxygen theory, which does not necessarily mean that the abandoned theory was not approximately true. Rather, all this challenge shows is that we cannot acquire the whole truth all at once and that we should be more cautious and refined in our judgments from evidence to approximate truth. By exercising caution and refining our theories we only commit ourselves to those theoretical constituents that benefit from evidential support and which led to the success

of the superseded theory (1996, 175). Thus, absolute judgments of approximate truth of current best theories are still rational as long those judgments are centered on the constituents of those theories that contain evidential support and as far as those constituents are actually retained in subsequent theories. To identify which theoretical constituents contribute to a theory's success and which do not, Psillos relies on a particular theory of reference. This theory of reference will be discussed in detail in the sections on reference in this chapter.

While Laudan thinks that the challenge of underdetermination can be overcome in the manner Psillos' describes, he does not agree with Psillos' epistemic characterization of this solution. Laudan doubts that science can access truth about the world, and his argument against the underdetermination thesis endeavors to save the methodology of science from skeptical attack, not the ability of science to access truth. He is committed to *instrumentalism*, or the view that the goal of science is to predict and control phenomena, not to generate knowledge or true beliefs about the phenomena in question. According to instrumentalism, knowledge and/or true beliefs about nature are not necessary for science to be successful, as long as success is taken to be accurate predictions and control of phenomena. For example, Newton claimed that he would not form any hypotheses as to what the nature of gravity was, but, while he could not explain how it worked, he was able to use his theory to accurately predict many phenomena, such as the orbits of planets. The nature of gravity does not need to be known for this theory to generate accurate predictions. The equations that make up Newton's theory of gravity are neither true nor false, just as a hammer that effectively forces nails through wood is

neither true nor false, but just a tool. In the section on theories of truth in this chapter, I shall show why Laudan does not think that truth is accessible to human beings.

Since Psillos does not share Laudan's instrumentalist intuitions he needs to find a way to epistemically justify the methodology of science. Psillos employs *reliabilism* as his theory of epistemic justification for scientific methodology. Reliabilism is an externalist theory of justification which holds that a belief counts as knowledge if the belief is true and was formed through a *reliable process*. A reliable process is one that consistently leads us to truth. For example, seeing is considered a reliable process since our sight does not often mislead us. Most of us use sight to navigate our way through the world, and most of the time we see obstacles in our path. Moreover, logic helps us to reliably form beliefs. For instance, ignoring the law of non-contradiction could make life very difficult if you believed that you were both underwater and not underwater *at the same time*. In this case you would not know whether to swim or walk, or whether to breathe or hold your breath. Psillos thinks that the methods of explanation and justification in science should not be decided *a priori*, but must be continuous with those of the natural sciences. This methodological view is called *methodological naturalism*.

Relying on the methods of science to justify the ability of scientific methods to uncover truth seems viciously circular. Against this charge of vicious circularity, Psillos contends that, though the reasoning and methodology are based on and evaluated empirically, naturalism is not trapped in a vicious circle. Naturalism can escape the charge of vicious circularity in the same externalist manner as the inference to the best explanation.²⁶

²⁶ In Chapter Four, I shall endeavor to show that Psillos should not simply dismiss the non-uniqueness thesis (where a theory is as well supported as a rival theory by the evidence and theoretical virtues). Not

2.5 The Pessimistic Meta-Induction and the Causal-Descriptive Theory of Reference

2.5.1 The Pessimistic Meta-Induction

The pessimistic meta-induction holds that the explanatory part of theories (where explanatory hypotheses posit unobservable entities and processes to account for the causes of scientific phenomena) are often abandoned as new theories replace old ones. Hence, current scientific explanations could also be false given the bad track record of explanation in science. Psillos claims scientific realism can survive the challenge posed by the pessimistic meta-induction. While past successful scientific theories seem to be false and thus abandoned, Psillos claims some of their theoretical constituents which were empirically supported and played a part in their success were retained in the successor theories. Thus, he concludes there is a significant continuity of theoretical constituents in scientific theory change. Psillos gives an in-depth analysis of the transition from “luminiferous ether” to “electro-magnetic field”. He claims that, since significant theoretical continuity through theory change often occurs, this stability should

only may a new theory arise that is equally supported by theoretical virtues and empirical evidence, but theories thought to be refuted are sometimes revived. Therefore, Psillos cannot rely on further evidence or theoretical virtues to eventually break the tie between two otherwise equivalent theories. Furthermore, in Chapter Three, I shall discuss van Fraassen’s argument that theoretical virtues are pragmatic and not epistemic. In Chapter Four, I will attempt to argue that van Fraassen’s arguments against the epistemic value of theoretical virtues shows Psillos has not conclusively shown that theoretical virtues are epistemic. Moreover, I will describe Chang’s arguments against Psillos’ treatment of “caloric” to show that Psillos’ identification of superfluous posits fails, and to demonstrate that explanatory power is not an indicator of a theory’s truth.

ground the epistemic optimism of the realists. However, Psillos is a cautious optimist. As he notes, “it should be acknowledge that the whole truth, and nothing but the truth, cannot be had in science; but this does not mean that scientific theories have not latched upon important truths, or near truths, about the unobservable structure of the world,” (1999: xxiv). These truths suggest that there is a secure system, “of theoretical principles and explanatory hypotheses [that] has survived revolutionary changes to become part and parcel of our evolving scientific image of the world,” (1999, xxiv). The security for this system rests on Psillos’ causal-descriptive theory of reference of theoretical terms.

Before discussing the causal-descriptive theory of reference, I will endeavor to give brief sketches of both the causal and the descriptive theories of references in the sections below. Through these sketches I shall attempt to show what Psillos uses from both theories in the development of his own theory of reference.²⁷

2.5.2 The Descriptive Theory of Reference

The *descriptive theory of reference* considers the referent of a term or expression to be determined by a particular description. For example, the term “human” has been determined by the descriptions “rational animal” and “featherless biped”. The descriptive

²⁷ By describing the strengths and weaknesses of the descriptive and causal theories, I will attempt to argue, in Chapter Four, that his own theory of reference does not escape the criticisms he launches against these theories.

theory of reference suffers from significant weaknesses. For instance, it does not provide necessary and sufficient conditions for fixing the reference of a term to a particular object or person. A proper name can refer to a person though the descriptions attached to that name are false, or if the users of the name cannot collect a cluster of descriptions that are true only of that individual. Moreover, an individual may fit a cluster of descriptions attached to that name, but that name may still fail to refer to that individual. An example of the latter problem is Kripke's (1980) Schmidt-Gödel case. The Schmidt-Gödel case assumes that the most people know about Kurt Gödel is that he proved the incompleteness theorem that bears his name. However, in this case, he did not actually prove the incompleteness theorem, rather he stole the theorem from his friend Schmidt. According to the descriptivist theory of reference, since this description of Gödel fits Schmidt, Schmidt is the reference of "Gödel". The main difficulty with the descriptive theory is that it attaches too rich of a description to a term or proper name. The individual referred to does not always meet all, or even most, of the descriptions attached to the name.

2.5.3 The Causal Theory of Reference

The *causal theory of reference* deems the referent of a term or expression to be the causal chain that links the entity with the term that was introduced to name the entity. The initial description of the entity may differ wildly from later descriptions but the term

that was given to that entity still refers to that entity. While Psillos discusses many objections to the causal theory of reference, I shall only focus on those that can also be applied to his causal-descriptive account.

Psillos notes that the causal theory of reference, through ostension, permits us “in principle” to give a name to something before we are able to say what this object is and what features of the object make it what it is (1999, 284). When the object has been identified and the features essential to it have been discovered, then it is placed into a category called a *kind*. Two broad categories of kinds are artificial kinds and natural kinds. An *artificial kind* is human made, such as an airplane or silly putty. A *natural kind* is an entity that is mind-independent, but not created by humans, and that has certain core properties that pick it out uniquely. Psillos claims that scientific theories identify natural kinds. An example of a natural kind is water, or H₂O. Water existed before humans, is naturally occurring, and its atomic structure uniquely picks it out from whatever impurities (e.g. minerals, toxic chemicals, sand, etc.) may be mixed in with it.

A proponent of the causal theory of reference, Hilary Putnam (1983) uses the examples of “water” and “tiger” to show how the causal theory of reference introduces natural kind terms that appear in folk and scientific theories. For instance, when introducing a natural-kind term, one picks out an object by ostension, connects a name to it, then claims that this name applies uniquely to those objects which have the same nature as that present when the term was introduced. As Putnam states, “a term refers (to the object named) if it stands in the right relation (causal continuity in the case of proper names; sameness of ‘nature’ in the case of kind terms) to these existentially given things” (1983:73 in Psillos 1999: 283). After the introductory event has taken place, the term is

spread through a linguistic community. If the users are linked to the introductory event in some causal chain of term diffusion, then the term is employed by other users and this use consequently preserves reference.

A variation of the same procedure applies for the fixing of physical magnitude terms. When a new observable phenomena appears, a physical magnitude, or entity, which is the putative cause is posited. This physical magnitude is then dubbed with a term, and this term is associated with this phenomena. This is the terms introductory event for referring to this magnitude. The term will then be surrounded by a description, a causal story, of the nature of the posited magnitude and for the characteristics through which the magnitude causes the observable effects for which it was first recognized. This first description will likely be incomplete or mistaken. However, according to the causal theory of reference, a term has been introduced existentially, that is, a physical magnitude that is causally responsible for particular effects to which the term in question refers.

As indicated above, the initial description of the putative physical magnitude's nature is often mistaken. The description changes through more sophisticated and comprehensive interaction. While the description of the magnitude's nature may change drastically in the course of our causal interactions with it, that there is a magnitude which is the cause of certain effects remains invariant. Psillos uses the example of "electricity" to show how the reference of a term is fixed existentially under the causal theory of reference. If all the descriptions attached to this term do not establish what this term refers to, then all the various theories of electricity refer to, and quarrel over, the same existentially given physical magnitude "electricity" (or the causal agent of prominent electrical effects).

Psillos contends that the causal theory of reference allows us to say that past scientists were investigating the same phenomena, though they had partial or mistaken beliefs about the nature of the phenomena in question. The investigations of past scientists continued with the work of subsequent scientists because they both seek to identify the nature of the same agent that is the cause responsible for the same effects. The successor theories have better descriptions of the same causal agent since they have had more sophisticated and comprehensive interaction with it. Psillos claims that the causal theory of reference allows us to compare theories, and to judge that the subsequent theories are closer to the full truth than their predecessors (1999, 284).

However, since drastic mistakes by past scientists, and drastic corrections by subsequent scientists, are permitted of the same putative entity “the causal theory reduces referential stability to the bare assertion that a causally efficacious agent operates behind a set of phenomena, continuity and sameness of reference become easily satisfiable” (1999, 290). If the unobservable cause behind a range of phenomena is only given existentially, and if no description of its properties is used to fundamentally fix the reference of the terms that claim to refer to it, then the term will *never* fail to refer to whatever it is that causes the phenomena in question.

For example, causal theorists claim that “water” refers to the substance with the chemical structure H_2O . Psillos notes that admitting that this chemical composition is the referent for “water” concedes that the manifest characteristics of water (such as translucence and wetness) are not enough to single out the extension of the term “water”. Yet, a causal theorist can respond that the internal structure of the entity in question fixes its reference, “*whatever that is*” (1999, 286). We may be completely mistaken about its

internal structure, but the term “water” would still refer to whatever substance displays the manifest characteristics of the samples employed when the term “water” was introduced. If it turned out that H₂O was the *not* the chemical composition of “water”, then the causal theorist has two options: either concede that the term “water” does not refer to water, or claim that it still does.

According to Psillos, the causal theory has to put the internal structure of the posited kind before the manifest properties of some samples to be able to argue that the resemblance between these samples is strong enough to justify positing a natural kind—in this case, water—to which “water” refers. However, when faced with the wrong internal structure of the posited kind, the causal theory of reference must put the manifest properties ahead of the internal structure to argue that those samples which have these properties in common still sufficiently resemble each other to be classified together as a natural kind with some internal structure, although they do not have the internal structure attributed to them previously by the scientific community. For Psillos, such a reversal of priorities seems to be ad hoc. He contends that if the internal structure (as described by our best theories) is employed to justify positing and identifying the referent of a natural-kind term, then it should be placed ahead of the manifest properties when it comes to the misidentification of the referent of a natural-kind term. Otherwise those using the causal theory of reference would have to show that manifest properties are sufficient to determine natural kinds. Psillos claims that such an approach is problematic.

While ostension is employed when introducing a natural-kind, prior to having a description of the internal structure that makes it into a kind, Psillos argues that ostension is not enough to unambiguously fix the reference of a kind-word. An *ostensive definition*

uses an example which points to the referent. Often ostension will establish connections between a term and more than one kind, or at least there is no assurance that it will not. Moreover, ostension provides us with a sample of the kind, but not with its extension in its entirety. The term is not meant to refer to only the sample present, but to everything that belongs to its extension. Ostension cannot possibly tie together the sample present during the introductory event to all of the other items that fall under the extension of that term since a kind have the same manifest properties as another kind, and some samples of the same kind have different manifest properties. Thus, reference-fixing must involve more than mere ostension. The extra component necessary to fix the reference of a term is a description of the internal structure common to all of the ostensively given samples. Furthermore, causally relevant similarities and differences in the behavior of items that are classified together are normally the result of similarities and differences of internal structure. As Psillos states, “Ice is a kind of water not because of its manifest properties but because of its internal structure. And a liquid which has the appearance of water might well kill you if you drink it, unless it is H₂O” (1999, 285).

Psillos asserts that an occurrence such as the term “water” not referring to H₂O after all is not a science fiction story, and notes that just such an occurrence took place with the term “phlogiston”. Phlogiston is an historical example of an entity whose internal structure was severely mistaken. It was thought to be given off in combustion. When no measurement of phlogiston could be made, this purported entity was given up. Phlogiston cannot be shown to exist, and is not considered as being causally involved in combustion, rather oxygen is. If we follow the causal theory, then one can claim that “phlogiston” actually refers to oxygen, since the latter is now seen as causally involved in

combustion. Other than the claim that both are involved in combustion, the two entities do not share many properties in common.

For instance, James Conant (1948) notes the following properties of phlogiston: when combined with metallic ore it produces metal, it combines with the air, when it saturates the air its combustion was no longer possible and the air no longer supports life since respiration removes phlogiston from the body (1948, 70). Lavoisier was able to show through his “doctrine of gases” that combustion involved chemical combination with some part of the air, and that part of the air was called “oxygen”. Moreover, oxygen clearly has different properties than phlogiston, such as when the former saturates the air combustion is facilitated, not impossible.

2.5.4 The Causal-Descriptive Theory of Reference

To safeguard against the skeptical challenge of the pessimistic meta-induction, Psillos relies on a hybrid theory of reference that is a cross between the descriptive theory of reference and the causal theory of reference. The theory of reference Psillos formed from the descriptive and causal theories is the *causal-descriptive theory of reference*. The causal-descriptive theory of reference takes the referent of a term or expression to be fixed by both the causal chain between the term and the entity named by that term, and by certain core descriptions of the entity. He argues that his theory of reference illustrates how scientific theories can identify natural kinds.

The identification of a natural kind is a referential success. However, if no entity is found that fits the core description attributed to it, then this is a referential failure. An example of such a failure is the term “caloric” which was no longer needed to explain certain phenomena, such as the production of heat by friction. The identification of natural kinds is crucial to Psillos’ realism since he assumes scientific realism must start with the assumption that the world is independent of the mind. Using his causal-descriptive theory of reference, Psillos holds that he can show the danger posed to scientific realism by the pessimistic meta-induction is far less than is supposed by the scientific anti-realists. To illustrate his point, he cites the change from the “luminiferous ether” to the “electromagnetic field” as a continuity of reference through theory change.

2.5.5 The Luminiferous Ether and The Electro-magnetic Field

As an example of past successful scientific theories, Psillos examines 19th century dynamical theories of light, which posited the luminiferous ether as the medium through which light travels, and Maxwell’s postulation of the electromagnetic field. The former theories postulated an ethereal medium as the carrier of light-waves that had some basic properties that it must possess if it were to be considered a natural kind. The luminiferous ether was posited to serve a dynamical role with two significant and interconnected clusters of properties. The first cluster of properties is kinematical since experiments show that light propagates with a finite velocity, and it should propagate

through a medium rather than through action-at-a-distance. The second cluster of properties is dynamical since the luminiferous ether stored potential and kinematic energy during the propagation of light. The term “ether” was used to denote the entity which, if it existed, should possess the above mentioned natural kind properties. These natural kind properties are the core causal description of the “ether”. The *core causal description* is the set of descriptions that depict the natural kind properties of the entity or magnitude putatively referred to by a particular term that perform the entity or magnitude’s causal role for a given set of phenomena (1999, 295). The term “ether” was connected to a core causal description of the properties through which the ether was claimed to fill its intended causal role.

Psillos contends that the “luminiferous ether” has the same reference as the electromagnetic field, not only because they fill the same causal role, but because, “the core causal description associated with the term ‘electromagnetic field’ takes up the same core causal description associated with the term ‘ether’” (1999, 296). That is, the electromagnetic field fills all of the same kinematic and dynamic descriptions that the “luminiferous ether” did before the term was abandoned. The kinematic and dynamical properties of both terms are the properties that make “luminiferous ether” and “electromagnetic field” denote the same natural kind. Moreover, by sharing the same core causal descriptions, the terms also share the same core explanatory structure. The shared ontology stems from the shared core descriptions of what is doing the explaining. These shared core descriptions of the natural kind-constitutive properties are what justifies the referential stability from luminiferous ether to electromagnetic field.

Psillos notes that one may object that there is an ad hoc element in deciding which descriptions are to be considered as core causal descriptions. He responds that “in principle” one can identify and delineate the descriptions associated with an entity and to examine them in terms of importance for the causal role attributed to the posited entity. The most important descriptions, according to Psillos, are those that pick out the causal role a putative entity plays. If the descriptions ascribed to the entity are not necessary for it to fulfill its causal role, then they are not as fundamental or important. For the luminiferous ether, the less fundamental descriptions are those involving its possible constitution, such as Green’s elastic-solid model and McCullagh’s rotational elasticity model. Psillos stresses that the identification of core causal descriptions is not ad hoc. The choice of which properties are taken to be core causal descriptions is determined by examining what properties are necessary for the posited entity to play its causal role with regards to a certain range of phenomena.

Furthermore, Psillos notes, “It is certainly constrained by the way in which the scientists who posited the entity described it” (1999, 298). Hence, it is the ampliative-abductive methods of the scientists themselves that delineate what is a core causal description and what is not. Returning to his examples of phlogiston and luminiferous ether, Psillos concludes that since the former does not fit the same core causal description as oxygen while the latter fits the same core causal description as the electromagnetic field, “phlogiston” does not refer, while the “luminiferous ether” does refer.

Another objection Psillos answers comes from Niiniluoto (1997), who points out that there are a number of actual cases where the core causal description is mistaken in some way. He uses the example of the HI-virus where the first assumptions of its causal

properties were simplified. Rather, there are many variants of HI-virus, and the causal mechanisms and the conditions that produce AIDS are still largely unknown (1997, 549). Psillos responds that the initial assumptions of the putative entity's causal properties may be "exploratory or speculative" (Psillos 1999, 299) and should not be taken seriously until the term becomes part of a well-established theory which links it with a core causal description. When discussing why it is that a term such as "luminiferous ether", despite being linked to firm theories, was dropped, Psillos responds that this is a question for sociology (1999, 298).²⁸

Psillos concludes that if the luminiferous ether/electromagnetic field case is typical, then realism is on solid ground. However, if future investigation shows that all or most core causal descriptions linked with theoretical terms are false, then scientific realism faces a major difficulty. In order for his theory of reference to work, he notes that the determination of the core causal properties must be neither too broad nor too narrow. If too broad, it may include properties that are not necessary for the putative entity to fulfill its attributed causal role. If too narrow, then reference is too easily had and becomes trivial. Thus, through his theory of reference, Psillos has putatively shown how scientific realism can cope with theory change.²⁹

²⁸ Different interpretations for the abandonment of the term "luminiferous ether" are possible. For example, Michael Friedman argues that the Michelson-Morley experiment led to the demise of the luminiferous ether when they showed that the luminiferous ether had no detectable effect on the velocity of light (2001, 87).

²⁹ In Chapter Four, I shall attempt to show that Psillos has not successfully avoided apparent arbitrariness in identifying the core causal properties that pick out natural kinds. One source for the arbitrariness of Psillos's theory of reference stems from his intuition that scientific realism is comprised of three theses about how the world is constituted and of our ability to access the world. His three theses are that the world is (1) mind-independent, (2) theories should be taken literally, and (3) science gives us the most accurate picture of the world through approximately true beliefs. Psillos finds scientific realism "an intuitively compelling philosophical position", so he seeks to make clear, and to defend, this intuition (xvii). The three theses he posits are meant to clarify what he means by "scientific realism". If his arguments support these theses, then he has successfully defended his version of scientific realism. If he cannot find sound arguments for one or more of these theses, then his defense has failed and his intuition is mistaken. Thus,

2.5.6 Reference to Abstracta

Psillos (2011a) contends that the referents of the terms and models that make up a scientific theory are the abstracta. *Abstracta* are entities and processes whose definitions are not satisfied by anything in the physical world. Thus, according to Psillos, abstracta both represent entities and process in the physical world and exist as entities that serve as the referents for particular posits. To show the ubiquity of abstracta in science, Psillos gives the following list of posits, “*imponderable fluids, frictionless planes, ideal gases, perfectly spherical objects, mass-points, perfectly isolated systems, as well as linear harmonic oscillators, Hilbert spaces, and the like*” (2011a, 5). As noted in Chapter One, abstractions and idealizations are used so that scientific theories and models are not so cognitively costly as to be untenable. However, abstracta do not exist in the physical world, and thus cannot be detected through naturalistic means. Hence, the scientific realist confronts a dilemma: either all scientific theories are false or scientific theories cannot be taken literally.

Psillos does not enter the scientific realism debate as a disinterested party, but is motivated by his desire to defend his intuition that scientific realism is true. Thus, Psillos is voluntarist. *Voluntarism* is the view that we choose certain epistemic and metaphysical assumptions as starting points for our philosophical positions, as well as the criteria for success, based on our prior intellectual commitments.

Furthermore, I shall endeavor to argue that Psillos’ epistemic voluntarism lead to an unintended ontological and semantic voluntarism. While Psillos is careful to show how his theory of reference works to identify natural kinds, because of his voluntarism, he has not shown why his particular criteria should be adopted rather than some other criteria. Following Hasok Chang (2003) and P. Kyle Stanford (2003), I will contend that Psillos’ reading of the history of science seems to be inaccurate. Chang argues against Psillos’ claim that the term “caloric” and the description of the entity the term putatively referred to was a superfluous addition to caloric theory. Rather, the term “caloric” fit all of Psillos’ criteria for inclusion as a natural kind in the 18th and 19th centuries and played a crucial explanatory role in the empirical success of the theory during that time. Stanford shows that Psillos’ standards were not adopted by all the important scientists in the historical episodes he cites. Thus, Stanford concludes that Psillos cannot use scientific consensus as a criterion for the identification of natural kinds since consensus in the scientific community on such matters is rare.

Psillos' response to the dilemma is to claim that abstract entities and processes are real, not imaginary fictions. These abstract entities have similar counterparts in the physical world, particular gases (such as hydrogen), particular spheres, and planes, but the abstract entities are not comprised of some of the features that their physical counterparts have. Unlike concrete objects, abstracta do not have causal efficacy. For instance, every physical plane observed thus far creates friction, and every physical object has a particular color, often a particular taste, and so on, that abstracta do not. But, because abstracta lack certain features they bring into focus certain characteristics of their physical counterparts that account for the latter's behavior. For example, when constructing a model to calculate the motion of a projectile, the gravitation of the Earth is assumed to be the only force acting upon the projectile. Taking into account the gravitational forces exerted on the projectile by other objects in the physical world are negligible, and including them would make the calculation cognitively costly. The Earth, Moon, and the force of gravity acting on them are modeled as a "two-particle Newtonian system" (2011a, 7). It is important to note that the Earth, Moon, and gravity are all existent in the physical world because they have causal efficacy. The two-particle Newtonian system exists because the model is required to account for the motions of the Earth and the Moon. As shall be discussed below, if a hypothetical entity or process is necessary for the explanation of well-established phenomena, then the entity or process must exist.

According to Psillos, abstracta must exist because they fit the *explanatory criterion*. The explanatory criterion states that a posit is real if its being posited is required in the explanation of well confirmed phenomena. Psillos claims that what is

directly referred to in scientific theories are the abstracta described by the theory's models, thus we get full knowledge of abstracta and not of the world. Rather, we only get approximate truth and approximate knowledge of the world through our theories. This result seems to conflict with the scientific realist intuition that science is the best generator of knowledge of the natural world. Psillos recognizes that his realism about abstracta carries serious metaphysical baggage (which he concedes but does not address). For instance, he admits that he can only give a sketch of how it is that these models are known if they cannot causally interact with the world. His response is that they come to be known "hypothetico-deductively", they are posits that must exist because they are necessary to account for the behavior of physical entities and processes (2011a, 19).³⁰

³⁰ However, as I shall attempt to argue in Chapter Four, Psillos assumes explanation is a requirement for a good scientific theory, but as shall be discussed in Chapter Three, van Fraassen contends that explanation may be safely ignored. An explanation presumably shows how it is that something is the cause of one or more effects, yet a scientific theory does not need to have posited a cause for every effect in the theory's domain to be considered a good theory. For example, quantum mechanics has enormous predictive power and thus is used by physicists even though there is no explanation for why the quantum universe is not deterministic. In a non-deterministic framework, the causes of phenomena cannot always be identified, if they exist at all.

Moreover, if the pessimistic meta-induction is true, then there are many incompatible explanations that have been given to the same phenomena. I shall return to Chang's argument that caloric theory is a clear counter-example to Psillos' theory of reference. Returning to Stanford, I shall attempt to show that "phlogiston" is another challenge to Psillos' causal-descriptive theory of reference. Lastly, I will endeavor to argue that Psillos' reliance on abstracta as the referents for scientific terms is untenable since he cannot show how we can know that abstracta exist independently of the explanatory criterion, nor can he account for how abstracta interact with us and the physical world.

2.6 Theories of Truth

2.6.1 The Correspondence Theory of Truth

The causal-descriptive theory of reference is tied directly to Psillos' position on truth. The test for whether or not a term or a statement refers to anything is also the test for whether or not it is true that the entity putatively referred to exists. As explained in the section above, the term "phlogiston" does not refer to anything, nor is it true that phlogiston exists, since a key element in the core description (i.e. weight) of "phlogiston" cannot be detected. Thus, having a referent in the world is what makes a term or a statement true. This theory of truth is called the *correspondence theory of truth* since a term or statement is true if and only if it corresponds to something in the world. For example, the statement "the cat is on the mat" is true if and only if there is a cat on the mat. This statement can be tested for truth by looking to see if a cat is on the mat, touching the cat, hearing it purr, and so on. The truth value for statements about unobservable scientific terms is tested through the observable effects they purportedly produce and from inferring their existence as the best explanation of a given phenomena. For instance, the silver-grey track in a cloud chamber is best explained by the existence of positrons, hence the track is both the means of detecting a positron and what is explained by the existence of the positron. Although this method of establishing truth conditions seems viciously circular, Psillos contends that it is not since the core

description of “positron” may not have matched with the expected experimental results (as was the case with phlogiston).

While Psillos thinks that the full truth about what is observable is accessible to us through our senses, he claims that the full truth of scientific theories and models is not directly accessible to us. Therefore, there is no direct correspondence relation between theoretical claims and the world. However, he defends the correspondence theory of truth from the claim that truth as correspondence may be attainable, but that we would not know if we have attained it or not.³¹

2.6.2 The Correspondence Theory of Truth and Our Indirect Access to the World

The correspondence theory of truth faces many skeptical challenges. A longstanding criticism against this theory of truth is that we cannot go beyond our senses to judge whether or not they accurately perceive the world. Moreover, as noted earlier in this chapter, our observations are theory-laden. If our theories affect how we perceive the world, then we do not have unmediated access to the world. Moreover, as described in Chapter One in the section on Catherine Elgin, experiments do not give us direct access to how the world works, but only how phenomena behaves in laboratory conditions. Lastly, scientific realists claim the inference to the best explanation can be used to pick the theory that is closest to the truth. However, as I shall show in Chapter Three, this

³¹ In Chapter Four, I shall argue that Psillos has not established that we can know when we have obtained a true belief.

inference also faces skeptical challenges, such as how we can know that the best explanation is the most true and how we can know we have arrived at the best explanation. Thus, these challenges imply that, in order to acquire truth as correspondence, one would need to be able to surpass our senses, theories, and inferences. If we cannot surpass them, then we cannot tell if we have acquired truth as correspondence with the world. Therefore, truth is an unattainable ideal. Moreover, surpassing one's senses, theories, and inferences is necessary for the attainment of approximate truth too so, *a fortiori*, approximate truth about the world is also unattainable.

Against these challenges, Psillos contends that truth as correspondence is attainable if the *truth conditions*, or the conditions that make a belief true, obtain. For example, a cat being on a mat makes the belief "the cat is on the mat" true. A charged particle's interaction with supersaturated vapor causes a silver-grey streak in the Wilson cloud chamber makes the belief "a charged particle's interaction with supersaturated vapor causes silver-grey streaks in the Wilson cloud chamber" true. These conditions are different from the *evidence conditions* which, when they obtain, make a belief rational (1999, 181). The evidence conditions make a belief rational and justified because the evidence is collected through processes that have reliably acquired truth in the past. Hearing the cat meow and purr, feeling her fur, and seeing her on the mat are all items of sensory evidence that make the belief "the cat is on the mat" justified and rational to believe. Seeing the silver-grey streaks in the Wilson cloud chamber is an item of sensory

evidence and inferences made on the basis of the Lorentz force law³² that make the belief “a charged particle’s interaction with supersaturated vapor causes silver-grey streaks in Wilson cloud chambers” justified and rational. However, whether or not the evidence one is collecting is true or not depends on the world, and not what one is experiencing since these processes are not perfect. The processes can be led into error by other conditions.

For instance, if one was given a hallucinogen and did not know it and perceived a cat *that was not there*, then the truth conditions are not met, though the evidence conditions have been obtained. Thus, while the belief “the cat is on the mat” is false, because it was formed through senses that are otherwise reliable the subject is justified and rational in believing there is a cat before her. Likewise, if the silver-grey streaks in a Wilson cloud chamber were not caused by positrons, then the belief that they do would be false, but, because of the evidence and reasoning mentioned above, the belief would be justified and rational.

Yet, it is still not clear how we can know that we are in a position to know that our evidence conditions and truth conditions merge to make a belief true, justified, and rationally believed. To this challenge Psillos responds that unless certainty is necessary to justify belief in a claim, then we can judge the truth of a claim. Following Rudolf Carnap (1945/6), Psillos claims that there is a point at which enough evidence has been gathered to justify a belief. Moreover, Psillos holds that the truth predicate is disquotational. The *disquotational theory of truth* states that true predicates added to an assertion is redundant. As Psillos explains:

³² The Lorentz force law states that positively and negatively charged particles in an electromagnetic field will curve in opposite directions. Positrons were posited to explain the appearance of streaks in the Wilson cloud chamber that curved in the opposite direction as electrons in an applied magnetic field.

Now, let us grant that assertion S , ‘Substance X is an acid’, is decidable - that is, confirmable to a high degree – by some scientific procedures. As Carnap rightly observes, if S is confirmed to a degree r , then sentence S' , ‘ “Substance X is an acid” is true’, is confirmed to exactly the same degree, since S and S' are equivalent, given the English language and the disquotational property of the truth predicate. Carnap concludes that ‘is true’, and ‘truth’, are legitimate scientific notions, precisely on the grounds that sentences which state truth-values are confirmable. (1999, 182)

If the belief that a particular sentence is true was generated by a reliable method, then the addition of the truth predicate is redundant. Clearly, it is possible that the belief warranted by a reliable method is false (e.g. a belief in phlogiston). Reliable methods do not guarantee certainty, they are fallible. As Psillos states, “The link between a reliably produced belief and a true belief is synthetic, not conceptual. But, notice that fallibility does *not* entail actual falsity” (1999, 182). Hence, we discover reliability through experience and not *a priori*, and reliable methods often, but do not always, generate true beliefs. However, unless one holds that no belief can be rationally judged to be true unless it is certainly not false, then belief in truth is rational despite the logical possibility that the belief could be false. Thus, if scientific methods are generally reliable methods for obtaining true beliefs, then we do not need to transcend time to know that our best theories are probably true. Psillos concludes that access to the truth is not a utopian ideal.

Now let us turn to Psillos’ defense of the correspondence theory of truth.

2.6.3 Correspondence and Our Mediated Access to the World

As noted in Chapter One, Catherine Elgin argues that scientific experimentation does not provide the full truth about how phenomena behave in the world unaffected by humans, but what truth experimentation does provide is of how phenomena behave in laboratory settings. Since scientific descriptions of the world make claims about the world based on experimental findings, and experimental findings refer to laboratory settings and not the world, then these claims are false. If Elgin is correct, then, according to the correspondence theory of truth, scientific descriptions of the world based on experimental findings do not correspond to the world. Thus, scientific descriptions of the world are false. If Psillos cannot respond to this challenge, then he cannot show how terms and statements about the world are to be understood as corresponding to the world.

An additional challenge against the correspondence theory of truth arises from theory-ladenness. As Kuhn shows in his (1970), the discoveries of oxygen and X-rays show that Priestly and 19th century physicists apparently could not observe oxygen and X-rays, respectively. The theories Priestly and 19th century physicists used to categorize the world did not allow for these new discoveries in their respective ontologies. Kuhn claims that according to the correspondence theory of truth, Priestly and 19th century physicists should not have been blinded by their theories and they should have recognized these discoveries. Because of the failure of many scientists to recognize novel phenomena, Kuhn concludes, ‘There is, I think, no theory-independent way to reconstruct phrases like “really true”; the notion of a match between the ontology of a

theory and its “real” counterpart in nature now seems to me illusive in principle” (1970: 206 in Psillos 1999: 246).

Against the first challenge, Psillos claims that our interaction with the world does not preclude our acquiring knowledge about the entities with which we have interacted. Rather, he asserts that our interactions with the world are a reliable way for us to gain knowledge about the world. If we could not interact with the world, then we could not have epistemic access to the world. Furthermore, it is through our interaction with the world that we can fallibly state that the truth conditions of our beliefs obtain (1999: 245). The same information that we conceptualize and theorize about is information about the interacted-with objects. Since our causal interactions and connections provide our knowledge of the world, we should have no trouble accessing truths about this world.

In response to the second challenge, Psillos argues that our interaction with the world facilitates epistemic access to the world instead of undermining it. He states, the main realist point about truth is that, “[it] is *logically* independent of human opinion: there is no conceptual or logical link between the truth of a statement and our ability to recognise it, assert it and the like. Our beliefs are about interacted-with objects – since they cannot be about *uninteracted-with* objects” (1999: 246). Without interacting with objects we cannot know anything about them. Simple passive observation of a substance thought to be carcinogenic will not conclusively tell us if the substance is carcinogenic or not. However, exposing mice to that substance, isolated from as many other substances as possible, can lead us to conclude that the substance is, or is not, carcinogenic. Yet, the conclusions arrived at about these interacted-with-objects - “insofar as they are true” - (1999, 246) is not logically dependent on methods of confirmation, justification, and so

on. Thus, returning to Kuhn's examples of oxygen and X-rays, these entities would exist whether we could theorize about them or not. These objects are independent of us in a logical, not causal sense, they are not the result of our theorizing and conceptualizations. Rather, we create our theories and concepts from our interactions with the world.

Moreover, to further defend the correspondence theory of truth from the challenges of interaction and theory-ladenness, Psillos invokes the *success argument*. This argument states, "that an appeal to truth-conditions is essentially involved in the explanation of why successful actions are successful. This explanation is based on the claim that the truth-conditions of the belief(s) on which successful actions were based have been realised" (1999, 247). In particular, it is grounded in the claim that a variety of reliable beliefs are necessary for success in various domains, and that a belief should reliably reveal its truth-conditions to be considered reliable.

An explanation of a successful action is basically dependent on the employment of truth-conditions. An action based on certain beliefs is successful because the beliefs' truth-conditions are realized. For instance, "what explains the efficacy of the physician's prescription in curing the disease is that the truth-conditions for his beliefs, e.g. that the patient is infected by such-and-such a virus and that this virus is killed by such-and-such an anti-biotic, were realised" (1999, 247). Thus, generally, a subject is more likely to meet with success when the truth-conditions of their beliefs obtain (1999, 247). The beliefs that produce a "systematic pattern of success" are those whose truth conditions are the realization of "*referred-to entities standing in the referred-to relations*" (1999, 248). Put more simply, successful actions are based on beliefs that "correspond to reality" (1999, 248). This property of correspondence with reality that is shared by all these

beliefs is what gives this account of truth its substance. Clearly, beliefs are expressed in terms of concepts and theories. Thus, while theory-ladenness is unavoidable, the success of actions based on these concepts and theories show that the concepts and theories are true.³³

2.7 Truth-likeness

2.7.1 Psillos' Intuitive Approach

Scientific realists typically argue that our best scientific theories should be considered truth-like, since, as Psillos notes, “In our interactions with the world, the exact truth cannot generally be had, especially concerning the unobservable and spatiotemporally remote aspects of the world. A perfect match between theories and the world is almost impossible” (1999, 276). Thus, we cannot obtain the full truth about the world and have to settle for approximations to the truth about the world in our scientific theories and models. The structure of natural phenomena is so interwoven and complex that it cannot be fruitfully studied and represented in complete theories without idealization and simplification. Scientific theories are often constructed with many

³³ However, as I shall attempt to show in Chapter Four, Chang (2003) and Stanford (2003) both contend that past theories have met the explanatory criterion of the success argument, but these theories are now seen as false. Furthermore, I will argue that Psillos has not shown that the success of science is not viciously circular. Moreover, even if his success argument is sound, he has only shown that concepts and theories are true of abstracta, not of the world.

idealizations, such as point masses and ideal gases, which give a simplified representation of the world. This simplified representation makes investigating it easier. For instance, our representations of the laws of nature are formed by bracketing off many obfuscating features and conditions, such as air resistance in the law of free fall. As Psillos states:

Theoretical predictions are tested against experimental results, but almost no prediction exactly matches the experimental results. Most predictions stand within an ϵ , however small, from the experimental outcome, which itself has an error-estimate. Demanding the truth in science would amount to demanding the exclusion of all approximations, simplifications, idealisations, approximate derivations, sources of error in measurements and calculations. (1999, 276)

Even if such an exact science were possible, it would be a different kind of science than that with which we are familiar (1999, 276).

However, in general, scientific results are self-corrective. Scientists specify the idealizations and specifications employed in theoretical laws and mechanisms.

Moreover, they specify, as precisely as possible, the respects and the degree to which natural phenomena diverge from their theoretical representations. Psillos employs the case of a law that is derived from a more fundamental law as an example of how simplification occurs in science:

Take for instance,...the derivation of Kepler's first law from Newton's inverse-square law. This derivation is, strictly speaking, false. Actually, from premises which are strictly speaking false, i.e. considering the revolution of Mars around the sun as a two-body problem, a false conclusion is derived – that Mars' orbit is elliptical. But, the degree of accuracy of the derivation is specifiable, as are the respects in and degrees to which the conclusion, Mars's orbit is elliptical, deviate from the actual orbit of Mars. This is where the idea of truth-likeness enters science. For both the premises of the derivation (i.e. the two body problem) and its conclusion(Mar's orbit is elliptical) are, in one sense, truth-like. (1999, 276)

Kepler's laws approximate to a high degree of accuracy the motion of the planets, and are thus truth-like. The two-body simplification in the derivation of Kepler's first law from the law of universal gravitation is truth-like, since the other planets' gravitational effects on the motion of Mars are negligible in contrast to the Sun's gravitational field.

To be accepted as true, a theory (or theoretical description) must fit the world. To fit the world the theory or theoretical description must give an isomorphic representation of the phenomena in the theoretical description's domain. This conception of truth Psillos calls *fittingness*. If truth is fittingness, "then truth-likeness should be understood as *approximate fittingness*: a description, statement, law, theory are truth-like if and only if there are respects and degrees to which they fit with the facts" (1999, 276-277).

According to Psillos, a statement is as approximately true to the extent that it is accurate in whatever it asserts (1999, 277). For instance, a theory which states that Mars has a square orbit is less true than a theory which states that Mars has a circular orbit.

Moreover, the theory which states that Mars' orbit is circular approximately fits the fact that Mars' orbit is elliptical. Clearly, the theory which states that Mars' orbit is elliptical is more approximately true since it more fully fits the facts.

Psillos considers his approach to be intuitive since it aligns with our intuitions that "a theory is approximately true if the entities of the general kind postulated to play a central causal role in the theory exist, and if the basic mechanisms and laws postulated by the theory approximate those holding in the world, under specific conditions of approximation" (1999, 277). He argues that the cognitive significance of false descriptions furnishes these false descriptions with their truth-likeness. These descriptions may have a better or worse fit with the facts they claim to describe, and they

may represent the domain they propose to describe to a low or high degree of accuracy. For example, “The wave theory of light is strictly speaking false. Yet, its description of the interference phenomena is a better approximation to the truth than is the relevant description of the theory of luminous molecules. Similar examples can be generated at will” (1999, 277). Rather than discard the notion of verisimilitude, “since all truth-like assertions are – strictly speaking – false” we should attempt to discover as sufficiently as possible the conditions where a representation fits as accurately as possible the pertinent facts (1999, 277).

Psillos give following clarification of his *intuitive approach* of truth-likeness:

A description *D* *approximately fits* a state *S* (i.e. *D* is approximately true of *S*) if there is another state *S'* such that *S* and *S'* are linked by specific conditions of approximation and *D fits S'* (*D* is true of *S'*). (1999, 277)

To further clarify his notion of truth-likeness Psillos states that a theoretical law is approximately true of the world, if it is fully true in a world which approximates ours under certain conditions (1999, 277). In his (2011a), Psillos further clarifies that the full truth of scientific theories and models are to be found in the abstracta that constitute a scientific theory’s descriptions of the phenomena in the theory’s domain. For example, “the law of gases, $PV = RT$. This is approximately true of real gases, since it is true of ideal gases and the behaviour of real gases approximates that of ideal gases under certain conditions” (1999, 277). Psillos concedes that this is a sketch of a theory of truth-likeness, but it also incorporates the proper intuitions that judgments of approximate truth entail some comparison between the actual world and the world, or state, the theory describes.

An objection to an intuitive approach to truth-likeness is that it makes the notion of truth-likeness vague. Psillos responds that the notion of truth-likeness operating in science is clear enough. Paradoxes, such as the *Liar's paradox* where difficulty arises in assessing the truth of a statement that asserts its own falsity³⁴, led to Alfred Tarski's formulation of the *semantic approach* to the truth: *x is a true sentence if and only if p* (1983, 155). In this formulation, *x* is the sentence and *p* is the states of affairs the sentence putatively represents. For example, the statement "The grass is green", is true if and only if the grass is green. But, no such formulation is necessary for the intuitive approach to truth since no such paradoxes arise for this latter approach. Furthermore, Psillos maintains that the vagueness of truth-likeness does not count against it. The charge of vagueness can be overcome by focusing on the notion of approximation. Different theoretical descriptions will have different degrees of similarity and different respects in which they are similar to the phenomena in their domain. Thus, the notion of approximation plays a crucial role in elucidating truth-likeness through particular examples.

A second objection against the intuitive approach to truth-likeness is that it is not robust enough to be used by scientific realists. The reliance of the notion on approximation may not be sufficient to support the epistemic optimism of scientific realism that current theories are approximately true. Psillos claims the "objection is misguided" (1999, 278). He gives the following answer to this objection:

For it is one thing to explicate the claim that a theory (or a theoretical statement) is approximately true, but it is quite another to *ground* the judgment that a theoretical description, or a theory, is approximately true. The first is, broadly speaking, an issue in semantics, whereas the second is an epistemological problem....The present aim has been to show that

³⁴ E.g., "What I am now saying is false".

insofar as the conditions under which a certain description (or state) approximates another are specifiable, to say of a statement D that it is approximately true of state S is to say that it is true of a state S' which approximates S . Whatever else it does, this explication at least legitimises ascriptions of approximate truth when there are specifiable conditions of approximation. (1999, 278)

When these ascriptions are shown to be legitimate, the question remains whether a certain statement (or theory) can be warrantably asserted to be approximately true. The epistemic optimism linked with scientific realism states that such ascriptions of approximate truth are capable of being warranted. The warrant for attributing approximate truth to mature and successful theories rests on the inference to the best explanation. As shown in previous sections, Psillos is confident that the inference to the best explanation has been shown to be a reliable method for generating true beliefs.

2.8 Psillos' Account of Scientific Knowledge

In this section, I shall argue that Psillos has an implicit justified true belief analysis of scientific knowledge. My argument will examine Psillos' development of this account of scientific knowledge from his (1999) to his (2011a). I will first discuss his account as presented in his (1999). Using Alexander Bird's approach for obtaining full truth of the world, I will argue that Psillos could use this approach for his (1999) account of scientific knowledge. In his (2011a), Psillos argues that the full truth of scientific theories is to be found in the abstracta employed in the theories. I shall argue that this

additional view of what constitutes scientific truth gives a fuller description of his account of scientific knowledge.

2.8.1 Psillos' (1999) Account of Scientific Knowledge

Psillos (1999) explicitly states that the goal of science is the attainment of approximately true beliefs justified through abductive reasoning. The theory of truth he is committed to is the correspondence theory of truth, the theory of justification he adopts is reliabilism, and he claims that scientific theories that have been shown through reliabilism to be true should be believed. However, despite showing how these three components of traditional knowledge work together to attain non-accidental approximate theoretical truth, Psillos does not claim to have an account of scientific knowledge. Yet, he does not explicitly deny that he has an account of scientific knowledge either.

However, as noted in Chapter One, Catherine Elgin argues that approximate truth is not sufficient for knowledge. Knowledge requires the full truth and nothing less. Scientific realists recognize this and thus claim only to have acquired approximately true beliefs and not knowledge. However, Psillos seems to think that approximately true beliefs can be justified in the same way that true beliefs can, and that these approximately true, warranted beliefs are worthy of the name “knowledge” in both of his (1999) and (2009) (at least in the case of “background knowledge”). While he does not explicitly deny the possibility of knowledge on the grounds that approximate truth of the world is

all that can be achieved and knowledge needs nothing but the truth, Psillos could follow Alexander Bird's account of truth used in the latter's account of knowledge. Bird's account of truth shall be described in the paragraph below. Rather, not until his (2011a) does Psillos state of what it is that scientific theories are true. There he argues that we acquire full truth only about the abstracta that represents the world and we only acquire approximate truth about the world itself.

In arguing for the possibility of full truth about the physical world, and thus knowledge, in science, Bird also relies on an informal and intuitive concept of approximate truth. He contends that "If [a proposition] p is approximately true, then the proposition q , that p is approximately true, is itself true, not merely close to the truth" (2007: 76). He uses the following propositions to illustrate his point, "'if planets travel in ellipses' is a scientific proposition, then so is 'approximately, planets travel in ellipses'". Even if p is not true and so not knowable, q ($q = \text{approximately } p$) might well be knowable" (2007, 76). Moreover, such propositions as q are true about the world, and can be used in explanations and predictions. For instance, "the fact that it is approximately true that the only force acting on the Earth is a central force directed towards the Sun explains why the Earth's orbit is approximately an ellipse" (2007, 77). Clearly, explanations that involve "approximately p " are frequently parasitic on what p would account for if p were true. However, this might not always be the case. As Bird explains:

That the motion of a pendulum is approximately simple harmonic is explained by the fact that small θ , $\sin \theta$ is approximately equal to θ . This is not best understood in terms of what would be explained if $\sin \theta$ were exactly equal to θ . For if $\sin \theta = \theta$ (exactly) then $\theta = 0$, and we do not have any pendulum motion at all. (2007, 77).

In some cases, increasing nearness to truth can also show increasing full truth. As Bird states, “And so those cases can in principle show the accumulation of knowledge also, should the required epistemic conditions be met” (2007, 77). In Psillos’ case, the epistemic conditions to be met for truth-likeness seem to be justification and belief. Bird illustrates how an accumulation of truth-likeness can lead to knowledge:

Let $\langle p_1, \dots, p_k \rangle$ be a series of hypotheses, accepted in that order over time, that monotonically get closer to the truth. This sequence shows progress according to the verisimilitude account. We can see that there is also a distinct but related sequence of propositions, entailed by the first, that exhibits the accumulation of truth and (potentially) the accumulation of knowledge. Let $A(\dots)$ be a propositional operator whose meaning is given thus: $A(p)$ iff approximately p . Assuming first, for simplicity, that all p_i are approximately true, the sequence of propositions $\langle A(p_1), \dots, A(p_k) \rangle$ will be a sequence of propositions each of which is fully true and adds to the truth provided by its predecessors. If these propositions are believed, then we have an accumulation of fully true propositions; and if they are sufficiently well supported by the evidence, the propositions will be known and we will have progress according [to] the cumulative knowledge account. The improving precision of our approximations can be an object of knowledge. (2007, 77)

The above illustration seems to cohere with Psillos account of “background knowledge”. As Psillos states, “Those beliefs for which scientists acquire overwhelmingly supportive evidence augment the mass of warranted background beliefs and become the pivots for new warranted beliefs” (1999: 219). If I am correct to assume that Psillos would find this account of truth acceptable, then he has the ingredients to formulate an account of scientific knowledge.

In this sketch of Psillos’ (1999) account of truth, it seems that he is arguing for something very much like the traditional justified, true belief account of knowledge for science. Perhaps, scientific knowledge could be seen as a special kind of knowledge whose components are justification (through abduction), truth (approximately of the

world), and belief. However, it is important to note that all of his (1999, 2009, 2011) accounts of scientific knowledge may not require justification since he never explicitly claims it is necessary for scientific progress or scientific knowledge. Hence, it appears his account of scientific knowledge can also be analyzed in terms of truth and belief. While Psillos may hold to a true belief account of scientific knowledge, such an account would seemingly not affect the arguments in Chapter Four regarding his commitment to facticity.

2.8.2 Psillos' (2009) Account of Scientific Knowledge

In Psillos' latest book *Knowing the Structure of Nature* (2009), he takes a more epistemically optimistic position. He claims that there should be no principled limit on our knowledge of nature, hence a large part of his book is aimed at those philosophers, such as the *structural realists*, who seek to impose such limits. The structural realists hold that we can only know the mathematical structure of nature and deny that we can obtain any description about the character of the unobservable entities and processes represented mathematically. Structural realists assume that pessimistic meta-induction is cogent, and hence have given up hope of giving true descriptions of unobservables. However, they are committed to the "no miracles argument" since they hold that we do discover the structure of the world and the structure accounts for the empirical success of science. For example, the equations representing Fresnel's elastic solid ether are largely

the same as those that represent the electromagnetic field in Maxwell's theory. Psillos contends we can have knowledge, not only of the structure of nature, but also of whatever fills in the structure of nature, such as the characteristics of the entities and processes that constitute the structure. In this section, I shall summarize the changes in his epistemic position.

Clearly, Psillos explicitly mentions "knowledge" in a scientific context in the title of, and throughout, his (2009). However, in this work too he fails to give an explicit account of scientific knowledge. Nowhere does Psillos state that scientific knowledge is constituted of particular components, such as truth, justification, and belief. Nor, does he explicitly discuss justification, truth, or belief as being components of scientific knowledge. Yet, does he give a more detailed account of the justification of beliefs in unobservable entities. He no longer relies on a purely externalist account of justification and supplements it with John Pollock's internalist perspective, "where justification and warrant are tied to the presence (or absence) of defeaters" (2009, xxiv). *Defeaters* are propositions that are true, and if one were to learn of the propositions in question, then they would undermine the justification for a belief. This condition blocks the original counter-examples. For instance, in one of Gettier's (1963) examples, Smith has a justified true belief that the man with ten coins in his pocket will get the job. However, unbeknownst to Smith, he has ten coins in his pocket and will get the job. Thus, Smith has a justified true belief, but it is not knowledge since the justification for his belief does not apply to him, but to Jones who Smith knows has ten coins in his pocket. Smith formed his belief that the person with ten coins in their pocket would get the job, and he was justified in believing that Jones had ten coins in his pocket. If Smith had known that

Jones would not have gotten the job, then this proposition would have defeated his justified false belief that Jones would get the job. Moreover, if Smith knew that Jones did not own a Ford, then he would not have the justified false belief that Jones owns a Ford. In both cases, Smith's justified true beliefs are defeated. If Smith acquired justified true beliefs that had not been undermined by defeaters, then he would have knowledge.

In particular, Psillos needs to safeguard ampliative reasoning, or inference to the best explanation, from two challenges. The first is the challenge of justification: "given that ampliative reasoning is not necessarily truth preserving, how can it be justified?" (2009, xxiv). For example, our ancestors were justified in believing that the Earth is stationary, flat, and the sun and other celestial bodies moved around it. The Earth does not seem to be moving, or round, and we can see the sun "rise" and "set" each morning and evening. Thus, they did not have evidence for the movement of the Earth or for its being round. Thus, our ancestors were justified in believing what they believed about the Earth though, by contemporary lights, they are wrong. The second challenge is to discover the structure of ampliative reasoning. For example *enumerative induction* is one form of ampliative reasoning where if something *A* has been observed to be a certain way *B* for long enough, then one can infer the generalization that "All *As* are *Bs*". Thus, if all observed eagles have talons, then one can infer the generalization "All eagles have talons". As Psillos states, "The first is *Hume's* problem; the second is *Peirce's* problem" (2009, xxiv). While these are distinct philosophical issues, a proper discussion of ampliative reasoning needs to deal with both. Specifically, a proper discussion should show how some epistemic warrant is bestowed on the results of ampliative methods.

According to Psillos, inference to the best explanation is the best description of the scientific method, and it is inference to the best explanation that confers warrant on the results of this method. Psillos justifies inference to the best explanation “within Pollock’s (1986; 1987) framework of defeasible reasoning. By thinking of justification in terms of the absence of *defeaters*, this framework makes possible the investigation of the conditions under which defeasible reasoning can issue warranted beliefs” (2009, xxiv). Clearly, ampliative methods of reasoning are defeasible since new information or evidence can defeat an outcome. Further evidence or information can remove the warrant for believing the outcome of the method. Psillos follows Pollock in calling, “‘*prima facie*’ or ‘defeasible’ any type of reason which is not conclusive (in the sense that it is not deductively linked with the output it is a reason for)” (2009, 177). Since ampliative reasoning is defeasible, such reasoning supplies *prima facie* warrant for an outcome (belief). Psillos agrees with Pollock that in calling a warrant (or reason) *prima facie* is to emphasize that it can be defeated by further reasons or information; and (b) its robustness, *qua* reason, is derived from the presence or absence of defeaters (2009, 177). The degree to which one is warranted in holding a particular belief is directly reliant on the presence or absence of defeaters. As applied to ampliative methods, the extent to which an ampliative method can bestow warrant on an outcome is directly linked to whether or not defeaters are present, and to what degree (2009, 177). As Psillos explains, “To say that *S* is *prima facie* warranted to accept the outcome *Q* of an ampliative method is to say that although it is possible that there are defeaters of the outcome *Q*, such defeaters are not actual” (2009, 177-178). Specifically, it is to say that *S* has thought over a range of possible defeaters of the reasons offered for this outcome *Q* and has shown

they are not present. It is logically possible that the subject in question is being constantly deceived by an omnipotent deceiving demon, but without any evidence to support this possibility this defeater can be considered as not actual. The existence of black swans is an actual defeater to the generalization that “All swans are white”, and of enumerative induction as a reliable method for conferring warrant to the generalizations produced by this method. If this no defeaters have been discovered, then we can assert there are no specific doubts regarding the outcome of the method, and that there is prima facie warrant in this belief.

Psillos contends that, “This talk of defeaters is not abstract. There are general *types* of defeater that one can consider” (2009, 178). Thus, when judging whether an outcome is warranted, there are particular things to pay attention to such that, if present, would remove the warrant for the outcome. Mere logical possibility of defeaters is not enough to warrant consideration, the threat has to come from an actual defeater. As Psillos states, “Besides, if the reasoner has done whatever she can do to ensure that such defeaters are not present in a particular case, there is a strong sense in which she has done what it can plausibly be demanded of her in order to be epistemically justified” (2009, 178). For instance, in the case of the omnipotent deceiving demon, by definition there is no way for an inquirer to know that she is being deceived since the demon constantly supplies her with false information. Without anyway to show how one could circumvent the alleged demon’s omnipotence to discover she is being deceived there is nothing for her to do to show that this logically possible defeater can be guarded against. Thus, she is epistemically justified in ignoring this defeater. However, if the inquirer is a building inspector who does not actually go onto the roof of a roofing job, then her epistemic

warrant cannot be conferred on her claims that the roof is being constructed according to code.

As in his (1999), Psillos (2009) stresses the importance of justification, truth, and belief in his account of the epistemic progress of science. Still he does not provide an explicit account of scientific knowledge. Perhaps he is afraid that an explicit analysis of scientific knowledge in terms of justification, truth, and belief would leave his account of progress open to the same vulnerabilities as the traditional analysis of ordinary knowledge. For instance, this latter analysis has been plagued for nearly fifty years by Gettier cases. Regardless of the motivation for his caution, Pollock's theory of justification originated as a response to Gettier cases. Therefore, Psillos would likely present Pollock's account of knowledge as justified true belief with no defeaters as his account of scientific knowledge.

2.8.3 Psillos' (2011) Account of Scientific Knowledge

As noted in section 2.4.6, Psillos claims that the full truth in scientific theories is to be found in the abstract models of natural phenomena. Since the full truth is to be found in abstracta, and not in the approximate truths of the world, we do not have knowledge of the world, but only of abstracta. Thus, while the domain of science seems to be the natural world, Psillos maintains that the knowledge science produces is of abstracta. We now have a complete description Psillos' account of scientific knowledge.

The theory of justification he is committed to is reliabilist, internalist (except he adopts externalism to justify the “no miracles argument” and the inference to the best explanation), and defeasible. His theory of truth is the correspondence theory of truth, and our beliefs correspond to abstracta and not directly to the world. Psillos’ theory of belief is that we should believe what has been shown to be true through reliable methods, rather than withholding judgment on their truth or falsity.

2.9 Alexander Bird

2.9.1 Scientific Knowledge as Unanalyzable

Alexander Bird (2007) explicitly argues, not only that scientific knowledge is possible, but that its accumulation is the proper measure of scientific progress (64). This knowledge is propositional and theoretical. He adopts Timothy Williamson’s (2000) account of knowledge as primitive and unanalyzable, and extends this account to scientific knowledge. Motivated by the failure to overcome Gettier cases, Williamson analyzes justification, truth and belief in terms of knowledge, rather than analyzing knowledge in terms of justification, truth and belief (and perhaps some fourth element that blocks Gettier cases).

Bird (2007) is motivated by two major views in the philosophy of science. The first is semantic realism, where truth-likeness is the goal of science. He argues that philosophers, such as Psillos, allow for truth to be acquired accidentally. Without justification to show how the belief was reliably acquired, the accidentally obtained true belief can just as easily be lost. For instance, a true belief gained through spurious means, such as reading tea leaves, can lead to the true belief being abandoned once the unreliability of the method has been revealed. Moreover, he notes that scientists spend tremendous energy justifying their conclusions through certain procedures, such as repeatable experiments. Thus, mere accumulation of true beliefs cannot be the goal of science since true beliefs can be easily gained and lost, and is not reflective of actual scientific practice. Moreover, Bird is keen to point out that he is talking about propositional knowledge of unobservables.

The other position motivating his approach he calls the *functionalist-internalist approach*, where scientific progress is measured through the fulfillment of a certain function, such as reliably predicting phenomena, and whether or not that function has been fulfilled can only be judged by the scientists employing that function. The fulfillment of a function (such as solving a puzzle) does not require truth. For instance, Nicole d'Oresme thought that hot goat's blood would split diamonds. Oresme's claim is now seen as false. On the functionalist-internalist conception of progress, scientific progress was achieved by Oresme if his putative solution was accepted by his contemporaries. However, Bird contends that since it is false that hot goat's blood does split diamonds (regardless of what any community says), then truth becomes relevant to whether or not a given solution really is a solution to a particular puzzle. Instead, he

argues that the goal of science should be knowledge. Bird calls his view the *epistemic approach*.

2.9.2 Bird on Truth

Bird notes some supporters of scientific realism, such as Karl Popper, argue that science cannot deliver the full truth; rather we can only ever achieve approximate truth. Thus, they claim science should not aim to generate knowledge, which requires the full truth, but should seek truth-likeness as its goal. As shown on pages 99-100 of this chapter, Bird contends that the full truth is possible, even if we only often obtain approximate truth. Yet, even with the acquisition of approximate truth we know fully that a particular proposition is approximately true, though we do not know the full truth. For instance, that the Earth travels in an ellipse is only approximately true because it is only approximately true that the only force exerted upon the Earth is a central force directed towards the Sun. Furthermore, Bird holds that an accumulation of (non-accidentally obtained) true beliefs can eventually lead to acquiring the full truth and, hence, knowledge. For instance, successive approximations of the speed of light become more and more accurate until we finally know the speed of light (within a certain margin of error).³⁵

³⁵ In Chapter Four, I shall attempt to show that Bird's notion of approximately true background knowledge cannot support knowledge claims since approximately true items of "knowledge" are false, and false claims cannot be considered to be evidence or knowledge.

2.9.3 Bird on Belief

In accord with Williamson, Bird holds that the aim of belief in general is knowledge, so the aim of scientific belief is scientific knowledge. Beliefs have an aim because mental capacities have aims. These functions are explained by their contribution to fitness. The function of the capacity to believe aims to generate knowledge and to provide that knowledge to other cognitive functions (Bird 2010, 1). If beliefs aimed only truth, then they could be easily acquired, but also easily abandoned. Thus, knowledge as the goal for belief blocks the possibility of true beliefs being easily acquired and abandoned. Moreover, the goal of knowledge acquisition accurately represents scientific practice and knowing whether or not a given solution is really a solution or not is a contribution to scientific progress. As an historical example of a true belief accidentally obtained, Bird claims that Alfred Wegener's theory of continental drift arrived at true conclusions with insufficient evidence that was confronted by strong counterevidence.³⁶

2.9.4 E=K

If the assumption is allowed that belief, and scientific belief specifically, aims at knowledge, then we can characterize evidence in functional terms. As Bird states, "the

³⁶ In Chapter Four, I shall use Naomi Oreskes' (1999) take on Wegener's theory to show that his theory appears to be scientific, and an item of scientific knowledge, by Bird's own standards.

function of evidence is to provide the propositional input into a process of reasoning that can achieve this aim. More precisely, then:

(E) the proposition p is in S 's evidence if and only [sic] S can gain knowledge by (non-redundant) inference from p ." (2010, 3)

The quality of the propositional input and of the reasoning process will determine whether or not the aim of belief has been achieved. Bird leaves discussion of the reasoning process for later in his paper, and merely mentions that we assume the reasoning process is impeccable. He focuses on what constraints (E) puts upon evidence. Knowledge only will suffice to constitute evidence. Bird contends that, "[i]f conclusion c depends inferentially on evidence e , then if e is not known then c is not known either" (2010, 3). Moreover, any bit of knowledge can be evidence, because there will always be conclusions drawn from this bit of knowledge that can thereby be known in principle. Thus, "(E) supports Williamson's (2000) claim that a subject's evidence is precisely what that subject knows, (E=K)" (2010, 4).

The distinction between when a proposition counts as "evidence" and when it counts as "theory" is contextual. For instance, Ignaz Semmelweis showed that infectious material from the hands of medical students from dissections had caused puerperal fever in post-partum women the medical students had subsequently examined. The finding that the cadaverous material on the medical students' hands was highly correlated with instances of puerperal fever is evidence that helps to establish the truth of the theory that cadaverous material is the cause of puerperal fever.

The evidence that comprises background knowledge is arrived at through the inference to the best explanation. Oftentimes, more than one hypothesis is put forth to account for some scientific phenomenon. Then, the hypotheses offered are judged for

how well they explain the phenomenon. The best explanation of the phenomenon is given the greatest possibility of being the actual explanation, or at least approximately so. Returning to the example of Semmelweis, the steps he took to explain the cause of puerperal fever illustrate how the inference to the best explanation proceeds. Semmelweis showed that infectious material from the hands of medical students from dissections had caused puerperal fever in post-partum women the students had subsequently examined. This hypothesis won out over alternative hypotheses for the cause of the fever, such as, the presence of a priest, or overcrowding.

According to Bird, this characterization of the inference to the best explanation faces three objections: *the problem of underconsideration*, *Hungerford's objection*, and *Voltaire's objection*. The first objection states that there may be explanations that have not been considered, and that the actual explanation may be in the set of unconsidered explanations. Hungerford's objection asserts that the notion of "goodness" used to decide which explanation is best is too subjective to have any connection with truth. The last objection maintains that, even if explanatory goodness could be objectively rated, there is still no reason to assume that the best explanation is the actual explanation since that would be to assume that we live in the best of all explanatory worlds, and that is not a safe assumption.

Bird first answers the second and third objections by appeal to the *inference to the only explanation*. This inference proceeds by eliminating the competing explanations as plausible explanations until the only explanation remains. Bird contends that Semmelweis proceeded in this manner when he showed that the competing explanations were not explanations of the phenomenon in question. Against the problem of

underconsideration, Bird argues that controlled experiments are designed precisely to reduce the number of hypotheses to one. Moreover, following Williamson's *principle of safety*, our evidence does not need to refute all false hypotheses, but only plausible false hypotheses. Thus, there is no need to be concerned about hypotheses that are radically different from the ones being considered.

A mechanism is required to ensure that only plausible hypotheses are considered. Kuhn's notion of an *exemplar* forms part of this mechanism. An exemplar is a model solution to a given scientific puzzle. When scientists are searching for a solution to a puzzle they will often search for a solution similar to an exemplar being used to solve an existing problem. For example, the equations that Newton used to predict the motions of celestial bodies are the exemplars that served as models for the creation of new sciences studying terrestrial motion, such as hydrodynamics. These exemplars are, according to Bird, true or approximately true. When potential solutions to a similar problem are put forward, and these potential solutions resemble the problems solved by existing exemplars, then the potential solutions are considered plausible.

Bird notes that sometimes not all of the plausible solutions are considered, and the actual solution to a problem is not found. When the existing exemplars fail to solve an increasing number of problems, then radically new exemplars are proposed. For instance, phlogiston theory was overthrown when the mass of metals was found to increase when burned. When a new exemplar has been found then a scientific revolution has occurred. Bird claims that large scale occurrences such as the replacement of phlogiston with oxygen or of Ptolemaic to Copernican astronomy, is rare. However, they often occur on a smaller scale, as when stress was replaced by *Helicobacter pylori* as the

principal cause of stomach ulcers. While exemplars are replaced in large scale scientific revolutions the defeated exemplars are rarely abandoned completely. For instance, Newtonian exemplars are limited to calculating the motion of middle-sized Earth bound objects, but neither celestial, nor sub-atomic objects. Thus, there is often continuity among exemplars in scientific revolutions.

2.9.5 Bird and The Causal Theory of Reference

From the above description of exemplars it seems we can surmise that Bird is committed to the notion that radical breaks from past scientific theories are possible during scientific revolutions. Furthermore, Bird claims that know-how can be interpreted in terms of knowing-that, but he does not specify when such a translation is appropriate and when it is not. Hence, radical changes in how science is practiced can be accompanied by radical conceptual changes. Since Bird believes that know-how can be translated into propositional knowledge it seems he will have to show in what way reference is preserved across theoretical change. In particular, Bird is keen to avoid Kuhn's (1970) challenge to the referential continuity of theoretical terms. Kuhn contends that during scientific revolutions the terms used by successor theories are often the same as those in the previous theories, but the meanings of the terms have changed so drastically that they no longer express the concept. Thus, the earlier term is homophonic with the later term, but the terms do not translate each other (Bird 2004, 40). For

instance, earlier astronomers described “Earth” as being immovable, the center of the universe, and orbited by planets (including the Sun), but now astronomers describe “Earth” as mobile, not the center of the universe, and as orbiting the Sun (which is not now seen as a planet). According to Kuhn, though the same word is used by the earlier and later astronomers, since the word does not have the same meaning between these two communities, the two communities are referring to *different entities* when they use the same word. Kuhn’s challenge is known as the *incommensurability thesis*. If the incommensurability thesis is true, then scientific progress cannot be characterized as the correction of mistakes made by past scientists about the same concepts referred to by the same terms as present scientists. If scientific progress cannot include the process of overcoming mistaken notions about the same concepts, then scientific progress cannot be seen as the accumulation of scientific knowledge.

In response to Kuhn’s challenge, Bird (2000) adopts a causal theory of reference. Bird notes that continuity of reference is crucial to understanding the scientific realist view of truth and falsity. A causal theory of reference allows scientific realists to claim that later scientists are referring to the same object picked out by the same term as earlier scientists even though the latter described gave vastly different descriptions of the object in question. Thus, the causal theory of reference allows scientific realists to claim that earlier scientists were mistaken in their views about the same objects that later scientists refer to with the same names, such as “Earth” and “Sun”.

Bird is aware of the criticisms of a purely descriptive theory of reference discussed earlier in this chapter. To avoid these shortcomings he adopts a causal-descriptive theory of reference. According to this theory of reference, a term’s

descriptions of the causal role of a putative entity in some specifiable phenomenon are crucial to the referential success of the term (2000, 186). Bird uses the example of “phlogiston” to show how the false descriptions of phlogiston’s causal powers make the term non-referential. Moreover, he uses the Newtonian and Einsteinian notions of “mass” to show how reference is preserved through revolutions. According to Bird, “mass” merely describes the quantity that we are causally acquainted with through the quantity’s presence in instances of forces leading to changes in the motion of objects. Since both Newtonian and Einsteinian mass measure the same quantity then “mass”, whether used in Newtonian mechanics or Einsteinian mechanics, refers to the same property. While Bird does not explicitly mention the role of other descriptions, such as descriptions of the constitution of the entity putatively referred to, presumably he does not consider them to be crucial to the referential success of the term.³⁷

³⁷ In Chapter Four, I shall use the same arguments against Psillos use of the inference to the best explanation against Bird’s use of the same inference. Against Bird’s use of the inference to the only explanation, I will attempt to show that Bird has not explained how we can know that we have actually arrived at the only explanation. The history of science has shown that an item of putative knowledge can be lost, and then reacquired in light of new evidence. Moreover, Bird is vague as to what is retained or discarded in revolutions. He mentions the appearance of new exemplars during revolutions, but relies on a causal theory of reference. Although he does not give as detailed an account of the causal-descriptive theory of reference as Psillos does, his theory seems to be very similar to the latter’s account. Thus, in Chapter Four, I shall use the same criticisms against Bird’s account of reference as I do against Psillos’ causal-descriptive theory of reference. Furthermore, I will attempt to argue that Bird seems to equivocate on the term “true”. He appears to be committed to the correspondence theory of truth, but he seems to be using the term in the pragmatic sense when he speaks of exemplars.

2.10 Conclusion

In this chapter, I have endeavored to describe the accounts of scientific knowledge as stated by the scientific realists Psillos and Bird, respectively. I chose Psillos' account since his is an example of the widely held justified true belief plus Gettier blocking fourth condition account of knowledge in general epistemology. I have tried to argue that, while Psillos never explicitly asserts that scientific knowledge is the analyzed in the form given above, his use of the term "scientific knowledge" in relation to justification, truth, and belief heavily suggest that this is his account of scientific knowledge. His treatment of justification as necessary for confirming that one has obtained truth (or truth-likeness) and as necessary for claiming that one has acquired a true (or truth-like) belief mirrors all the steps needed to acquire knowledge in general epistemology. Moreover, I have chosen Bird's epistemic approach to scientific progress since he follows Williamson's radical departure from the longstanding justified true belief account of knowledge with his knowledge as primitive term account of knowledge. Furthermore, I have attempted to argue that facticity is a feature of both Psillos and Bird's accounts of scientific knowledge.³⁸

³⁸ In Chapter Four, I shall attempt to contend that neither Psillos nor Bird's accounts of scientific knowledge meet the criterion of facticity. Since their accounts of scientific knowledge do not meet the facticity criterion, I will attempt to show that their respective accounts of scientific knowledge seem to fail.

Chapter 3.

Bas van Fraassen's Anti-realist Account of Scientific Knowledge

Bas van Fraassen is a leading figure against scientific realism and against justification. In the course of his arguing against various forms of scientific realism, van Fraassen develops his scientific anti-realist positions. In *The Scientific Image* (1980), he introduces *constructive empiricism*. In *Scientific Representation* (2008), van Fraassen modifies his views on semantics and adopts a form of structuralism called *empiricist structuralism*. In *Laws and Symmetry* (1989), van Fraassen attacks the notion of justification in traditional epistemology and he champions *probabilism*, which takes prior opinion to be the starting point of inquiry. Moreover, he argues for a *voluntarist* epistemology in *The Empirical Stance* (2002).

In this Chapter, I shall examine the implicit and explicit features of Bas van Fraassen's account of knowledge in his constructive empiricism, empiricist structuralism, probabilism, and voluntarism. While discussing these positions, I will describe van Fraassen's arguments against scientific realism, analytic metaphysics, the inference to the best explanation, and traditional epistemology. Since van Fraassen seemingly does not make a distinction between knowledge and scientific knowledge he appears to use the same account for both. Thus, I will refer only to his account of knowledge whether I am speaking of general knowledge or scientific knowledge. Since van Fraassen is the foremost contemporary scientific anti-realist I have chosen his account of knowledge as

an exemplar of a scientific anti-realist account of knowledge. Moreover, his general epistemology, which informs his philosophy of science, differs radically from mainstream epistemology because he argues against justification. Despite van Fraassen's significant departure from traditional epistemology, he still holds to facticity in his account of knowledge.³⁹

3.1 van Fraassen's Epistemology

3.1.1 Probabilism and Knowledge

As noted in Chapter One, van Fraassen's starting point for both his epistemology and his philosophy of science is his commitment to empiricism. While van Fraassen began propounding an empiricist philosophy of science before he developed his empiricist epistemology, I shall start my description of his thought with his epistemology rather than with his philosophy of science. In many cases, van Fraassen seems to have been creating the foundation for his philosophy of science through his epistemology only

³⁹ In Chapter Four, I shall argue against his positions in the philosophy of science and epistemology, and thus his account of knowledge appears to fail. In particular, I shall argue that the probabilism is undermined by the mode of hypothesis and thus seemingly cannot succeed from the start. Furthermore, I shall describe how van Fraassen's account of knowledge seems to fall short of facticity. Moreover, I shall use van Fraassen's views as a counterbalance to the respective scientific realist claims of Psillos and Bird leading to a draw between the three positions.

after developing his philosophy of science.⁴⁰ Since van Fraassen's epistemology is the foundation for his philosophy of science it will be clearer to begin an examination of the latter with the former.

As noted above, van Fraassen does not accept the framework of traditional epistemology which assumes that there is some rule or method that if one were rational, one would have to adopt some conclusion adjudged rational by the framework (2000, 274). For example, the *rationalists* assumed that deductive reasoning was the method to use to arrive at infallible knowledge. Descartes' method of doubting everything that could be doubted to arrive at least one item of infallible knowledge is one such method. Rather, van Fraassen focuses on the rationality of opinion and opinion change (2007, 337). This change in focus commits him to (1) a voluntarist epistemology, (2) probabilism, and (3) a permissive view of rationality. I shall describe each of these aspects of his epistemology below. His voluntarism stems from his agreeing with William James (1948) that we are motivated in the search for truth by the desire to believe truth and avoid error. There is nothing in the canons of logic that can tell us which of these attitudes to adopt over the other, or to what degree we should reconcile them. The epistemic enterprise cannot begin without a certain *stance* as a starting point. A stance is an attitude, commitment, approach, or any combination thereof. Clearly, van Fraassen's stance is epistemically cautious. He follows Blaise Pascal's probabilism which disregards traditional concerns with justification. To illustrate how his epistemology works, van Fraassen uses Bertrand Russell's (1912) response to external

⁴⁰ For example, only in his (2007) did he finally explain why the privileging of sense perception over instrumentation and inference is a rational choice as a starting point for epistemology. I shall return to this aspect of his epistemology and philosophy of science later in the chapter.

world skepticism. Russell notes that we do not come to the belief that the world is mind independent through argument, rather we cannot think of a good reason to reject this assumption. Thus, we cannot justify our opinion that the external world exists, but we are rational to continue holding this opinion because this belief seems to be reliable. According to van Fraassen, we assume our beliefs to be true and reliable, to hold beliefs we think to be false and unreliable would be irrational. Dissolving the problem of external world skepticism (or any skepticism) in this way, the epistemological focus has switched from justifying our beliefs to the rationality of changing opinion.

The form of rationality van Fraassen maintains is permissive. Rationality constrains what we can believe, but does not compel us to believe anything. In particular, rationality provides two constraints on belief. The first is logic, the belief must be consistent. Thus, one is not permitted to believe p and $not-p$. The second is that, when we create bold new hypotheses about the world, we can embrace these hypotheses if the evidence supports them, even if the hypotheses go beyond what the evidence permitted by previous opinion. For example, scientific realism is rational since the proponents of this stance are more inclined to believe truth than avoid falsehood. This position on rationality seems to lead to *irenical relativism*, or the view that (1) there is no objective standard for judging the rightness of an opinion, and/or (2) that all standards of rationality are trivial making truth a subjective matter (1989, 176). Moreover, one may object that this form of rationality leads to skepticism. For van Fraassen, “skepticism” is the dual claim that, whether objective truth exists or not objective truth cannot be obtained, and/or rational opinion is impossible.

Van Fraassen considers the possible charge of irenic relativism and claims in response that there is objective truth. Our opinions about the world are right or wrong, and what makes them right or wrong relies on how the world is. For example, I correctly have the opinion that I am seeing a cat on the mat if there really is a cat on the mat before me. While this notion of truth seems to suggest that truth is the relation of a statement to the world, van Fraassen maintains he is committed to the *disquotational theory of truth*. The disquotational theory of truth states that to assert a statement, and to assert that the statement is true, is redundant. Hence to say, ‘The cat is on the mat’ is true is to say that the cat is on the mat. However, whether or not any of our assertions are true is a matter of luck. Thus, van Fraassen maintains that it is rational to hold onto unjustified opinion, and that we cannot have an independent justification for our opinion. He contends that we start out, as in Neurath’s boat, with the opinions handed down to us by our parents, society, etc., and that we can only revise these opinions, not find some solid justificatory foundation for all of our beliefs.

Van Fraassen’s account of knowledge is true belief that is rationally held. According to his permissive version of rationality, *to be rational is to be logically consistent and not to ignore evidence contrary to one’s belief*. On this view, to hold a belief when there is convincing evidence against it is irrational. *Since van Fraassen argues that there is an objective world that determines whether or not a belief is true or false he is committed to facticity.*⁴¹

⁴¹ In Chapter Four, I shall argue that van Fraassen’s views on true belief and rationality are pragmatic rather than epistemic, and thus his commitment to facticity seems to be incoherent.

3.2 van Fraassen on Theory Change

In his (2002), van Fraassen seeks to provide a sketch of what empiricism is today as opposed to the earlier incarnations of empiricism that relied on ontologically suspect entities, such as sense-data. However, I shall concentrate on his discussion of revolutionary episodes in science. While van Fraassen does discuss opinion change in his (1989), in his (2002) he goes into detail on how one can rationally change one's opinion. The history of science provides a particularly interesting case of opinion change since the changes are so radical and asymmetric compared to everyday changes in opinion. As shall be shown below, from the previous point of view of a scientific theory, the new position being put forth is absurd, and the transition to it impossible to justify. Everyday opinion change (such as, changing one's belief that the weather is rainy rather than sunny) does not involve a radical change in one's worldview.

3.2.1 The Seeming Incoherence of Scientific Theory Change

According to van Fraassen, scientific realists hold to epistemologies that see the current scientific picture of reality as being true, and past scientific pictures of reality as false. However, scientific realists also maintain that they are open to their current scientific image of the world being false and overturned by an incompatible theory in the

future. Van Fraassen notes that this style of epistemology cannot countenance radical conceptual change. As we have seen in Chapter Two, Psillos' causal-descriptive theory of reference is a scientific realist attempt to block radical conceptual change during scientific revolutions. Incoherence arises if one assumes that they were rational to believe what is now seen as absurd. To eliminate the threat of incoherence, scientific realists attempt to ameliorate or dissolve the threat of radical conceptual change during revolutions.

The solution van Fraassen puts forth to this problem is to eschew the style of epistemology that assumes there is only one true way of viewing reality and that the epistemic position in question has acquired this one true way of seeing the world. As shown above, in place of such an epistemology, van Fraassen adopts a permissive theory of rationality and a voluntarist epistemology. The pursuit of epistemology is an enterprise with cognitive goals and a "volitional, intentional activity" (2002, 82). The pursuit of knowledge is an "enterprise" because we are finite, imperfect beings. While our knowledge on this account still depends on how the world is, this dependence relies on what we count as "successful". The criterion of success itself depends on what are our goals. Only after we have set ourselves clear goals can we begin to measure how much our enterprise centers on how well we have done something, how badly, and how lucky we were.

To illustrate how standards of adequacy apply he uses the example of mistakes in one's checkbook arithmetic leading to bouncing checks. Moreover, mistaken calculations can result in inaccurate weather predictions, with potentially greater consequences than bouncing checks. Only after we have decided we want our bank

accounts to be solvent, or that we want to know what weather to expect in the future, can we see what methods will get us to these goals most effectively, and which shall not.

3.2.2 Voluntarism and Scientific Theory Change

In terms of scientific revolutions, the standards for change become more complicated. Since van Fraassen takes a liberal view of rationality he can show how previous views were rationally permitted in retrospect. However, this liberal position on rationality still does not give an account of how revolutions take place. The account given by Kuhn (1970) is that two conditions must be met for a scientific revolution to occur: (1) the old framework deteriorates, but (2) is not abandoned until a suitable rival arrives on the scene. While van Fraassen accepts that the first condition for a revolution is often recognized at the time, in the form of disappointing empirical expectations, the latter condition is often not recognized at the time. This is because the rival is often seen as absurd by many who are committed to the currently accepted theory. For example, after the widespread acceptance of oxygen theory by the scientific community, Joseph Priestly was considered to be irrational and no longer a scientist because of his continued commitment to phlogiston theory (1970, 159).

The voluntarist approach to epistemology cannot help us to account for how scientists arrive at the second condition since we cannot choose to accept a new option unless it is seen as a genuine option. Clearly, there must be a transition from seeing the

new view as absurd, to entertaining it, to seeing it as a plausible option, then finally renouncing the old view and accepting the new. To be in a position to entertain the new one has to see the old view as deteriorating, and then a rival must appear.

For instance, Bohr's new model of the atom was absurd in the context of classical physics. However, his model could fit much of the recent experimental findings resistant to theoretical representation. For example, this new model of the atom was able to explain the lines observed in the spectral line emissions of various chemical elements. The success of Bohr's model raised many questions for the classical viewpoint: "When an electron passes from one stationary state to another, how does it choose which (lower energy) state to jump to? How does it decide at what frequency it is going to vibrate? How does a photon, emitted during such a transition, choose the direction to move?" (van Fraassen 2002, 67). If these questions could not be answered, then nature would appear to be indeterministic. However, according to the classical view, a theory is incomplete if it cannot show that the theory is part of a deterministic process. The reactions of top physicists such as Rutherford, Einstein, and Raleigh were "skeptical, dismayed, or dismissive" (2002, 67). Yet, none of them could deny the incredible empirical success of the model. Despite these initial reservations, this model was widely accepted, and questions of its intelligibility were mostly moot.

According to probabilist epistemology, the way one must proceed in a time of crisis has been answered by Blaise Pascal in his famous wager. Following van Fraassen's permissive view of rationality, one is not compelled to believe any one answer, so reason cannot provide an answer. Thus, one must weigh the available options and choose the option that provides the most value. Clearly, before any of the options

can be weighed, the options have to be genuine to the person deciding. Moreover, which decision one will make rests on her aversion to risk or willingness to take chances.

In the scientific context, a framework deteriorates when more and more obvious anomalies appear, calculations become more difficult, predictions fail, and ad hoc explanations are put forth. Then an alternative arises that seems absurd and transgresses common sense notions of what nature is like. To change this evaluation one needs to be forced into desperation by the previous view's anomalies enough to entertain alternative views. This consideration takes place not just in the individual scientist's mind, but within the scientific community. The acceptance of a new view within the scientific community is a matter of negotiation, confrontations and reconciliations, and not merely one of logic. The reason an individual scientist changes from one framework to another is that the stability of the old view is not worth the cost in anomalies, that the old framework's cost overshadows the costs taken in adopting a radically different viewpoint. Such a change cannot be rationally compelled, but is based on emotion. If the scientist can tolerate the frustration of increasing anomalies, then she can rationally hold to the old view. Hence, those who choose to attempt to fix anomalies in the old view rather than accept the new view cannot be derided for their decision. If she cannot handle these frustrations, then she can rationally move on to a radically new view as long as the new view fits the facts, and the new view provides novel experimental discoveries. Thus, not just any consistent new framework can come along and derail the old framework.⁴²

⁴² In Chapter Four, I shall argue that van Fraassen's view of theory change relies on a criterion of success similar to the criterion of success that scientific realists use to defend the epistemic value of theoretical virtues. Van Fraassen argues that the theoretical virtues are likely just pragmatic and not epistemic. However, I shall contend that his use of the criterion of success falls victim to the same criticism he aims at scientific realists, and that his view of theory change appears to be inconsistent.

3.3 Constructive Empiricism

Constructive empiricism holds that the proper aim of science is to construct *empirically adequate* theories. A theory is empirically adequate when everything the theory says about observable entities and events is true (1980, 12). The *constructive* aspect of constructive empiricism is in the practice of building models in science that, van Fraassen contends, need only have true descriptions of the *observable* phenomena, while the truth value of the description of unobservable phenomena is false since only observable entities exist. What is observable is what can be perceived with the unaided senses. For instance, Newton's model of the motions of the planets (with the exception of Mercury and the Moon) was empirically adequate because the model was predictively accurate and thus true. However, whether or not the hidden force of gravity that was posited to account for this predictive accuracy was real or not was left undetermined. The empiricism in constructive empiricism refers to van Fraassen's antagonistic stance towards explanation and *analytic metaphysics*, and his admiration for the scientific attitude of keeping an open mind. Analytic metaphysics is a sub-division of western philosophy that seeks to uncover the nature of reality. Different metaphysical theories posit different accounts of how reality is constituted. For instance, *materialists* claim that all reality is composed of matter in space and time, while *subjective idealists* hold that reality is made up of nothing more than sensations and the minds that perceive them. Van Fraassen sees himself as carrying on the *nominalist* tradition in philosophy against the Aristotelian practice of positing unobservable causes, dispositions, qualities, and so

forth to account for why the observable phenomena behaved as they do. The observable phenomena do not show a clear causal connection with any unobservable entities or processes. Van Fraassen mentions Molière's ridiculing of *virtus dormitiva* as an example of the superfluity of metaphysical explanations (1980, 2). Furthermore, he contends that the inference to the best explanation may not be reflective of scientific practice since scientists may be looking for empirically adequate theories, and not theories that give a true description of an unobservable world. Moreover, from a Bayesian standpoint the inference to the best explanation is incoherent. These arguments against the inference to the best explanation will be discussed more thoroughly in the section on the inference to the best explanation below.

3.3.1 Observability

As noted above, van Fraassen's philosophy of science is a reaction against analytic metaphysics. The motivation for this rejection of metaphysics stems from his epistemological commitment to empiricism. Since empiricism claims that we can only know what we can perceive, a key difficulty for van Fraassen is to distinguish between what is observable and what is unobservable. Van Fraassen notes that he is not marking out the boundary between observation terms and theoretical terms since all terms are theory-laden. He concedes, "All our language is thoroughly theory infected" (1980, 14). As noted on page 49 of Chapter Two, Hanson illustrated how merely looking at the sun at

dawn cannot provide conclusive evidence in favor of terra-centrism or heliocentrism. Following Grover Maxwell's (1962), van Fraassen takes the term "observable" to classify entities which may or may not exist. For instance, a leprechaun is observable, and that is why we are so confident such a creature does not exist. However, the number 72 is not observable, only the numeral 72. Furthermore, an act of observation is an act of perception without the aid of instruments such as telescopes or microscopes. A mathematical calculation is not an observation, as van Fraassen notes, "A calculation of the mass of a particle from the deflection of its trajectory in a known force field, is not an observation of that mass" (1980, 15). Rather, the calculation is a representation of an observed regularity that is used to predict phenomena. Thus, the calculation is not a representation of some unseen reality, but a device used to carry out experiments.

However, distinguishing calculations from perceptions is not enough to draw a sharp dichotomy between what is observable and unobservable. The predicate "observable" is vague, but van Fraassen contends that it is still viable because it has clear examples and counter-examples. For instance, a clear case of observation would be looking through a telescope at Mars, since astronauts would be able to see it very well from close up. But, the alleged observation of micro-particles in a cloud chamber is not a case of direct observation of micro-particles, if what the theory says about the phenomena is correct. The theory states that when a charged particle traverses a chamber filled with saturated vapor some atoms in the vicinity of its path are ionized. When this vapor is decompressed, and thus becomes supersaturated, then it condenses in droplets on the ions, hence marking the path of the particle. Van Fraassen compares the silver-gray line marking the path of the particle to the vapor trail left in the sky by a passing jet. In the

latter case, we may notice the trail first, and then look ahead of it to see the jet producing the trail. In the former case, we can observe the trail, but not the particle that purportedly produced it (1980, 16-17). Furthermore, for van Fraassen, observability is not constrained by what we can in principle observe, but what we can and cannot observe is constrained by physics and biology. Thus, he states, “It is these limitations to which the ‘able’ in ‘observable’ refers—our limitations, *qua* human beings” (1980, 17). For instance, certain wavelengths of light are not visible to the naked eye, such as infrared, because our eyes are not attuned to longer or shorter wavelengths outside of a certain range.

While van Fraassen is an empiricist, he contends that this privileging of the perceptible is epistemic or pragmatic, depending on context. Following Nancy Cartwright (2007), he argues that we have good reason to privilege the perceptible because the perceptible is what we experience and want to control in order to reach our goals. Our sensations affect us against our wills and we must react to them to preserve ourselves. He calls this position *commonsense realism*. Commonsense realism is a metaphysical view, but it is not metaphysical in the analytic sense that van Fraassen rejects. His understanding of metaphysics is that it supplies an interpretation of the world, rather than a true description of the world. Different interpretations lead to different contexts from which to judge the truth of claims about the world.⁴³

⁴³ In Chapter Four, I shall contend that van Fraassen’s epistemic voluntarism falls prey to the mode of hypothesis and his commitment is incoherent in light of his commitment to facticity.

3.3.2 The Inference to the Best Explanation

The main rule of inference used by scientific realists to support their position is *inference to the best explanation*. This rule states that when we are considering several hypotheses to explain the evidence we have, we should infer the hypothesis that is the best explanation of that evidence. Scientific realists contend that we follow this rule in ordinary cases, and, if we are consistent, then we will be led to scientific realism. For instance, if we hear scratching in the walls, the movement of little claws across the floor, and notice missing cheese, then we should conclude a mouse exists in the house with us. The scientific realist argues that this pattern of reasoning should also lead us to belief in unobservable entities.

However, van Fraassen argues that the scientific realists' supposition that we follow a certain rule of inference is a psychological hypothesis, and thus an empirical hypothesis to be tested. He then offers a rival hypothesis that, "we are always willing to believe that the theory which best explains the evidence is empirically adequate (that all the observable phenomena are as the theory says they are)" (1980, 20). Hence, van Fraassen can account for the many appearances of inference to the best explanation in the history of science. Moreover, he objects that the scientific realist needs an extra premise for his argument. This extra premise is that we are committed to the notion that every uniformity in nature needs an explanation. Following in the empiricist tradition of eschewing explanation, van Fraassen denies this premise (1980, 21).

Furthermore, the range of hypotheses offered may all be bad hypotheses. If we have only these hypotheses to choose from, then we do not have any reason to believe that the best among this bad lot is true. Moreover, we have no reason to believe that we have some special power for differentiating true from false hypotheses. Van Fraassen recognizes two arguments in support of the notion that humans have a unique faculty for the identification of true and false beliefs. The first idea is that we have evolved to choose the theories that are most true based on certain characteristics of the theory (such as, simplicity). However, there is no reason to think that true theories offered in the future will have the same characteristics as true theories stated in the past (a complex theory may be true and simple ones false). The second notion is that God created us such that we can differentiate truth from falsity, but it is not clear that God would have created us to be able to distinguish truth from falsity in all circumstances. Perhaps God only gave us the ability to distinguish moral truths from wickedness rather than truth from falsity in scientific matters (1989, 143-145).

Perhaps the most damaging of van Fraassen's criticisms toward the inference to the best explanation is that the inference is probabilistically incoherent. He begins his critique using Bayesian probability theory as his starting point. Following this theory, van Fraassen explains that an agent will assign a prior probability to some opinion and then correct the probability of the opinion in response to experience. According to proponents of the inference to the best explanation, after fixing the posterior probabilities of hypotheses, greater weight should be given to the hypothesis that is the best explanation of the phenomena in question. However, van Fraassen contends that if one follows this rule of belief revision, then one will rapidly encounter a situation where the

probability of the hypothesis that is the best explanation is not increased over other hypotheses. Rather, the hypothesis that is the best explanation becomes as likely as the rival hypotheses. Since assigning increased posterior probability to the best explanation does not increase the probability of that hypothesis van Fraassen argues that doing so is probabilistically incoherent. Thus, the inference to the best explanation is probabilistically incoherent.⁴⁴

3.3.3 The Scientific Realist Demand For Explanation

Scientific realists hold that the supreme criterion for theory choice is explanatory power. Explanatory power is the supreme virtue for the scientific realist because without it the regularities we observe in nature would be “miraculous”. While van Fraassen agrees with scientific realists that explanatory power is a criterion in theory choice, he does not agree that it is the supreme criterion. According to van Fraassen, the argument that a theory’s explanatory power is needed to dispel the miraculous nature of regularities leads to absurdity.

Van Fraassen begins his argument against explanation by examining Hans Reichenbach’s (1958) *principle of the common cause*. This principle states that if two

⁴⁴ In Chapter Four, I shall give a sketch of Psillos’ response to van Fraassen’s argument that the inference to the best explanation is probabilistically incoherent. Psillos’ argues against van Fraassen’s claim that scientific realists use Bayesian theory alone to fix the posterior probabilities of rival hypotheses. Both Psillos and Bird’s counter-arguments to all of van Fraassen’s challenges to the inference to the best explanation rely on the epistemic value of theoretical virtues. I will conclude that, since Psillos and Bird appear not to have shown that theoretical virtues are not merely pragmatic but epistemic, they seem to have failed to have defeated van Fraassen’s challenges to the inference to the best explanation.

observable events occur more frequently than they would independently of each other, then there exists a common cause for both events. For instance, there is a high statistical correlation between frequent smoking and lung cancer. To account for why smoking seems to be correlated to lung cancer it is necessary to posit an unobservable common cause for the cancer (1980, 25-26). Reichenbach argued that an unobservable common cause must be posited to explain observable events since observable events do not often have the same observable cause. For example, genes are posited to explain the unobservable transmission of observable traits from one generation to the next. Thus, most scientific explanations would be impossible without unobservable entities, and since (for the scientific realist) explanation is the aim of science, then this aim can only be realized if these entities actually exist. As shall be shown below, while van Fraassen does not believe that this principle should be universally applied in science, he does argue that it can be coherently applied in some instances without a commitment to scientific realism.

The principle of the common cause cannot be applied as a general principle because it requires that the universe be deterministic. With the advent of quantum mechanics such a view of the world is no longer viable. According to the Heisenberg Uncertainty Principle, both the location and the velocity of a particle cannot be measured at the same time, thus definite values cannot be given in quantum physical experiments. Without definite values the trajectory of a particle can only be measured statistically and not deterministically. In order to account for the lack of definite values in quantum physical experiments, and make physics deterministic again, *hidden variables* are posited by some physicists. For instance, The De Broglie-Bohm theory posited a hidden guiding

wave that determined the motion of sub-atomic particles. Since the waves are represented in multi-dimensional space instead of three dimensional space, they are considered to be abstractions and not as physically existing. David Bohm concedes that he has not shown that these waves exist, rather he hopes that his posit will facilitate the discovery of an underlying reality behind quantum phenomena. However, van Fraassen notes that hidden variables demand an explanation too, leading to a regress of explanation unless the demand is dropped. Despite the lack of explanation for quantum phenomena, quantum mechanics is accepted as a successful branch of physics because of its impressive predictive power. Therefore, the demand for explanation ceases to be the ultimate criterion for theory choice (1980, 30).

While van Fraassen concludes the principle of the common cause should not be used as a general principle, he argues that there are two ways it can be used in science. The first is where applying the principle aids in acquiring greater knowledge of what is observable. For instance, when past frequent smoking is posited as the cause of lung cancer, this hints there is a further correlation between cancer and irritation of the lungs, or that chemicals such as nicotine are present in the bloodstream, or both. The postulation will be justified if such hinted further correlations are discovered, and if the postulation aids the discovery of larger scale correlations among observable events. In this sense, the principle is a *tactical rule*. The second way the principle can operate is as advice for the construction of theories and models. A model can be constructed for a set of observable correlations to posit hidden variables with which observed variables are specifically correlated. The hidden variables in quantum mechanics are not taken to be actually existing entities, but as mathematical objects. As van Fraassen states, “This

[positing of hidden variables] is a theoretical enterprise, requiring mathematical embedding or existence proofs. But, if the resulting theory is then claimed to be empirically adequate, there is no claim that all aspects of the model correspond to ‘elements of reality’” (1980, 31). In this sense, the principle is a *theoretical directive*. Therefore, whether the principle is a tactical rule or theoretical directive it is not a demand for explanation. A demand for explanation would create the superfluous metaphysical weight of hidden variables that provide no new discoveries (1980, 31).

3.3.4 Putnam’s Ultimate Argument for Scientific Realism

What Psillos calls the “no miracles argument”, van Fraassen calls the “the Ultimate Argument” for scientific realism (Cf. Ch.2, pp.43-46). Van Fraassen muses that perhaps Putnam’s argument is the final explanation that the scientific realists seek. Since “the Ultimate Argument” claims that theoretical terms must refer to unobservable entities and processes, van Fraassen notes this argument’s claim that the structure of nature is mirrored by the structure of ideas is a traditional realist argument. However, while he does not think that science operates as the result of “miracles”, he also does not accept the Ultimate Argument. Instead, he reformulates the question as “why we have successful scientific theories at all” (1980, 39), and gives a “Darwinian” answer. Competition among scientific theories is analogous to biological phenomenon in which an organism (or theory) facilitates its interaction with the environment (1980, 39). Those

organisms that do not correctly perceive dangers in their environment become extinct, and so it is with scientific theories. Only those scientific theories that have discovered actual regularities in nature survive competition with rival theories (1980, 39).⁴⁵

3.3.5 The Empirical Content Of Theories

Constructive empiricism adopts the *semantic approach* to theories where a theory is a family of models. This view is in opposition to the *syntactic* theories of models where a theory consists of a body of theorems stated in a language chosen to express that theory. In the former approach, the language used to express a theory is not unique since the same collection of structures could be described in drastically different ways. For example, the Bohr model of the atom does not refer to one specific structure, but to a type of structure, or class of structures, all with the same general characteristics. Thus, the Bohr model is applicable to hydrogen atoms, carbon atoms, and so on.

To better illustrate the semantic approach to theories van Fraassen uses Newton's attitude towards what he observed and the reality he posited in his *Mathematical Principles of Natural Philosophy* and *System of the World*. For example, Newton distinguished between the true and apparent motions of bodies. *True motion* is whatever

⁴⁵ In Chapter Four, I shall argue that van Fraassen's Darwinian explanation for the success of science does not succeed in defeating the challenge posed by the "no miracles argument". While his explanation putatively works within the framework of his empiricism, his explanation seems to fail because empiricism relies on the mode of hypothesis. Moreover, he does not account for how past theories that seemed to be empirically adequate and successful, such as the Ptolemaic model of the universe, are now seen as false. I shall argue that his likely response would rely on a distinction between reality and appearance similar to his scientific realist opponents.

the real motion of an object may be, and *apparent motion* is relative to an observer. The apparent motions are defined by time intervals, angles of separation, and measuring relative distances. These relational structures van Fraassen calls *appearances*.

According to the mathematical model supplied by Newton's theory, bodies are located in Absolute Space, where they have real or absolute motions. Absolute Space is a posit and Newton did not believe it to be real. As van Fraassen notes, "within these models we can define structures that are meant to be exact reflections of those appearances, and are, as Newton says, identifiable as differences between true motions" (1980, 45). Newton's theory has some model for every appearance that is isomorphic to motions in that model, and is thus empirically adequate.

According to the syntactic approach to theories there is an *observational vocabulary* and a *theoretical vocabulary*. The empirical significance of a theory is matched with the set of its observational, or testable, consequences. However, van Fraassen argues that this view of the empirical significance of a theory is mistaken since the empirical consequences of a theory cannot be isolated by delineating observational from theoretical terms. If such a demarcation could take place, then the set of the theory's theoretical terms could not be cashed out in observable terms since the theoretical term will differ from the observable term by the former's lack of observable characteristics. However, this is clearly not the case because a theory's unobservable terms are described using the observational vocabulary. Van Fraassen notes that, "The quantum theory, Copenhagen version, implies that there are things which sometimes have a position in space and sometimes have not. This consequence I have just stated without using a single theoretical term" (1980, 54). Moreover, he mentions that Newton's

Absolute Space, which has neither position nor occupies a volume, can be imagined in terms of what is observable. Thus, what counts as an observable term or a theoretical term is based on context. The greatest mistake the syntactic approach made was assuming that there could be a pure observation language. Such a language would have to be theoretically neutral, but even natural language has presuppositions that give a context through which to describe the world (1980, 54-56).

3.3.6 Underdetermination

As noted in Chapter One on pages 19-20, underdetermination arises in quantum mechanics. While underdetermination is a problem for a scientific realist, it seems not to be a problem for the constructive empiricist. For example, the experimental results for both non-relativistic quantum mechanics and quantum field theory are the same. Underdetermination is not confined only to the foundations of physics, but to experimental physics. For instance, depending on the convention codes for visual representation and the instruments used, a scanning tunneling microscope produces images of atoms that are conical in structure. However, the same instrument using different convention codes will produce representations of atoms as spherical in structure (Bueno, forthcoming). The observable phenomena are the same for each theory and constructive empiricism is not concerned with the truth or falsity of what a theory posits beyond the observable phenomena, but only with the theory's empirical adequacy (1980,

64). Thus, whether or not the world is fundamentally composed of particles or waves, or if instruments create or discover conical or spherical atoms, does not matter to a constructive empiricist. Rather what matters is that a theory can account for all of the measurements and experimental results that are observed with our unaided senses. If a theory meets this criterion, then the constructive empiricist will consider the theory to be a good one. If one or more theories are equally empirically adequate but make different claims about unobservables, then they will be considered good theories too. A constructive empiricist can choose which theory to use based on other considerations than empirical adequacy, such as simplicity or explanatory power.

3.3.7 Unification and Conjunction

In practice scientists seem to strive for theories that can be applied in various domains. These domains do not just include areas that are within the scope of one science, but also between scientific fields such as physics and biology. The practice of applying the same scientific theory to disparate phenomena within or between scientific fields is called *unification*. An example of unification within a scientific field is the search for a unified theory in physics that will combine a theory of gravity that does not break down at the quantum level. The currently accepted theory of gravity, general relativity, works well to predict the motions of large objects, such as stars and galaxies, and medium sizes objects, such as cars and bullets, but not the movements of sub-atomic

particles. An illustration of unification between scientific theories in different scientific fields is molecular biology which combines biology and chemistry to study the molecular basis, such as DNA, of biological activity, (e.g. inheritance).

According to classical logic, the conjunction of two true statements is true:

A

B

Therefore, *A* & *B*. (1980, 85)

Scientific realists assume that theories that are true should be able to be unified following the rule of conjunction. The successful unification of putatively true theories seems to confirm the scientific realists' intuition that true scientific theories accurately represent an unseen reality. The successful combination of theories covering disparate phenomena within or between different scientific fields seems to show that successful scientific theories reveal a unified underlying reality. Furthermore, scientific realists, such as Hilary Putnam, argue that scientific anti-realism cannot account for why the conjunction of scientific theories is a standard scientific practice and why it is often successful.

According to scientific realism, unification is evidence for their belief that the world is a coherent whole. Clearly, the assumption that the world is a coherent whole is also what causes underdetermination to be a problem for the scientific realist.

Van Fraassen responds to the challenge that scientific anti-realism cannot give a coherent account of unification by noting that on his view successful scientific theories are empirically adequate, not true. According to scientific realism, a successful scientific theory is comprised of discretely true statements. However, on van Fraassen's view, a scientific theory is a family of models and not composed of discretely testable sentences. To be empirically adequate only one model that purports to represent observable

phenomena needs to correspond to the observable phenomena, but the models that putatively represent unobservable phenomena do not correspond to anything. While a theory may have syntactic content, the sentences in the theory are not separately testable for empirical adequacy. Thus, the empirical adequacy of a scientific theory is defined by the correspondence of at least one of the theory's models to all of the observable phenomena. The model may represent the observable phenomena mathematically or visually and not necessarily syntactically. Thus, the conjunction rule in logic does not apply to van Fraassen's account of theories since scientific theories are not comprised of discretely testable sentences organized into arguments. In order to successfully conjoin two incompatible theories all that is necessary, on van Fraassen's account, is that the union of the two theories is empirically adequate.

Moreover, van Fraassen argues that the scientific realist account of conjunction as the combination of two true theories is simplistic and does not reflect scientific practice. Scientists do not assume that merely uniting two true theories will automatically deliver a true conjunction just because the rule of conjunction states this must be the case. Furthermore, the scientific realist view assumes that the goal of science is to pursue truth, but oftentimes the unification of theories occurs for practical reasons. Van Fraassen uses the example of combining physiology and physics to illustrate his point. To practice Earth-bound physiology successfully a physiologist must take into account the effects of gravity on the tensing of various muscles in various postures. However, difficulties arise when a theory needs to be devised to account for the motion of a person in a spacesuit walking across the surface of the Moon. Unless the physiological and physical theories have models in common that can be applied to this context, then one must either abandon

the attempted conjunction or create a theory that can cover the inanimate suit and the living person inside the suit on the surface of the Moon. The physical theory used is deterministic since relativistic corrections are not necessary for physiological phenomena. As van Fraassen states, “Physiologists need not make relativistic corrections in their mechanical calculations, and can treat almost all processes deterministically (and some stochastically which physics implies to be near deterministic” (1980, 87).

Thus, our interests compel us toward unification, not necessarily a search for an underlying coherent whole. Van Fraassen notes that, in practice scientists rely on many “mini-theories” rather than global theories. While van Fraassen speculates that the world may not be a coherent whole, he does state that unification of mini-theories is an aim of empirical adequacy. Moreover, in practice the theories are not combined wholesale. Instead, adjustments are made to both theories to make them fit together. All that is needed to combine theories to reach our goals is to find some models in common, thus not every model in each theory must be compatible with each other to be successful. Hence, van Fraassen states that, “the process of unification is mainly one of correction and not of conjunction” (1980, 87).⁴⁶

⁴⁶ In Chapter Four, I shall describe how Psillos uses the conjunction of theories in science to argue against van Fraassen’s notion of *acceptance*. The concept of acceptance will be sketched later in the section on pragmatic virtues. Furthermore, Psillos contends, against van Fraassen, that belief in the truth of a scientific theory’s unobservable posits is preserved under conjunction while empirical adequacy is not. Moreover, Psillos argues that belief in the conjoined theories’ unobservable posits lead to greater empirical success. He holds that the inference to the best explanation accounts for the success of conjunction. Psillos then critiques van Fraassen’s arguments against the inference to the best explanation and concludes that these arguments lead to unintended skeptical consequences for van Fraassen. If Psillos’ arguments are sound, then van Fraassen’s notion of acceptance is untenable, and his rejection of the inference to the best explanation leads to unwanted skepticism.

3.3.8 Metaphysical baggage

As mentioned earlier, van Fraassen holds to the semantic view of theories. On this view, the models represent empirical phenomena in terms of experimental results and measurement reports called *appearances*. For example in classical mechanics, to represent the appearances a theory lays down principles as to which mathematical operators will represent energy, momentum, position, and so on. A theory is empirically adequate if it has a model that is isomorphic to the appearances represented by that model. In the course of representing appearances idealization occurs, but empirical adequacy is preserved in the process of idealization. For instance, as Cartwright (1983) has shown, Snell's Law states only one ray of light is refracted through isotropic dielectric media, such as glass. However, most media, such as crystals, are anisotropic. When light passes through anisotropic media two rays of light are refracted very close to each other. Since the refracted rays are very close to each other accurate calculations can still be made using the law as stated despite the lack of isomorphism between what is observed and the law. According to van Fraassen, 'empirical adequacy, like truth, is "preserved under watering-down"' (1980, 67).

Since van Fraassen recognizes the need for idealization in theory building and claims that empirical adequacy can be preserved through idealization he does not hold that we should avoid idealization and the watering-down of empirical adequacy. Rather, he argues that we should create theories that posit unobservable entities and processes because this practice often leads to further fruitful results. Van Fraassen calls the posited

unobservable entities and processes *metaphysical baggage*. Any theory without metaphysical baggage (i.e. non-observable claims) would not be sophisticated. To be a sophisticated theory “metaphysical baggage” is necessary to allow for detours through theoretical variables to arrive at viable descriptions of the phenomena. For instance, Newton’s gravity was considered “metaphysical baggage” during his lifetime, but this attitude was dropped because of the posit’s successes, such as the discovery of Neptune. However, when the detour leads to practical gain it is not considered “metaphysical baggage” anymore, rather the term is used only for those detours that do not yield any practical gain.

Yet, even the useless baggage can be intriguing since it may be useful in the future. For instance, the “hidden variables” theories in quantum mechanics may lead to fruitful results. From a mathematical point of view hidden variable theories are equivalent to orthodox quantum theory in their representation of observables. Considered in this way, theories in this sense are empirically equivalent. Except that the hidden variable models have extra structure, now seen as “metaphysical baggage”, but which could be useful if radically new phenomena appear in the future (1980, 68). For example, if hidden variable proofs could give a deterministic prediction of the trajectory of a particle, then hidden variables would no longer be considered “metaphysical baggage”.⁴⁷

⁴⁷ In Chapter Four, I shall contend that van Fraassen’s position on unobservable entities and processes as “metaphysical baggage” can be counterbalanced by Psillos’ arguments that unobservable entities and processes must exist to explain the success of science. I will argue that the seeming strengths and weaknesses of these positions lead to a draw between the two views.

3.3.9 Constructive Empiricism and Skepticism

One might assume that since constructive empiricism will not accept what a theory says about what is unobservable as true that, by parity of reasoning, constructive empiricism never allows evidence to warrant what goes beyond it. Van Fraassen denies this claim. As shall be discussed in detail in the section on van Fraassen's epistemology, he abandons traditional epistemology's project of justifying the beliefs we already have and allows us to hold onto our prior opinions as long they are consistent with our other beliefs. Moreover, if our prior opinions do not fit the world, then they must be discarded otherwise the subject holding the opinion is irrational. For instance, if one held that mountain gorillas were mythical creatures, then one would be irrational to hold that opinion after being shown a mountain gorilla at the zoo or in the wild. Thus, van Fraassen contends that since in everyday life we infer conclusions beyond the evidence we have (such as inferring that footsteps in the sand were made by an unseen person), and will defend this practice against any philosophical theory that labels us irrational only for that reason.

Another assumption one may make is that van Fraassen considers observable objects and processes to be posits, to be believed because they do the best job of explaining and systematizing the sense-data which is ultimately the only evidence for the world that we have. He rejects this assumption since the notion of sense-data has been refuted. Moreover, he remarks that sense-data are not our most fundamental evidence, but are themselves posits. As van Fraassen states, "such events as experiences, and such

entities as sense-data, when they are not already understood in the framework of observable phenomena ordinarily recognized, are theoretical entities” (72). However, has been noted earlier in this chapter, van Fraassen (2006) claims that the privileging of the perceptible may be epistemic or pragmatic in different contexts.⁴⁸

3.3.10 Empiricist Methodology

According to van Fraassen, “The real importance of theory, to the working scientist, is that it is a factor in experimental design” (1980, 73). This view is in opposition to the scientific realist approach where the aim of science is to know the structure of the world, particularly the unobservable structure that is the putative cause of observable phenomena. Thus, the main effort for the scientific realist is to construct theories that describe this structure. Experiments are designed to test theories for their truth or falsity. However, for van Fraassen, scientists aim to discover regularities in the observable part of the world, not to find truth. In order to make these discoveries experiments are necessary, not mere reasoning. These regularities can be complex and far from obvious, hence experimental design can be extremely difficult. Therefore, theories are required to help with experimental design.

Theories are used in experimental design in two ways. The first is that of formulating and answering questions raised about the observable phenomena. The

⁴⁸ In Chapter Four, I shall argue that van Fraassen’s contextualist privileging of the perceptible is inconsistent with his commitment to objectivity and to facticity.

second is where background theories are used to facilitate the design of an experimental apparatus. Van Fraassen emphasizes the second aspect. Experimentation is significant in the construction of theories in two ways. The first is testing the empirical adequacy of a theory. The second is answering questions about the observable phenomena such that this activity guides the further construction and completion of the theory (1980, 73-74).

To illustrate his view, van Fraassen notes episodes in the history of science where experimentation was used to test the empirical adequacy of a given theory:

Dominic Cassini's attempt to measure the curvature of the earth in order to adjudicate between Newtonian and Cartesian physics, Halley's prediction of the comet's return and its observation, the famous watch at the eclipse that bore out Einstein's theory implying the deflection of light rays in the gravitational field. This sort of experimental activity fits neatly into the empiricist's scheme, for clearly it is designed to test claims of empirical adequacy. (1980, 74)

However, this sort of experimentation is not the kind used when we speak of discovery. In some cases a theory states that there must be some entity or value, answering to certain conditions, and that entity's nature is discovered by experimental scientists. For instance, Darwin's theory implied that there were "missing links" in the evolutionary chain. A search for these links commenced which led to surprising discoveries, such as Java man and Peking man, which were still in agreement with the theory. Clearly, missing links are observable entities, so the question remains how the "discovery" of unobservables works in physics if constructive empiricists will not believe in them.

To answer this question, van Fraassen cites the measuring of the charge of the electron by Robert Millikan. Using the theory of electricity as his background for developing his experimental apparatus, Millikan connected brass plates to a battery and switch arrangement, which could be used to create an electrical field of strength between

3,000 and 8,000 volts per centimeter between the plates. Van Fraassen is quick to note that when he is using the terminology of the background theory of electricity he is doing so on the macro-level. Hence, he interprets “this field is created” as meaning a reading on a voltmeter. Dropping oil onto the lower plate in the apparatus, Millikan observed that when the field was on, some of the droplets would rise to the upper plate. When he short-circuited the plates just before the drop hit the roof, the droplet would fall.

The background theory stated that we could expect some droplets to rise when the field was switched on, since some of the droplets would receive a charge due to friction. The theory also states that there may be variations in the speed of rising, and that this speed will sometimes be zero (in this case, the droplet hovers over one place). According to the background theory, the droplet hovers because it may catch an ion from those already existing in the air. Thus far, the experiment is empirically adequate, as van Fraassen states, “what is happening fits well into various models provided by the theory, since all these variations are observed in some droplets” (1980, 76).

Now we can use the established part of the theory and observations on the speed of rising, to calculate the charges on the droplets. Van Fraassen shows how this experiment filled in the value for a variable in an established theory:

But we can furthermore use the established part of the theory, and the observations on the speed of the rising, to calculate the charges on the droplets. The apparent mass of the droplet is the difference between the actual mass and the buoyancy of the air; call this m . Let its charge at a given time be e , its speed under gravity v , and its speed when the electric field F is on w ; the relation among these quantities are given by the equation

$$\frac{v}{mg} = \frac{mg}{Fe - mg}$$

(1980, 76)

Because all of the other variables except e are known, we can calculate e .

Van Fraassen describes how Millikan measured the charge of the electron below:

When a variation in rising speed occurs, this must then be attributed to a change in the charge, from e to e' , say. If electrical charge comes only in multiples of a unit u , the charge on the electron, there must be a number k such that $e' - e = ku$. Gathering sufficient data of this sort, Millikan arrived at the mean value for u , which is very close to the one at present accepted. (1980, 77)

In van Fraassen's telling of the story, Millikan was filling in a value for a quantity which had thus far been left open in the construction of the theory. As van Fraassen states, "Hence, in this case, *theory construction consists in experimentation*" (1980, 77). One could use the terminology of discovery for Millikan's results, but the constructive empiricist position on this episode is that he was writing theory through his experimental apparatus. In cases such as these van Fraassen asserts, "*experimentation is the continuation of theory construction by other means*" (1980, 77).

I have included van Fraassen's position on experimentation to describe his alternative to the scientific realist claim that science discovers an unseen world through experimentation.⁴⁹

3.3.11 The Pragmatic Virtues

Empirical adequacy is not the only criterion one has for choosing a theory. Other virtues a theory can have are mathematical elegance, scope, consistency, simplicity, explanation, and unification. For the scientific realist, judgments concerning simplicity

⁴⁹ In Chapter Four, I shall use van Fraassen's view on experimentation as a counterbalance to the scientific realist take on experimentation as an activity of discovery.

and explanatory power are *epistemic virtues* because they are considered the most intuitive avenues for the expression of epistemic evaluation. Van Fraassen sees the value placed on these latter two judgments as evidence of our interests, and not as criteria for gauging the truth content of a theory. Thus, all of these virtues reflect our interests and are *pragmatic virtues*, and not epistemic virtues.

For van Fraassen, pragmatic virtues are often sought in theory choice because they lead to empirically adequate theories. Often the theory with the most pragmatic virtues is the one that best fits the observable phenomena. For example, Darwinism had more pragmatic virtues than Natural Theology and thus supplanted the latter. Natural Theology assumed that the Earth was created 6,000 years ago by an omniscient, omnipotent, omnibenevolent God. However, geological findings, such as the slow rate of geological change and the fossil record revealing the existence of extinct species, contradicted the Biblical account. The Earth's mountains, valleys, coastlines, etc. could not have formed in the short timeframe given by the Bible. Furthermore, the extinction of species contradicts the notion of an all-loving God who creates all creatures with a purpose. Darwin was able to do away with these inconsistencies by abandoning the Bible as a starting point of scientific investigation. With the removal of God, Darwin could create a theory that was empirically adequate because it could be unified with observed phenomena in geology and had greater explanatory power than Natural Theology.

As noted in the first paragraph of this section, van Fraassen considers the putatively epistemic virtues of simplicity and explanation as being pragmatic virtues. He does not see why the world should be assumed to be simple rather than complicated, thus van Fraassen does not consider simplicity to be an indication of truth. Rather, we choose

simple theories because they are easier to understand and to use. For instance, Newtonian mechanics was used instead of Einsteinian mechanics to send astronauts to the Moon because the former has simpler equations than the latter. Yet Newtonian physics is regarded as false in terms of Einsteinian physics. Moreover, van Fraassen sees our drive to seek explanations as an expression of our desire to have a certain kind of question answered more than another, but we have no reason to think that the theory that can answer that question is closer to being fully true than the theory that cannot provide an answer. For example, Newton's theory of gravity was adopted because of its predictive accuracy though Newton could not explain what gravity is, especially within the framework of mechanistic philosophy. According to mechanistic philosophy, causation only occurred when two objects physically came into contact with one another.

However, as Newton's theory became more entrenched the notion of action-at-a-distance was taken on as a legitimate explanation for how gravity operates. Since the notion of action-at-a-distance fit the observed phenomena the mechanistic view was abandoned, and a new view was adopted that allows for action-at-a-distance. But, current physics does not accept the Newtonian description of gravity as true or even approximately true. According to Einsteinian physics, gravity is a curvature in space-time caused by the mass of an object, not a force separate from space and time. Thus, while scientists in the 18th and 19th centuries had a satisfactory explanation for what gravity is, this explanation is now seen as false. The Newtonian explanation for gravity led to the cessation of wonder among its adherents, but did not show that the theory was true.

Thus, theoretical virtues do not lead to true theories, rather theories are chosen on the basis of these virtues because they help facilitate scientists' interests. As shown above, a theory can provide a satisfactory explanation and later be found to be false. Explanatory power is no guarantee of the truth of a theory. Furthermore, a theory that is considered false may still be used because it is simpler to use than the reigning theory. The more a theory has these virtues, the more useful it will be. If it unifies phenomena, then it has a wider scope of application. Simple theories require less cognitive effort to apply than complex theories. Clearly, if a theory has both of these qualities, then it will be more desirable than a theory that lacks one or more of these qualities. Therefore, those theories that include a greater number of pragmatic virtues will be more attractive than those that lack them.

According to van Fraassen, the combination of empirical adequacy and pragmatic virtues does not show we should believe the unobservable parts of a theory are nearly true, rather this combination shows that we should *accept* the theory. Acceptance involves believing what a theory states about the observable world to be true, but not what the theory states about a putatively unobservable world as being true. Moreover, acceptance is a commitment to use the theory to account for, manipulate, and predict new phenomena. For example, the application of Newtonian theory was expanded beyond the movement of planets and projectiles to the movement of water in hydrodynamics. Those scientists committed to Newtonian mechanics bet that the theory could be applied successfully to predict and manipulate the movement of water. To put one's faith into a theory's ability to account for and control new phenomena is not a matter of truth or falsity. Rather, accepting a theory means having an optimistic attitude that the theory one

is committed to will be successful when applied in new domains. Optimistic or pessimistic attitudes are not true or false; instead they are vindicated or not vindicated by future events.

Therefore, theoretical virtues may not be, as the scientific realist claims, indicators of truth of the entire theory and reasons to believe in the truth of all parts of a theory. Instead, they are reasons to accept a theory as being true of what is observed and as useful in attaining our goals. Hence, the theoretical virtues are not always epistemic, but they are pragmatic.⁵⁰

3.3.12 Explanation

For the scientific realist, explanation is the supreme virtue of a theory. As shown in Chapter Two, scientific realists assume that the inference to the best explanation uncovers a hidden world beneath the appearances that causes observable phenomena. Hence, when a successful scientific theory has explanatory power then the scientific theory has accurately represented the world. The accurate representation of the world is synonymous with truth and, according to scientific realism, uncovering what is true of the world is the goal of scientific inquiry. Since the attainment of truth or knowledge is the goal of science, and explanation acquires truth, then explanatory power is the highest virtue of a theory. Thus, those theories with the greatest explanatory power should be

⁵⁰ In Chapter Four, I shall use van Fraassen's view to counterbalance the scientific realist position that theoretical virtues are not merely pragmatic, but epistemic.

believed rather than competing theories with less explanatory power. Clearly, scientific realism has a straightforward criterion for rational theory choice.

However, van Fraassen argues that the explanatory power of a scientific theory is not an indicator of the theory's truth. In his (1977) he contends that scientific realists hold to three false ideals of explanation in the philosophy of science. In his account of explanation, van Fraassen (1977) attacks three false ideals of explanation in the philosophy of science. The first is that explanation is simply a relation between a theory and phenomena. This view of explanation is similar to the correspondence theory of truth, which is an ideal of truth. Van Fraassen seems to hold to this ideal of truth when he points out that, "a statement is true exactly if the actual world accords with this statement" (1980, 90), however, as shown above, he claims to hold to a disquotational theory of truth. Just as a statement's truth consists in its "mirroring" the world, a theory's explanation "mirrors" the phenomena.

The second false ideal is that explanatory power cannot be logically detached from other virtues of a theory, particularly truth and acceptability. For instance, Darwin recognized that his theory of natural selection was false, but was still explanatory, "It can hardly be supposed that a false theory would explain, in so satisfactorily a manner as does the theory of natural selection, the several large classes of facts above specified" (1977, 143-44). Furthermore, van Fraassen notes that acceptability is not the same as truth. For example, Newton's theory of gravity was accepted though it could not account for the perihelion of Mercury. The inability to fit this anomaly with the theory is a failure of empirical adequacy of the theory, and thus the theory is false. Yet, the theory was still accepted. Moreover, 'Huygens theory explained the diffraction of light, Rutherford's

theory of the atom explained the scattering of alpha particles, Bohr's theory explained the hydrogen spectrum, Lorentz's theory explained clock retardation' (1980, 98 in Monton 2014). All of these theories are now seen as being false.

The third ideal, mentioned in the paragraph above, is that explanation is the supreme virtue of science and the end of scientific inquiry. Rather than rely on explanation as the supreme virtue of theory, van Fraassen maintains that empirical adequacy is the supreme virtue of a theory. A virtue, he argues, can be dominant in one of two ways. The first is that it is the basic criterion of acceptability. For example, the theory is consistent with the facts in the domain of application. Explanation does not work that way, otherwise a scientific theory would not be acceptable at all unless it accounted for all facts in the theory's domain. The second way is that of being necessary when this can be accomplished. In other words, if two theories succeed in terms of other virtues (simplicity, empirical adequacy) equally well, then the one which accounts explains more must be accepted.

However, quantum theory is an extremely successful theory and has enormous explanatory power. The latter seems to be a virtue except that in explaining so much hidden variables must be introduced to correct the indeterminism. The indeterminism appears because the more exactly the position of a particle can be measured, the less exactly can one measure the particle's momentum and vice versa (Hilgevoord and Uffink, 2006). The introduction of hidden variables was attempted by many physicists, including Einstein, to give quantum theory deterministic laws and increased explanatory power. Yet, according to the received Copenhagen interpretation of quantum mechanics, hidden variables are an ad hoc hypothesis. Thus, van Fraassen concludes, "...hidden variables

are rejected in scientific practice as so much ‘metaphysical baggage’ when they make no difference in empirical predictions” (1977, 144).⁵¹

3.4 Empiricist Structuralism

In his (2008), van Fraassen introduced empiricist structuralism. *Structure* in the philosophy of science refers to the mathematical content of scientific theories. *Structural Realism* holds that the only theoretical knowledge possible is to be found in the mathematical structure of scientific theories. Hence, this view is not committed to belief in the unobservable entities and processes, such as atoms and genes, as they are described in scientific theories. *Empiricist structuralism* rests on two theses:

1. Science represents the empirical phenomena as embeddable in certain *abstract structures* (theoretical models).
2. Those abstract structures are describable only up to structural isomorphism (2008, 238).

The abstract structures van Fraassen refers to are mathematical structures. The mathematical structures that represent the empirical phenomena are chosen and used by a particular person. Thus, the only relation between the model and the phenomena is what the person using the model in question has chosen the model to represent. For example, mathematical models used to represent the structure of harmonic motion can be used in

⁵¹ In Chapter Four, I shall use van Fraassen’s account of explanation as a counter-balance to the scientific realists’ view on explanation as the supreme virtue of a theory.

relation to vibrations in electromagnetic radiation and vibrations in a diatomic gas molecule. Furthermore, van Fraassen introduces a distinction between appearances and phenomena. *Phenomena* are observable entities, objects, and processes. *Appearances* are how the phenomena appear from different perspectives, such as from different instruments or different locations. An example of phenomena is the orbit of Mars around the Sun, while Mars' occasional retrograde motion is the appearance. In Ptolemaic astronomy the retrograde motion is represented as an epicycle. In the Copernican model, Mars only appears to have occasional retrograde motion, while in reality Mars' motion is uniformly circular. Thus, Mars' real motion is unobservable.

While constructive empiricism takes a semantic realist approach to reference, empiricist structuralism does not. In the former case, terms purporting to refer to unobservable entities were taken as possibly referring to entities that may or may not exist. Constructive empiricism seems to take an agnostic stance on the existence of unobservable entities and regards the truth value of these terms as neither true nor false.⁵² According to empiricist structuralism everything that can be spoken of is observable, what is unobservable does not exist. As he states, "If appearances are what appear to us, then by definition, we never do see beyond the appearances...! This insight, clear enough in Locke and Berkeley,...could be the slogan for our entire discussion" (2008, 99). When scientists see entities that are putatively made observable through instrumentation, as when a paramecium is said to be observed through a microscope, empiricist structuralism avers that they are observing an image created by the microscope, in this case a paramecium image. Thus, under empiricist structuralism, talk of the existence of imperceptible entities is simply false.

⁵² Though van Fraassen denies this (Cf. 2007, 343).

Van Fraassen (2008) notes that, in the past, a scientific theory was considered incomplete unless it could show a process by which an appearance is produced. He calls this completeness condition the *appearance from reality criterion*. While historically this requirement has been honored in practice since the Scientific Revolution, it has recently been abandoned in quantum mechanics. Quantum theory is unable to meet the demand in the following question, “*Does this scientific theory specify, explicitly or implicitly, a process, whether deterministic or stochastic, by which this appearance is produced?*” (2008, 299). Any theoretical description of any quantum measurement process “does not seem to provide a place for the specific outcome in question” (2008, 300). He is not clear on whether or not this criterion is to be expanded beyond the quantum realm to all of the sciences, but in light of his stance against analytic metaphysics, this seems to be the case. Thus, the image of a paramecium, or whatever else we see through the use of an instrument, does not need to be accounted for once the appearance from reality criterion has been abandoned.⁵³

3.5 Conclusion: van Fraassen and Knowledge

In this chapter, I have attempted to sketch van Fraassen’s epistemology and philosophy of science and how they relate to his account of knowledge. I chose to examine van Fraassen’s philosophy since he is the foremost scientific anti-realist and

⁵³ In Chapter Four, I shall contend that van Fraassen’s distinction between appearance and phenomenon mirrors the scientific realist’s metaphysical distinction between appearances and the unseen reality that causes the appearances.

because his account of knowledge is based on Blaise Pascal's often overlooked epistemology. While van Fraassen develops an epistemology that does not include justification, I have endeavored to show that he is as committed to facticity as those, such as Psillos, who adhere to the traditional justified true belief analysis of knowledge, and those, such as Bird, who do not. Rather, van Fraassen accounts for our ability to acquire truth through a commitment to commonsense realism. Thus, despite his claim that he follows the empiricist tradition of eschewing metaphysics, he seems to be reliant on metaphysics to explain how it is that we can know anything about the world.⁵⁴

⁵⁴ In Chapter Four, I shall argue that van Fraassen's account of knowledge seems to fall short of facticity. Moreover, I will use his arguments against scientific realism as a counter-balance to the scientific realist arguments of Psillos and Bird. By doing so, I hope to show that no one account of scientific knowledge so far examined has met the facticity criterion and thus we should suspend judgment on them.

Chapter 4.

A Pyrrhonist Examination of Psillos, Bird, and van Fraassen

How would a Pyrrhonist approach the scientific realism debate? The Pyrrhonist will critically investigate the claims made by each side of the debate using representative samples and by comparing and contrasting arguments for and against their respective claims. The Pyrrhonist will note that there is significant disagreement among the claims made by both sides of the debate—one side argues that truth (and ultimately knowledge) can be acquired through induction and the inference to the best explanation; the other denies this claim and argues that knowledge comes through our senses. The Pyrrhonist will investigate this disagreement to see if they can decide which of the two views gives the true account of scientific knowledge. Both scientific realists and scientific anti-realists give seemingly persuasive reasons for their respective accounts of scientific knowledge, but what their accounts are incompatible. Since their accounts cannot both be true, how is one to choose between these views?

In the previous chapters, I have endeavored to sketch the skill set the Pyrrhonists use to test truth claims, and to describe representative accounts of scientific knowledge. As noted in Chapter One, this skill set includes the modes of hypothesis, relativity, circularity, and infinite regress. In this chapter, I intend to do the following:

- (1) Describe the conflict between the sides in the debate to show that it *seems* an incontrovertible choice cannot be made between these views.
- (2) Instill doubt in the reader's mind by recounting the criticisms made by both sides against their opponents, identifying which mode these criticisms fall under.
- (3) Put forward an original argument against Psillos' causal-descriptive theory of reference.
- (4) Attempt to argue that both sides of the scientific realism debate fall prey to the mode of hypothesis because they explicitly or implicitly rely on stances as the bases for their accounts of scientific knowledge.
- (5) Conclude that it seems none of the accounts of scientific knowledge examined can meet the facticity criterion, and thus they all appear to fail.

4.1 Psillos

In the subsections that follow, I shall:

(1) Use the Pyrrhonist modes to criticize Psillos' philosophy of science and his account of scientific knowledge. I will begin with his three stances of scientific realism since these stances are the assumptions from which Psillos develops the rest of his philosophy of science.

(2) Criticize his defense of the "no miracles argument". Since the stances and the defense of the "no miracles argument are meta-level justifications for the rest of his philosophy of science if the stances and his defense fail, then the rest of his philosophy cannot even get off the ground.

(3) However, if these critiques are not powerful enough to instill doubt in the reader, I then describe the objections that I and others raise against the components of his philosophy of science, such as the semantic component detailed in his causal-descriptive theory of reference.

(4) Endeavor to contend that his account of scientific knowledge appears to fall short of the facticity criterion.

4.1.1 Psillos' Three Stances of Scientific Realism

While Psillos never openly commits himself to voluntarism, he begins his defense of scientific realism with the intuition that it is the best generator of knowledge about the natural world. Ultimately, Psillos' defense of this putative form of knowledge rests on the problems of epistemic risk versus epistemic security. He states that he is willing to risk more epistemically when he claims that background theories are approximately true since this increase in risk is necessary in order to move away from ignorance. By taking on an extra epistemic risk, the scientific realist aims to know more about scientific theories than their empiricist colleagues. He concludes that the risk is worth the expense. This intuition leads to the claim that scientific realism is the incorporation of three theses: the metaphysical thesis, the semantic thesis, and the epistemological thesis. The first thesis is the fundamental metaphysical presupposition that the world is made up of natural kinds, that this structure has definite characteristics, and is mind-independent. The second thesis is crucial to scientific realism, it is that scientific theories are to be taken literally in the descriptions of their intended domain, both observable and unobservable, and that these descriptions are truth-conditioned. The third thesis is the epistemically optimistic stance that mature and genuinely predictive scientific theories are approximately true of the world. Rather than defend his intuition or the adoption of these assumptions, Psillos attempts to show that, once these assumptions are adopted, they can do a better job of explaining the empirical success of science than any epistemically pessimistic competitor.

Although Psillos' intuition that science is the best generator of knowledge of the natural world may be true, it is unjustified. Moreover, he does not give an argument for why this intuition is properly followed or for why it is not in need of justification. This intuition leads, not only to the three assumptions he thinks are the basis of scientific realism, but to assumptions about the desirability of explanation and of our current state of scientific knowledge. Without a justification for Psillos' intuition and his epistemic optimism he cannot show why we should value explanation. Furthermore, if Hasok Chang (2003) and P. Kyle Stanford (2003) are correct, Psillos' epistemic optimism may bias him towards thinking that current scientific theories are closer to the truth than past theories, especially successful, though abandoned theories. This bias leads to Psillos' privileging those theories that most resemble current theories as being truer than those that do not.

Moreover, as described in Chapter Three, van Fraassen argues that one can be just as disposed to think that science is not the generator of knowledge of an unperceived world and that explanation is not desirable. If Psillos cannot show why one we should follow one intuition over another, it is unclear why we should take seriously his defense of scientific realism.

Furthermore, even if one were to agree with Psillos that science is the best generator of knowledge of the natural world and Psillos' three stances, this would not compel them to adopt Psillos' version of scientific realism. For instance, Psillos contends that his theory of reference shows how we can identify natural kinds and block the pessimistic meta-induction. However, as will be shown in a subsequent section, Psillos' epistemic and metaphysical voluntarism allow for one to adopt his theory of reference

and come to very different metaphysical conclusions than Psillos. Chang and Stanford make this clear in their examination of Psillos' mistaken take on 19th century scientists' attitudes toward now abandoned theoretical terms.

In a subsequent section, I shall attempt to argue that Psillos' voluntarism can be consistently extended to his theory of reference leading to referential and ontological voluntarism, and thus relativity. Since Psillos sought through his theory of reference to identify natural kinds, these last two forms of voluntarism should be blocked from his theory of reference, but I will argue this does not seem to be the case. If my contention is correct, then his voluntarism undermines his goal of objectively identifying natural kinds. Thus, he has not presented a compelling defense of scientific realism, but a detailed account of how he identifies natural kinds. Therefore, his stances fall prey to the modes of hypothesis and relativity.

4.1.2 The "no miracles argument"

Psillos defends against the charge that the "no miracles argument" is circular by relying on an externalist justification for the reliability of the argument. However, he recognizes that the "no miracles argument" seems to belie externalism since the argument gives reasons for the reliability of abduction. In response to this challenge against externalism, Psillos claims that the "no miracles argument" does not add anything to the reliability of abduction, rather it simply produces a new belief about the reliability of

abduction, which is justified if the latter is reliable in the first place. But, even if the “no miracles argument” did purport to defend the reliability of the inference to the best explanation, then this move would not be blocked by a commitment to externalism. Instead, this move would just be optional. The justification would be rule-circular, but this is rationally acceptable. If rule-circularity were not justifiable, then there would be no defense for any of our basic inferential practices (such as, *modus ponens*). Even internalists have to rely on rule-circularity to defend our ampliative and deductive practices. Psillos claims that if rule-circularity is not permitted for the justification of these practices, then these practices cannot be justified. Lewis Carroll noted this dilemma in “What the tortoise said to Achilles” where it was shown that any deductive rule, such as *modus ponens*, needs a deductive rule at the meta-level to demonstrate that the first deductive rule is sound. However, the deductive rule at the meta-level needs yet another deductive rule at the meta-meta-level to show that the meta-level rule is sound, and so on. Clearly, the mode of infinite regress threatens to arise if Carroll is correct.

In answer to this objection, Psillos argues that we should trust deductive rules, such as *modus ponens*, because we have no reason to distrust them. We can test the rule through examining various instances of the rule and see that we cannot create a situation where all the premises are true and the conclusion false. Psillos claims that ultimately “no justification of *modus ponens* is possible which does not rest on some presuppositions. All we can do is engage in a process of *explanation and defence*” (1999, 87). To explain and defend *modus ponens* (and other deductive rules), Psillos suggests that we systematize the rule(s), give an account to ourselves of how we should use it, and demonstrate that, from the meaning assigned to the logical connectives and truth-tables,

the rule(s) are truth-preserving. A similar approach is employed in the defense of induction. Induction is not truth-preserving, but, if one is disposed toward the use of induction, then one can still learn from experience. Hence, in defense of both deductive and inductive experience, Psillos relies on a disposition to accept these rules in the first place. He admits that he cannot attempt to persuade those without this disposition that these rules are reliable without using the rules to show their reliability (1999, 88).

In his defense of deductive and inductive practices, Psillos' view falls prey to the mode of circularity because he answers the question of why we have these practices by stating we have a disposition to reason in these ways. Moreover, by rejecting the need to account for why we should accept logic merely on the basis of our putative disposition, Psillos makes logic into a "black box". Even if our apparent disposition to accept logic leads us to rules that preserve truth or reliably generate true beliefs, we cannot show how it is that these rules preserve or generate truth. Psillos stops further inquiry on this matter when he claims that logic requires a disposition to accept logic. Furthermore, he does not explicitly allow for the search for an explanation to continue, hence he falls prey to the mode of hypothesis again. Considering Psillos' commitment to naturalistic explanations, he would be more consistent if he suspended judgment on the matter and deferred responsibility for an account of our reasoning onto cognitive psychology. However, he could attempt to defend his response by appealing to the reliability of inductive and deductive practices. If he took this approach, then Psillos has to show that reliability is a mark of truth. In the sections below shall describe how the criticisms of Chang and Stanford seem to show that what Psillos would count as reliability applies to

theories he considers false. Moreover, van Fraassen's arguments against the view that reliability has epistemic value shall be sketched.

4.1.3 Chang and Stanford contra Psillos

Chang (2003) and Stanford (2003) challenge Psillos' understanding of the "no miracles argument" by attacking his causal-descriptive theory of reference. Since the causal-descriptive theory of reference is designed to block the challenge from the pessimistic meta-induction, I shall use this section as a bridge between the sections on the "no miracles argument" and my criticisms of the causal-descriptive theory of reference. Thus, this section shall serve as an introduction to those criticisms. Moreover, Chang and Stanford's respective arguments also seek to undermine the inference to the best explanation.

In the course of their criticisms of Psillos' treatment of revolutionary episodes in the history of science, Chang and Stanford show that past scientists held theories that are now discredited as being the best explanation for the phenomena in the theory's domain. In particular, both implicitly challenge Psillos' causal-descriptive theory by criticizing the latter's treatment of the history of science. I shall describe Stanford's criticisms first since he deals with Psillos' main example of the transition from the "luminiferous ether" to the "electromagnetic field". However, Psillos does devote enough attention to the history of caloric that Chang's detailed examination of caloric will be addressed as well.

Chang's discussion of caloric shows in greater detail than Stanford how selective Psillos' history of this particular posit seems to be.

Stanford contends that scientific realists employ a strategy of *selective confirmation* in response to challenges to the historical record posed by successful but abandoned scientific theories. Selective confirmation is the practice of putatively identifying those parts of successful but discarded theories that were involved in the success from those that were superfluous. Stanford calls Psillos' version of selective confirmation "Trusting Scientists" (2003, 918). Rather than devise an explicit criteria for selecting confirmation, Psillos appeals to the judgment of the scientists themselves to identify the successful parts of theories. He claims that the judgment of scientists is often reliable in these circumstances. However, Stanford claims that Psillos' reading of the historical actor's judgments in the historical examples Psillos uses is "highly selective" (2003, 919).

Stanford notes that the scientists themselves can be greatly mistaken in their ontology. He states that the embryologists and physiologists of the 19th century argued for the existence of vital forces on the basis of the inherently teleological nature of biology. Furthermore, it was the 19th century physicists who claimed that the existence of the ether was well-established by the numerous successes of the wave theory of light. Stanford contends that if a leading scientific expert such as Maxwell could be wrong about the existence of the ether "in so spectacular a fashion", and then this instance should show how dubious scientist's ontological judgments can be (2003, 919). Stanford concedes that Psillos rightly notes the agnosticism many ether theorists had concerning the constitution of the ether, and of the doubt these theorists cast on the details put forth

in various mechanical models of the ether. Stanford claims that Psillos takes these theorists' agnosticism regarding the constitution of the ether as agnosticism regarding the existence of the ether.

However, As Stanford notes, Green's statement that ether theorists were "perfectly ignorant of the mode of action of the elements of the luminiferous ether on each other" is evidence that the ether theorists believed ether existed, but that they simply did not know how it was constituted (2003, 919 in Psillos 1999, 132). Moreover, Stanford contends that Psillos is assuming that scientists' judgments are homogenous. Yet, such homogeneity cannot be found in the historical record, or in contemporary accounts of science. Bueno's (forthcoming) example of the disagreement on the shape of atoms is similar to the disagreement shown by ether theorists on the constitution of the ether (11). Thus, Stanford concludes that the historical record contains enough superseded posits that were crucial to the genuine success of past mature scientific theories that the scientific realist strategy of selective confirmation has failed.

Chang attacks what he calls *preservative realism* in the philosophy of science. Preservative realism is the view that there are key characteristics of successful scientific theories that are preserved through revolutionary changes in science, and those preserved key characteristics are what the scientific realist is committed to accepting as true of the world. In particular, Chang challenges various problems with Psillos' treatment of the caloric theory of heat. He chooses to address Psillos specifically since the latter attempts to seriously examine episodes in the history of science to support his theory of reference. However, Chang shows that Psillos' treatment of these historical episodes appears to be seriously flawed. Although Psillos' principal example of theory change is the transition

from the luminiferous ether to the electromagnetic field, Chang concentrates on Psillos' discussion of the caloric theory of heat since Chang knows it well, and because it deserves more historical scrutiny.

Psillos claims that he can differentiate between those parts of scientific theories that are crucial to the success of the theories and those that are not. With regards to the caloric theory of heat, Psillos contends that the laws of the caloric theory of heat can be shown to be approximately true without a commitment to the existence of caloric.

Against this claim, Chang shows that the successes of the caloric theory of heat were the result of certain (now seen as mistaken) assumptions about the nature of caloric as the material substance of heat. In beginning his examination of the successes of the caloric theory, Chang notes that Psillos selects only those successes that seem to be clear antecedents to contemporary beliefs. However, even if such an effort were successful, it would only show that those elements of past science which have survived into contemporary science have survived.

Chang employs Psillos' definition of *success* for scientific theories: theory that explains phenomena, and generates better accounts than alternative theories can, is successful (2003, 906). Using this measure of success produces a record of the caloric theory's successes that diverges greatly from Psillos' list. Psillos only mentions calorimetry, the law of adiabatic expansion of gases, the calculation of the speed of sound, and Sadi Carnot's theory of heat engines. This list of successes only mentions those successes where the beliefs and practices involved were not central to caloric theory, and where theoretical beliefs in caloric theory were later abandoned (2003, 905-906). However, As Chang states:

The highlights of success in the actual history include the explanations of the following: the flow of heat toward equilibrium, the expansion of matter by heating, latent heat in changes of state, the elasticity of gases and the fluidity of liquids, the heat released and absorbed in chemical reactions, combustion, the radiation of heat, and the gas laws. (2003, 907)

He then uses Psillos' strategy of paying attention to what parts of caloric theory the scientists of the time considered as being responsible for its successes. The most significant theoretical assumption was that heat was a 'self-repulsive' material substance that was attracted to normal matter (2003, 907). This assumption, when added to the notion that temperature was the density of caloric, produced elegant explanations for the flow of heat from warmer to colder places, and for the expansion of most things when heated. The chief competitor to caloric was the idea that heat was a form of motion, and this theory could not give explanations that were seen as equally elegant (2003, 907).

Furthermore, the postulation that caloric existed in a sensible and latent state produced further significant successes. The *latent* state of caloric meant that the substance was undetected by our senses or a thermometer. This particular postulation led to Joseph Black's most salient achievement in the theory of heat was his explanation for *changes of state* as the addition or withdrawal of latent heat. Changes of state are the changes in physical properties of certain substances. Examples of changes of state are freezing, boiling, melting, and condensation. This notion of latent heat accounted for why the addition or subtraction of heat did not cause temperature changes during changes of state. Moreover, it explained the fundamental changes of physical properties (such as fluidity and elasticity) as a result of the chemical combination (or dissociation) of matter with sizeable quantities of caloric (2003, 907). The postulation of the sensible and latent

states of caloric was not a superfluous metaphysical fancy, but a notion that was seen as being crucial to Black's explanations (2003, 907).

The assumption that caloric existed in these two states was integral to the materialistic conception of caloric in Lavoisier's *Traité élémentaire de chimie*. According to Chang, this work marks that maturity of caloric theory. Lavoisier confidently identified caloric as a chemical element, so much so that he placed it in his table of chemical elements. The notion that caloric was a chemical element served as a direct and compelling framework for the explanation of the heat absorbed and released in assorted chemical reactions. During this period, the dynamical theory of heat could not account for these phenomena. According to Lavoisier, latent heat was caloric when chemically combined with matter. If the chemical bond was broken, then free caloric was produced and this free caloric appeared as perceptible heat. The most famous case of this was the generation of heat in combustion. In combustion, the oxygen 'base' mixes with inflammable substances, leaving behind the large amount of caloric with which it had mixed. Lavoisier's notion of oxygen gas as a chemical compound composed of oxygen and a large amount caloric was securely founded in the overall calorist conception of the three states of matter. According to this conception of caloric, the addition of latent caloric changed solids into liquids, and liquids into gases. Hence, as Chang notes, "his explanation of combustion constituted a particularly satisfying synthesis" (2003, 908).

Caloric was central to accounting for the phenomenon of radiant heat. Radiant heat was simply caloric being tossed about between objects at very high speeds. Those who were not committed to the existence of caloric did not have an explanation for why

heat radiated. The scientific consensus of the day accepted that caloric theory explained radiant heat, and did so better than any other competitor.

The height of theoretical sophistication for caloric theory came from Laplace in the 1820s. Psillos does not address Laplace's work during this period instead Psillos claims that this omission will not affect his argument. Chang disagrees with Psillos' claim and contends that Laplace's caloric theory accounted for a large number of significant phenomena, such as, "(the speed of sound, the adiabatic gas law, and the regularities that are summarized by the ideal gas law), and it explained these things better than any other competing theory did" (2003, 909). Not until the molecular-kinetic theory of gases in the latter half of the nineteenth century did any serious contenders to the Laplacian theory of gases appear.

Chang contends that, contra Psillos, the claims about the material composition of heat were assumed to be a critical function in accounting for many significant phenomena. Chang notes, "Laplace's central premises were that the caloric fluid was made up of point-like particles of caloric, and that the caloric particles, most of which were contained within molecules of matter, repelled each other with a force that was a function of distance only," (2003, 909). Temperature was identified as the density of 'free caloric space,' which was a small quantity of caloric moving about in intermolecular spaces, removed from the molecules by intercaloric repulsion (2003, 909). Because Laplace did not know the exact form of the intercaloric form function, he needed the following additional assumptions: "the force is negligible at any sensible distances; each molecule in a gas in equilibrium contains the same amount of caloric; in equilibrium, the caloric-filled molecules are spherical and stationary; and so on," (2003, 909). Chang

claims that it is unlikely that Laplace's derivation of the gas laws could be understood in an approximately true sense by contemporary theories. The Laplacian ontology of mutually repelling caloric particles has been entirely abandoned by contemporary science. The gas laws are now derived by completely different assumptions.

According to Chang, the review of the most important and uncontroversial successes of the most mainstream line of the caloric theory shows what Psillos has all but ignored in his alleged refutation of the pessimistic meta-induction. The assorted assumptions regarding the nature of caloric and its relationship with ordinary matter did carry out crucial work in creating successful explanations, and they were unambiguously abandoned by later science. Furthermore, the assumption of the materiality of caloric was critical to the explanatory success of the theory since the many auxiliary assumptions concerning the nature of caloric cannot be interpreted into terms that do not rely on the supposition of its materiality except with great difficulty.

Moreover, while Psillos accurately quotes the scientists of the period, Chang contends that he is taking these quotes out of context. The scientists seemed not to have been committed to the reality of caloric even though they did usually favor the caloric theory over its rivals. Psillos quotes Black and Lavoisier as being reluctant to accept the reality of caloric theory for certain. However, Chang claims, Lavoisier and Black were only displaying an insincere commitment to the epistemic caution toward all theories which was popular at the time. This apparent caution is difficult to take seriously when Lavoisier put caloric into his table of elements. Yet, Chang's main point is that the materiality of caloric was necessary for the accounts given of the phenomena that were accepted, and not the epistemic modesty of the scientists themselves (2003, 910).

Employing Psillos' own strategy of taking what the scientists had to say about caloric, Chang has seemingly shown that Psillos' own strategy cannot support his scientific realist conclusions. Specifically, Chang showed how crucial "caloric" was to the explanatory and empirical success of that now discarded theory. In the final section of his (2003), Chang discusses the scientific realist preservationist strategy, reviewing what it is that is preserved in theory change and examining whether this review supports scientific realism or not. He claims there are four features that are preserved in scientific theory change. First, much observational data is retained, though in rare cases even the data can change. Chang does not give an example of this kind of change; however the retrograde motion of Mars would be one example. Second, phenomenological laws, the mathematical representations of the data, are also largely immune to change. Chang is careful to note that phenomenological laws do not identify causes, mechanisms, or "far-reaching theoretical principles" (2003, 911). However, these features of scientific theories are uncontroversial, many anti-realists (such as, constructive empiricists) recognize the preservation of observable phenomena and regularities in theory change. The third and fourth features are the techniques of representation and well-entrenched metaphysical systems. Chang contends that these two features will not help the scientific realist since they do not rely on how the world actually is. Chang notes that metaphysical systems, as Quine argued, 'can be held true come what may,' (2003, 911). While certain representational techniques and metaphysical commitments have been consistently held through the centuries in Europe does not, by itself, speak to how the world is.

Chang concludes that the above list of four features is all that is maintained in theory change. Structural realists claim that they have discovered something more than

data, mere regularities, oft employed representations, and entrenched metaphysical commitments that have survived theory change. Yet, Chang argues, the structural realists “structures” are only particular data-sets represented mathematically in phenomenological laws, and (maybe) extremely abstract mathematical structures. According to Chang, the foregoing discussion illuminates what it is about preservative realism that fails: the elements that are preserved do not make clear what can be inferred from them about the world. Since certain features of science may be preserved for different reasons it is unclear if these features work because they correspond to nature, because we like them, or because of our cognitive limits. If the latter two possibilities can be excluded, then an inference from preservation to truth can be properly made (2003, 911).

At the end of his (2003) Chang uses the following metaphor to argue against the “no miracles argument”:

I conclude with a metaphor designed to express this last point. The metaphor sees the development of scientific knowledge as a process of putting up a building. A building will collapse if its design goes against the laws of nature in certain crucial ways; however, if we always work certain features into the buildings we build (such as external decoration, or even certain structural aspects), that does not necessarily mean that those constant features are linked to laws of nature in any straightforward way. Likewise, in building scientific knowledge the systems we construct will collapse if they disagree with nature in certain crucial ways; however, certain features being constantly present does not mean that we can read off anything inherent about nature from them. Even if our cognitive activities are stable, we may not be able to fathom the reasons for that stability. (912)

Chang’s conclusion seems to present a serious challenge to the “no miracles argument” as understood by scientific realists, but he stops short of calling the success of science a miracle. However, he casts significant doubt on the scientific realist claim that

theoretical terms refer to unobservable entities, and that this accounts for the success of science.

While Stanford and Chang do not employ the Pyrrhonist modes against Psillos, they implicitly employ the mode of relativity since they respectively give alternative interpretations to Psillos' history of science. In particular, Chang seems to show that Psillos' strategy for identifying superfluous posits in past scientific theories is self-defeating since the latter's definition of success can be applied to posits which Psillos identifies as superfluous. Moreover, Stanford and Chang appear to show how Psillos' strategy for identifying crucial posits is *post hoc*. In the section below, I shall endeavor to show that his strategy seems to be more generally *ad hoc* as well.

4.1.4 Voluntarism and Psillos' Causal-Descriptive Theory of Reference

In the above section, Chang and Stanford contend that Psillos' reading of the history of science in support of his theory of reference is mistaken. While Psillos' treatment of the history of science does seem flawed, I attempt to argue that even if his retelling of historical events was less controversial, his theory of reference does not have the normative power that he claims that it has. Stanford uses the history of science to show that scientists do have a consensus on ontological matters. In this section, I hope to:

(1) show that on philosophical grounds Psillos does not compel one to adopt his method of identifying natural kinds.

(2) contend that Psillos' implicit epistemological voluntarism leads to metaphysical and referential voluntarism.

In particular, I endeavor to argue that Psillos' voluntarism, when consistently applied to his theory of reference, cannot compel one to accept a certain set of descriptions as being fundamental. Rather, his voluntarism allows one to choose, within a certain range, what properties are fundamental. The range of "fundamentals" one can choose is constrained by whether or not the referent seems to exist, and the referent can only be said to exist if at least one core description, or core descriptions, fit with the world. Thus, what is considered fundamental seems to be at least partially intentional.

Using the example of the electro-magnetic field, Psillos notes that one may object to his theory of reference by stating that there is an ad hoc element in deciding which descriptions are to be considered as core causal descriptions. He responds that "in principle" one can identify and delineate the descriptions associated with an entity and to examine them in terms of importance for the causal role attributed to the posited entity. The most important descriptions, according to Psillos, are those that pick out the causal role a putative entity plays. If the descriptions ascribed to the entity are not necessary for it to fulfill its causal role, then they are not as fundamental or important. For the luminiferous ether, the less fundamental descriptions are those involving its possible constitution, such as Green's elastic-solid model and McCullagh's rotational elasticity

model. The fundamental aspects of the luminiferous ether are its dynamic and kinematic features.

Psillos stresses that the identification of core causal descriptions is not ad hoc. The choice of which properties are taken to be core causal descriptions is determined by examining what properties are necessary for the posited entity to play its causal role with regards to a certain range of phenomena. Furthermore, he notes, “It is certainly constrained by the way in which the scientists who posited the entity described it” (1999, 298). For instance, Psillos argues that “phlogiston” could not refer to the same entity as oxygen since the former does not fit the same core causal descriptions as the latter. James Conant (1948) notes the following properties of phlogiston: when combined with metallic ore it produced metal, it combined with the air, when it saturated the air it combustion was no longer possible and the air would no longer support life since respiration removed phlogiston from the body (1948, 70). Lavoisier showed through his “doctrine of gases” that combustion involved chemical combination with some part of the air, and that part of the air is oxygen. Moreover, oxygen clearly has different properties than phlogiston, such as when the former saturates the air combustion is facilitated, not impossible. According to Psillos, since the “luminiferous ether” possesses the same core causal description as the electromagnetic field and “phlogiston” does not share the same core causal characteristics as oxygen, he concludes, “although it may be reasonable to argue that the term ‘luminiferous ether’ refers, it is not equally reasonable to maintain that the term ‘phlogiston’ does” (1999, 298).

Niiniluoto (1997) objects that there are a number of actual cases where the core causal descriptions are mistaken in some way. He uses the example of the HI-virus

where the first assumptions of its causal properties were simplified. Psillos responds that the initial assumptions of the putative entity's causal properties may be "exploratory or speculative" (1999, 299) and should not be taken seriously until the term becomes part of a well-established theory which links it with a core causal description. When discussing why it is that a term such as "luminiferous ether", despite being linked to firm theories, was dropped, Psillos responds that this is a question for sociology.

Psillos concludes that if the luminiferous ether/electromagnetic field case is typical, then scientific realism is on solid ground. However, if future investigation shows that all or most core causal descriptions linked with theoretical terms are false, then scientific realism faces a major difficulty. In order for his theory of reference to work, he notes that the determination of the core causal properties must be neither too broad nor too narrow. If too broad, it may include properties that are not necessary for the putative entity to fulfill its attributed causal role. If too narrow, then reference is too easily had and becomes trivial.

However, as Stanford appears to have shown, Psillos misrepresents the attitude ether theorists had toward the luminiferous ether. The former contends that ether theorists believed that the ether existed although they did not know how it was constituted. Thus, Psillos has only shown that he thinks knowledge of the core causal properties of an entity are necessary for belief in its existence, but that the entities constitution is superfluous. This, as noted by Chang, is a function of Psillos' precursoritis since the kinematic and dynamic properties of the luminiferous ether are to be found in the electromagnetic field. Since the constitutive features were not carried over, then they must not have been important. Moreover, Chang seems to have shown

how the constitution of caloric played a vital role in the explanations of the empirical success enjoyed by that theory. Furthermore, as Stanford observed, the scientific community is not homogenous in their judgments. For instance, as Bueno illustrates, there is still a lack of consensus on the shape of the atom in the physics community (forthcoming: 11). These points blur the boundaries Psillos set down to stipulate what counts as too narrow or too broad a determination of the relevant properties necessary to fix reference.

The above examples of the lack of consensus concerning ontological matters rely on historical and current scientific practice. But, I shall argue that Psillos' causal-descriptive theory of reference is implicitly voluntarist. This elaboration on Psillos' theory of reference shall be called the *voluntarist theory of reference*. The voluntarist theory of reference expands on Psillos' theory of reference by allowing one to choose what is to be considered a fundamental description for the fixing of reference. The voluntarist theory of reference fits within the three stances Psillos set down for scientific realism. For example, as Chang appears to have shown of the caloric theorists, their attitudes seem to have fallen within the bounds of these three stances. They believed that caloric was an independently existing element with a definite structure (or, according to the metaphysical thesis, a natural kind). Caloric theorists assumed that the term "caloric" had factual reference and that what the theory said about it should be taken literally (all of which is compatible with the semantic thesis). Whether or not caloric theorists believed in the full truth of caloric theory, there seems to be enough evidence to show that they were very confident that it existed. Moreover, these beliefs were generated through ampliative-abductive methods. This last facet fits within the epistemic stance. Thus, for

these scientists, the constitutive features were considered crucial to establish the entity's existence, not just its dynamic and kinematic properties. Furthermore, all of these beliefs are compatible with Psillos' three stances of scientific realism. These historical examples seem to show that it is possible to agree with Psillos' three stances of scientific realism, but not thus be compelled to agree with him on what constitutes a natural kind.

According to William James (1948), objective rules alone (such as, the law of non-contradiction) cannot determine what we believe. According to James' we are pulled in opposite directions in our search for knowledge: we wish to pursue truth and to avoid error. Those who are more referentially liberal are those who are more inclined to avoid error than pursue truth. Clyde L. Hardin and Alexander Rosenberg's (1982) "sameness-of-causal-role" account of reference is an example of a referentially liberal view since this position allows causality alone to fix reference. Psillos gives the following sketch of their account, "For instance, Aristotle's *natural place*, Newton's *gravitational action-at-a-distance*, Einstein's *space-time curvature* can all be said to have played the same causal role *vis-à-vis* gravitational phenomena" (Psillos 1999, 292). Hardin and Rosenberg's account is a response to Laudan's (1981) which argues that scientific realists must adopt a theory of reference where the central terms in a scientific theory must be referential for the scientific theory to be, at least, approximately true. They claim that there is no theory of reference that one must commit themselves to in order to be a scientific realist. Rather, one can be a scientific realist and hold to any theory of reference, or suspend judgment on the matter. Laudan uses the example of "gene" to argue that one cannot be a scientific realist and believe in the approximate truth of genetic theory since the central term "gene" is non-referential: "If there were nothing

like genes, then a genetic theory, no matter how well con-firmed it was, would not be approximately true. [A] necessary condition—especially for a realist—for a theory being close to truth is that its central explanatory terms must genuinely refer" (Laudan 1981, 33 in Hardin and Rosenberg 1982, 606). In response to Laudan, Hardin and Rosenberg claim to show how one can be a scientific realist and not consider Mendel's concept⁵⁵ of "gene" as referential:

It is, however, by no means clear that this thesis must be embraced by a realist, and the concept of the gene together with its theories provides an excellent example of why it need not. Thus, Mendel's 1866 theory, embodying its laws of segregation and assortment, clearly constitutes the first in a sequence of successive theories which are held by life scientists to constitute a series converging on the truth. Mendel's theory is often credited with approximate truth and still taught because of its simplicity, and the ease with which it can be complicated in the direction of presumably more accurate and more complete genetic theories, theories more nearly approximate to the truth. Yet it may plausibly be reported by a realist that there is nothing like genes, or phenotypes, as Mendel or his immediate successors construed them. The particular properties of whole organisms, which they construed as phenotypes, and the kinds of particulate units that are transmitted between generations and generate these properties, which they construed as genes, can reasonably be asserted not to have existed. This is because all of the functions indicative of the Mendelian gene are now credited to widely different amounts, highly complex combinations, and incredibly diverse sequences of DNA; indeed in some areas of genetics the term 'gene' has pretty much dropped out of sophisticated presentations of the theory in favor of terms that more accurately discriminate between the units of hereditary functions. Accordingly it might be said, by a realist, that there is nothing, no one thing, like the gene. The claim is even clearer in the case of Mendelian phenotypes, which have been superseded by the immediate polypeptide products of DNA expression. Notwithstanding these facts, Mendelian genetics is still represented as an approximately true theory, even though its central theoretical terms can, on this account, plausibly be said not to refer. The causal role Mendel accorded to genes is parceled out to other entities. In brief, this is done by showing that the diverse units of genetic functions—of mutation, of replication, of expression—work together often enough to give a false impression of unity and to yield an approximately true set of predictions about the distribution and transmission of paradigmatically heritable properties, which Mendel mistakenly took to be phenotypes. The units of function, the 'muton', 'recon', 'cistron' do not "add up" to the 'gene'. This is what makes for the problem of reductionism in genetics. To insist that they have jointly the same referent as the term 'gene' is a proposition on which the realist need not take sides. The scope for according Mendel's laws approximate truth, while denying that there is "anything like the gene", is even broader in the light of discoveries at the molecular level. These discoveries multiply the entities relevant to the explanation of the approximate truth of Mendelian genetics so much that they render the greatest plausibility to the view that there is nothing like a gene in the sense envisioned by Mendel. The role of the single nucleotide, the non-coding intervening sequences and their eliminators, the orders of ribosomal, messenger,

⁵⁵ Hardin and Rosenberg note that Mendel did not use the term "gene", nor did he trouble himself with how hereditary characteristics were represented in germ cells (1982, ftnt. 3). However, Mendel's theory does mention particulate units that are transmitted across generations, and this is what is meant by subsequent scientists when they speak of Mendel's concept of "gene" (Hardin and Rosenberg 1982, 606).

transcribing RNA, the epistatic and redundant biosynthetic pathways to the simplest peptide expressions which constitute their phenotypes, all these make it plain that the realist account of the approximate truth of Mendel's laws need make no appeal to anything Mendel or his first half century of successors would recognize as a gene.

(1982, 606-607)

And how one can be a scientific realist and consider this term to be referential:

On the other hand, the changes in genetic theory in the last century can be presented in a manner which suggests that the central theoretical terms of Mendel's theory do successfully refer. They may be taken to refer to configurations of DNA and their polypeptide products, even though of course neither Mendel nor any other geneticist before the 1950's realized that it was these sorts of things to which the terms refer. Here reference is severed from detailed beliefs of the theorist, and its success is accorded retrospectively in the light of subsequent further approximations to the truth. The realist can accept either account of the matter. If he accepts the first he must be (and is) able to trace out the relations between Mendel's theoretical claims and currently accepted ones in a way that shows why his laws were approximately correct, even though they failed to secure reference. If the realist accepts the second construction of what happened in the history of genetics, he must buy into a somewhat different account of reference, at least for the nonce. Since no theory of reference is as yet fixed in the philosophy of science, the realist may avail himself of several alternative accounts of the referential success of particular theories, and can demand that his claims about these theories' referential successes be judged on a case by case basis, and not in the light of a univocal account of reference that he must establish and justify *ab initio*. Similarly, he is in a position to credit theories with approximate truth even where he considers it more accurate to say that the theories did not secure reference to what, with hindsight, we now hold to constitute the fundamental ontology of their domains.

(Hardin and Rosenberg 1982, 607-608)

Hardin and Rosenberg's desire to avoid erroneous descriptions leads to their arguing that sameness of causal role is all that is necessary to fix reference since the pessimistic meta-induction shows that the explanations for phenomena based on their descriptions seem to be regularly abandoned. Psillos does not address their treatment of "gene". If Hardin and Rosenberg's take on the history of the term "gene" is correct, then presumably Psillos would take the first option described by Hardin and Rosenberg above and he would deny that Mendel's "gene" is referential. However, if he took the second approach suggested by Hardin and Rosenberg in the quote above, then it is not clear how Psillos would fill in the details of referential continuity from Mendel's usage of the term to today's usage. Yet, to connect Mendel's usage to today's usage reference he would have to be "severed from the detailed beliefs of the theorist" (Hardin and Rosenberg

1982, 608). If they are correct, then Psillos' notion of core causal descriptions will fail here and thus his account of reference fails. Moreover, today's usage of "gene" is not clear. As Jan Sapp describes:

The complexity of genetic information and modifications with RNA editing greatly complicated the definition of the gene. The gene was first defined simply as a unit of inheritance, then as a locus on a chromosome, and then as a specific nucleotide sequence of DNA, but the "gene" has become increasingly abstract. As [Philip] Sharp noted in his Nobel lecture of 1994, what exactly the gene is has become somewhat unclear. (2003,204-205)

Psillos again faces the challenges presented by Chang and Stanford. If Psillos choose the first approach, then he must carefully choose which of Mendel's posits most closely match the posits used by biologists today. But, if follows the second approach, then he will have to carefully choose which of Mendel's descriptions of "gene" best match the description given by contemporary biology. However, whichever approach he chooses his account will fall victim to Chang's charge of precursoritis. Furthermore, if he looks to the scientists themselves to guide him in his search for core causal descriptions, then he will discover the lack of consensus Stanford observes in the scientific community. Seemingly, the last resort for Psillos is to accept that the gene exists as an abstract entity that must exist due to his commitment to the explanatory criterion. However, a subsequent section shall aim to cast doubt on the explanatory criterion and the existence of abstracta.

Although Psillos does not deal with the gene in his account of reference, he does discuss the luminiferous ether. Hardin and Rosenberg argue that a scientific realist is free to choose whether or not the term "ether" is referential. They argue that a scientific realist could see the incompatible models of the ether as referentially significant and, because they all failed to refer, as reason to believe "luminiferous ether" does not refer.

However, Hardin and Rosenberg regard “luminiferous ether” as referential since it shares the same causal role as the electromagnetic field. Thus, their causal account of reference is more cautious than Psillos’ theory of reference because it does not claim that the descriptions attributed to scientific terms are true, or nearly so. Furthermore, those scientists, such as Green and McCullagh who regarded the constitution of the luminiferous ether to be fundamental to fixing the reference of the term “luminiferous ether” were willing to risk error in their pursuit of truth. Hence, they abandoned the term “luminiferous ether” when the descriptions posited did not fit the empirical evidence. By choosing a middle ground between these two extremes all Psillos has shown is how much he tries to balance the pursuit of truth with the avoidance of error.

In arguing for his theory of reference, Psillos does not directly address how it is that the term “luminiferous ether” was abandoned. Rather, he merely states that this can be explained sociologically and not philosophically. The argument he does give for the abandonment of the constitutive properties of the luminiferous ether are that they were “heuristic devices for the possible constitution of the carrier of light-waves” (1999, 140). Moreover, he states that since no solid account of the constitution could be carried over into the subsequent theory of electromagnetism, then the alleged constitutive properties were not fundamental.

The term was dropped, or so the standard story goes, because ether drag could not be detected in the famous Michelson-Morley experiment. If the luminiferous ether had the same properties as, for example, a liquid or elastic solid, then the light travelling through it would displace the luminiferous ether and this displacement could be detected. Clearly, the physics community of the 19th century considered the detection of this entity

to be important, and that the constitution of the entity would lead to observable phenomena. When the expected phenomena could not be produced, the entity was considered to be non-existent and the term abandoned.

As noted above, Psillos argues that referential continuity was maintained because the dynamical and kinematic properties of the terms “luminiferous ether” and “electromagnetic field” overlap. However, if one considered constitutive as well causal properties to be fundamental to an entity, then the referential continuity between these two terms could not be warranted. The electromagnetic field does not have a liquid or elastic-solid constitution. According to the voluntarist theory of reference, the abandonment of “luminiferous ether” as a term that refers is as permissible as holding that this term denotes the same entity as “electromagnetic field” as long as the properties held to be fundamental can be observed. He claims there is referential stability between the two terms since the kinematic and dynamic properties of these terms overlap. Those who held that the constitutive properties are fundamental to the reference of a term were correct to abandon “luminiferous ether” when no such evidence could be discovered of these properties. Furthermore, if anyone wanted to continue using the term “luminiferous ether”, then they were justified in doing so if the dynamic and kinematic features of an entity count as central to its reference. If one is to be consistent, then what is not permitted is the continued use of a term when what is considered a fundamental property for a term cannot be observed. For instance, those who maintain that combustion is impossible when the air is saturated with “phlogiston”, and who hold that this characteristic is a fundamental causal description of the entity picked out by that term, are not permitted to use the term since no such entity seems to answer to that description.

4.1.5 The Voluntarist Theory of Reference and Scientific Realism

The implications for scientific realism of a voluntarist theory of reference are that it weakens the force of any kind of robust scientific realism, especially of the kind that Psillos wants. Scientific realism cannot be compelled since there seems to be no stable ontology upon which to rest scientific realist claims that science discovers natural kinds. The criteria for what constitutes a natural kind, or whether or not natural kinds exist, can also be subject to a voluntarist approach. As we have seen, some scientists will claim an entity exists even if its constitutive properties have not been discovered, but will stop seeking an entity, and using a term, if its constitutive features cannot be detected. They are not willing to be mistaken about such properties. But, for Psillos the dynamic and kinematic features of an entity are what are required for referential continuity through theory change. Still others, such as Hardin and Rosenberg, allow the same term to be used no matter how radical a change in the description of its constitutive properties. This occurred in the cases of “atom” and “Earth”. An atom was by definition indivisible, while the Earth was by definition immobile.

All these different approaches show is, within certain boundaries, different scientists have taken different features as being more important than others when deciding whether or not to believe in a given entity. But all of these approaches are permissible since they do not violate any of the three basic scientific realist theses. Thus, whether or not there is continuity of reference across theory change for theoretical terms is mainly a matter of risk assessment. Furthermore, whether or not one is to be a

scientific realist is also mainly a matter of epistemic risk assessment. The only impermissible approach is the one that allows for the continued use of a term after one or more of the crucial descriptions fail to fit the world. Which description(s) are crucial for deciding this criterion will differ from person to person and depend on how much the person in question prizes pursuing truth over avoiding error.

Clearly, if one does not share Psillos' scientific realist intuitions and, thus does not accept his three stances, then one will adopt different basic stances or no stances at all. For instance, a constructive empiricist would deny that natural kinds exist, hold to the semantic stance, but not to the epistemological optimism of realism. A constructive empiricist holds that a good theory does not require any explanatory power to be acceptable, nor do they accept ampliative-abductive methods. Thus, their choice as to what is fundamental to the identity of an entity would vary radically from a scientific realist of the semantic stripe. For instance, van Fraassen notes that the ontological status of theoretical terms is indeterminate because they may be mere equations or imperceptible independently existing entities (1980: 11). Furthermore, if there is a lack of homogeneity in the scientific community about the existence of various theoretical entities, then, following Psillos' suggestion of taking scientists seriously and following his implicit voluntarism, we should be allowed to pick which scientists to take seriously. For instance, one who has empiricist intuitions may choose to adopt Ernst Mach's take on unobservable entities as a way of systematizing sensations.

Psillos may object that he has arguments which show that the success of science can be better accounted for by belief in the inference to the best explanation, and that he has refuted global versions of the pessimistic meta-induction and underdetermination.

However, as I shall show below, his arguments against these positions falter without the support of the causal-descriptive theory of reference as he understands it. If my examination of his implicit voluntarism and theory of reference are correct, then Psillos cannot put the kind of restraints on his ontology that he seeks. Since natural kinds are characterized as existing independently of the human mind, then Psillos must want his identification of them not to be reliant on one's intuitions. Rather, if natural kinds exist, then they are the type of entity that exists no matter our intuitions. For example, in the ontology of non-relativistic quantum mechanics the world is ultimately composed of particles, but in the ontology of quantum field theory the world is fundamentally composed of fields. In this case, if the participants in the debate are scientific realists, there is nothing about scientific realism that helps them come to a consensus on what form of natural kind the world takes. Furthermore, if dynamic and kinematic effects are enough to establish that something is a natural kind, then scientific realists should agree that various quantum entities and processes exist. However, Psillos suspends judgment on problems in the philosophy of quantum mechanics (2009: xxii). Thus, he has not shown how our intuitions about what counts as fundamental can be blocked. If Psillos cannot block these intuitions, then his theory falls prey to the modes of hypothesis and relativity. In the first instance, he merely claims the world has a natural kind structure without argument. In the second instance, his implicit stance voluntarism leads to relativity.

4.1.6 The Pessimistic Meta-Induction

Psillos argues that his causal-descriptive theory of reference allows scientific realism to overcome the challenge posed by the pessimistic meta-induction. While he acknowledges that past mature and genuinely successful scientific theories are false, and hence discarded, some of the theoretical constituents which were empirically supported and played a part in their success were retained in the successor theories. As shown above, Psillos example of such an enduring theoretical constituent is the entity that is denoted by both “luminiferous ether” and “electromagnetic field”. Although, this continuity across theory change is what grounds his epistemological optimism, Psillos concedes that we cannot access the whole truth in science. But, he does argue that approximate truth can be had in science. Hence, he claims that the entity denoted by “luminiferous ether” exists since science has latched on to its dynamical and kinematic properties, but was mistaken about its constitution.

However, there are still clear cases of the pessimistic meta-induction. For instance, leeching (or the use of leeches to suck “bad blood” out of an ailing patient) was prescribed for a variety of ailments, such as pneumonia, scurvy, jaundice, and indigestion. This procedure was largely abandoned when the loss of blood seemed to be ineffective. However, leeches are now employed to relieve some patients of hemochromatosis and polycythemia. Moreover, Jean-Baptiste Lamarck’s theory of the inheritance of acquired characteristics seems to have been resurrected in the form of *epigenetics*. Lamarck claimed that characteristics acquired by parents are inherited by

their offspring. For example, he held that giraffes acquired their long necks over many generations of stretching their necks to reach high branches on tall trees. Contemporary biology does not maintain that giraffes acquired their necks in the manner Lamarck claimed. However, in the branch of biology known as epigenetics, which studies variations in gene expression that are not due to the underlying DNA sequence, evidence has been found to support Lamarck's claim that acquired characteristics can be passed on to offspring. For example, a Swedish study showed that the paternal grandsons of men exposed to famine as preadolescents were less susceptible to heart disease (Pembrey ME, Bygren LO, Kaati G, Edvinsson S, Northstone K, Sjöström M, Golding J, 2006). Furthermore, neo-Lamarckism is being taken seriously again by researchers in Australia who claim that their study of obesity may be the "first report in mammals of non-genetic, intergenerational transmission of metabolic sequelae of a HFD [high fat diet] from father to offspring" (Sheau-Fang Ng, Ruby C.Y. Lin, D. Ross Laybutt, Romain Barres, Julie A. Owens & Margaret J. Morris, "Chronic high fat diet in fathers programs β -cell dysfunction in female rat offspring", *Nature* 467, 963). Thus, epigenetics is seen in biology as neo-Lamarckian. Both the effectiveness of bloodletting and neo-Lamarckism can be explained with existing theoretical resources. These examples show that, if history is accurate, then we should be cautious when declaring a "confirmed" scientific theory true or an apparently refuted scientific theory false.

Psillos would likely reply that the pessimistic meta-induction is only a threat at the local level, but not globally. The rediscovery of bloodletting as an effective treatment, or of the inheritance of acquired characteristics does not count as revolutions. If the latter refuted current genetics rather than complemented it, then this would be a

global case of the pessimistic meta-induction. However, the voluntarist theory of reference shows that the pessimistic meta-induction can work globally. Some theories that are seen as false in certain areas are seen as approximately true in other domains. For example, Newtonian physics is seen as an approximation to Einsteinian physics when applied to medium sized objects. In terms of the accuracy of prediction for such objects this seems to be true. Psillos argues that for objects that are very small or large, Newtonian mechanics is no longer approximate with Einsteinian physics or quantum mechanics. While Psillos may argue that these concepts have overlapping core causal properties that allow for these terms to be used interchangeably in this case, he does not make this claim. As shall be shown above, what counts as a core causal property seems to rely on one's epistemic stance. Thus, Psillos has not conclusively shown that these theories share the same ontology.

For instance, as Thomas Kuhn notes in his (1970) that one of the characteristics of Newtonian mass is that it is conserved while Einsteinian mass is convertible with energy (102). As Kuhn states, "Only at relatively low velocities may the two be measured in the same way, and even then they must not be conceived to be the same" (1970, 102). Psillos seems to be confusing similarity of measurement with similarity of ontology. The overthrow of Newtonian theory and the acceptance of Einsteinian theory is an example of the pessimistic meta-induction on a global scale. If Kuhn is correct, then Psillos will have to show how it is that his theory of reference works in this case. Moreover, if Stanford and Chang are correct, then Psillos' own examples fail to show how his theory of reference blocks the pessimistic meta-induction. Lastly, if my argument is sound that Psillos' implicit voluntarism undermines the normative force of his theory of reference,

then his theory of reference cannot objectively block the pessimistic meta-induction.

Thus, the mode of relativity seems to defeat Psillos' arguments against the pessimistic meta-induction.

4.2 Underdetermination

Psillos contends that he has refuted the empirical equivalence thesis and the entailment thesis, two premises necessary to argue for underdetermination. He argues that history shows the former is not diachronic. Psillos seems to claim that when a rival theory has been refuted it has been refuted for good. However, the previous section noted leeching and neo-Lamarckism as instances where seemingly refuted practices and theories have been resurrected. Thus, while Psillos seems to have shown how the empirical equivalence thesis can be overcome temporally, he has not shown that the threat of this thesis can be permanently banished. Psillos attacks the second thesis by maintaining that many of the examples of auxiliary hypotheses used by the proponents of the Duhem-Quine thesis are likely just trivial. He thus dismisses this thesis as a "promissory note" (1999, 165).

Moreover, Psillos' thinks that he has found a way, through the causal-descriptive theory of reference, to distinguish between trivial and non-trivial hypotheses. However, as I have argued above, Psillos seems only to have shown how it is that he distinguishes trivial from non-trivial hypotheses. Following the voluntarist theory of reference, what is

considered trivial and non-trivial differs from one person to the next. Returning to Psillos' example of the luminiferous ether and the electromagnetic field, he regards the proposed constitution of the luminiferous ether to be trivial, non-fundamental, and expendable. Yet, those scientists who ceased to search for the luminiferous ether did so because the constitution of this proposed entity was supposed to create certain phenomena (in this case, ether drag). Clearly, the constitution of the luminiferous ether was a fundamental, and not a trivial, hypothesis to these scientists.

Moreover, Psillos uses examples of underdetermination that he can easily dismiss as trivial since the differences between the underdetermined theories seem to be ad hoc additions to both theories. For example, he examines the following instance taken from van Fraassen in his (1980) where the latter putatively seeks to support the empirical equivalence thesis:⁵⁶

Let *NM* stand for Newtonian Mechanics, *R* be the postulate that the centre of mass of the solar system is at rest with respect to absolute space, and *V* be the postulate that the centre of mass is moving with velocity v relative to absolute space. Then *NM* & *R* and *NM* & *V* will be empirically indistinguishable given any evidence concerning relative motions of bodies and their absolute accelerations. (Psillos 1999:166)

Psillos claims that *NM* & *R* and *NM* & *V* use the same ontology and ideology for space, time, and motion. He agrees with Earman (1993) that there is no significant difference between *R* and *V*. Thus, this example does not support the empirical equivalence thesis.

While the above example employed seemingly trivial ad hoc additions to create underdetermination between theories, there are historical cases of underdetermination can be found involving theories of the physical structure of space. These theories of space do

⁵⁶ According to Maarten Van Dyck (2007), this example is not an instance of underdetermination but meant illustrate how empirical content can be isolated from non-empirical content. Van Fraassen (2007) agrees with Van Dyck's characterization of this example.

not appear to be ad hoc additions to the theories in which they are a part, but these theories of space are not directly testable. Poincaré's conventionalism is often cited as an example of this kind of underdetermination. However, as noted in Chapter Two, Psillos dismisses underdetermination involving theories of space since they are beyond the reach of science to test them. Hence, Psillos concedes to underdetermination in these cases, but is more concerned with historical cases of underdetermination where one theory was accepted over its empirically equivalent rival on empirical grounds (such as the acceptance of the Copernicus' theory over Ptolemy's theory).

Yet, Psillos' attitude toward theories of space is not consistent with his position on abstracta. Psillos holds that the abstracta featured in scientific models must exist to explain how it is that the models can be approximately true of the entities and processes they represent. He is committed to the explanatory criterion as *the* criterion for establishing the real existence of an entity. Yet, it would seem that one's theory of space is as necessary for the explanatory success of a theory as any idealization or abstraction in science. Psillos concedes that there are several problems that accompany the commitment to the existence of abstract entities. Psillos does not enumerate them, but perhaps he has in mind such problems as explaining how these entities could be discovered if they do not occupy time and space or have any causal powers.

However, theories of space are necessary to provide a framework from which physical theories can be constructed and tested. For example, Michael Friedman notes that when Carl Friedrich Gauss attempted to empirically determine the curvature of space by measuring the angle sum of a terrestrial triangle determined by three mountain tops, Gauss failed (Friedman 1999, 7). The triangle Gauss employed implicitly presupposes

that light rays travel in straight lines, thus the concept of “straight line” is already well defined as the path taken by a ray of light. However, Arthur Eddington’s 1919 experiment seemed to show that light travelled along a curved path. Yet, the results of Eddington’s experiment could be interpreted as showing that light travels in a straight line, but in *Riemannian space* where space is curved (Berlinghoff, Grant, and Skrien 2001, 121). Thus, there is no way that is independent of the geometrical and optical principles being assumed in the above examples to test these same principles (Friedman 1999, 6-7). Since there is no direct way to connect sensory experience to physical geometry, non-empirical factors, called *conventions*, must mediate between sensory experience and geometrical theory. For instance, the concept of a straight line is a convention mediating between the representation of a finite segment between two objects in the world (say, two telephone poles) and the distance between the objects being represented. Friedman concludes, “The upshot is that it is in no way a straightforward empirical matter of fact whether space is Euclidean or non-Euclidean” (1999, 7). As noted in the paragraph above, according to the explanatory criterion, the indispensability of theories of space as frameworks from which to construct theories should make them explanatorily more crucial than the abstracta that represent observable phenomena. If Psillos is to consistently apply the explanatory criterion, then he must believe in the existence of all of the theories of space that fit this criterion.

Furthermore, abstracta are as untestable as theories of space yet Psillos remains agnostic about theories of space, and believes that the explanatory indispensability of abstracta shows that abstracta must exist. While he concedes that some areas of inquiry are beyond our ability to grasp, Psillos does not remain agnostic about the existence of

abstracta though the former are as necessary for the explanation of scientific phenomenon as the latter. If Psillos is to be consistent, then it seems he must believe in one or more theories of space. For example, he could rely on the theoretical virtue of simplicity to choose between theories of space in different contexts. Euclidean geometry works best for medium sized objects, such as cars and parking lots, while Riemannian geometry works best when dealing with large scale objects, such as galaxies. Thus, Psillos could commit himself to an ontically pluralist view of physical space. However, if he chooses this option, then he will fall victim to the mode of relativity. If he believes in only one theory of space, then he must show how he has escaped the underdetermination among them or fall victim to the mode of hypothesis. Or, Psillos can abandon the explanatory criterion upon which he bases his belief in abstracta, and thus consistently remain agnostic concerning theories of space. But, he if chooses the latter option, then Psillos will lose what he claims is fully true of scientific theories and, implicitly, will lose the objects of scientific knowledge.

4.2.1 Theoretical Virtues

Psillos appeals to the epistemic character of the theoretical virtues to help putatively undermine the threat posed by the underdetermination of theory by evidence. The theoretical virtues, such as coherence with other established theories, unifying power, lack of ad hoc features, and the capacity to generate novel predictions” comprise

the explanatory power of a theory (Psillos 1999: 171). While one or more theories may fit the empirical evidence equally well, it seems unlikely that more than one theory will also have equal explanatory power to another. The theory with that is empirically adequate and has the greatest explanatory power is the best theory.

However, as described in Chapter Three van Fraassen has putatively shown in his (2008) many paradigm cases of great scientists who have eschewed the scientific realist's requirement that a good theory must explain. In the following quotation, van Fraassen argues against what he calls the "Aristotelian ideal" that science must give explanations, "Galileo, Gassendi, Boyle, Descartes, and Newton consciously and explicitly refuse to take on this Aristotelian task for science, or to accept it as a criterion of success for science. Indeed, they claim that the modern era's scientific success derives largely from their rejection of that tradition" (van Fraassen 2008: 278). For example, Newton refused to give an account of gravitation in his mechanics, but his theory was still seen as successful because of its predictive power. Moreover, quantum phenomena cannot be given causal explanations, but quantum mechanics is also considered successful because of its predictive accuracy. Furthermore, as sketched in Chapter Three, van Fraassen appears to have shown how theoretical virtues are used to generate pragmatic results rather than for their putative epistemic virtues.

Psillos contends that the theoretical values, which together constitute a theory's explanatory power, are the same values that meet the criteria for calling a statement true. He notes that the challenge is to show which explanatory virtues are related to the likelihood of a theory's truth. As shown in Chapter Two, he contends that the virtues which constitute the explanatory power of a theory are indirectly evidential. The

background theories, which scientists use to gauge the theoretical plausibility of new theories, are those with the greatest evidential support and theoretical virtues. The background theories' evidential support and theoretical plausibility are preserved, and appear in, the new theories which they warrant. Thus, the virtues which make up explanatory power become evidential because they appear in theories which have achieved theoretical plausibility and evidential support.

Psillos recognizes that, though the best theory we have now may be replaced by another that possesses more comprehensive and superior evidential support, this possibility does not undermine the nearness to the truth of the overthrown theory. Rather, such an occurrence only shows that we cannot attain the truth all at one time, and that our conclusions from the empirical evidence to verisimilitude can become more polished and that we should be more cautious. Furthermore, "they should commit us only to the theoretical constituents that do enjoy evidential support to the successes of the superseded theory" (1999, 175). The theory of justification Psillos invokes to support his claims is reliabilism. This method of justification rests on naturalism, and hence, reliabilism itself is judged empirically by its ability to produce and maintain true beliefs. However, Psillos has not shown that reliabilism is epistemic. As sketched above in the section of Chang and Stanford, both have indirectly argued that theories that are now seen as false would have been considered true by reliabilist standards. Both do directly contend that these discarded theories fit the theoretical virtues that Psillos claims are epistemic. If Psillos has failed to show how these theories could have been reliable but false, then he has not shown that theoretical virtues are epistemic.

In the quote in the paragraph above, Psillos implicitly argues that there is referential continuity among certain key terms during theory change. He makes this implication based on his causal-descriptive theory of reference. Based on this theory of reference, Psillos claims certain terms in the theory are explanatorily necessary and the reference of these terms is carried over into the theories that supersede them in revolutions. Yet, following the voluntarist theory of reference, Psillos cannot compel us to believe that underdetermination has been defeated. As shown above, using the examples of the change from Newtonian to Einsteinian mechanics and from the luminiferous ether to the electromagnetic field, what counts as a fundamental constituent of a theory has a voluntarist aspect to it. In the former case, the similarity of the measurement of mass is similarity enough for Psillos and Earman, but for those, such as Kuhn, who attach fundamental importance to the constitutive description of mass, the similarity of measurement is not enough to allow for referential continuity. As argued by Stanford, the same applies in the example of the luminiferous ether and the electromagnetic field. In the case of the Einsteinian revolution, for those who do not accept referential continuity between the Newtonian and Einsteinian theories, the impressive number of theoretical virtues that Newtonian mechanics possesses does not make it true. Therefore, the theoretical virtues do not appear to make a theory true, or even nearer to the truth. While Einsteinian mechanics superseded Newtonian mechanics because it possessed more of the theoretical virtues, if referential continuity is not allowed, then the theoretical virtues do not show an accumulation of nearness to truth. If an increase in nearness to truth is blocked, then the theoretical virtues are not epistemic.

4.3 Truth

Psillos is committed to a correspondence theory of truth. He holds that if we do not have to be certain of the truth of a claim to warrant its belief, then we can judge the truth of the claim. Psillos claims that there is a certain point at which there is sufficient evidence to warrant a belief. Furthermore, if the warrant for the belief is based on a reliable method, then no extra assurance for the belief is required. He concedes that the belief warranted by a reliable method could be false, but this is to be expected since reliable methods do not guarantee certainty. For instance, sight is regarded as a reliable method for obtaining true information about the world, however sometimes our sight tricks us, as with the apparent retrograde motion of Mars. Moreover, he contends that once a certain threshold of evidence has been reached, then belief in the claim in question is rational, despite the logical possibility that the belief could be false.

However, Psillos does not tell us how much evidence is necessary before a sufficient amount of evidence has been accumulated to warrant belief. If I have successfully shown that Psillos is implicitly committed to a voluntarist epistemology, then, (following a voluntarist epistemology) the amount of evidence to warrant a belief depends on the interests of the inquirer. Furthermore, the judgment of how fallible reliable methods are for obtaining true beliefs also relies on the interests of the inquirer. In the both cases, the threat of underdetermination and the pessimistic meta-induction vary with the amount of risk an inquirer is willing to take in being wrong. In the latter case, the voluntarist theory of reference becomes salient. Psillos seems to suggest that

the fallibility of reliable methods is something remote since his causal-descriptive theory of reference claims to show ontological stability in successful theories during scientific revolutions.

But, as I have attempted to show in this chapter, which descriptions are crucial to warrant belief in an entity varies with the interests of the inquirer. As Stanford has putatively shown, for some inquirers the constitutive properties are crucial to the reference of a term, for others (such as Psillos) they are not. Thus, if a putatively reliable method cannot give the correct description of a posited entity, and this happened often in the history of science, then the pessimistic meta-induction is more than a mere logical possibility, but a genuine threat to truth claims. If the transition from the luminiferous ether and caloric are to be considered test cases for the viability of Psillos' epistemology and theory of reference, then Stanford and Chang have presented seemingly serious challenges to Psillos' claim that he has refuted the pessimistic meta-induction. Moreover, as noted in the example of the theorists and experimenters in quantum mechanics, there is a lack of agreement in ontology even within the contemporary community of physicists. As this instance appears to have shown, the two groups of inquirers different interests shapes their ontology, and, despite their incompatible ontologies, they are still able to reliably produce predictions.

4.3.1 Psillos' Success Argument

As noted in Chapter Two, Psillos responds to two challenges to the ability of science to acquire objective truth about the world. The first challenge is that the world cannot be independent of us since we causally interact with it; the relation we have with the world is a reciprocal one. The second challenge is that, because of this reciprocal relation, all of the information we receive from interaction with the world is contaminated. The upshot for the correspondence theory of truth is that we cannot have a correspondence relation between statements and a world that is independent of us.

Psillos' response to these challenges is that our causal interactions provide us with our knowledge of the world, and that our knowledge is thus of interacted with objects. Whatever truth we acquire of them is not logically dependent on methods of verification, justification, and so on. These objects are independent of us in a logical, not causal sense, they are not the result of our theorizing and conceptualizations. Psillos concludes that, accepting that we acquire truth about entities causally, these challenges can be defeated by the scientific realist.

Bolstering his response to these challenges is Psillos' success argument which states that, "an appeal to truth-conditions is essentially involved in the explanation of why successful actions are successful. This explanation is based on the claim that the truth-conditions of the belief(s) on which successful actions were based have been realised" (1999, 247). Thus, what makes an action based on certain beliefs successful is that the belief's truth-conditions are realized. The beliefs that are systematically successful are

those whose truth conditions are the realization of “*referred-to entities standing in the referred-to relations*” (1999, 248). This reference relation is the correspondence relation between beliefs on which successful actions have been carried out and the world.

Psillos’ responses seemingly do not refute these challenges. Psillos has not address the odd results of quantum mechanics where the very act of observation seems to affect an experiment’s outcome. For instance, in Wheeler’s delayed choice experiment, the measurement of the particle seems to determine how the particle passes through the double slits, hence also determining its state as a wave or particle. Moreover, Psillos’ argument against these challenges is based on nothing more than the metaphysical stance. Psillos claims that the “basic philosophical presupposition of scientific realism” is that there is a natural-kind world independent of our minds (1999, xix). Hence, his argument that the truth we acquire about natural kinds presupposes there are natural kinds for us to discover the truth about. Since he does not argue for this claim his commitment to the metaphysical stance falls prey to the mode of hypothesis. He relies on the causal-descriptive theory of reference to show how natural kinds can be identified and how the terms that putatively refer to them does so.

However, if my argument that the causal-descriptive theory of reference is implicitly voluntarist is correct, then Psillos has not presented an objective method for accomplishing these tasks. Rather all Psillos has shown is that he assumes the existence of natural kinds and his theory of reference shows what he considers to indicative of natural kinds. Moreover, if Stanford and Chang are correct, then Psillos has not refuted the pessimistic meta-induction. In particular, Chang describes how Lavoisier went so far as to include caloric in his table of elements. Therefore, Lavoisier seems to have

regarded caloric to have been what Psillos describes as a “natural kind”. While Psillos claims that his epistemology is fallibilist, Chang and Stanford chose examples of discarded theories that Psillos is confident are clear instances of non-referential, non-explanatory, and trivial theoretical terms. Thus, Psillos’ theory of reference appears not to have shown how we can know that a scientific theory is false, much less that science can acquire truth about a mind-independent world. If Psillos cannot show that science acquires truth about a mind-independent world, then scientific realism fails.

Psillos’ success argument rests on his argument that reliability is an epistemic value. However, predictive success and control of nature can be had with beliefs that are now seen as false, or false by a certain community. Chang has apparently shown how it is that the caloric theory, though now seen as false, was successful. Stanford seems to have to have shown that the constitutive properties of the luminiferous ether were crucial in the abandonment of this concept in favor of the electromagnetic field. If my argument that Psillos’ theory of reference is implicitly voluntarist is sound, then whether or not entities referred-to stand in the referred-to relations depends on the interests of the inquirer.

In response to Psillos’ argument that we gain true beliefs about the objects we are manipulating when we manipulate them, I contend that it is not clear what it is that we are apparently manipulating. If the pessimistic meta-induction is cogent and the underdetermination thesis is sound, then successful experiments do not give us knowledge of what is being manipulated to create successful scientific outcomes. If Chang and Stanford are correct, then Psillos has not defeated the challenge posed by the pessimistic meta-induction. If Peter Galison (1997) is correct, then there are many

instances of underdetermination that do not pose a problem for the acquisition of reliable results. Psillos may respond to this latter objection that cases of underdetermination are eventually settled. However, this is a promissory note. As noted earlier in this chapter, the ontological status of the gene has gone from being an abstract entity early in the twentieth century to being a locus on a chromosome, then a specific nucleotide sequence, and now back to being an abstract entity (Sapp 2003: 204-205). Thus, if we cannot settle once and for all what it is that we are manipulating (especially concerning posits as explanatorily crucial as the gene), then we do not seem to gain propositional knowledge of what we are apparently manipulating.

To counter the above objection, Psillos may rely on instrumentation to argue that we now have greater knowledge of the imperceptible world because we can observe it through instruments. Yet, van Fraassen (2008) contends that when we employ instruments to “aid” our senses we seem to be creating phenomena instead of uncovering a hidden world of natural kinds. For instance, the microscope creates new phenomena, such as paramecium, to be accounted for by our theories. These phenomena are also created by nature as optical phenomena on a par with rainbows and reflections in the water. These phenomena are spoken of as if they were things, but we are wrong to do so since they are not material objects. For example, the rainbow is not a material arch though it looks that way. Moreover, the rainbow seems to be in different locations, but with respect to the different visual fields of two different people since “we would see the colors ‘attached’ to the same part of the cloud, modulo parallax” (2008, 102). If the people say they are seeing two rainbows, then they are not counting the same thing, especially since what they would be counting is not a thing. Furthermore, they are not

hallucinating since the former are subjective and private experiences while the experience of seeing a rainbow is public. However, seeing a rainbow is akin to a subjective hallucination because the people who see them do not see real things. A rainbow is a public hallucination created by nature. As van Fraassen states, “So public, in fact, that the camera captures them as well!” (2008, 103).

The images a microscope produces can be considered of real things. The scientific realist will use the inference to the best explanation to conclude that unobservable entities are responsible for the images being seen in the microscope. Van Fraassen claims to agree with the core of their argument, but not with its conclusion. He contends that, while the success of the microscope does partly derive from their potential to represent the images they produce as things that exist independently of the instruments that produce them, they are like the rainbow in that they can be examined publicly and are produced optically. However, unlike the rainbow (which is not real), the phenomena produced by the microscope need not be regarded as real (there is an element of choice that does not exist with the rainbow). The image we see in the microscope may be a copy of a real thing invisible to the unaided eye, or a public hallucination.

Adopting an agnostic stance on this topic does not stop us from gathering empirically presentable evidence through a microscope, or to base our practice (e.g. medical practice) on what the microscope shows us. In particular, the significant correlations between the products of different instruments in like situations, and between the products of the same instrument in a situation which can vary, do not need to be accounted for by unobservable external causes to be coherent or useful. We can make

images in predictable ways since we are capturing regularities in the phenomena between the objects and events being examined, and the relations between them (2008, 109).

Psillos would object that the inference to the best explanation shows that unobservable entities are necessary to account for the cause of what we are seeing under the microscope. However, the pessimistic meta-induction and underdetermination are two challenges to this form of inference. In the first instance, there have been theories (such as Newton's) that were seen as the best explanations of some phenomena (e.g. the orbits of the planets) that are radically different from the theories used to explain the same phenomena now. Moreover, the underdetermination thesis shows that the same phenomena can be explained by two or more theories. Galison has described how different sub-communities within physics can be committed to different ontologies since one ontology works better in one context rather than another. As argued earlier, Psillos' use of the inference to the best explanation relies on his theory of reference. However, his theory of reference is a voluntarist theory of reference, where what counts as a fundamental description of a posited entity relies significantly on the interests of the person positing the entity. Depending on the interests of the inquirer, different explanations will arise based on different fundamental descriptions of posits. Hence, what counts as the best explanation for a given phenomenon relies on the interests of the inquirer.

For instance, as Mara Beller (1999), in her examination of quantum physics, notes: "The proliferation of opinions, or perspectives, on the wave-particle issue was connected with the ambiguity of designating terms. There was no agreement on the necessary and sufficient attributes of a particle" (1999, 232). For example, Erwin

Schrödinger thought the concept of the particle was obsolete since particles could no longer be distinguished from one another under the new quantum statistics. Thus, he adopted the wave ontology. Niels Bohr saw this phenomena as a limit on the classical concept of “particle”, not a reason to abandon the concept altogether. According to Richard Feynman, the electron was neither a wave nor a particle (1999, 233). As Beller states, “Difficulties in the discussion of the wave-particle issue were further aggravated by disagreement about which (wave or particle) attributes are ‘essential’ and which are merely artifacts of interaction” (1999, 233). She goes on to note that this ambiguity has been theoretically fruitful from the beginnings of quantum theory until today. For example, William Duane, Pascual Jordan, and other physicists did not see the diffraction of light (or matter) to indicate that it had a wave nature. Rather, diffraction was simply an artifact of the quantized structure of the grating used in the experiment.

Thus, from the examples given above, Psillos has seems not to have given sound arguments in favor of science’s ability to acquire objective truth. In particular, the arguments of Chang and Stanford appear to undermine Psillos’ success argument. Van Fraassen seems to give a compelling for suspending judgment on whether or not instrumentation provides a window into an unseen world. Galison describes how different ontologies are useful in different contexts. Beller describes the lack of consensus among physicists regarding wave/particle duality. As with Galison, she appears to show that this lack of consensus does not affect the empirical success of quantum physics. All of these examples support the voluntarist theory of reference’s claim that reference is fixed by the interests of the inquirer. Therefore, Psillos has seems

not to have successfully argued that we can gain knowledge of entities through our interactions with them.

4.3.2 Truth-likeness

Psillos argues that, thus far, formal accounts of truth-likeness are untenable, since the truth-likeness of scientific theories can only be measured in relation to each other. Hence, a non-relative measure of a theory's truth-likeness cannot be obtained. However, if Psillos' causal-descriptive theory of reference is voluntarist, and thus relative, then his intuitive approach to truth-likeness, which relies on his theory of reference, is also plagued by relativism. I shall argue, following Psillos' arguments against existing formal accounts of truth-likeness and my arguments against his intuitive account, that there does not seem to be a viable account of truth-likeness. If there is no tenable account of truth-likeness, then scientific realism fails.

As sketched in Chapter Two, Psillos recognizes that scientists idealize and abstract away from the phenomena in their theoretical representations making the theoretical representations of the phenomena false. But, Psillos claims false descriptions can be cognitively significant, and thus truth-likeness is achievable because the false descriptions specify the degree to which their representations diverge from the phenomena. Moreover, Psillos contends that if truth is considered to be fittingness, then approximate truth is "*approximate fittingness*: a description, statement, law, theory are

truth-like if and only if there are respects and degrees to which they fit with the facts” (1999, 276-277). Furthermore, Psillos claims that the intuitions behind his theory are that “a theory is approximately true if the entities of the general kind postulated to play a central causal role in the theory exist” (1999, 277). The justification for the theory’s approximate truth relies on the inference to the best explanation.

Psillos seems to assume that his intuitions are shared by all scientific realists. However, as shown in the section on his causal-descriptive theory of reference, one may be committed to Psillos’ three stances of scientific realism, but understand what these stances mean in ways that are different from Psillos. Hence, while Psillos has attempted to show why we should adopt his intuitions regarding approximate truth and approximate fittingness, because of the incompatible ways that his three stances can be interpreted, he seems only to have succeeded in described his own version of scientific realism.

Therefore, his intuitions and his intuitive account of approximate truth both fall prey to the mode of relativity. For instance, using the causal role of entities as the criterion for claiming their existence is not a compelling criterion. Depending on the interests of the inquirer, detection of the constitutive elements of the postulated entity may be the deciding factor in whether or not an entity can be said to exist. For some, if the constitutive elements cannot be shown to exist, then the entity does not exist.

Moreover, specifying to what degree the theoretical descriptions differ from the phenomena will not convince an inquirer with criteria for truth that are more demanding than Psillos’ criteria. Again, following the example of the “luminiferous ether” this term was abandoned when the constitutive characteristics of the posited entity could not be

detected. Thus, the same argument used against Psillos' casual-descriptive theory of reference can be applied *mutatis mutandis* to his intuitive account of truth-likeness.

4.3.3 Background Knowledge

Chapter Two discusses how Psillos counters van Fraassen's attacks on background knowledge. Ultimately, Psillos' defense of this putative form of knowledge rests on the problems of epistemic risk. He states that he is willing to risk more epistemically when he claims that background theories are approximately true since this increase in risk is necessary in order to move away from ignorance. By taking on an extra epistemic risk, the scientific realist aims to know more about scientific theories than their empiricist colleagues. He concludes that the risk is worth the expense.

However, as I have argued above, Psillos does not initially provide arguments for why he holds the intuitions he holds, and in this respect he succumbs to the mode of hypothesis. By allowing voluntarism to inform his epistemology and philosophy of science, Psillos falls prey to the mode of relativity. However, once Psillos has laid bare his intuitions, he seems to seek arguments that support his intuitions while arguing against those that challenge his intuitions. For example, he supports the inference to the best explanation because his intuitions tell him that the success of scientific theories requires an explanation. Moreover, he assumes that good scientific theories are ones that explain, rather than those that empirically successful.

However, if van Fraassen is correct, then many scientific theories that have been predictively successful were not explanatory but still accepted as good scientific theories. Hence, if Psillos did not have the intuition that scientific theories must be true to be successful, then perhaps he would not believe in the inference to the best explanation. Thus, by beginning his defense of scientific realism with intuitions unsupported by arguments, seeking arguments that support his intuitions, and then using these arguments to justify his intuitions Psillos is undermined by the mode of circularity. Furthermore, if the arguments presented above against his causal-descriptive theory and his use of the inference to the best explanation are sound, then Psillos has fewer resources to justify his epistemic optimism. If Psillos' epistemic optimism is enervated, then he should not hold that background knowledge is approximately true.

4.4 Psillos' Account of Scientific Knowledge and The Facticity Criterion

In Chapter Two, it was noted that Psillos' account of scientific knowledge seems to be modeled after John Pollock's account of knowledge.⁵⁷ However, Psillos has not established that his account of scientific knowledge meets the facticity criterion because he has not shown that his intuitions about science as a generator of knowledge are true. His three stances of scientific realism are merely his opinion that science arrives at true beliefs on a reliable basis. Thus, the foundation for his philosophy of science falls prey to the modes of hypothesis and relativity. His defense of the 'no miracles argument' fails

⁵⁷ Pollock's account of knowledge is that knowledge is justified true belief with no ultimate defeaters.

because his defense of our use of induction and deduction are undermined by the modes of relativity and hypothesis. His defense of these logical practices is also used to block challenges to the inference to the best explanation.

However, van Fraassen appears to have shown that the inference to the best explanation can be seen in a purely pragmatic light, along with the theoretical virtues. If van Fraassen's argument that we cannot be in a position to know that we have arrived at the best explanation is sound, then we cannot know that we have a belief that can be undefeated. Moreover, Psillos' success argument is meant to be a defense of the epistemic value of scientific theories, but Chang and Stanford show that the constituents of a theory that played a crucial role in the empirical and explanatory success of past theories, are now seen as false. Furthermore, the criterion of success used by Chang and Stanford is Psillos' own criterion. Galison also indirectly undermines the success argument when he shows the indeterminate ontological status among physicists of entities that are putatively crucial to the success of quantum mechanics. Thus, Psillos appears not to have safeguarded ampliative-abductive reasoning. Hence, his theory of justification seems to fail. According to his own account, without a theory of justification Psillos cannot show how it is that we can acquire true beliefs, and why we should believe one belief over another. Therefore, his account of scientific knowledge seems to fail.

The inference to the best explanation, the theoretical virtues, and the success argument are all used to justify the ability of Psillos' theory of reference to objectively pick out natural kinds. According to Psillos' version of scientific realism, what are real about the world are natural kinds. Natural kind terms are made true by their reference to entities and processes that answer to the core descriptions given by the terms that

putatively represent them. Yet, as argued earlier, Psillos' implicit voluntarism leads to referential relativity, and thus he cannot claim that his theory of reference picks out natural kinds in a non-*ad hoc* fashion. Thus, Psillos has not shown that scientific theories represent the world in a true, or nearly true, fashion since his implicit voluntarism falls prey to the mode of relativism. Therefore, since relativism makes objective truth indeterminate, his account of scientific knowledge fails to meet the facticity criterion.

If Psillos' theory of reference fails, then he has not blocked the challenges from underdetermination and the pessimistic meta-induction. If the argument against his theory of reference is sound, then we cannot know which of two or more rival theories is undefeated. If the latter challenge is true, then we cannot know whether or not our current theory is true. Thus, Psillos' response to the challenges of underdetermination and pessimistic meta-induction fail to meet the facticity criterion.

Moreover, as discussed in the section on underdetermination, Psillos allows for the existence of abstracta based solely on the explanatory criterion. Abstracta are so crucial to explanation in scientific theories that Psillos holds the full truth of scientific theories is not about the world, but about the abstract entities used to represent the world. If his account is true of abstracta and not the world, then his account fails the facticity criterion. He recognizes that his view is extremely problematic due to many problems that come with showing how it is that we can know, independently of the explanatory criterion, such entities exist. Psillos mentions these challenges, but does not address them. It appears that, since existence is widely seen as being established by a putative entity's causal powers, we cannot know if abstract exist in this manner because they do seemingly cannot cause anything. By ignoring this challenge, and whatever problems he

admits to be significant, Psillos' commitment to the existence of abstracta is undermined by the mode of hypothesis. If Psillos cannot show that abstracta exist, then his account of scientific knowledge cannot show what is fully true. If his account of scientific knowledge cannot show what is fully true, then it cannot meet the truth condition of the justified true belief or true belief accounts of knowledge. If his account of scientific knowledge cannot meet the truth condition, then his account fails.

Further problems arise for Psillos' account of scientific knowledge in terms of what he counts as truth-likeness and background knowledge. In the former case, he states that his account of truth-likeness is intuitive. However, it is not clear that everyone shares his intuitions as to what constitutes truth-likeness. Thus, his notion of truth-likeness falls prey to the modes of relativity and hypothesis. Moreover, Psillos relies on his intuitions to describe background knowledge. He employs the inference to the best explanation to justify his belief in background knowledge, but concedes that he is committed to the inference to the best explanation because he is an epistemic optimist. Psillos' defense of background knowledge rests merely on his optimism and not upon a sound argument. Thus, this view too is undermined by the modes of relativity and hypothesis.

Therefore, the arguments and claims Psillos' presents to support his account of scientific knowledge *appear* not to meet the facticity criterion. Hence, his account of scientific knowledge *seems* to fail in several respects since Psillos' arguments *appear* not to meet the facticity criterion. If the facticity criterion is a sound criterion by which to measure accounts of scientific knowledge, then Psillos' account of scientific knowledge fails.

4.5 Alexander Bird

In the subsections below, I will employ the Pyrrhonist modes to raise objections to Bird's account of scientific knowledge:

(1) First, I shall use the mode of hypothesis to challenge Bird's commitment to Williamson's primitivist account of knowledge.

(2) If this argument is not strong enough to arouse doubt, then I will attempt to instill equipollence in the reader by objecting to Bird's notion of what is "scientific". Since Bird uses Alfred Wegener's theory of continental drift as a clear example of what is not scientific, I shall use Naomi Oreskes' (1988) description of Wegener's theory to in an effort to show that Bird's notion of "scientific" seems to be untenable. I shall endeavor to contend that if his notion allows for Wegener's theory to count as "scientific", then Bird's notion is untenable. If his notion is untenable, then it seems he cannot have an account of scientific knowledge without first identifying what is scientific and what is not.

Bird seems to share the same views with Psillos on to the inference to the best explanation and the correspondence theory of truth. Thus, to avoid redundancy:

(1) I shall not discuss these features of Bird's philosophy of science.

(2) However, Bird includes a stronger form of inference than the inference to the best explanation in his philosophy of science. This stronger inference form he calls “the inference to the only explanation” which shall be discussed in a subsection below. I shall attempt to argue that this inference appears to fail.

(3) Furthermore, both Bird and Psillos have a very similar take on the causal-descriptive theory, but I shall discuss it in a subsection below because Bird’s version gives fewer details on what counts as a core casual description. Since Bird’s lack of clarity is different kind of objection to his causal-descriptive theory I have included it in this section.

(4) Moreover, I will discuss his distinction between full truth and approximate truth since he does not seem to be committed to the view, adopted by Psillos, on abstracta as being what is fully true of scientific theories.

(5) I shall attempt to contend that Bird’s position of truth and approximate truth appear to fail, and thus his account cannot meet the facticity criterion and fails also.

4.5.1 The Primitive Account of Knowledge and The Mode of Hypothesis

As noted in Chapter Two, Bird follows Williamson's primitivist account of knowledge where knowledge is not defined, but is assumed to be a fundamental term not in need of definition. Williamson contends that every attempt to analyze knowledge into component parts has met with failure because of Gettier cases. From this history of failure Williamson concludes that knowledge is unanalyzable because it is a fundamental concept. Since knowledge is a fundamental concept it is not in need of explanation in terms of component parts. While Williamson seems to have sound arguments against the analyzability of the concept of knowledge, his solution falls prey to the mode of hypothesis.

4.5.2 Bird and Scientific Methodology

In this subsection, I hope to generate equipollence in the reader by giving what seem to be plausible alternative accounts of Bird's notion of what is "scientific". Bird contends that the methods of science cannot allow for the acquiring of a true belief by accident. He claims that Alfred Wegener's theory of continental drift is a theory that hit upon the truth accidentally since the method Wegener used is not recognized by Bird as rigorous enough to be considered scientific. Bird (1998) standard historical reason given

for the initial rejection of Wegener's theory is that it lacked a causal mechanism (88).⁵⁸ However, as shall be shown below, Wegener's theory was as scientific as Newton's theory of gravity and quantum mechanics, both of which lack causal mechanisms.

Bird describes Wegener's theory of continental drift as a lucky guess since it was not supported by enough evidence and faced strong counter-evidence. However, Naomi Oreskes (1988) contends that Wegener's theory was supported by sufficient evidence, and that the alternative theory of land bridges was *ad hoc*. For example, Wegener's theory could explain why fossil assemblages were alike in distant lands since the same species occupied land that was directly connected before breaking off. Moreover, his opponents could not explain this phenomenon and relied on *ad hoc* adjustments to their theories of static continents, such as the positing of land bridges between continents that had since sunk into the ocean (Oreskes 1988, 315). Furthermore, the continents were discovered to be made of less dense material than the ocean basin, and the denser material that composes the ocean floor also underlies the continents (Oreskes 1988, 318).

Thus, the continents could float on the material that makes up the ocean floor, as "Ices floes in water" (Oreskes 1988, 317). These discoveries together are known as *isostasy*. Isostasy was widely confirmed when Wegener presented his theory. Moreover, Wegener included in his theory the long standing geological notion that the continents could move through the underlying denser material because the latter acts over geological time as an extremely viscous fluid (Oreskes 1988, 318). From isostasy and this latter notion, Wegener hypothesized that major geological formations—mountain ranges, islands, rift valleys, and so on—were created by the horizontal movement and collisions

⁵⁸ Cf. Oreskes, N. (1999) *The Rejection of Continental Drift: Theory and Method in American Earth Science*, Oxford: Oxford University Press.

of continents. Furthermore, he proposed that the entire surface of the earth was once covered by one continental layer which eventually broke apart to form separate continents.

Wegener developed his account by comparing the similar coastlines of South America and Africa, and Europe and North America. He assumed that these similarities were too close to be mere coincidences. To show how the continents may have fit together into one contiguous continent, Wegener did not match the coastlines of the continents, but the edges of the continental shelves since the coastlines would have changed more through erosion and fluctuating sea levels (Oreskes 1988, 319). Well-established geological evidence supported Wegener's theory:

The stratigraphic successions of South America and Africa were strikingly similar throughout the nearly 200 million years of the Mesozoic era. Likewise, certain igneous complexes occurred in both places, and folds ran continuously when the continents were reunited. Comparable continuities could be achieved by uniting North America with Europe, and India with Africa. (Oreskes 1988, 319).

Further evidence from Philip Sclater's observations showed that the island of Madagascar possessed nearly none of the animals common to Africa, such as giraffes and lions, but did have a host of lemur species common to India. Many of the lemur species found on Madagascar were almost identical to those found in India. To explain these observations, Sclater postulated the sunken continent of Lemuria as a land bridge between Madagascar and India. Darwin's theory of evolution supported Sclater's notion that Madagascar's lemurs originated in India since the lemurs were too alike to have evolved independently (Oreskes 1988, 319). While other zoological similarities were observed between fauna across the oceans, the most compelling evidence in favor of Wegener's theory came from earthworms, as Oreske notes:

Wegener pointed not only to Sclater's lemurs, but also to Glossop teris flora, Mesosauridae reptiles, lumbricidae earthworms, and many other species as

indicators of continental contiguities. The earthworm's distribution seemed to be particularly significant since earthworms can neither swim nor fly, nor do they have resilient seeds or a dormant stage that might permit passive distribution. Further more, in many cases the species could be found only on the coastlines on either side of oceans, suggesting a fragmented habitat. (If the continents had not moved, the required connections would have been extremely long and crossed several climatic zones: It seemed unlikely that a species would persist, morphologically unchanged, over such a distance only to stop its migration precisely at the other end of the land bridge.) (Oreskes 1988, 321)

Thus, Wegener's theory of continental drift is supported by non-*ad hoc* evidence from the independent science of biology. If Oreskes is correct, then the geological and biological evidence that Wegener used to advance his theory were well-established. Furthermore, his theory seems to have been confirmed independently by well-confirmed biological observations. Moreover, the alternative theory of sunken continents acting as land bridges could not explain phenomena, such as the distribution of earthworms, in a non-*ad hoc* fashion. The *ad hoc* character of this latter hypothesis appears in the lack of evidence for sunken continents and for the failure to be supported by well-confirmed biological observations. Since the *ad hoc* hypothesis appears to be the only competitor against Wegener's theory, it would seem that the later theory is an instance of the inference to the only explanation. Therefore, there seems to have been sufficient evidence for Wegener's theory to count as an item of scientific knowledge *by the standards of E=K*. Thus, *a fortiori*, Wegener's theory is scientific by Bird's standards.

According to Oreskes, the standard story as to why continental drift was not rejected in Wegener's lifetime is because his theory lacked a causal mechanism. Oreskes denies that Wegener did not present a causal mechanism in his theory, but I shall not describe it here since it is not relevant to the argument against Bird that Wegener's theory is not scientific knowledge. However, Bird may believe the standard story to be true and that is why he does not consider Wegener's theory to be scientific knowledge. If this is

the case, then he will have to explain why he uses certain scientific theories that do not posit causal mechanism as examples of scientific knowledge.

For instance, the scientific status of Newton's mechanics was suspect since it could not explain how objects influenced each other at a distance. Newton called this mysterious influence "gravity" but remained agnostic as to its nature. However, Newton's theory was well-supported by background theories, such as Kepler's laws and by the empirical success of the theory, such as its accurately predicting the motions of celestial bodies (except Mercury). If Newton could not explain a key component of his theory, then it would seem that Newton's theory does not count as scientific knowledge, but as a lucky guess. Moreover, many quantum phenomena lack causal explanations, but Bird seems to think that quantum field theory is scientific (Bird 2007, 75). If a theory needs causal explanations to be scientific then, these two theories seem, by Bird's standards, to be lucky guesses. Thus, Bird seems to have failed to have blocked lucky guesses as being instances of scientific progress. If he cannot block such instances (especially in revolutionary cases such as those mentioned above), then his account of scientific knowledge does not accurately describe how scientific progress occurs. If Bird has given the wrong description of scientific progress, then he cannot claim that scientific progress is the accumulation of scientific knowledge.

4.5.3 The Inference to the Only Explanation

If the previous subsection failed to achieve equipollence in the reader, then I shall hope to instill it here and the subsequent subsection on Bird's take on the causal-descriptive theory of reference. While Bird assumes that he has refuted the pessimistic meta-induction and underdetermination, his inference to the only explanation and the mechanism that supports seem to be defeated by both of these challenges. For instance, there are cases where the only explanation had seemingly been decided upon, but has since been overturned. An example of the pessimistic meta-induction is the theory of inheritance of acquired characteristics. According to Jan Sapp, the standard history of biology states that this theory was refuted by Darwin's alternative account of natural selection. However, Darwin used this theory in his theory of evolution (Sapp 2003, 7).⁵⁹ Moreover, in the 20th century the inheritance of acquired characteristics was dismissed as unscientific Stalinist dogma because of its having been adopted by the Soviet biologist Trofim Denisovich Lysenko. Since Lysenko and his followers put forward a vague theory that was not scientifically testable Western scientists charged the theory with being unscientific (2003, 171-173).

Since the inheritance of acquired characteristics was thought to have been refuted, *genetic determinism* was regarded by the mainstream of biologists to be the only

⁵⁹ If the positing of a causal mechanism is necessary for Bird to consider a theory to be scientific, then Darwin's theory of evolution may not fit this criterion. If the theory of the inheritance of acquired characteristics is the causal mechanism behind natural selection, then Darwin's theory of evolution is in danger of being unscientific since the former theory is considered to be unscientific. If Darwin's theory is true despite its lack of a scientifically acceptable causal mechanism to account for natural selection, then what is often regarded in the history of science as a scientific theory would not be scientific according to Bird's criterion. If Bird does consider Darwin's theory to be scientific, then he must explain how it is despite its commitment to the inheritance of acquired characteristics.

explanation for the inheritance of traits across generations. Genetic determinism states that the expression of genes is not effected by the environment. Rather the genes expressed in an organism will either allow the organism to survive and reproduce or to die without offspring. Those organisms fortunate enough to survive and reproduce will pass on the genes that allow for survival onto their offspring. However, if the environment changes or the offspring express different genes, then the offspring with not survive and reproduce. Since genetic determinism was seen as the only explanation for the inheritance of traits across generations the mapping of the human genome was thought to be the 'holy grail' of genetics (2003, 201-202). It was thought that mapping the human genome would allow humanity to identify those genes that are desirable and those that are not. Once these genes were identified then geneticists would be able to control which genes would be expressed and which would not. For example, genetic defects such as cystic fibrosis and sickle cell anemia could be eradicated along with depression, alcoholism, and impulsivity (2003, 202).

However, the successful mapping of the human genome seems to have shown that genetic determinism is false. Instead of discovering one gene that expresses a given trait, geneticists appear to have discovered that one gene can express many different traits since the gene is modified by RNA (2003, 204). However, as noted in subsection 4.1.6 of this chapter, the theory inheritance of acquired characteristics is now known recognized as a genuine alternative to genetic determinism. Because earlier the inheritance of acquired characteristics seems not to have been scientific by Bird's standards it appears that genetic determinism would be the only explanation for biological inheritance. Yet, as has been shown, genetic determinism seems not to be the

only explanation available for biological inheritance, and the putatively refuted theory of the inheritance of acquired characteristics now seems to be a plausible candidate as the best explanation for how inheritance occurs in certain contexts.

If genetic determinism fits the criteria for being the only explanation, then it seems that epigenetics undermines this inference. Bird may reply that this case is not typical, but if other cases can be found, then it seems the inference to the only explanation fails. Other possible cases are Aristotle's theory that the Earth is stationary and the notion that space is absolute. In the former case, Aristotle noted that we do not feel the Earth moving beneath us, nor do we feel the movement of air around us as we would if we were moving (say, on a galloping horse). It seems that with the means for testing these hypotheses available to Aristotle he had the only explanation for why the Earth is not moving. Moreover, Newton's mechanics seemed to refute the notion that space was relative since his theory's empirical predictions seemed to be empirical support for the hypothesis that space is absolute. Again, it seems that Newton's explanation was the only explanation. If the above cases seem plausible, then Bird's inference seems to be false.

4.5.4 Bird and The Causal Theory of Reference

Similar to Psillos, Bird employs a causal-descriptive theory of reference. However, he does not give as much detail as Psillos as to how the causal-descriptive theory preserves reference through revolutions. Given the sketch of the causal-descriptive theory he gives, it is not clear how he would deal with the challenges to the causal-descriptive theory put forth by Stanford and Chang. For instance, Bird does not deal with the historical actors and their attitudes toward the putative entities in their theories. Thus, while it appears Bird would claim referential continuity from the “luminiferous ether” to the “electromagnetic field”, he does not discuss why the change in terminology took place.

As noted earlier in this chapter, Stanford seems to show that the scientists who believed in the “luminiferous ether” believed that the constitutive descriptions of this putative entity were crucial to the referential success of the term. To avoid the charge of precursoritis, it seems Bird would do well to explain why the scientists of the time thought that the constitutive properties of the “luminiferous ether” were important to the referential success or failure of the term. Since Bird does not mention voluntarism in his philosophy of science perhaps his version of the causal-descriptive theory will not fall victim to the charge of relativity. However, Stanford’s description of the historical scientists’ attitude toward the constitution of the ether should be explained. By accounting for this feature of the historical record Bird can explain why the description of a putative entity’s causal role is crucial to a term’s referential success.

Furthermore, it would appear a more damning criticism comes from Chang's treatment of how the explanation for the caloric theory's successes fundamentally relied on the descriptions of the causal role of "caloric". As shown earlier in this chapter, Chang seems to have shown how the descriptions of "caloric" were the best explanation of the phenomena in the theory's domain. Moreover, Bird follows Kuhn's take on exemplars as primary examples for how the puzzles that arise within a paradigm are to be solved. Bird considers exemplars to be a form of know-how and he considers know-how to be translatable into knowing-that. He does not clarify how this translation would be carried out nor how it fits in with his apparent commitment to the correspondence theory of truth. However, if the truth of an exemplar relies on its empirical success, then Bird would seem to be relying on a pragmatic theory of truth. To criticize the pragmatic theory of truth here would be redundant as I have done so in the section on Psillos. Nevertheless, an account of how it is that empirically successful scientific theories could be false would help to clarify Bird's scientific realism since it would clarify how what is putatively true of a scientific theory explains a theory's success. However, until Bird's causal-descriptive theory can meet this challenge and the others mentioned in this subsection, it is unclear if his theory of reference is tenable or not.

4.5.5 Full Truth, Approximate Truth, and Facticity

Bird argues that, from the approximate truth given to us by the idealizations and abstractions from the phenomena used in the construction of scientific theories, we can trace our steps back to the full truth necessary for knowledge. His argument for arriving at the full truth from approximate truth seems to be untenable since the path from approximate truth to full truth is not always visible. Many approximations in science cannot be traced back to their complicated truth. Many scientific conclusions are based on models that deviate wildly from the phenomena. For instance, as noted in Chapter Two, Elgin notes that experimental design deviates substantially from what is found in nature, since the laboratory is an artificial setting. She appears to show how experimental results that allegedly represent nature are manipulated into being realized.

For instance, the mice that are used in cancer experiments are bred to be susceptible to contracting cancer. Such breeding apparently does not occur naturally, and thus is not an approximation that can be traced back to the full truth as observed in the wild. Experimentation involves creating conditions that do not exist in the natural world, and using the findings from these artificial conditions to make claims about the natural world. Hence, if Elgin is correct, then experimentation relies on falsity. Moreover, Psillos accepts that the approximate truth science putatively acquires about the natural world is strictly false, while the abstracta of scientific theories is what is fully true. If Psillos is correct, then we do capture full truth by retracing our steps from idealization and abstraction back to the concrete phenomena.

According to E=K, the evidence that is used to support an item of knowledge must be an item of knowledge itself, and hence, cannot be false. However, if Elgin and Psillos are correct, then the empirical evidence used in science is only approximately true and strictly false. Thus, it seems much of the “evidence” that is the “background knowledge” for all significant scientific conclusions cannot be evidence or knowledge by Bird’s own standards. If the evidence has to be knowledge and knowledge is true, then Bird’s notion of scientific knowledge appears not to fall short of the facticity criterion.

4.6 Bas van Fraassen

In this section, I shall endeavor to show that van Fraassen’s account of knowledge appears to fail. Throughout the subsections below I will:

- (1) employ the arguments against views held by van Fraassen, as well as the Pyrrhonist modes, in an effort to instill doubt in the reader as to the tenability of van Fraassen’s account of knowledge.

- (2) In particular, I shall use Psillos’ arguments as a counter-balance to van Fraassen’s empiricism and voluntarism.

4.6.1 van Fraassen and The Empirical Stance

As with Psillos, van Fraassen assumes the empirical stance as the starting point for his epistemology and philosophy of science. Because a stance is an assumption unsupported by argument, the only dialectical support for his taking a particular stance is his permissive view of rationality and his observation that the inescapability of sense experience makes this experience a natural place to start our inquiries. I first discuss the former justification in this paragraph and the latter justification in a subsequent paragraph in this subsection.

According to voluntarism, we are as justified in choosing to pursue truth and avoid error to whatever extreme since these dispositions are not irrational. Van Fraassen's take on rationality is that one is permitted to believe what one wants as long as it is not contradicted by the world. However, as shall be discussed in a subsection to follow, because of van Fraassen's commitment to voluntarism and the theory-ladenness of language, it is unclear what counts as being true. Distinguishing truth from falsity is crucial to van Fraassen's position if he is to avoid irenic relativism and skepticism. According to van Fraassen, irenic relativism holds that there is no objective truth only truth for someone, while skepticism maintains that if there is objective truth, then it cannot be acquired and that rational opinion is impossible. Yet, van Fraassen apparently recognizes that different theories have different interpretations of the same observations. He seems to claim that the appearances of everyday objects, such as trees and snow, are

veridical. Moreover, he assumes that these appearances are not dependent on our minds for their existence.

However, according to van Fraassen's take on rationality, Berkeley's sensationalist idealism or Cartwright's agnostic sensationalism are rational. If these positions are rational, then there is a significant difference in what constitutes an appearance. In the Berkeley's case, they are mind-dependent sensations produced by the Mind of God, in the Cartwright's case, their ontological status is indeterminate. If these positions are permitted to be rational, then van Fraassen seems to fall prey to the irenic relativism and skepticism he hopes to avoid since these incompatible stances are allowed as long as they are consistent, and there seems to be no way to judge which stance is true. Hence, it seems van Fraassen boldly accepts the mode of hypothesis since he argues that we can choose a given position although we do not (and perhaps cannot) know if the choice we have made is true or not.

Furthermore, van Fraassen's voluntarism and permissive rationality seem to lead him into the following contradictory position: while he embraces metaphysics as a field which interprets the world in many, sometimes incompatible ways, van Fraassen does think that the world plays an important role in telling us which interpretation works and which does not. For example, he claims that scientific theories latch onto regularities, or they do not, and that is how we know whether or not scientific theories are successful. Furthermore, his commonsense realism seems to be the standard for judging the truth of statements and of theories. However, as noted in Chapter Three, van Fraassen claims he is not committed to commonsense realism on epistemic grounds, but on pragmatic grounds. This ambiguity between the epistemic and pragmatic threatens to undermine his

theory of rationality since what counts as true depends on the stance one takes. This ambiguity shall be discussed in detail in the subsection below in James Ladyman's (2007) objection to van Fraassen's epistemic contextualism.

4.6.2 Ladyman's Dilemma

James Ladyman (2007) argues that using a position adopted on pragmatic grounds to come to an epistemic conclusion leads to a dilemma. The dilemma is that if a true belief can be considered knowledge even if it was accepted for pragmatic reasons, then scientific realists' claims to knowledge about unobservables seems to be equal to the empiricists' claims of knowing a theory is empirically adequate. Yet, if epistemic reasons are a necessary condition for a true belief to count as knowledge, then the only scientific knowledge we have is of what has been so far observed (2007, 345). Van Fraassen's reply to this dilemma is that he adopts a contextualist view of knowledge where in a specific context, following David Lewis' (1996), after all relevant alternatives have been eliminated one can make a knowledge claim. For instance, in a specific domain the classical gas theory is empirically adequate and useful but beyond that domain it is neither empirically adequate nor useful (van Fraassen 2007, 349). Van Fraassen notes that whether or not something is really true cannot be answered in a context neutral sense.

However, van Fraassen claims that we can have objective knowledge. If “objective” means free of any particular perspective and a context is a particular perspective, then van Fraassen’s position seems to be self-contradictory. In Chapter Three, I attempted to argue that van Fraassen seems to be committed to facticity, especially since he asserts that, in spite of his relativism, truth is something objective and accessible. Furthermore, because van Fraassen concedes that all of our observations are theory-laden (and thus contextual) it would seem that what counts as true is contextual too. Despite this, van Fraassen argues as if everyday objects and events are observed non-contextually and that these observations can be used to judge the truth of a particular context. According to van Fraassen, a context is false if it does not fit the world in the way that is alleged. Phlogiston theory is one example of a context that does not fit the world. Moreover, van Fraassen claims that successful scientific theories are successful because they have discovered genuine regularities in world. Yet, what counts as a regularity may change with time.

For example, the retrograde motion of Mars and the flat, stationary Earth were both considered regularities, but now are now only appearances. The inheritance of acquired characteristics appeared to have been refuted, but now seems to be accepted by epigeneticists as a genuine regularity. As described in Chapter Two, van Fraassen relates how we can move beyond the appearance of retrograde motion to the phenomenon Mars’ true orbit. As shall be discussed in a subsequent subsection, van Fraassen relies on a distinction between appearance and reality that seems much like the distinction employed by his scientific realist opponents. However, before discussing van Fraassen’s distinction between appearance and reality I shall discuss a seemingly deeper problem for van

Fraassen's empiricism, viz. what counts as experience according to his version of empiricism. If van Fraassen cannot clarify this notion, then it is unclear how his empiricism is supposed to work as method of inquiry.

4.6.3 Nagel on What Counts as Experience

Jennifer Nagel claims that van Fraassen does not give an account of how we can tell what an experience is (2000 in van Fraassen 2007, 370). The latter argues that her objection does not count against him because he does not hold that experience is the sole source of our knowledge. Rather, he adopts experience as the starting point for our epistemic queries. Thus, he is not making a metaphysical claim about how knowledge is obtained, instead this is his stance. However, Nagel notes that skeptical challenges of how van Fraassen can distinguish reality from hallucinations and dreams still arise. Van Fraassen answers that experience is to be understood in the commonsense way of our being aware of what is happening to us. Sometimes he wonders if he is dreaming, but he dismisses such concerns, stating that he can rely on other people around him to let him know if he is dreaming or not. Van Fraassen states that such skeptical concerns are unanswerable by design and can be safely ignored. He again emphasizes that we are sailors aboard Neurath's boat and dependent on our own memories, the testimony of others, and new experiences to test our opinions. Furthermore, he notes that our experience includes the collective experience of our predecessors just as the ancient

empiricist school of medicine included the accumulated experience of their profession (2000: 371). Thus, he concludes such radical skeptical worries can be safely ignored.

However, seemingly unanswerable skeptical doubts cannot so easily be dismissed. Van Fraassen's response that he can rely on others to tell if he is dreaming (or sane) relies on the fallacious mode of circularity. If asked how he knows he is not dreaming, van Fraassen will reply that the people around him assure him he his not dreaming. If asked how he knows that they are not characters in his dream, presumably he will reply that the people around him have assured him he is not dreaming. Thus, he does not show in a non-circular manner that he is not dreaming. If experience is to be our starting point, then it seems subjective experience should be included in this starting point. If subjective experience is included in this starting point, then the old skeptical conundrums of how we know we are not dreaming become salient again.

4.6.4 van Fraassen and Observability

As noted in the subsection above, van Fraassen's view on the contextual nature of observability leads to the mode of hypothesis because of his reliance on the empirical stance to justify the verity of our senses. In this section, I describe Psillos' arguments against van Fraassen's notion of observability. These arguments seem to show that, even if van Fraassen can show how the contextual nature of observability is coherent, he cannot show that the verity of observation is tenable in the epistemic context. As

mentioned in Chapter Three, van Fraassen claims that the distinction between observable and unobservable entities is an empirical one. Our current scientific theories regarding the perceptual abilities of humans in the world decide for us which entities are observable and unobservable. Moreover, our ability to construct scientific theories also hinge on our physiological states. Thus, what is observable and unobservable is to be determined by our best theories of human biology, physiology and psychology (Psillos 1999, 195). These theories dictate that humans cannot use their senses to observe electrons, but can observe tables and could observe remote celestial objects.

Psillos counters van Fraassen's view by arguing that the biological and physiological theories van Fraassen relies on must be regarded as true to make his distinction between observability and unobservability work. However, these biological and physiological theories are committed to the existence of unobservable entities. Hence, van Fraassen will, "have to accept that what is observable is delineated by theories whose empirical adequacy can be judged only if they know *in advance* which entities (and phenomena) are observable" (1999, 195). Observability depends on empirically adequate theories, yet whether or not these theories are empirically adequate depends on a prior theory of observability. Hence, van Fraassen's account of observability seems to be viciously circular (1999,195).

Additionally, as noted in a previous subsection, the observable-unobservable distinction serves an epistemological function for van Fraassen, it shows what is epistemically acceptable and what is not. As Psillos notes,

all statements about the unobservable world are undecidable in that no evidence can warrant belief in theoretical claims about the unobservable world. If correct, this point would least motivate a (radical) empiricist epistemology: belief in theoretical assertions can never be justified

because no evidence can sway the epistemic balance in their favor. (1999, 198).

Van Fraassen seems to presuppose that unobservability is equivalent to epistemic inaccessibility, while observability is equivalent to epistemic accessibility. Psillos contends that these assumptions are untenable. Unaided senses have a poor epistemological track record. They are unable to adjudicate claims involving the empirical adequacy of theories. For example, our theories hold the temperature of Pluto is extremely low, yet no one can verify this by being sent to the surface of Pluto since no human being can survive such low temperatures.

Psillos further argues that it is mistaken to suppose that beliefs about observables are either immediately justifiable, or in no need of justification in a manner in which theoretical beliefs are not. As Psillos states, “Any plausible reason to think that a different *kind* of justification is always required for non-observational beliefs (e.g. beliefs based on instruments) would end up requiring *this* very kind of justification for observational beliefs as well” (1999, 199). For instance, one could argue that for an instrument-based belief to be justified one must first justify the belief that the instrument operates reliably. However, the human eye itself is a complex, and fallible, instrument, and thus beliefs originating from it are subject to the same need for justification as the instrument in question. Hence, eye-based beliefs cannot be assumed to be immediately justifiable, while instrument based beliefs need extra justification. According to Psillos, there is no difference in quality between the evidence gathered by the unaided senses and that gathered by instruments. Both warranted belief, and beliefs generated by the unaided senses are sometimes less warranted than instrument-based beliefs (1999, 199).

Thus, Psillos argues that van Fraassen's distinction between observables and unobservables is epistemically untenable. However, as shown in Chapter Three, van Fraassen now thinks that instruments create phenomena. Assuming that the senses are instruments then it would seem that they too create phenomena. If this is not the case, then van Fraassen will have to show how it is that biological instruments should be given epistemic privilege over artificial ones. Van Fraassen's views on instrumentation will be discussed in a subsequent subsection.

4.6.5 van Fraassen and Justification

Van Fraassen's empirical stance seems to reject the need for justification for observations because observation is regarded as being immediately justifiable. However, van Fraassen's probabilism eschews justification, not just for observational claims, but all claims. Thus, according to probabilism, one can hold a true belief supported by other beliefs whether these beliefs are true or false or with no support from other beliefs at all. Apparently, if one makes a lucky guess that his true and one believes their lucky guess to be true, then one has knowledge. This view conflicts with the traditional intuition that knowledge should somehow be tethered (Cf. Plato *Theaetetus* 201) and not a matter of luck. Furthermore, one can hold onto a belief if one has no reason to give it up and it is consistent, but whether or not it is true is a matter of luck (van Fraassen 2007, 352).

However, van Fraassen seems to contradict himself since one is supposed to know what is true through commonsense realism. Yet, if Psillos' arguments against van Fraassen's notion of observability are sound, then this notion is untenable. Moreover, commonsense realism will not help us to distinguish what is true from what is false since there are many interpretations given as to what seems evident to the senses. For instance, Bishop Berkeley (2000) assumed that what we directly experience are sensations, and not material objects that exist independently of us. Instead, he argued that the belief in material objects is something that is arrived at through inference, that the belief in material objects is the result of a theory and not direct experience. Moreover, Berkeley argued that his theory was commonsense (2000, 74). Presumably, we are capable of making vastly different starting assumptions about the nature of perception and what needs to be supported with a theory. Our experience does not tell us which assumptions are true.

Furthermore, starting from the presuppositions we already have falls prey to the mode of hypothesis. Van Fraassen claims we assume that our senses are reliable and the world is independent of us because this assumption has worked well for us, but that does not make the assumption true. By his standards, there are rational alternatives, such as indirect realism, to commonsense realism. *Indirect realism* states perception is not directed, but mediated. Historically, the medium of perception in Western philosophy has been sense-data. *Sense-data* are mental objects that represent the objects putatively perceived. For example, according to indirect realism, color is a mental quality. The apple before is not green, the color green does not exist apart from my perception of it. Rather, I just perceive a particular wavelength of light this way. Yet, van Fraassen

simply dismisses sense-data since it is a theory of perception, and he does not believe that theories provide true descriptions of the world or of ourselves. Unless he can show why believing in sense-data theories is irrational, then van Fraassen will have to show how it cannot also count as an acceptable form of commonsense realism. Moreover, Cartwright's defense of van Fraassen's commonsense realism seems to depart from van Fraassen's claim that sensations are of a mind independent world when she notes that however sensations are to be understood is of no consequence for her argument. Rather, what matters is that sensations "whatever they are" force themselves upon us (Cartwright 2007, 43).

Thus, the commonsense realism to which van Fraassen is committed leads to very different views about what is true. Van Fraassen's view of the world is very different from Berkeley's in the past and Cartwright's nowadays since he claims that the world is independent of us, and he explicitly states that Berkeley's sensationalism is wrong. However, van Fraassen's reliance on pragmatic success to show that his view is likely to be true seems untenable against these other positions because they seem to be pragmatically underdetermined. For instance, if one wants to remain alive, then it seems to be pragmatic to step out of the bus' way no matter if it exists independently of our minds, depends on a mind to exist, or if its composition is beyond our ken. Since commonsense realism can support incompatible world views based on pragmatic considerations then pragmatic considerations do not seem to resolve the above mentioned underdetermination.

Moreover, Psillos relies on the pragmatic success of theoretical virtues to argue that these virtues are epistemic. Van Fraassen contends that pragmatic virtues are not

clearly epistemic since many false but successful scientific theories contained certain theoretical virtues (such as the explanatory power of the phlogiston and caloric theories). Hence, van Fraassen appears to contradict himself when he allows for pragmatic considerations to support his claim that commonsense realism is an epistemic position, but does not allow the pragmatic success of theoretical virtues to support the claim that they are epistemic.

4.6.6 The Darwinian Explanation for the Success of Science

While van Fraassen (2007) claims that the “no miracles argument” is question begging because it relies on the inference to the best explanation, he attempts to give his own version of the “no miracles argument” by claiming that successful theories have latched onto actual regularities in nature. He contends that the inference to the best explanation is question begging because it assumes that the truth is contained in the specified range of answers to the question at hand (2007, 140).⁶⁰ However, this response is a promissory note since he cannot show that the interpretation given to regularities will always be the same, as we saw with the retrograde motion of Mars. Furthermore, if he were true to his eschewing of explanation, then he would not attempt to account for why science has predictive success. Moreover, his Darwinian account of the success of science, by relying on the presumption that successful scientific theories have latched on

⁶⁰ Van Fraassen’s objection to the inference to the best explanation shall be discussed in a subsequent subsection.

to actual regularities in nature, seems at odds with his view on laws of nature. As noted in Chapter Three, van Fraassen argues that “law” is a misnomer, and that what philosophers of science call a “law of nature”, is merely a symmetry argument. Thus, van Fraassen should state that a successful theory has found a temporarily satisfactory solution to a problem, or set of problems, not that it has discovered a regularity in nature. But, van Fraassen argues we can only know what comes to us in the immediacy of experience, but regularities dependent on past experience. He defends the reliance on past experience as a well established practice that is not in need of defense until it is shown not to be reliable anymore (following his probabilism). But, as noted in a previous subsection, probabilism relies on the mode of hypothesis.

Additionally, it seems that van Fraassen seemingly cannot account for why past theories that were seemingly empirically adequate are now seen as unsuccessful and remain true to his empiricist stance. To explain why a seemingly successful theory, such as the Ptolemaic model of the universe, was successful, is now seen as false relies on a distinction between reality and appearance. According to van Fraassen, phenomena are what are real and appearances are how things appear to us whether real or not. As mentioned earlier, the retrograde motion of Mars is only apparent, but the reality is that the motion of the Earth causes the appearance of retrograde motion. The motion of the Earth is not something is perceived through our five senses. As Aristotle noted, if the Earth were in motion, then there would be a constant wind moving across its surface. Furthermore, an object thrown straight up into the air does not fall back behind the person throwing as happens when someone throws something up while on a moving surface. Hence, Ptolemaic astronomy seemed to be empirically adequate, and now is not.

Empirical adequacy entails the truth of what is perceived, but what is perceived can be false. According to facticity, Ptolemaic astronomers did not have knowledge, though constructive empiricism would have conferred knowledge upon them had it existed in ancient times. In this case, belief in empirical adequacy is no more risky than belief in truth. Thus, just as it seems we cannot know we have truth, it appears that we cannot know we have empirical adequacy either.

Moreover, van Fraassen's scientific realist opponents may be correct in arguing for the epistemic value of explanation. Their arguments may be sound, though they appear not to be. Van Fraassen recognizing that explanation has pragmatic value if it has any value at all. While he claims that the epistemic value of commonsense realism can be inferred by this views pragmatic success, he denies the epistemic value of explanatory power. He does not make clear why pragmatic success suggests epistemic success for commonsense realism, but not for explanatory power. Furthermore, his argument for the believing in the epistemic value of commonsense realism is the same as the scientific realists' argument in favor of the inference to the best explanation. Thus, van Fraassen seems to hold incompatible views on the relation between pragmatic success and epistemic success.

Van Fraassen's inconsistency on the issue of the relationship between pragmatic value and epistemic value obfuscates his position on explanation. It would appear that his rejection of the need for explanation in scientific theories relies on the mode of hypothesis. While he seems to employ the pessimistic meta-induction and underdetermination arguments against the view that explanatory power is the most epistemically robust of all the theoretical virtues, he claims he does not. Hence, it seems

that his eschewing of explanation stems completely from his empirical stance. If this is the case, then his commitment to the rejection of explanation as epistemic relies on the mode of hypothesis.

4.6.7 van Fraassen and Theory Change

Van Fraassen relies on a criterion of success to argue for his version of scientific theory change. He claims that his version does not fall victim to the incoherence of scientific realist views on scientific theory change. However, van Fraassen argues that the criterion of success cannot be used to support the scientific realist's claim that theoretical virtues are epistemic and not merely pragmatic.

Van Fraassen argues for his criterion of success by asserting that epistemology is a volitional activity where we set the parameters for what counts as success. Yet, he allows that one can rationally hold onto scientific frameworks that are seen by some as failures due to an excess of anomalies. Van Fraassen appears to have succumbed to an inconsistency himself. On the one hand, he argues that our beliefs are rational if they are consistent and are not refuted by the world. But, he also says that it is a matter of volition as to what counts as too many anomalies in the context of scientific frameworks. Thus, one can be rational and hold onto a theory that may be considered defeated by a large number of experts in the field.

For instance, Lamarck's notion of the inheritance of acquired characteristics was seen as having been refuted by Mendelian genetics. However, in the 1920s and 1930s Mendelian genetics was confronted by various anomalies, such as the inability of geneticists to create a new species of fruit fly in the laboratory through the mutation of specific genes. Rather, lapsed Mendelian geneticists such as Carl Correns, turned to the notion of the inheritance of acquired characteristics in the cytoplasm egg cells to account for the evolutionary processes leading to speciation (Sapp 2003, 112-113). Moreover, Ptolemaic astronomy seemed to be rationally held because it seemed to fit the world and make accurate predictions.

If theories such as Lamarck's seem so obviously wrong but reemerge in the eyes of many scientists as worthy of consideration, then it seems we do not know when a theory does not fit world and thus irrationally held by its adherents. Moreover, if cases such as Ptolemaic astronomy exist where a theory seems obviously to fit the world but apparently does not, then we again seem not to know when a theory fits the world and is rationally held. Since Lamarck's theory seems to have been vindicated by further evidence after his death, it is not clear if it was always rational to be committed to a Lamarckian theory or not. Therefore, van Fraassen seems to contradict himself when he asserts that there is an objective from which we can measure the truth and falsity of statements, and that holding onto beliefs that do not fit the world is irrational, but that one can rationally hold onto seemingly refuted theories.

If the world is our standard by which to judge scientific theories true or false, then van Fraassen should tell us how many anomalies make a theory false. He seems to think that this is a matter of disposition, but if he allows for such relativity, then it is not clear

how the world is an objective measure of truth. If these objections are sound, then it is not clear why van Fraassen seems to think that we can speak of scientific theories in epistemic terms. Rather, it seems that what van Fraassen is arguing for in his account of scientific theory change is commitment to problem solutions instead of truth. Furthermore, he appears to be arguing for this position on the basis of our ignorance as to world is. Hence, his account of scientific theory change seems to be incoherent.

4.6.8 Psillos' Defense of The Inference to the Best Explanation

Psillos (2009) contends that van Fraassen's description of how the inference to the best explanation is applied in probabilistic reasoning commits the straw person fallacy. Van Fraassen claims that, in the Bayesian framework, bonuses should be assigned to posterior probabilities of hypotheses that are seen as the best explanations of the phenomena in question (in Psillos 2009, 196). Psillos agrees with van Fraassen that following this method of fixing posterior probabilities will lead to probabilistic incoherence. However, van Fraassen's description of how probabilities are fixed using the inference to the best explanation is not the only way. Psillos argues that the theoretical virtues should be taken into consideration when assigning prior probabilities. Theoretical virtues are taken into account relative to the "background knowledge" supporting the theory, or in judging the likelihood of the theory. In this way, the prior probabilities are not given equal probabilistic weight as van Fraassen claimed. For him,

theories that are underdetermined by the observable evidence have the same likelihood of being true. But, Psillos contends, the inference to the best explanation is used to break observational ties, and thus two or more competing hypotheses will not have the same prior probabilities. Therefore, the probabilistic incoherence van Fraassen described will not arise.

Van Fraassen could reply that Psillos has not shown that theoretical virtues are not merely pragmatic, but epistemic as well. Psillos' argument above seems to be aimed at van Fraassen's specific argument against the inference to the best explanation based on Bayesian probability. Furthermore, van Fraassen's seemingly likely reply would appear to lead to a standstill between the two positions since Psillos ultimate retort to van Fraassen's objection to the inference to the best explanation is that he is an epistemic optimist while van Fraassen is an epistemic pessimist. However, in the subsection below I shall discuss Psillos' objections to van Fraassen's arguments against the inference to the best explanation.

4.6.9 Truth, Empirical Adequacy and Metaphysical Baggage

Psillos claims that van Fraassen's rejection of the inference to the best explanation for observables leads him into skepticism. As mentioned in a subsection above, van Fraassen maintains he escapes skepticism because skepticism for him is, by definition, self-refuting. When he thinks of skepticism he assumes it to be of the

Cartesian variety where one cannot be sure of what one is immediately perceiving (since one could be dreaming, insane, deceived by an all-powerful demon, and so forth). Thus, by believing in the truth of what we can see immediately in experience van Fraassen claims he is not a skeptic. It is important to note that van Fraassen appears not to believe in the truth of past experience, and hence he is not committed to any form of induction.⁶¹ However, Psillos argues that posits (observable or unobservable) are used to causally unite our immediate experiences. He notes that van Fraassen's position does not cover unobserved entities that could be observed.

For example, the inference to the best explanation is used when we suppose that a book that we own still exists when we leave the room. Furthermore, the positing of extinct animals through fossil remains is another instance of the inference to the best explanation. Psillos contends that, while Cartesian skepticism is avoided, Humean skepticism looms around the corner. Whether or not van Fraassen would accept Humean skepticism is unclear. However, if van Fraassen continues to claim that his philosophy of science accurately depicts scientific practice, then Psillos seems to have shown this is not the case since biology seems to rely heavily on the fossil record as crucial evidence for the theory of evolution. Moreover, it appears that the argument above could be extended to van Fraassen's take on what he calls "metaphysical baggage" in scientific theories. Otherwise, without posits, it would seem that the regularities we see in nature miraculously arise anew in the immediacy of every experience we have.

⁶¹ This is why he does not believe in the pessimistic meta-induction.

4.6.10 Empiricist Structuralism and Instrumentation

Since his (2008), van Fraassen considers what other philosophers of science call “entities” and “processes” to be the products of instruments. As shown in Chapter Three, he contends that “entities”, such as paramecium, are not denizens of an unobservable world. Rather, they are phenomena created by a microscope. Given his commonsense realism, it is not clear what metaphysical status the “entities” created by instruments occupy. He seems to think that what is observable with the unaided senses is real, but that the phenomena in microscopes, for instance, are not ontologically on a par with the entities and processes that we sense without the aid of instruments. If this is the case, he again has to show why our unaided senses are to be epistemically privileged above what we sense through instruments.

Furthermore, if instruments (such as the microscope) operate under the same physical principles as the human eye, then why is it that the former create phenomena and the latter do not? For instance, he claims there is a distinction between what appears to our senses (which may be false) and phenomena (scientific measurements which are taken to be true). However, as noted earlier in this chapter, the appearance of the retrograde motion of Mars is false, while the phenomenon of Mars’ elliptical motion is true. This distinction seems to be at odds with the empiricist assumptions that reality is as we perceive it. Is there something special about organic material versus glass and metal that allows the former access veritable access to the world while the latter do not have veritable access to an unseen world? Van Fraassen’s answer seems to be that we

rely on our naked senses out of pragmatic considerations, but he has not shown why we cannot have the same trust in our instruments for the same reason. For example, the apparent discovery of germs has revolutionized medicine, now cleanliness in hospitals (especially in operating rooms) is seen as a necessity for maintaining health and keeping people alive. If the phenomena observed under microscopes have such significant survival value, then why are they not trusted for the same pragmatic reason as our senses? Moreover, if instruments, such as microscopes, do not help us to see small things accurately (say, the eyes of fly), then do eye glasses and magnifying glasses create phenomena too? Van Fraassen does not seem to have answers to these questions.

4.6.11 Voluntarist Reference?

Considering the scientific realism debate in science seems to hinge on which theory of reference one adopts, van Fraassen would do well to make explicit which theory of reference he is using. Furthermore, he is unclear as to what it is that theoretical terms refer to, if they refer at all. Perhaps that they refer to phenomena created in the laboratory, but he does not specify what the features are that identify a referent. As we have already seen, for Psillos it is a combination of specific descriptive properties and a dubbing event that fixes the referent of the theoretical term. In his (1980), he seems to be committed to taking scientific theories at face value. However, in his (2008), van Fraassen argues that science uses structures to represent certain phenomena, and that

what determines which structure is used to represent which phenomena depends on the person employing the representation. If van Fraassen holds that the terms used by the scientist are also representations, then what features of the term are essential depends on the user of that term. If this is how reference works for van Fraassen, then a voluntarist theory of reference is in line with his view. If he does not see reference as working in this way, then a voluntarist theory of reference can still be used in accordance with a voluntarist epistemology such as the one he employs.

However, if van Fraassen uses a voluntarist theory of reference, then the objections employed against Psillos work against van Fraassen *mutatis mutandis*. While Psillos was apparently attempting to create a theory of reference that would compel scientific realists to adopt it as the theory of reference for scientific realism, van Fraassen seems to allow for a given context to determine reference and meaning. Yet, there appears to be an ambiguity in van Fraassen's indexical characterization of reference between what is normative and what is descriptive. As noted earlier, there is a tension between whether or not truth is only to be found within a context or if it can be found outside of a context, as when a scientific theory latches on to a regularity or fails to do so. This tension bleeds over into reference and meaning since meaning and reference are defined within a context, but whether or not someone correctly refers to something or not seems to be independent of any given context. For instance, van Fraassen notes that if one were to say that phlogiston is escaping to describe combustion, then this person would be mistaken since phlogiston theory does not capture any regularity in nature (2006: 147).

Furthermore, it is not clear when we should dismiss a theory or abandon a term since, according to voluntarism, our interests, dispositions, and emotions play an inescapable role in our decision to regard something as true or false. For instance, as shown in a previous subsection, van Fraassen's account of theory change relies on voluntarist epistemology to argue that holding onto an anomaly ridden scientific theory is rational. For example, epigenetics has shown that once discredited theories and their terms, such as "the inheritance of acquired characteristics" can be resurrected. If this is the case, then it is not clear when a theory has failed to latch onto actual regularities in nature or not. If this it is not clear when a theory has failed to latch onto actual regularities, then there are fewer clear cases for van Fraassen to point to as instances of irrationality and failure of reference.

4.6.12 Facticity and van Fraassen

While van Fraassen eschews the correspondence theory of truth and does not hold to the metaphysical view that there is one true story of the world, he seems to hold onto a view of truth that requires some kind of non-contextual common denominator of truth with which to judge the acceptability of a particular context. Thus, he seems to need to have a notion of truth that has some kind of correspondence with the world. If he does not have a way to judge the truth and falsity of contexts, then his view will fall victim to irenic relativism. However, if he denies that truth has any connection to the world but

only to a particular context, then his view will be idealist. Because of his commitment to commonsense realism it seems idealism is a view he wants to avoid. Therefore, a notion of truth that corresponds with the world in some way is necessary to avoid these two positions. But, as shown above, his permissive position on rationality allows for contradictory notions of what is true and what is false since these are decided contextually. Hence, van Fraassen's attempts to constrain irenic relativism fail since the cases he cites to show are unacceptable appear to be acceptable by his own standards. Furthermore, theories that have been refuted, presumably for failing to attach themselves to regularities in nature, seemingly latch onto regularities after all. Hence, van Fraassen's permissive rationality leads to an untenable view of truth that undermines his empiricist epistemology and philosophy of science. Since the view of truth used in his account of knowledge seems to fail it appears that van Fraassen's account of knowledge does not meet the facticity criterion.

4.7 Conclusion

In the preceding sections and subsections, I have sought to:

(1) Instill a suspension judgment on the accounts of scientific knowledge examined in the previous chapters of this dissertation.

(2) Show that none of the accounts of scientific knowledge examined seems to meet the facticity criterion of acquiring truth about the world.

(3) Begin each section with a discussion of the assumptions made by the philosopher in question to show that they fall prey to the mode of hypothesis. For Psillos the mode of hypothesis appeared in his three stances of scientific realism, for van Fraassen in his epistemic voluntarism and commitment to the empirical stance, and for Bird with his commitment to the primitivist account of knowledge.

(4) If the mode of hypothesis failed applied to the above mentioned philosopher's respective views did not lead to the suspension of judgment, I then attempted to instill this state through particular arguments that seemed to undermine particular aspects of these views.

(5) In particular, I focused on those views that entailed the facticity criterion, such as the inference to the best explanation.

(6) I sometimes used other philosopher's objections to the views in questions or developed my own. As noted in Chapter One, the modes are used by all sides in a debate to undermine the positions of their opponents. My goal was not show that the views in question failed, only that they *seem* to have failed. The arguments appear to have failed because they seem to fall prey to one or more of the modes.

(7) I attempted to show the reader that some of the objections to each account seem to be equipollent in terms of their persuasive power.

(8) Thus, one of these accounts may be true, but they do not seem to be because they do not appear to overcome the Pyrrhonist modes or to be able to break the apparent tie with one or both of the other accounts examined.

(9) Therefore, if all of the alternatives examined seem to be false, then the search for the true account of scientific knowledge continues. However, if the accounts I have chosen are representative of the majority of accounts existing, then it appears that there is as yet no true account of scientific knowledge.⁶²

⁶² In the next chapter, I shall conclude this dissertation by endeavoring to instill doubt about the possibility of adding a positive cognitive feature to the traditional modes of Pyrrhonism. Specifically, I will examine the respective neo-Pyrrhonist positions of Robert Fogelin and Otávio Bueno.

Chapter Five.

Neo-Pyrrhonism and Scientific Knowledge

In all of the accounts discussed throughout this dissertation, the philosophers presenting their particular version of scientific knowledge seem to state that they are able to access the truth, whether of the natural world (Bird, van Fraassen) or of abstracta (Psillos). Each philosopher seems to argue that the success of a scientific theory is evidence for the truth of some part of that theory. Thus, they all appear to agree that whether or not some part of a scientific theory is true is to be found in the relation between theory and practice. While the previous chapters have endeavored to show that the three accounts of scientific knowledge examined fail to defeat the Pyrrhonist modes, I shall attempt to show that the Pyrrhonists themselves may have held an inconsistent view concerning the relation of theory to practice.

As noted in Chapter One, the Methodist school of ancient medicine would consider various doctrines about the nature of disease to improve their practice without belief in the truth of those theories. There is evidence in the writings of Sextus Empiricus⁶³ that the Pyrrhonists followed the Methodists in the practice of medicine, and

⁶³ No evidence appears anywhere else. Moreover, the title “Empiricus” seems to indicate that Sextus was a follower of the *Empiric* school of medicine. Furthermore, Sextus notes that it is commonly assumed that the Empirics is allied to Pyrrhonism (*PH* 1 236-41). However, he rejects the association between the Empirics and Pyrrhonism. These apparent discrepancies in Sextus’ writings will be discussed in section 5.1.1 of this chapter.

thus also considered the practical value of theories while not taking the theories under examination as being true of the world. In this chapter, I shall attempt to assess whether or not the Pyrrhonists were consistent in considering theories as guides to better practice. I will employ the Pyrrhonist modes to test whether or not a connection between theory and practice can be assumed by the Pyrrhonist. If this connection cannot be maintained, then the Pyrrhonists were inconsistent in their practice of considering theories for their practical value. I shall then consider what this inconsistency means for the neo-Pyrrhonist views of Robert Fogelin and Otávio Bueno, respectively.

5.1 Pyrrhonism and Practice

5.1.1 The Relation of Theory to Practice

In Chapter One, I summarized Frede's (1983) description of how the Methodist school of medicine justified its use of theory in the practice of medicine despite its skeptical stance toward the truth of all theories. Sextus Empiricus seems to have endorsed the Methodical school of medicine in (*PH I* 238-241). Moreover, Frede assumes that the Methodists and Pyrrhonists took a contextualist position on knowledge claims. The Methodists claimed to know much about characteristics that reappear, and whose manifestation and disappearance could be determined through inspection (1983,

3). These characteristics they called *generalities*. However, the Methodists used the term in a non-metaphysical way, denying that they knew whether generalities exist or do not exist. Rather, they used generalities as indications for treatment (1983, 4-5). Thus, while Methodists *used* the term in a non-metaphysical way, it they considered “generalities” to be a metaphysical term since it was not apparent, but had to be uncovered. These generalities did not only apply to medicine but to any art at all. An example of a generality in the art of medicine is pneumonia. The Methodists did not claim that a natural kind called “pneumonia” exists, just that this name picks out a certain set of symptoms (such as coughing, fever, difficulty breathing).

The Methodical school was a reaction to both the Rationalist and Empiric schools of medicine. The *Rationalist* school strove to understand diseases and their treatment through inferring from the symptoms of a disease to the hidden state that causes the malady. They held that reasoning showed that the human body was composed of atoms and pores. The Empiric school⁶⁴ relied solely on experience to guide their treatments. They assumed that the symptoms make clear how the disease is to be treated (1983, 2). For instance, if suffering is caused by the constriction of some body part, then dilation is called for as a treatment. The treatment is immediately apparent, thus there is no inference to be made. In the following two paragraphs, I shall endeavor to show how the Methodists differed from the Rationalists and the Empirics, respectively.

According to Frede, the knowledge the Methodists claimed to have is *ordinary knowledge*, or knowledge of what appears to be the case, and not *theoretical knowledge* that makes claims about reality. The Methodists, like the Pyrrhonists, do not assent to anything except for appearances. This commitment to appearances extends to a rejection

⁶⁴ Cf. footnote 1 of this chapter.

of logic since having to use inferences to arrive at a conclusion means that the conclusion arrived was not immediately evident. The Methodists rejected belief in the truth of theories since theories are inferences to and from non-evident states to the causes of diseases. Thus, against the Rationalists, the Methodists did think that reason should be used in the construction of scientific theories. Rather, an evident feeling makes obvious what course of action is needed (1983, 7). They agreed with the Pyrrhonists that we do not need theories to tell us to drink water when thirsty or to eat when we are hungry. An instance of evident feeling in medicine is the extraction of foreign objects from a person's body, as when a dog removes a thorn that was stuck in its paw (*PH I* 238).

As with the Pyrrhonists, the Methodists would study all theories of medicine, though they did not believe in any of them, because they did not want to look like they were rejecting belief in them out of ignorance. If they did so, then they would appear to be what Sextus called *negative dogmatists*, or those who claimed to know we could not know something. Rejecting theories without carefully considering them would be negatively dogmatic since one of them may turn out to be manifestly true. Therefore, the Methodists disagreed with the Empirics' sole reliance on experience. However, Frede contends that there is more to the Methodist consideration of theory since they seem to have been heavily influenced by the physiology of Asclepiades.

According to Asclepiades, the body was composed of atoms and invisible pores. All diseases arose from three generalities: the constriction of these pores, the dilation of these pores, or both. Clearly, the constriction and dilation of hidden pores, and the existence of these pores and of atoms, were not manifest. Moreover, Asclepiades claimed to have arrived at knowledge of the existence of these hidden entities and

processes through reasoning. However, the Methodists did not believe in the existence or non-existence of these hidden entities and processes, nor did they accept the reasoning from observed constriction and dilation to corresponding hidden states of constriction and dilation. Yet, there is some evidence that the Methodists did accept Asclepiades' theory on some level (1983, 14-15).

If Frede's argument is sound, then at least some Methodists thought that Asclepiades' account seemed more plausible than other accounts of disease since the latter's theory, while speculative, seemed to correspond to careful attention to the appearances. Thus, according to the Methodists, there are certain manifest generalities that we may never have become aware of if it were not for Asclepiades' theory. Frede gives the following elucidation of their view: "After all, it is extremely unlikely that by merely looking at the phenomena without the guidance of some theory we would ever have realised that all diseases are forms of three generalities" (1983, 16). However, the Methodists seemed to hold that it is observation which furnishes us with the knowledge of these generalities which were brought to our attention by speculation. Furthermore, it is only what is clearly manifest that can be reliably known and hence guarantee the safety of a given treatment.

Yet, as noted by the physician and philosopher, Galen, there were disagreements among the Methodists as to what features constituted generalities. He further observed these disagreements should not have arisen since generalities are supposed to be manifest, and what is manifest should be clear for all to see (1983, 16). The Methodists would have likely replied to Galen's objection that he is assuming complex phenomena can be broken down into simpler, basic phenomenal features, and this assumption is not

obviously true. Therefore, generalities still are manifest, but what features characterize them is not. The disagreement as to what is manifest goes to show that something can be obvious without being immediately obvious to everyone. Training is needed to be able to recognize certain manifest phenomena. If medical phenomena were obvious to everyone, then there would be no need for doctors (1983, 17-18).

While Sextus Empiricus endorses them, the Methodists do not seem to have been skeptics. As shown above, the Methodists claimed certain knowledge must be obtained in order to practice medicine safely, and that they had acquired this knowledge. Thus, as Frede notes, “it comes as a considerable surprise when we are told by Sextus Empiricus (*PH I* 236ff) that of all the medical sects the Methodical school is the one which is most attractive for a sceptic” (1983, 20). Sextus does not claim that the Methodists are skeptics, but that their view is the most in line with Pyrrhonism than Empiricism. He notes certain common features between them such as the undogmatic use of terms, their agnostic stance toward unobservable entities, and their reliance on manifest affections for guidance. Hence, Sextus does not link Pyrrhonism with any particular group of Methodists, but only with the Methodical school in general (1983, 21).

Frede argues that the general view of the Methodical school is compatible with Pyrrhonist skepticism since the former speak of certain knowledge in an undogmatic manner. He assumes that the Pyrrhonists and the Methodists were contextualists of a sort where the word “knowledge” is understood in an ordinary sense, including “certain knowledge”. By speaking within an ordinary context they do not make any claims as to the existence of certain knowledge. The Methodists claimed the Rationalists had failed to

acquire theoretical knowledge that was certain and had failed to obtain ordinary, manifest knowledge. Since the Methodists only sought to acquire certain knowledge in an ordinary sense they were able to acquire at least that much. According to Frede, the Methodist view of knowledge is compatible with the Pyrrhonist suspension of judgment on all theoretical matters and their assent to appearances.

One may object that an ancient skeptic could not endorse the Methodical school since the latter allowed for theoretical speculation or for one to approve of one theory over others. The Methodists did both. Frede replies that the Academic skeptics allowed for these practices, therefore, Methodism was compatible with them. Moreover, while he does not show how, Frede claims that Sextus was consistent in following the Methodical school. Perhaps the contextualist construal of knowledge is what allows for this consistency.

Furthermore, Sextus' title "Empiricus" seems to indicate that he practiced following the Empirical school.⁶⁵ Sextus notes that the Empirical school and Pyrrhonism are commonly associated with each other (*PH I 236*).⁶⁶ However, according to Sextus, the Empirics appeared to hold that knowledge of non-evident matters is impossible rather than suspending judgment on these matters as a Pyrrhonist ought to (*PH I 236*). If the Empirics did hold that such knowledge was impossible to acquire, then they were assuming to know that we could not know these matters. Thus, they seemed to be negative dogmatists. Yet, he does not say whether or not a Pyrrhonist can align themselves with the Empirics if the latter did suspend judgment on non-evident matters.

⁶⁵ At least in *PH I 236-41* Sextus does not explain how the title "Empiricus" was bestowed upon him.

⁶⁶ Cf. footnote 1 of this chapter.

Therefore, it is unclear whether or not Methodism was the only school of medicine compatible with Pyrrhonism.

5.1.2 A Pyrrhonist Examination of Methodism

In this section, I endeavor to contend that Pyrrhonism and Methodism appear to be incompatible. As noted in Chapter One, it is not clear that the Pyrrhonists had the kind of contextualist view of knowledge that Frede and Fogelin claim they had. Furthermore, as I shall attempt to show in this subsection, the skeptical modes seem to be applicable to the Methodical school. If the modes do undermine the Methodists' views, then their theoretical speculation was inconsistent with their claim that they only assent to the appearances. In particular, their latter claim seems to fail because the Methodists seemingly do not have a way to deal with conflicting appearances.

The Methodists fall prey to the mode of hypothesis when they claim that training is necessary to recognize what is evident. If training is necessary to recognize what is evident, then there does not seem to be a way to judge what is evident and what is not evident. What is evident should be evident to everyone or else it is not evident by definition. Hence, a contradiction arises if they claim that what is evident is supposed to be evident to all. Moreover, they reject inferences because inferences are not immediately evident, but allow for theoretical training to lead to what is not immediately evident.

The mode of relativity seems to work against the Methodists' claim that theoretical speculation leads to new phenomena because it is not evident why we should follow one form of training over another. For instance, training in Eastern medicine, such as acupuncture, leads to very different treatments than training in contemporary Western medicine. Furthermore, in contemporary Western medicine there is disagreement as to what is evident and what treatment should be given for a particular ailment. For example, there is controversy over whether the benefits of electroconvulsive therapy on depressed patients outweigh the risks of the long-term effects on general cognition, such as memory loss. By claiming that training is necessary to recognize what is evident, the Methodists seem to be in the same position as their dogmatic opponents of showing that what is made evident to them should be evident to all. However, they do not and thus fall prey to the mode of hypothesis as well.

It appears the Methodists would likely respond that what differentiates them from the other schools of medicine is that they do not make any claim to knowledge of what is real. However, this response will not do. The Methodist claim that training is required to recognize what is evident is analogous to the Rationalists' use of reason to purportedly uncover reality. As shown in the preceding paragraph, the same modes of hypothesis and relativity can be used against the claim that something evident has now been recognized through training as against claims that reason has revealed reality. Yet, many claim to use reason to uncover reality and they disagree on the nature of reality. The Methodists will claim that what their training has shown them is truly evident, but it appears to be evident only to them and thus there is disagreement as to what is evident and what is not.

Therefore, using the same modes they use against claims about reality, Sextus should have suspended judgment on the Methodists' claims of what is evident through training.

The Pyrrhonists may have two responses to the above objection. The first is that they could still follow the general Methodist stance against theory, even if it leads to training in different theories and different treatments, because how a particular person responds to any given situation seems to depend on their dispositions and upbringing. As Sextus notes in *PHI* 23, the Pyrrhonist responds to the appearances according to their nature, affects, culture, and the training they received in their chosen trade. Thus, different Pyrrhonists will be moved by different appearances to act in different ways according to their different affects, training, cultures, etc. Therefore, when appearances conflict, they will seemingly be disposed to act on one appearance over another based on their training, acculturation, affects and so on.

Their second response appears to be that the goal of Pyrrhonism is to attain ataraxia. Their primary goal is not quality medical care for all, but their own peace of mind. Thus, while there are problems with conflicting appearances within the practice of medicine, these conflicting appearances need not concern the Pyrrhonist doctor. Rather, the Pyrrhonist doctor will simply follow their training, affections, and so forth. If they are encountering a new phenomena that they have not been trained to handle (such as a new illness), then they will likely wait for others to try different treatments on this new phenomena. Once different treatments have been tried, they will then use the one that seems to be the most effective. How long a particular Pyrrhonist will wait before choosing a treatment will depend on the dispositions of that Pyrrhonist.

5.2 Fogelin and Bueno

While the appeal to ataraxia in the preceding subsection may work for the traditional Pyrrhonist, Fogelin and Bueno's versions of neo-Pyrrhonism abandon this notion, and thus they cannot appeal to it. The adoption of the epistemological side of Pyrrhonism, while abandoning the practical side of attaining ataraxia, leads to difficulties that may undermine the purely epistemological side of Pyrrhonism which can be remedied by appealing to the attainment of ataraxia. As argued above in the examination of the Methodical school of medicine, the attempt to show Pyrrhonism is compatible with any kind of practice may be undermined by the Pyrrhonist modes. In the subsections below, I shall endeavor to argue that the objections to the Pyrrhonist and Methodist attempts to explain how they can adopt certain practices over others can be applied *mutatis mutandis* to the neo-Pyrrhonist views of Fogelin and Bueno. In particular, I will attempt to contend that their aim of wedding an account of cognitive success to the Pyrrhonist modes seems untenable.

5.2.1 Fogelin

As noted in Chapter One, Fogelin seems to be mistaken about how Pyrrhonists regard knowledge claims because Pyrrhonists do not seem to deal with levels of scrutiny.

However, even if I am wrong on this point and Fogelin is right, then Pyrrhonists may have an incoherent position on knowledge. Fogelin seems committed to a view close to epistemological contextualism with his notion of levels of scrutiny. The difference between his view and contextualism lies in his claim that low levels of scrutiny allow one to be justified in their being said to have knowledge, not in making a claim to actual knowledge in a certain context. While this is a significant difference, it seems that the same objections that apply to contextualism also apply to Fogelin's levels of scrutiny. I intend to deal with Fogelin's neo-Pyrrhonism in detail in future research, but here I will endeavor to give a sketch of some of the difficulties his position may face.

For instance, although Fogelin does not address scientific knowledge in particular, his levels of scrutiny seem to apply to all types of propositional knowledge. Thus, I shall examine whether or not his notion can be applied to scientific knowledge. If there is scientific knowledge, then it appears not to be a problem for levels of scrutiny since the levels of scrutiny seem to be well established by the centuries of scientific practice. For there to be stable levels of scrutiny across the history of scientific revolutions of the kind Kuhn (1970) argues for cannot have occurred. Perhaps Fogelin could rely Bird's response to Kuhn's arguments described in Chapter Two. If Bird's argument against Kuhn's methodological incommensurability is sound, then established levels of scrutiny remain stable through theory change. As noted in Chapter Two, Bird contends that exemplars (in the Kuhnian sense), such as the equations Newton used to predict celestial orbits, can be applied to different domains, such as the measure of water, to create new sciences, such as hydrodynamics. But, it is not clear that exemplars can be so easily applied to new domains.

However, levels of scrutiny may not be stable even in science that is generally seen as non-revolutionary. For instance, in chapter six of his *Image and Logic*, Galison describes how two instrument traditions in physics and the epistemological assumptions behind them. The *image tradition* relied on instruments such as cloud chambers and bubble chambers, while the *logic tradition* relied on devices such as the Geiger-Müller counter, spark counter, and the sonic chamber. For the image tradition, human intervention was necessary for recognizing patterns, whereas for the logic tradition electronic counters statistical arguments lead to discovery. The fusion of the two traditions occurred with the development of electronic imagery in such instruments as the Time Projection Machine.

Fogelin's epistemology does not make clear how or why one instrument tradition would compromise its notion of what counts as evidence to make such a machine. It is not obvious which tradition has the higher level of scrutiny. Thus, it is not clear that there can be established levels of scrutiny in science in one field of science (physics), much less in science generally. If Fogelin's account of ordinary knowledge relies on established levels of scrutiny, then established levels of scrutiny are required for scientific knowledge too. However, if levels of scrutiny cannot be established even among subdivisions within a scientific field, then it seems Fogelin cannot give an account of knowledge within subdivisions of a field of science. *A fortiori*, he cannot give a general account of scientific knowledge.

Furthermore, if Kuhn's (1970) arguments are sound, then during crises and revolutionary periods the levels of scrutiny become higher. When levels of scrutiny become higher the justification one has in thinking that one knows something in this

context disappears. When levels of scrutiny reach this high level it is not clear how to establish a paradigm that can bring the level of scrutiny back down to normal working levels.

For example, Natural theology reached a crisis when it could not account for the recent findings in geology, such as the fossils of extinct species. Darwin put forward his theory of evolution to resolve the crisis and make biology consistent with the findings of geology. Yet, while the consensus of the scientific community eventually accepted evolution as the new paradigm, there were unresolved inconsistencies. For instance, the mechanism of inheritance was left unexplained, and there were missing links in the evolutionary chain. These gaps in the theory could very well be reasons to suspend judgment on it. Fogelin may respond that a neo-Pyrrhonist can wait out the revolution until consensus is reached on a new paradigm, but this is not in keeping with scientific practice. It is not clear that Pyrrhonists could take up the practice of science, since it requires the creation of new contexts, or paradigms, which fix the levels of scrutiny back to normal working levels. Yet, even at the lower levels of scrutiny, where puzzle-solving takes place, creativity is necessary, and it is not clear how Pyrrhonists can be creative.

For instance, Ignaz Semmelweis needed to be creative when he sought a cure for puerperal fever. In doing so he developed antiseptic techniques for use in hospitals. While it is logically possible for a Pyrrhonist to have carried out the same experiments Semmelweis did to reduce mortality by puerperal fever, the Pyrrhonists seem to be passively affected by appearances. The Pyrrhonist does not act, but seems only to react to appearances. Thus it is difficult to see where the impetus for experimentation would come from. If Pyrrhonists also only use the techniques taught to them in their chosen

trades, then there would be no Pyrrhonist scientists. Rather, they can only be engineers and technicians since these trades have well established theories and methods. Hence, no creativity is necessary to carry out their tasks (cf. Kuhn 1970, 30). Therefore, it is not clear that Fogelin's notion of levels of scrutiny can handle areas of putative knowledge, such as science, where the levels rise and fall.

If Fogelin's neo-Pyrrhonism leads to the same kind of passivity of the historical Pyrrhonists, then there are certain trades that the neo-Pyrrhonist cannot adopt. If this is the case, then Fogelin should be explicit in saying so. But, if he takes this approach, he will also have to accept that there may be no way of accounting for the apparent empirical success of science. If he accepts such a position, then he should explicitly claim it. However, if Fogelin thinks that the neo-Pyrrhonist dismissal of ataraxia allows the neo-Pyrrhonist to be active, then they can adopt trades with fluctuating levels of scrutiny. If they can adopt such trades, then Fogelin has to show how one can actively find a way out of a higher level of scrutiny where there is no precedent for doing so, as in a scientific revolution. If my objections are sound, then it seems that Fogelin's levels of scrutiny face the same problems Pyrrhonist and Methodist doctors face when confronted by new phenomena.

5.2.2 Bueno

As shown in Chapter One, Bueno argues that neo-Pyrrhonism is consistent with scientific practice. The same arguments lodged against Fogelin's neo-Pyrrhonist scientist seem to apply to Bueno's view as well. There appears to be no motivation for the neo-Pyrrhonist to engage in the creative activity that appears to be integral to science. Yet, Bueno claims it is possible to add a positive feature to the epistemological side of traditional Pyrrhonism. Bueno seems to think that knowledge is possible for Pyrrhonism and neo-Pyrrhonism "in a non-dogmatic way" (forthcoming, 13). Such a claim seems to be close to Fogelin's account of knowledge where one is *said* to have knowledge, but it is not known whether one has knowledge or not. Bueno appears to go beyond Fogelin's account of knowledge by apparently allowing for know-how (or at least apparent know-how). He states that "Knowledge is something that emerges from a certain practice of investigation (the skeptical practice), in a non-dogmatic way" (Bueno forthcoming, 13). Moreover, since he seems not to be committed to the traditional Pyrrhonist goal of ataraxia he seems to hold that the neo-Pyrrhonist does not passively react to appearances: "Clearly the skeptic is concerned with the practical outcomes of such knowledge, by being able to control relevant phenomena, and explore technological applications that can be developed from them" (Bueno forthcoming, 13). Yet, know-how is not the only cognitive goal that seems to be acquired through scientific practice. As noted in Chapter One, this additional epistemic feature is van Fraassen's notion of understanding.

However, this notion of understanding allows for too much. If understanding is merely putting forth different interpretations of how the world *could* be, then there are many ways (some that seem scientific and some that seem not to be) that the world could be. Thus, it is unclear why a neo-Pyrrhonist would look to scientific theories and not to science fiction, or Scientology, or astrology, and so on for understanding. Bueno may respond that these interpretations appear not to be empirically successful. If he gave such a response, then he will have to show why we should adopt the scientific theory of success over, say, the astrological one. The adherents of astrology seem to think that astrology has reliable predictive power. If Bueno chooses to argue as the Methodists to justify their commitment to some theories over others, then it seems Bueno will fall prey to the modes of hypothesis and relativity as well.

Putting aside what counts as empirical success (or perhaps the appearance of it), the challenge from the pessimistic meta-induction seems to have show that scientific theories can be empirically successful and yet false in their descriptions of the world. Thus, there does not seem to be anything special about the empirical success of science that makes it more successful at giving true, or approximately true, interpretations of the world. Therefore, the world may be understood in any way that does not share the features of theories that are considered scientific, such as empirical testability, simplicity, and coherence with other findings in science.

Moreover, if increasing understanding simply means accumulating interpretations of how the world could be, then understanding would be increased by studying religious texts, New Age philosophies, alchemical manuals, graphic novels, and fairytales. Some of these examples may be consistent with certain appearances, or create other

appearances, but appearances may be very different from reality. If Bueno wants to exclude these examples as examples of understanding⁶⁷ and claim that science has some special purchase on how the world could be, then he has to show why this appears to be the case. Hence, he seems to encounter the same modes that the Pyrrhonists, Methodists, and Fogelin face when attempting to show why we should take some theories more seriously than others.

5.3 Conclusion

Of the accounts of scientific knowledge that have been examined, none of them seem to have been able to establish an account that met the facticity criterion. These accounts appear to be representative of many of the implicit accounts of scientific knowledge given in contemporary literature in the philosophy of science. However, there are other accounts of scientific knowledge, such as that put forth by the Barry Barnes and David Bloor that claim truth and scientific knowledge are socially constructed. The Pyrrhonist examination of this account of scientific knowledge shall be pursued in future research. Returning to the accounts examined, including the neo-Pyrrhonist attempts to develop a positive epistemology, *it seems that the Pyrrhonist modes are too corrosive to allow for any positive epistemology, whether for science or in general.* Following Pyrrhonist agnosticism, this conclusion is not a claim about whether or not the accounts

⁶⁷ Or perhaps he would just say that they seem to be inferior examples.

have *really* failed, or whether or not a facticity account of scientific knowledge that does not fall prey to the Pyrrhonist modes will not be come about in the future.

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